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The Impact of an Individual Health Insurance Mandate On Hospital and Preventive Care: Evidence from Massachusetts
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ABSTRACT

In April 2006, the state of Massachusetts passed legislation aimed at achieving near universal health insurance coverage. A key provision of this legislation, and of the national legislation passed in March 2010, is an individual mandate to obtain health insurance. In this paper, we use hospital data to examine the impact of this legislation on insurance coverage, utilization patterns, and patient outcomes in Massachusetts. We use a difference-in-difference strategy that compares outcomes in Massachusetts after the reform to outcomes in Massachusetts before the reform and to outcomes in other states. We embed this strategy in an instrumental variable framework to examine the effect of insurance coverage on outcomes. Among the population discharged from the hospital in Massachusetts, the reform decreased uninsurance by 28% relative to its initial level. Increased coverage affected utilization patterns by decreasing length of stay and the number of inpatient admissions originating from the emergency room. We also find evidence that outpatient care reduced hospitalizations for preventable conditions. At the same time we find no evidence that the cost of hospital care increased. The reform affected nearly all age, gender, income, and race categories. We identify some populations for which insurance had the greatest direct impact on outcomes and others for which the impact on outcomes appears to have occurred through spillovers.

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I. Introduction

In April 2006, the state of Massachusetts passed legislation aimed at achieving near universal health insurance coverage. This legislation has been considered by many to be a model for the national health reform legislation passed in March 2010. A key provision of the Massachusetts and national reforms is an individual mandate to obtain health insurance coverage. Although previous researchers have studied the impact of expansions in health insurance coverage, these studies have focused on specific subpopulations - the indigent, children, and the elderly (see e.g. Card, Dobkin, and Maestas (2008); Currie and Gruber (1996); Finkelstein (2007)). The Massachusetts reform gives us a novel opportunity to examine the impact of an expansion in health insurance coverage that achieved near universal health insurance coverage among the entire state population.

In this paper, we are the first to use hospital data to examine the impact of this legislation on insurance coverage, patient outcomes, and utilization patterns in Massachusetts. We use a difference-in-difference strategy that compares Massachusetts after the reform to Massachusetts before the reform and to other states. These estimates provide evidence on the reduced form impact of the reform. We also use an instrumental variable strategy to estimate the impact of coverage on outcomes by combining estimated reduced form impacts on outcomes and utilization with estimated first stage impacts on coverage. Finally, we employ these strategies within demographic subgroups to identify the groups that were most affected by the reform and to test for spillover effects from one subgroup's coverage on another subgroup's health care outcomes. If changes in coverage are not proportional to changes in outcomes across demographic subgroups, then there could be spillover effects from universal coverage.

The first question we address is whether the Massachusetts reform resulted in reductions in uninsurance. There exist a multitude of theoretical models which predict market failures in health insurance that produce less than complete coverage in equilibrium (see, e.g. Arrow (1963); Cutler and Zeckhauser (1998); Pauly (1968); Rothschild and Stiglitz (1976)). Whether the combination of regulatory mechanisms employed by Massachusetts, notably individual and employer mandates, subsidies, and a centralized exchange to purchase plans, overcame these is an empirical question. We consider overall changes in coverage as well as changes in the composition of types of coverage among the entire state population and the population who were hospitalized. One potential impact of expansions in publicly subsidized coverage is to crowd out private insurance (Cutler and Gruber, 1996). The impact of the reform on the composition of coverage allows us to consider crowd out in the population as a whole as well as among those in the inpatient setting. The incidence of crowd out also has implications for policy, particularly if relatively sick patients are more likely to be crowded onto public budgets. On the one hand, this could raise the cost of administering a reform of this type while, on the other hand, it redistributes resources to a relatively sick population and may also reduce adverse selection in the private market.

After estimating changes in the presence and composition of coverage, we turn to the impact of the reform on hospital and preventive care and, to a limited degree, on health. We first consider length of stay. Starting from a simple partial equilibrium model of physician agency where an individual's coverage has a direct impact on his own treatment (see e.g. McGuire (2000)), expansions in insurance coverage could impact length of stay through two primary mechanisms. First, if newly insured individuals demand more medical care because its price is lower, we expect that length of stay will increase. Second, if newly insured individuals have coverage that incorporates mechanisms to manage care (i.e. prospective payment to constrain practice patterns), we expect length of stay to decrease. We also expect length of stay to decrease if doctors substitute inpatient for outpatient care in the absence of coverage. That is, they are willing to release insured patients earlier, trusting that they can access appropriate outpatient follow up care. We test the predictions of this simple model by examining changes in coverage and length of stay overall and within demographic groups.

If we maintain the assumption that there are no general equilibrium impacts of the reform on the intensity of treatment, we expect that we should only see changes in length of stay among demographic groups that experienced changes in coverage through the reform. The sign of the change in length of stay should tell us which of the aforementioned effects dominates. Furthermore, if we impose the exclusion restriction that the reform only affected a group's length of stay through changes in coverage, the ratio of the change in length of stay to the change in coverage yields an instrumental variable estimate of the impact of changes in coverage on changes in length of stay. We report the instrumental variable estimate, but we do not argue for its validity. Instead, we begin from the presumption that heterogeneous treatment effects are possible and that expansions of coverage to individuals other than oneself could have spillover effects.

When there are large changes in insurance coverage, as we expect in Massachusetts, spillover effects are possible through changes in the hospital production function and through changes in insurer bargaining power. Observed changes in the hospital production function could result from physical limits on the number of beds in the hospital, efforts to increase throughput in response to changes in profitability, or changes in care provided when physicians face a more homogeneous pool of patients in terms of coverage (Glied and Zivin (2002)). Changes in insurer bargaining power could also reduce prices or facilitate non-price quantity constraints (i.e. prior authorization) across all groups, even those previously insured, thereby decreasing length of stay.

After considering length of stay, we study measures of quantities of care, prices of care, and operating costs. Our measures include the number of procedures performed for each discharge, the total charges for each discharge, and a hospital-level measure of operating costs (e.g. overhead, salaries, and equipment). Our cost measures include both fixed and variable costs, a relevant metric given the potential for hospitals to respond to the reform by expanding their size or by investing in technology to attract newly insured patients.[1]Combining estimates across these three outcomes, we perform a simple decomposition of the impact of the reform on the price of hospital care, the quantity of care provided, and hospital operating costs.

In addition to changes in the production process within a hospital, we are interested in the impact of insurance coverage on how patients enter the health care system and access preventive care. We first examine changes in the use of the emergency room (ER) as a point of entry for inpatient care. Because hospitals cannot turn patients away, the ER has become a popular way for the uninsured to access health care. ER usage has the potential to impact welfare for a variety of reasons. First, the cost of treating patients in the ER is likely higher than it would be to treat the same patient in another setting. Second, the emergency room is designed to treat acute health events. If the ER is a patient's primary point of care, then he might not receive preventive care that could mitigate future severe and costly health events. Uninsured individuals who access inpatient care after a visit to the emergency room also have barriers to receiving follow up treatment (typically dispensed in an outpatient setting), potentially reducing the efficacy of the inpatient care they receive. To the extent that uninsurance led people to use the ER as a point of entry for

[1]Hospitals facing consumers who are less price elastic (or more quality elastic) increase use of costly services and may also increase use of variable inputs as well as investments in large capital projects in order to attract price-insensitive customers (Dranove and Satterthwaite (1992)). In the extreme, large expansions in coverage might induce a so called "medical arms race," in which hospitals make investments in large capital projects to attract customers and are subsequently able to increase demand to cover these fixed costs. The resulting equilibrium is a socially excessive level of "entry" into capital intensive services (Cutler et al. (2010); Mankiw and Whinston (1986); Robinson and Luft (1985)).

treatment that they otherwise would have sought through another channel, we expect to see a decline in the number of inpatient admissions originating in the ER.

Using a methodology developed by the Agency for Healthcare Research and Quality (AHRQ) we are able to study preventive care in an outpatient setting using inpatient data. We identify inpatient admissions that should not occur in the presence of sufficient preventive care. If the reform facilitated increased preventive care, then we expect a reduction in the number of inpatient admissions meeting these criteria. These measures also indirectly measure health in the form of averted hospitalizations.

Our analysis relies on two main data sets. To examine the impact on coverage in Massachusetts as a whole, we analyze data from the Current Population Survey. To examine coverage among the hospitalized population, health care utilization, and preventive care, we analyze the universe of hospital discharges from a nationally-representative sample of approximately 20% of hospitals in the United States from the Healthcare Cost and Utilization Project (HCUP) National Inpatient Sample (NIS).

We find that the reform increased insurance coverage among the general and inpatient hospital populations. Among the population of hospital discharges in Massachusetts, the reform decreased uninsurance by 28% relative to its initial level. We see some evidence of crowd of out private coverage by subsidized coverage for the hospitalized population but we do not find evidence for crowd out in the general population, suggesting the incidence of crowd out is not uniform. We also find that the reform affected utilization patterns through decreased length of stay and a decrease in the number of inpatient admissions originating from the emergency room. Furthermore, we find evidence of increases in preventive care outside of the hospital setting. Our results indicate that hospital costs did not increase following the expansion in coverage. We also find evidence for declines in the intensity of treatment and weaker evidence for declines in prices paid by private payers. We make several attempts to investigate and address potential selection of healthier or sicker patients into or out of hospitals after the reform. We do not find evidence of a change in the composition of the patient pool large enough to affect the robustness of our findings.

In addition to its relevance for health economics and policy, our work represents a contribution to the public economics literature on the incidence of public spending and coverage mandates. Although there are many studies that examine the incidence of taxation (see Kotlikoff and Summers

(1987) for a review), far fewer studies examine the incidence of public spending (see Costa (1999) for one exception). Using measures of changes in coverage and outcomes across demographic groups, we capture measures of the incidence of the substantial increase in public spending associated with the Massachusetts reform. We find that the reform had an impact on nearly all ages, genders, incomes, and races. For some groups, the impact of the reform stems directly from gains in coverage, but others benefit from the reform through spillovers.

In the next section, we describe the elements of the reform and its implementation, as well as the limited existing research on its impact. In the third section, we describe the data. In the fourth section, we present the difference-in-difference results for the impact of the reform on insurance coverage and hospital and health outcomes. In the fifth section, we present the instrumental variable results and examine heterogeneity in these results by patient demographic characteristics. In the sixth section, we conclude and discuss our continuing work in this area.

II. Description of the Reform

The recent Massachusetts health insurance legislation, known as Chapter 58, included several features, the most salient of which was a mandate for individuals to obtain health insurance coverage or pay a tax penalty. All individuals were required to obtain coverage, with the exception of individuals with religious objections and individuals whose incomes were too high to qualify for state health insurance subsidies but too low for health insurance to be "affordable" as determined by the Massachusetts Health Insurance Connector Authority. For a broad summary of the reform, see McDonough, Rosman, Phelps, and Shannon (2006); for details on the implementation of the reform see The Massachusetts Health Insurance Connector Authority (2008).

The reform also extended free and subsidized health insurance to low income populations in two forms: expansions in the existing Medicaid program (called "MassHealth" in Massachusetts), and the launch of a new program called CommCare. First, as part of the Medicaid expansion, the reform expanded Medicaid eligibility for children to 300% of poverty, and it restored benefits to special populations who had lost coverage during the 2002-2003 fiscal crisis, such as the long-term unemployed and those enrolled in the HIV program. The reform also facilitated outreach efforts to Medicaid eligible individuals and families. Implementation of the elements of the reform was staggered, and Medicaid changes were among the first to take effect. According to one source, "Because enrollment caps were removed from one Medicaid program and income eligibility was raised for two others, tens of thousands of the uninsured were newly enrolled just ten weeks after the law was signed" (Kingsdale (2009), page w591).

Second, the reform extended free and subsidized coverage through a new program called CommCare. CommCare offered free coverage to individuals up to 150% of poverty and three tiers of subsidized coverage up to 300% of poverty. Some funding for the subsidies was financed by the dissolution of existing state uncompensated care pools.[2] To limit crowd-out of federal coverage, individuals with coverage through CHAMPVA, the federal health insurance program for veterans, or Medicare, the federal health insurance program for the elderly, were not allowed to purchase subsidized CommCare plans. CommCare plans were sold through a new state-run health insurance exchange. For the first three years, only four existing Medicaid managed care organizations were allowed to offer plans through CommCare.

In addition, the reform created a new online health insurance marketplace called the Connector, where individuals who did not qualify for free or subsidized coverage could purchase health insurance coverage. Unsubsidized CommChoice plans available through the Connector from several health insurers offered three regulated levels of coverage – bronze, silver, and gold. Young Adult plans with fewer benefits were also made available to individuals age 26 and younger. Individuals were also free to continue purchasing health insurance through their employers or to purchase health insurance directly from insurers.

The reform also implemented changes in the broader health insurance market. It merged the individual and small group health insurance markets. Existing community rating regulations, which required premiums to be set regardless of certain beneficiary characteristics of age and gender, remained in place, though it gave new authority to insurers to price policies based on smoking status. It also required all family plans to cover young adults for at least two years beyond loss of dependent status, up to age 26.

Another important aspect of the reform was an employer mandate that required employers

[2] Addressing costs associated with the reform remains an important policy issue. In 2008, facing a recession, the Massachusetts legislature passed new legislation that scaled back subsidies for low-income legal immigrants. Current policy debates in Massachusetts focus on cost-control - the recent "Special Commission on the Health Care Payment System" proposed a system of "global payments," which would require Federal waivers for Medicaid and Medicare (Bebinger (2009)). No additional health reform legislation has been enacted in Massachusetts at the time of this writing. However, on April 1, 2010, Governor Patrick's administration used existing regulatory authority for the first time to deny premium increases in the individual / small group market.

with more than 10 full time employees to offer health insurance to employees and contribute a certain amount to premiums. The legislation allowed employers to designate the Connector as its "employer-group health benefit plan" for the purposes of federal law. Employer-sponsored coverage through the exchange could combine employer contributions from multiple part-time employers or from spousal employers.

The national health reform legislation passed in March 2010 shares many features of the Massachusetts reform, including an individual mandate to obtain health insurance coverage, new requirements for employers, expansions in subsidized care, state-level health insurance marketplaces modeled on the Massachusetts Connector, and new requirements for insurers to cover dependents to age 26, to name a few. For a summary of the national legislation, see Kaiser Family Foundation (2010). Taken together, the main characteristics of the reform bear strong similarity to those in the Massachusetts reform, and the impact of the Massachusetts reform should offer insight into the likely impact of the national reform.

As Chapter 58 was enacted very recently, there has been very little research on its impact to date. Long (2008) presents results on the preliminary impact of the reform from surveys administered in 2006 and 2007. Yelowitz and Cannon (2010) examine the impact of the reform on coverage using data from the March 2006-2009 Supplements to the Current Population Survey (CPS). They also examine changes in self-reported health status in an effort to capture the effect of the reform on health. Using this measure of health, they find little evidence of health effects. The NIS discharge data allow us to examine utilization and health effects in much greater detail. Long, Stockley, and Yemane (2009) perform an earlier analysis using one fewer year of the same data. Long et al. (2009) and Yelowitz and Cannon (2010) find a decline in uninsurance among the population age 18 to 64 of 6.6 and 6.7 percent respectively. We also rely on the CPS for preliminary analysis.[3] Our main results, however, focus on administrative data from hospitals.

III. Description of the Data

For our main analysis, we focus on a nationally-representative sample of hospital discharges. Hospital discharge data offer several advantages over other forms of data to examine the impact of

[3]Our estimates using the CPS are similar in magnitude to the prior studies, though our sample differs in that we include all individuals under age 65 and at all income levels.

Chapter 58. First, though hospital discharge data offer only limited information on the overall population, they offer a great deal of information on a population of great policy interest – individuals who are sick. This population constitutes the group most vulnerable to changes in coverage due to illness itself and demographics correlated both with health and insurance coverage (i.e. race, income, etc.). Furthermore, inpatient care represents a disproportionate fraction of total health care costs. Second, hospital discharge data allow us to observe the insured as well as the uninsured, regardless of payer, and payer information is likely to be more accurate than it is in survey data. Third, hospital discharge data allow us to examine treatment patterns and some health outcomes in great detail. In addition, relative to the CPS, hospital discharge data allow us to examine changes in medical expenditure, subject to limitations discussed below. One disadvantage of hospital discharge data relative to the CPS is that the underlying sample of individuals in our data could have changed as a result of the reform. We use many techniques to examine selection as an outcome of the reform and to control for selection in the analysis of other outcomes.

Our data are from the Healthcare Cost and Utilization Project (HCUP) Nationwide Inpatient Sample (NIS). Each year of NIS data is a stratified sample of 20 percent of United States community hospitals, designed to be nationally representative.[4] The data contain the universe of all hospital discharges, regardless of payer, for each hospital in the data in each year. Because such a large fraction of hospitals are sampled in each year, and because of stratification, a large fraction of hospitals appear in several years of the data, and we can use hospital identifiers to examine changes within hospitals over time.

We focus on the most recently available NIS data for the years 2004 to 2007. These data include a total of 32,117,859 discharges. One limitation of these data for the analysis of Massachusetts health reform is that only a limited amount of data is available after the April 2006 reform. However, as we demonstrate below, the reform had an impact in this period. Another advantage of these data relative to the annual March Supplement to the CPS is that they allow us to examine the impact of the reform using more frequent time intervals.

In what follows, we define the *After* reform period to include 2007 Q3 and later. The *After*

[4] "Community hospitals" are defined by the American Hospital Association as "all non-Federal, short-term, general, and other specialty hospitals, excluding hospital units of institutions." (HCUP Nationwide Inpatient Sample (2007)) The sample is stratified by geographic region – Northeast vs. Midwest vs. West vs. South; control – government vs. private not-for-profit vs. private investor-owned; location – urban vs. rural; teaching status – teaching vs. non-teaching; and bed size – small vs. medium vs. large. Implicit stratification variables include state and three-digit zip code.

period represents the time after July 1, 2007, when one of the most salient features of the reform, the individual health insurance mandate, took effect. Because some reforms such as Medicaid expansions were implemented immediately after the reform, but other reforms were staggered, we do not want to include the period immediately following the reform in the *After* or the *Before* period. Accordingly, we define the *Before* period to include 2004 Q1 through 2006 Q2. We denote the *During* period as the year from 2006 Q3 through 2007 Q2, and we use this period to analyze the immediate impact of the reform before the individual mandate took effect.

One of the main limitations of the NIS data for our purpose is that Massachusetts did not provide Q4 data to the NIS in 2006 or 2007.[5] To be conservative, we define the *After* period for Massachusetts to include only 2007 Q3, the fifth quarter after the reform. Since our *After* period only includes three months, we take precautions to account for seasonal trends in hospital utilization.

In total, from 2004-2007, the data cover 40 states – Alabama, Alaska, Delaware, Idaho, Louisiana, Mississippi, Montana, North Dakota, New Mexico and Pennsylvania are not available in any year because they did not provide data to the NIS. The data include the universe of discharges from a total of 2,697 unique hospitals, with 44 in Massachusetts.[6]

The unit of observation in the data and in our main analysis is the hospital discharge. To account for stratification, we use discharge weights in all summary statistics and regressions. The individual claims that make up the discharges are not available. Although it would be interesting to examine the reform controlling for individual fixed effects, individual identifiers are not available.

IV. Difference-in-Difference Empirical Results

A. Impact on Uninsurance

A.1. Impact on Insurance Coverage in the Overall and Inpatient Hospital Populations

We begin by considering the issue that was the primary motivation for the Massachusetts reform – the expansion of health insurance coverage. Coverage is only an intermediate input into risk

[5] To address the missing data, the NIS relabeled data from the first three quarters of the year in 2006 and 2007 MA as Q4 data, but we recover the original quarter. We also undo related adjustments to charges.

[6] Only one hospital is missing all primary payer data. With the exception of one Massachusetts hospital, if a hospital is present in MA in 2006 or 2007, it is present in all of the first three quarters of that year. Outside of Massachusetts, only 51 hospitals are present in a given year but are not present for all four quarters. All other states and hospitals in the sample each year are present in the data for the entire year.

protection, health care and health. However, before estimating the reduced form impact of the reform on health and health care outcomes, we want to first consider whether the reform actually expanded coverage and, if so, where the effect was largest.

Before focusing on inpatient hospitalizations, we place this population in the context of the general population using data from the 2004 to 2009 March Supplements to the Current Population Survey (CPS). In most of our results, we focus on the nonelderly population because the reform was geared toward the nonelderly population (elderly with coverage through Medicare were explicitly excluded from purchasing subsidized CommCare plans, but they were eligible for Medicaid expansions if they met the income eligibility criteria).[7] Though we focus on the nonelderly, we present a breakdown of results by age in the NIS in Tables 6 and 7.

Figure 1 depicts trends in total insurance coverage of all types among nonelderly in the CPS. The upper line shows trends in coverage in Massachusetts, and the lower line shows trends in coverage in all other states. From the upper line, it is apparent that Massachusetts started with a higher baseline level of coverage than the average among other states. The average level of coverage among the nonelderly in Massachusetts prior to the reform (2004-2006 CPS) was 88.2%.[8] This increased to a mean coverage level of 93.8% in the 2008-2009 CPS. In contrast, the remainder of the country had relative stable rates of nonelderly coverage: 82.7% pre-reform and 82.5% post-reform. For the entire population, including those over 65, coverage in Massachusetts went from 89.5% to 94.5% for the same periods while the remainder of the country saw a small decline from 84.6% insured pre-reform to 84.4% insured post-reform.

The initial coverage level in Massachusetts was clearly higher than the national average, though it was not a particular outlier. Using data from the 2004, 2005 and 2006 CPS, we rank states in terms of insurance coverage. Massachusetts has the seventh highest level of coverage among the

[7] Because the reform was geared toward the nonelderly, we considered using the elderly as an additional control group in our difference-in-difference estimates. However, we did not pursue this identification strategy for three reasons: first, the elderly were eligible for some elements of the reform; second, the elderly are less healthy overall and suffer from different types of health shocks than the younger individuals of interest to us; and third, we find some increases in coverage for the elderly. Although many assume that the elderly are universally covered through Medicare, some estimates suggest that 4.5% or more of the elderly population are not eligible for full federally subsidized coverage through Medicare Part A, so coverage increases are possible in this population (Birnbaum and Patchias (2008), Gray et al. (2006)).

[8] We follow the Census Bureau in defining types of coverage and uninsurance. These definitions and the associated code to implement them are available from http://www.census.gov/hhes/www/hlthins/hlthinsvar.html. For individuals who report having both Medicaid and Medicare ("dual eligibles") we code Medicaid as their primary insurance type. We make the additional assumption that individuals who are covered by private health insurance but not by an employer-sponsored plan are in the private market unrelated to employment.

nonelderly in the US. It is one of 17 states with 88 percent or higher share of the population insured, and its initial coverage rate was only 1.7 percentage points higher than the 86.5 percent coverage rate in the median state.

Appendix Table 1 formalizes these means with difference-in-difference regression results from the CPS. These results suggest that the Massachusetts reform was successful in expanding health insurance coverage in the population as a whole. The estimated reduction in nonelderly uninsurance of 5.7 percentage points represents a 48% reduction relative to the pre-reform rate of nonelderly uninsurance in Massachusetts.

A.2. Regression Results on the Impact on Uninsurance

Using the NIS data, we begin by estimating a simple difference-in-difference specification:

$$Y_{dht} = \alpha + \beta(MA * After)_{ht} + \gamma(MA * During)_{ht} + \rho MA_h + \psi After_t + \varphi During_t + \varepsilon_{dht} \quad (1)$$

where Y is a binary variable that indicates no insurance coverage for hospital discharge d in hospital h at time t. The coefficient of interest, β, gives the impact of the reform – the change in coverage after the reform relative to before the reform in Massachusetts relative to other states. Analogously, γ gives the change in coverage during the reform relative to before the reform in Massachusetts relative to other states. The identification assumption is that there were no factors outside of the reform that differentially affected Massachusetts relative to other states after the reform. We estimate this specification using a linear probability model. Because our specifications are saturated in the independent variables, our coefficients are identical to average marginal effects from a probit model. Under each coefficient, we report asymptotic 95% confidence intervals, clustered to allow for arbitrary correlations between observations within a state. Following Bertrand, Duflo, and Mullainathan (2004), we also report 95% confidence intervals obtained by block bootstrap by state.[9]

The first column of Table 1 shows the results from the base specification in equation (1). The estimated coefficient estimate on $MA * After$ indicates a 2.86 percentage point reduction in uninsurance after the reform relative to before the reform, relative to other states. This represents

[9] See Appendix 1 for more detail on the implementation of the block bootstrapped confidence intervals. In practice, the confidence intervals obtained through both methods are very similar. To conserve space, we do not report the block bootstrapped standard errors in some tables.

a decline of almost 44% relative to baseline (2.86/6.43). Baseline uninsurance is much lower in the hospitalized population than it is in the CPS, suggesting *ex ante* adverse selection among the hospitalized population

We next turn to our preferred specification, which accounts for seasonality with time fixed effects by quarter and accounts for sampling variation with hospital fixed effects. We estimate the following equation:

$$\begin{aligned} Y_{dht} &= \alpha + \beta(MA * After)_{ht} + \gamma(MA * During)_{ht} + \\ &\sum_h \rho_h(Hosp = h)_h + \sum_t \psi_t YearQuarter_t + [X'_{dht}\delta] + \varepsilon_{dht} \end{aligned} \tag{2}$$

Identification from this specification comes from comparing hospitals to themselves over time in Massachusetts compared to other states, after flexibly allowing for trends in coverage across time. However, since hospitals are observed in an unbalanced panel, some identification comes from comparing hospitals to each other within Massachusetts or within the other states.[10] Column 2 presents results for this specification. The effect of the reform is a 1.82 percentage point reduction in uninsurance. Both sets of confidence intervals show that the difference-in-difference impact of the reform on uninsurance is statistically significant at the 1% level. The estimated impact of Chapter 58 also represents an economically significant reduction in uninsured discharges of roughly 28% (1.82/6.43). All future specifications follow this preferred specification. Since this specification obscures the main effects of *MA* and *After*, in later tables, we also report mean coverage rates identical to those obtained in the previous specification to aid in the interpretation of the magnitude of our estimates.

Our preferred specification, which includes time and hospital fixed effects, is the most conservative model in our view. We also estimate models that incorporate a vector X of patient demographics and other risk adjustment variables. In column 3, we include demographic characteristics – the patient's age in year at admission, the patient's gender, the income quartile of the residents of the patient's zip code, and the patient's race. We do not control for these demographic characteristics in our main specifications because we are interested in measuring the impact of the

[10]Restricting the sample to the balanced panel of the 52 hospitals that are in the sample in all possible quarters (2004 Q1 to 2007 Q4, with 2006 Q4 and 2007 Q4 not required for MA) severely restricts power, and it possibly makes the sample less representative, so we do not make this restriction in our main specifications. However, in the first specification in the third panel of Appendix Table 7, we present results for the population of all ages using only the balanced panel, and the results are not statistically different from the main results.

reform as broadly as possible. To the extent that the reform changed the composition of the sample of inpatient discharges based on these observable characteristics, we would obscure this effect by controlling for observable patient characteristics. With the inclusion of demographic characteristics, the estimated impact of the reform on uninsurance is almost identical (a 1.85 percentage point impact), and is not significantly different from the coefficient estimate in column 2. An important implication of this finding is that it appears the reform had little effect on the observable case mix of inpatient admissions. In Section B1 and Appendix 2, we examine changes in case mix more formally by examining changes in case mix as an outcome variable and by including several other types of risk adjustment variables in our specification of X. We find that though there is some evidence of selection, it is not large enough to alter the robustness of our findings.

In addition to the specifications we present here, we consider a number of robustness checks to investigate the estimated impact and external validity of our results. We find that the conclusions presented in Table 1 are robust to a variety of alternative control groups and do not appear to be driven by unobserved factors that are a unique to Massachusetts. For brevity, we present and discuss these results in Appendix 3. In Figure 2, we present quarterly trends for each of our outcome variables of interest for Massachusetts and the remainder of the country. Each line and the associated confidence interval are the coefficient estimates for each quarter for Massachusetts and non-Massachusetts states in a regression that includes hospital fixed effects. The omitted category for each is the first quarter of 2004, which we set equal to 0. Given our short time period, we are particularly concerned about pre-trends in Massachusetts relative to controls. While the plots show slight variation, none of our outcomes of interest appear to have strong pre-reform trends in Massachusetts relative to control states that might explain our findings. One noticeable outlier in Figure 2 is the dramatic decline in ER admissions in control states in Q2 2008. We note, however, that because we do not have data from Massachusetts in this period and our specifications include quarterly fixed effects, these data do not identify any of our results.

A.3. Effects on the Composition of Insurance Coverage among Hospital Discharges

Until now, we have not differentiated among different types of insurance coverage. In this section, we investigate the effect of the Massachusetts reform on the composition of health insurance coverage. We divide health insurance coverage (or lack thereof) into five mutually exclusive types – *Uninsured*, Medicaid, *Private*, Medicare, and *Other*. CommCare plans and other government plans such as Workers' Compensation and CHAMPUS (but not Medicaid and Medicare) are included in *Other*. We estimate the preferred specification separately for each coverage type and report the results in columns 1 through 5 of Table 2. Because these represent mutually exclusive types of coverage, the coefficients sum to zero across the first five columns. We focus on results for the nonelderly here, and we report results for the elderly in Table 6.

We see from the difference-in-difference results in Column 2 of Table 2 that among the nonelderly, the expansion in Medicaid coverage was larger than the overall reduction in uninsurance. Medicaid coverage expanded by 4.70 percentage points, and uninsurance decreased by 1.82 percentage points. Consistent with the initial roll-out of the expansion of Medicaid, the coefficient on *MA*During* suggests large fraction of impact of the Medicaid expansion was realized in the year immediately following the passage of the legislation. It appears that at least some of the Medicaid expansion crowed out private coverage, which decreased by 3.54 percentage points. All of these effects are statistically significant at the 1% level.

We also find that Medicare coverage did not exhibit a statistically significant change in the nonelderly or elderly populations. Since the reform was not geared to the elderly, this result is reassuring that we are not picking up unobserved changes in Massachusetts relative to other states. *Other* coverage, the category including CommCare, increased by a statistically significant 0.63 percentage points. However, when we restrict the dependent variable to include only CommCare in specification 6, it appears that there must have been some crowd out of *Other* coverage because CommCare itself increased by 0.92 percentage points – more than the increase across all types of *Other* coverage. By definition, CommCare coverage is zero outside of Massachusetts and before the reform. We would have liked to have used Massachusetts residents that were prohibited from obtaining coverage through CommCare because of eligibility for federal coverage as a control group, but the Massachusetts data in the NIS do not include enough detail (e.g. eligibility for coverage through CHAMPUS) for us to do so.

An important issue to consider is whether the reform induced changes in how coverage was coded. In the last specification, we report the results from a specification that includes all discharges in the sample and includes a dependent variable for the probability that a discharge did not include coverage information. Reassuringly, the probability that coverage information was not included in the discharge record did not change differentially after the reform in Massachusetts relative to other states. The estimated coefficient is small, and it is not significant at the 5% level.

For comparison, in Appendix Table 1, we report difference-in-difference results by coverage type in the CPS. The coverage categories reported by the CPS do not map exactly to those used by the NIS. In the CPS, insurance that is coded as private coverage in the NIS is divided into employer sponsored coverage and private coverage not related to employment. Furthermore, to deal with the new types of plans available in Massachusetts, CommCare and CommChoice, the Census Bureau decided to code all of these plans as "Medicaid."[11] Thus the estimated impact on Medicaid is actually the combined effect of expansions in traditional Medicaid with increases in CommCare and CommChoice.[12]

Medicaid expansions are larger among the hospital discharge population than they are in the CPS. This is not surprising because hospitals often retroactively cover Medicaid-eligible individuals who had not signed up for coverage. Furthermore, the discharge population seems to disproportionately represent poor individuals, and these individuals could have multiple discharges.[13] Comparing changes in types of coverage in the NIS to changes in types of coverage in the CPS, we find that crowd out of private coverage only occurred among the hospitalized population. In Appendix Table 1 the magnitudes of the *MA*After* coefficients are 0.0345 and 0.0351 for ESHI and Medicaid respectively. That is, both employer-sponsored and Medicaid, CommCare or CommChoice coverage increased following the reform, and those increases were roughly equivalent to the total decline in uninsurance (5.7 percentage points). The only crowding out in Appendix Table 1 seems to be of non-group private insurance, though this effect is relatively small at 0.86 percentage points. Combining coefficients for ESHI and private insurance unrelated to employment gives us a predicted increase in private coverage (as it is coded in the NIS) of 2.59 percentage points. This contrasts strongly with the 3.54 percentage point decrease in private coverage that we observe in the NIS.

[11] We thank the Census Bureau staff for their rapid and thorough response to the many calls we made to confirm this decision on categorizing the new types of plans.

[12] In unreported regressions, we divide the sample by income. The results suggest an increase in "Medicaid" coverage for people above 300% of the FPL of 0.6 percentage points. Thus, the bulk of the effect on Medicaid reflects some form of publicly subsidized coverage and not unsubsidized CommChoice plans coded as Medicaid.

[13] Another factor that affects the comparison between CPS Medicaid changes and NIS Medicaid changes is that CPS Medicaid changes should overstate changes in Medicaid eligibility because they also include increases in take up that occurred to comply with the mandate. Medicaid changes in the NIS should only reflect changes in eligibility because hospitals, in order to maximize reimbursement, facilitate take-up for Medicaid-eligibles before and after the reform. That said, changes in Medicaid take-up reflected in the CPS could have real effects on utilization if the reform encouraged previously eligible individuals to take up Medicaid, potentially making them more likely to consume outpatient care, instead of retroactively taking up Medicaid after a hospitalization.

We also note that since the CPS coded CommChoice plans as Medicaid, this effect likely masks some transition from the private non-group market to purchases made through the Connector of equivalent CommChoice plans, which would appear to be crowd out even though it is a transition from one form of private coverage to another.

B. Impacts on Health Care Provision

B.1. Impact on Utilization

In Table 3, we investigate the impact of the reform on outcomes beyond insurance coverage. We estimate equation (2) with a set of dependent variables that capture changes the way in which health care is delivered and consumed. We first examine the extensive margin decision to consume care by considering the total number of discharges as well as changes in the health of the overall patient population. We then turn to the impact of the reform on intensive margin choices of health care consumption by studying length of stay.

One potential impact of the reform could be to increase the use of inpatient hospital services. Whether more people sought care after being insured is relevant in understanding the impact of the policy but also presents an important empirical hurdle to estimating the impact of the reform on subsequent measures of care delivered. If the number of patients seeking care after the reform increased and the marginal patients were healthier, changes in treatment intensity could reflect this. We investigate this possibility in two ways: first, we examine changes in the number of discharges at the hospital level; second, we control for observable changes in the health of the patient pool in our estimates for the impact of the reform on intensive margin outcomes.

In Table 4, we investigate selection into hospitals by estimating a series of specifications with the number of discharges at the hospital-quarter level as the dependent variable. In column 1 of Table 4, which includes hospital and quarter fixed effects to mitigate the impact of changes in sample composition, the coefficient on *MA*After* in column 1 indicates that, if anything, total discharges by hospital decreased after the reform relative to before the reform in Massachusetts relative to other states. This effect is not statistically significant, and the 95% confidence interval rules out increases greater than 2.0% or decreases greater than 4.7% of the initial number of discharges in Massachusetts. Columns 2 and 3 also show no statistically significant change in total elderly or nonelderly discharges. These findings suggest that any change in the composition of patients would have to have occurred through substitution as the total number of discharges remained unchanged. We return to this question below by re-estimating all of our intensive margin regressions including detailed covariates on patient severity. These results confirm our general finding from analysis of total discharges; the estimated impact of the reform is qualitatively unchanged and, if anything, more pronounced after including covariates suggesting the patient population did not expand and was not relatively healthier.

Moving beyond the question of the extensive margin care delivery, we turn to the intensity of services provided conditional on receiving care. The most direct measure of this is the impact of the reform on length of stay. As discussed earlier, we expect length of stay to increase in response to increased coverage if newly insured individuals (or their physician agents) demand more treatment, or we expect length of stay to decrease in response to increased coverage if newly insured individuals are covered by insurers who are better able to impact care through either quantity restrictions or prices. Regardless of which effect dominates, we expect variance in length of stay to decrease as the population becomes more homogenous with respect to insurance coverage.

The results in the first columns of Table 3 show that length of stay decreased by 0.057 days on a base of 5.42 days in the specification in levels – a decline of approximately 1 percent. Estimates in column 2 show a 0.2 percent decline in the specification in logs. These two results are slightly different because of skewness in length of stay, but they both indicate a statistically significant reduction, though the log specification is only significant at the 10 percent level using the block bootstrap confidence intervals. Because taking logs increases the weight on shorter stays, this difference suggests that the reform had a larger impact on longer stays. In specifications 17 and 18 of Table 4, we estimate models with the standard deviation and coefficient of variation in length of stay at the hospital level as the dependent variable. The results of both regressions also show statistically significant reductions, suggesting that variation in length of stay within a hospital also declined following the reform. The fact that the effect is evident in the coefficient of variation, which is invariant to scaling, shows that this decline is not merely an artifact of a reduction in average length of stay; rather, variation relative to the new mean declined. While we cannot observe the precise mechanism, this convergence is consistent with changes in length of stay induced by convergence in the types of coverage a physician sees.

Despite our finding that the total number of discharges was unchanged by the reform, we also re-estimate our model of length of stay incorporating detailed controls for patient severity. To control for observable changes in the health of the patient pool, we use seven sets of risk adjustment variables: demographic characteristics, the number of diagnoses on the discharge record, individual components of the Charlson Score measure of comorbidities, AHRQ comorbidity measures, disease staging, All-Patient Refined (APR)-DRGs, and All-Payer Severity-adjusted (APS)-DRGs. We discuss these measures in depth in Appendix 3A. These measures are a valid means to control for selection if and only if unobservable changes in health are correlated with the changes in health that we observe. We interpret our risk-adjusted specifications assuming that this untestable condition holds, with the caveat that if it does not hold, we cannot interpret our results without a model of selection. In the last row of all specifications in Table 3, we report results controlling for risk adjustment variables. The estimated decreases in length of stay and log length of stay are at least twice as pronounced in the specifications that include risk adjusters. We interpret this to indicate that holding the makeup of the patient pool constant, length of stay declined. The comparison between the baseline and risk-adjusted results suggests that, if anything, patients requiring longer length of stays selected into the patient pool in post-reform Massachusetts. In Appendix 3B, we investigate changes in the patient pool directly, and we do not see any statistically significant change in predicted length of stay. The impact on predicted charges is statistically significant but the estimated impact is an order of magnitude smaller than our results, again suggesting that differential selection does not drive our findings.

In Column 4 of Table 4, we further investigate the possibility that sampling variation led to an observed larger size of hospitals after the reform relative to before the reform using a separate measure of inherent hospital size as the dependent variable. In this measure, Hospital Bedsize, as collected by the American Hospital Association (AHA) annual survey of hospitals, hospitals were categorized in small, medium, or large using categories for the number of hospital beds that did not change during the sample period. We recode the data so that small hospitals have a value of zero, medium hospitals have a value of 0.5, and large hospitals have a value of one. The coefficient estimates from the reported regression, which includes hospital and quarter fixed effects, indicates that hospital size increased by 2.5 percent of the possible range from 0 to 1.[14]This

[14]We also estimate the same model using a continuous measure of general adult beds from the full sample of hospitals in the AHA database from 2004-2007. Our differences-in-differences coefficient estimate for the post reform impact is 2.56 (s.e.=3.10). Compared to the hospital mean in Massachusetts in 2004-05 of 84, this constitutes a slightly larger change of 3.03%, though the effect is not statistically different from zero.

estimate, which is statistically significant, indicates that the hospitals that identify impacts on length of stay increased in size, perhaps to accommodate the increases in insurance coverage, even though we observe no statistically significant change in total discharges. The discrepancy between the difference-in-difference mean estimate of 8 percent from the means at the bottom of the table and the difference-in-difference regression estimate of 2.5 percent estimates suggests that larger hospitals are differentially selected into the sample after the reform in Massachusetts, meriting our use of hospital fixed effects in our preferred specification. Specification 5, which includes saturated controls for hospital size, still indicates no statistically significant increase in discharges. Overall, it is possible that patients were healthier in ways unobservable to us after the reform, but this effect is not evident in the number of discharges.

It is also possible that supply-side constraints limited any increase in total discharges that would have been driven by expanded coverage. Capacity is, however, endogenous and changed with the reform, as we saw in the model with number of beds as the dependent variable. In a simple queuing model of hospital demand and bed size, Joskow (1983) shows that, under general assumptions, the probability that a patient is turned away from a hospital is endogenously determined by the hospital when it selects a reserve ratio (the difference between the total number of beds and the average daily census (ADC) relative to the standard deviation in arrival rates).[15] Because beds are a fixed cost, capacity and utilization are a source of scale economies for hospitals. If hospitals seek to improve efficiency in the face of increased pricing pressure from insurers after the reform, we would expect to see improved throughput in an effort to lower cost. One means of accomplishing this is to make smaller increases in capacity relative to demand following the reform.

Using the result in Table 4, we can provide some insight into whether changes in length of stay seem to be related to capacity by comparing the additional capacity that could result from the change in length of stay relative to the magnitude of the change in discharges. Because hospitals care about total changes in capacity, not just among the nonelderly, we use estimates for the change in length of stay among the entire population reported in column 1 of Table 7. The new, lower average length of stay is thus 5.88-.12=5.76 days. This would make room for an extra (.012*5,616)/(5.88-0.12)=117 discharges. An extra 117 discharges exceeds the upper bound of the 95% confidence interval for the estimated change in total discharges (110). Thus, decreased length of stay could

[15]Joskow (1983) formally model the stochastic arrival rate as a Poisson process which yields a standard deviation equal to $\sqrt{ADC}$.

have been a response to increased supply side constraints, although this explanation would be more convincing if the point estimate for the change in the number of discharges were positive and closer to the supply-side constraint of 117.[16]

B.2. Impact on Prevention, Quality of Care, and Safety

One potentially important role for insurance is to reduce the cost of obtaining preventive care that can improve health and/or reduce future inpatient expenditures. In this case, moral hazard can be dynamically efficient by increasing up front care that results in reduced cost later. One manifestation of a lack of coverage that has received substantial attention is the use of the emergency room (ER) as a provider of last resort. If people do not have a regular point of access to the health care system and, instead, go to the emergency room only when they become sufficiently sick, such behavior can lead them to forego preventive care and, potentially, increase the cost of future treatment. In addition, emergency room care could be ceteris paribus more expensive to provide than primary care because of the cost of operating an ER relative to other outpatient settings. Although we do not observe all emergency room discharges, we can examine inpatient admissions from the emergency room as a rough measure of emergency room usage. A decreases in admissions from the ER after the reform is evidence that a subset of the population that previously accessed inpatient care through the emergency room accessed inpatient care through a traditional primary care channel or avoided inpatient care entirely (perhaps by obtaining outpatient care).

In specification 3 of Table 3, we examine the impact of the reform on discharges for which the emergency room was the source of admission. We see that the reform resulted in a 3.2 percentage point reduction in the fraction of admissions from the emergency room, relative to an initial mean in Massachusetts of 38.6 percent, suggesting that the reform did impact patient utilization patterns substantially. The risk-adjusted estimate reported in the bottom row of specification 3 is very similar. As a further specification check, we decompose the effect by zip code income quartile in section V. To the extent that income is a proxy for ex ante coverage levels, we expect larger effects of the reform on ER usage among relatively poorer populations. The results in section V support this claim. We find that the reduction in emergency admissions was particularly pronounced among

[16] Although we do not find much evidence for capacity constraints in the inpatient setting, there is anecdotal evidence for capacity constraints in the outpatient primary care setting. Investigating constraints in that setting is beyond the scope of this paper.

people from zip codes in the lowest income quartile. The coefficient estimate suggests an 8.4 percent reduction (significant at the 1 percent level) in inpatient emissions from the emergency room. The effect in the highest income quartile, on the other hand, is not statistically significantly different from zero (coefficient estimate of -.0105). Taken together, these results suggest that the reform did reduce use of the ER as a point of service for care that resulted in inpatient admissions and that this effect was driven by expanded coverage, particularly among lower income populations.

In addition to the use of the ER, we are interested in directly measuring the preventive care effects of the Massachusetts reform. To investigate the impact of the reform on prevention, quality of care, and safety, we use the four sets of measures developed by the Agency for Healthcare Research and Quality (AHRQ): prevention quality indicators (PQIs), inpatient quality indicators (IQIs), patient safety indicators (PSIs), and pediatric quality indicators (PDIs).[17] See Appendix 4 for more details on these measures. Each set of quality indicators includes several outcomes developed to measure quality. Since the hospital's production function is complex, it is possible for the reform to have improved some quality measures and negatively impacted others. In general, we expect widespread health insurance to increase prevention quality, but the impact on the other measures is ambiguous.

We turn first to the prevention quality measures. These measures were developed as a means to measure the quality of outpatient care using inpatient data, which are more readily available. The appearance of certain preventable conditions in the inpatient setting, such as short term complications from diabetes, or a urinary tract infection, is evidence that adequate outpatient care was not obtained. All of the prevention quality measures are indicator variables that indicate the presence of a diagnosis that should not be observed in inpatient data if adequate outpatient care was obtained.[18] Because our post-reform period is short, we are concerned that we might not expect to see any impact of prevention on inpatient admissions. However, validating these measures with physicians suggest that the existence of a PQI admissions is likely due to short term

[17] We thank Carlos Dobkin for suggesting the use of these indicators.

[18] The calculation of each one of these quality indicators is often based on a complicated algorithm because the diagnosis first needs to be identified, and then specific discharges must be excluded based on secondary diagnoses and risk factors that mitigate the potential for prevention. Each quality measure includes a numerator – a specific condition, as well as a denominator – the population at risk for this specific condition – sometimes the entire population in a geographic area. The presence of the numerator and denominator allows a researcher to calculate rates with data for a single hospital. Since our research design compares hospitals to each other and to themselves over time, we do not calculate any of the measures as rates. Instead, we use a dummy variable dependent variable to indicate inclusion in the numerator - the presence of a quality measure on a specific discharge. For each quality measure, we restrict the sample to discharges included in the denominator.

management of disease in an outpatient setting (e.g. cleaning and treating diabetic foot ulcers to avoid amputations due to gangrene), that we expect would be manifest within the post-reform period.[19]We run our difference-and-difference estimator separately for each quality measure using the binary numerator as the outcome variable, and the denominator to select the sample.

Table 5 presents regression results for each of the prevention quality indicators. Each regression is a separate row of the table. In the first row, the outcome is the "Overall PQI" measure suggested by AHRQ – a dummy variable that indicates the presence of any of the prevention quality indicators on a specific discharge.[20] As shown, there is a statistically significant decrease in this measure of approximately 1.7%. One advantage of examining this measure relative to the individual component measures is that doing so mitigates concerns about multiple hypothesis testing. The following rows show that of the 13 individual PQI measures, 6 exhibit a statistically significant decrease, 4 exhibit no statistically significant change, and 3 exhibit a statistically significant increase (though we note that for two of these the coefficient is only statistically significant at the 10% level). Though we have not corrected our confidence intervals for the testing of multiple hypotheses, it appears that the rate of statistical significance for these outcomes is much larger than what would occur by chance. Taken together, these results suggest that the coverage expansion in Massachusetts significantly reduced preventable inpatient admissions.

Appendix Tables 3 and 4 report the results for the Inpatient Quality indicators, the Patient Safety Indicators, and the Pediatric Quality indicators. As discussed above, we do not have a strong prior about the impact of coverage expansions on these indicators. Although there were decreases in some measures and increases in others, no general pattern is visible. Since many children were already covered by Medicaid before the reform, it seems plausible that gains in pediatric quality had already been realized before the reform. Indeed, Dafny and Gruber (2005) examine the effect of Medicaid expansions on pediatric hospitalizations, and their results are broadly consistent with the results that we find for the general population. They use a measure of "avoidable" hospitalizations that seems to have been a precursor for the AHRQ measures. They find that pediatric length of stay decreased (as do we among the nonelderly), pediatric admissions increased (we find no change among the nonelderly), and avoidable pediatric hospitalizations did not increase as much

[19]We thank Katrina Abuabara for discussing each of these PQI measures and the associated treatment regime and potential for inpatient admission.

[20]PQI 02 is excluded from this measure, presumably because it has a different denominator.

as unavoidable pediatric hospitalizations (we find a decrease in preventable hospitalizations among the nonelderly).

B.3. Impact on Hospital Costs

In this section, we investigate the impact of the reform on hospital costs. In the presence of moral hazard or income effects we expect that the large coverage expansion in Massachusetts would lead the newly insured to seek additional care and, conditional on use, more expensive care (e.g. Pauly (1968); Manning, et al. (1987); Kowalski (2009)). We have addressed this possibility to some degree in considering discharges and length of stay. Measures of charges and costs, however, capture both changes in quantity of care and the intensity with which care is delivered. The impact on cost, as we discuss above, depends on the relative changes in incentives facing hospitals and physicians in treatment and investment decisions. Consequently, increases in health insurance coverage among a hospitalized population could either raise or lower cost. Whether the moral hazard and income effects of insurance dominate changes in care management and the bargaining position of insurers vis a vis hospitals is an empirical question.

Before turning to direct measures of cost, we begin by investigating the impact on measures of hospital prices and the quantity of care provided – measures of the impact of the reform on hospital-insurer bargaining. To provide some insight into the impact on prices, we estimate the impact on hospital charges for each discharge. Hospital charges are effectively the "list price" of a given diagnosis related group (DRG) – a grouping system Medicare uses to identify a particular procedure or treatment – scaled by the quantity of services provided. Hospitals and insurers establish the actual prices paid for services delivered based on a negotiated discount from the "list price." If there are N insurers each with a discount of d_n, which multiplies the list price $\overline{P}$, for each homogenous unit of care q, then we can express total charges C as:

$$C = q * \overline{P} = q * \left(\frac{1}{n} \sum_{n=1}^{N} d_n \overline{P} + \varepsilon \right) \tag{3}$$

where ε is an error term capturing unobserved components of the list price that are unrelated to actual payments. The error term accounts for the fact that, in the absence of any party paying the full list price, there is no requirement that the list price be tied directly to the actual discounted prices. It is clear from (3) that list prices need not bear any relationship to the actual amount paid if ε is large. Whether this is the case is an empirical question and one that we cannot answer directly in our data. However, in California – a state in which hospital level discount information is available – the evidence suggests that there is a positive correlation between the average price paid to a hospital $\left(\frac{1}{n}\sum_{n=1}^{N} d_n\overline{P}\right)$ and the reported list price $\overline{P}$.[21] We thus assume that changes in charges that cannot be explained by changes in quantities alone capture the impact of the reform on a rough measure of prices.[22] We note that this assumption is necessary to identify price effects separately from quantity effects, but we cannot test this measure directly in our data. If this relationship does not hold, changes in total charges only reflect changes in quantities. However, our results would still be relevant in interpreting the impact of the reform on insurer bargaining power and pricing to the extent that quantities are a function of prices.

Column 4 of Table 3 presents difference-in-difference estimates for the impact of the reform on the total number of procedures provided for each person discharged. Comparing the coefficient estimate of -0.057 to the pre-reform mean of 1.62 suggests that the reform reduced the total number of procedures per discharge by 3.5%. Incorporating patient risk adjusters changes the coefficient only slightly, suggesting this is not an artifact of a change in the composition of the patient pool. In column 5 of Table 3, the difference-in-difference estimate shows that total charges per discharge decreased by $1,985 per discharge, roughly a 13 percent reduction relative to initial mean total charges per Massachusetts discharge of $15,862. Column 6, which accounts for skewness in total charges by examining the logarithm of charges, indicates a smaller 5.6 percent reduction, suggesting that decreases in charges were the greatest for discharges with the highest initial charges.

From equation (3) we see that the observed decrease in total charges can reflect decreases in quantities (the number of procedures done), decreases in prices, or both. They can also reflect substitution toward less expensive procedures, which we acknowledge as an important caveat as we proceed with our simple decomposition. Comparing our estimated decline in total procedures to our estimated decline in total charges, we can make a rough assessment of the relative impact of prices and quantities in the decline in total charges. In percentage terms, the estimated impact of the reform on procedures (3.5%) is lower than the estimated impact of the reform on either

[21] The correlation at the hospital level is .93. We thank Kate Ho for estimating this relationship using data from the state of California in 2003.

[22] This approach also follows Anderson et al. (2003) who study OECD country level expenditures and quantities. They find evidence that the majority of differences between the US and other OECD members in health expenditures can be explained by price differences and not by differences in the quantity of care delivered.

the levels or log of total charges (12.5% and 5.6%, respectively). This suggests that at least some portion of the total reduction in charges reflects a decline in prices that we would expect if insurers gained market power with the reform. Furthermore, because quantities are a function of price (particularly in a setting like health care where suppliers may have some impact on demand), any observed impact on quantities is an upper bound on the impact of non-price rationing. The finding that enhanced insurer market power is manifest in prices is consistent with prior work studying the impact of managed care (Cutler et al. (1999)).

To measure hospital costs beyond changes in quantities and charges, we obtained hospital level all-payer cost to charge ratios. Hospitals are required to report these ratios to Medicare on an annual basis. The numerator of the ratio represents annual total costs of operating the hospital such as overhead costs, salaries, and equipment.[23] The denominator of this ratio represents annual total charges across all payers, which we observe disaggregated by discharge in the NIS. With our information on total charges from the NIS, we can get an accurate measure of total costs at the hospital level. We also measure total costs at the discharge ratio by deflating total charges by the cost to charge ratio, which has been done by other papers in the economics literature (see, for example, Almond, Doyle, Kowalski, and Williams (2010)). However, our hospital-level measure of costs is the most accurate if the ratio of costs to charges varies within the hospital. We report results at the discharge level for comparison but focus on results at the hospital level.

In column 7 of Table 3, we report the difference-in-difference estimate for charges deflated by the cost to charge ratio at the discharge level. It shows that costs per discharge decreased by $210, roughly 2.8 percent of costs per discharge in Massachusetts before the reform. This estimate is significant at the 5% level. The analogous estimate from the logarithmic specification yields an estimate of a smaller magnitude and is significant only at the 10% level using the block bootstrap confidence intervals. In the next columns, to examine whether the reduced charges and costs stem from the estimated decrease in length of stay, we estimate specifications for charges per day and costs per day. Both show a statistically significant decrease. For charges and costs in levels and logarithms, risk adjusted estimates shown in the bottom row reflect even larger changes, suggesting that our findings are more pronounced after accounting for selection.[24] One concern in estimating

[23]We also note that costs do not include the cost of uncompensated care, which presumably declined with the increase in insurance coverage.

[24]Through a complex interaction between daily costs and charges and length of stay, selection decreases the magnitude of our estimated reductions in charges per day and cost per day. However, the point estimates are not statistically

our base specification with costs as a dependent variable is the fact that cost to charge ratios are computed on an annual basis while our regressions are on a quarterly basis. To account for this issue, we estimate three alternate versions of our models. Our findings are robust to these alternate specifications. These results are presented and discussed in Appendix 5.

In Table 4, we report estimates of changes in the cost to charge ratio itself at the hospital level. Because these estimates are computed for the entire inpatient population at each hospital, they account for costs associated with treating both elderly and nonelderly patients and, thus, provide a more reliable estimate for the overall impact of the reform on hospital charges and costs. In column 6, we see that the cost to charge ratio increased by about 3% relative to its initial level. In the second row for Table 4, confirming the results from Table 3, we show that charges declined in levels and logarithms and the coefficient estimates for the impact of the reform on total costs are negative both in levels and logarithms but they are not statistically significant. Thus, the estimated increase in the cost to charge ratio reflects the ratio of stable or decreased costs to decreased charges.

Finally, we expect that large increases in coverage should lead both charges and costs to converge if the production process and the prices paid for a given procedure are becoming more homogeneous across patients as more people are insured. In the last row of the Table 4, we show that the standard deviation of charges and costs within hospitals, and the standard deviation of costs and charges relative to their initial means also decreased.

Overall, we find no evidence that costs to hospitals increased following the reform. We also find strong evidence that quantities and prices of care declined following the reform, suggesting insurer bargaining power may have been enhanced. Within hospitals, we find that there was less variance in costs that could have resulted from greater homogeneity in the patient pool with respect to insurance coverage.

V. Direct and Spillover Effects of the Reform

A. IV Estimates Relating Gains in Coverage to Outcomes

Using our difference-in-difference strategy, we have shown a reduced form impact of the reform on utilization, outcomes, and hospital costs. These impacts are of direct policy interest in Massachusetts, and they provide suggestive evidence on the potential impact of the national reform.

lower, and they remain statistically significant at the 5% level.

To estimate the impact of insurance coverage on outcomes more generally, we can combine these reduced form estimates with first stage estimates of the impact of the reform on insurance coverage. To do so entails imposing the exclusion restriction that the reform only affected the reduced form outcomes through the expansion in insurance coverage. This exclusion restriction could be violated for several reasons, for example, if the type and not just the expansion in coverage matters. We report the instrumental variable estimates subject to this caveat.

Returning to the estimated coefficients on length of stay, emergency admissions, and overall prevention quality, we combine these results with the estimated coefficient from the uninsurance regression. These estimates are approximate because the two underlying estimates are estimated on slightly different samples due to data availability. We find that a one percentage point increase in insurance coverage decreases length of stay by 0.031 days (0.57/1.82), decreases emergency admissions by 1.7 percentage points (3.24/1.82), and decreases the overall prevention quality indicator by 0.08 percentage points (0.15/1.82), about 0.1% of its initial level.

These effects, derived from the broadest possible sample, allow for spillovers from one patient pool to another. If there are not any spillovers, analogous instrumental variable estimates derived from the reduced form and first stage from separate demographic groups should yield the same estimates. We investigate the possibility of spillovers in the next sections by estimating separate reduced form and first stage specifications within observable demographic groups.

B. Heterogeneity by Age, Gender, Income and Race

There are several reasons to examine heterogeneity in the impact of the reform by age, gender, income, and race. The first is to understand the incidence of the reform. Another is to understand the impact of the coverage mandate on disparities in coverage and outcomes. A third, as discussed above, is to identify heterogeneous impacts of expansions in coverage within groups and to look for evidence of spillovers across groups.

In Table 6, we examine changes in coverage by age. As expected, increases in coverage are most pronounced in the nonelderly, but the elderly did experience small gains in coverage, mostly through Medicaid. Our insurance variable reports the primary payer for the discharge. Increases in Medicaid coverage could indicate that some individuals eligible for Medicare also became eligible for Medicaid, and Medicaid became the primary payer. As discussed above, Medicare does not cover all elderly individuals, and some elderly people might have gained coverage through the Medicaid expansions or through CommCare. Among the elderly, 8,525 discharges in the post-reform period report CommCare as the primary payer.

From the bottom rows of Table 6, we see that decreases in uninsurance were largest among individuals aged 19-26. These individuals predominantly obtained coverage through Medicaid and CommCare. Individuals of all ages obtained CommCare, and CommCare's share of coverage is largest among the near elderly population, aged 55-64. All nonelderly age groups experienced a statistically significant decline in private coverage, suggesting that other types of coverage crowed out private coverage within the hospitalized population.[25]

When we turn to outcomes by age in Table 7, we see some heterogeneity in estimated effects of length of stay across age groups, with some groups experiencing statistically significant increases and others seeing a decline. Since all groups experienced expanded coverage, this implies variation in the within group instrumental variables estimates of the impact of coverage on length of stay. In contrast, admissions from the emergency room declined for all ages, as did the vast majority of measures of procedures, costs, and charges. The IV estimates for the effect of coverage on inpatient admissions from the ER are uniformly negative and monotonically increasing with age. This provides further evidence that, in the absence of the newly provided coverage, uninsured individuals were seeking care through the ER. We would expect health status to decline with age and thus, use of the ER as a point of entry into the health care system should also increase with age. The IV estimate for the impact of a 1 percentage point increase in coverage among individuals 27-30 is a 0.31 percentage point reduction in the probability of an inpatient admission originating in the ER. The estimated impact for those from 31-40 and 41-54 is a 2.12 and 3.35 percentage point reduction respectively. The estimated impact for the near-elderly, 55-64, was an even larger reduction in inpatient admissions from the ER of 9.05 percent.

In the top panel of Table 8, we report difference-in-difference results for insurance coverage by gender. From the mean coverage rate in the lower rows, we can see that Massachusetts males were almost twice as likely to be uninsured as Massachusetts women before the reform, and though males experienced larger gains in coverage than women during the reform, males were still almost twice as

[25]Crowd out among young adults is likely to be less pronounced than in the general population because young adults with CommCare coverage tend to be healthy and less likely to have inpatient stays.

likely to be uninsured after the reform.[26] If changes in outcomes for a particular group occur directly through changes in insurance coverage for that group and there are no heterogeneous treatment effects, men's outcomes should show a larger change than women's because their coverage changed more. We do find larger decreases for men for length of stay, inpatient admissions from the ER, and some and cost measures, as well as smaller decreases in procedures. The IV estimate for the impact of coverage on the use of the ER is relatively uniform across the two groups with a predicted reduction of 1.96 and 1.58 percent for a 1 percent increase in coverage for men and women respectively.

In the second panel of Table 8, we report difference-in-difference results for insurance coverage by the income quartile of the patient's zip code.[27] People from the lowest income zip codes are over-represented in hospital discharges, making up 29% of the sample. People from these poorest zip codes experienced the largest gains in coverage, mostly driven by increases in Medicaid and CommCare. People from the richest zip codes were the only group to experience gains in private coverage, perhaps because means-tested coverage from other sources was not available. The largest increases in CommCare coverage occurred for patients in the second lowest income quartile, which seems plausible because Medicaid was aimed at the poor, and CommCare was particularly targeted at the near poor. However, CommCare coverage reached people in zip codes of all income quartiles, suggesting that there is heterogeneity in income within zip codes.

The second panel of Table 9 shows changes in outcomes by zip code income quartile. Most outcomes show minimal heterogeneity across the income categories, suggesting spillover effects. However, as discussed above, decreases in emergency admissions are particularly pronounced for individuals in the lowest income zip codes, for which Medicaid expansions were the most pronounced.

Finally, we examine heterogeneity in coverage and outcomes by race in Tables 10 and 11. From the means in the bottom rows of each cell, we can see that whites had the highest levels of insurance coverage before the reform. In percentage point terms, all races experienced gains in coverage,

[26] It is not surprising that males had lower initial rates of coverage than females because Medicaid programs explicitly have more lenient eligibility thresholds for women, especially single mothers. Furthermore, policies on the individual market are often more expensive for women because childbirth is an expensive and common medical expense.

[27] Income thresholds that determine the quartile of income for each zip code are different in every year. For 2007, the lowest quartile ranged from $1 to $38,999, the second ranged from $39,000 to $47,999, the third ranged from $48,000 to $62,999, and the fourth was greater than or equal to $63,000. The quartile of income is suppressed for any zip code with a population below a certain threshold and for any zip code that is the only zip code in its state in the given quartile.

but people identified as black, Hispanic, or of unknown race, experienced the largest increases in coverage through the reform. Medicaid expansions were also largest among these groups. Native Americans, which make up less than one percent of the population, experienced the largest gains in private coverage. People of all races took up coverage through CommCare at varying rates. The reform reduced disparities in coverage by race, but it did not eliminate them.

In Table 11, we examine outcomes by race. We see more heterogeneity in the estimated changes in outcomes across the race categories than we do across other demographic categories. Although we generally see decreases in length of stay in all demographic groups, length of stay increased among Asian patients. Admissions from the emergency room also increased for Asian patients. Overall, it appears that within-race changes in outcomes are not directly related to within-race changes in insurance coverage.

Our instrumental variable estimates for some outcomes vary substantially across the subgroups we analyze. Furthermore, though there is less variation in the direction of the results for length of stay, the IV estimates are not proportional across subgroups. We take this as evidence for spillovers from insurance coverage in the hospital production function. One plausible mechanism is that hospitals, facing convergence to almost complete insurance coverage, alter the way in which they provide care to all patients, not only those who are newly insured. Put differently, our estimates suggest that expanding coverage to near universal levels, particularly among the relatively young, impacts care for other populations, including the elderly, who typically have coverage through Medicare. If overall coverage levels impact care among those who are already covered, an externality exists in individual's choice to purchase health insurance. Thus the private equilibrium coverage levels are likely to be too low and government invention might be justified.

We find less evidence for spillovers in inpatient admissions from the ER. However, barring network effects within a community or supply side constraints within an ER, we would not expect a change in coverage of one individual to change the ER usage of another. Thus our results suggest that those who gained insurance were less likely to use the ER as their point of entry into the health care system. This had little effect on the use of the ER among other groups who maintained their pre-reform level of coverage, either through changes in the operation of the ER or learning about alternate options for treatment. Finally, the weak evidence of in favor of cost spillovers is hard to interpret because our measure of costs is most reliable on the hospital level.

VI. Conclusion

In this paper, we show that the Massachusetts health insurance reform expanded coverage among the inpatient hospital population by approximately 28 percent relative to its pre-reform level. Among this population, we see some evidence of crowd of out private coverage by subsidized coverage but we do not find evidence for crowd out in the general population, suggesting that the incidence of crowd out differs between the general population and those with inpatient hospitalizations.

Next, we show declines in length of stay and admissions from the emergency room following the reform. Using new measures of preventive care, we also find evidence that prevention increased outside of hospitals, resulting in a decline in inpatient admissions for preventable conditions. In the midst of these gains, we find no evidence the hospital costs increased. We are unable to make precise welfare statements as we do not capture increased costs to the government and to the purchasers of health insurance that resulted from the reform.

Combining estimates of coverage expansion with estimates of outcome changes, we estimate the instrumental variable impact of expanded coverage on hospital outcomes and find economically significant impacts. To capture the incidence of the reform, we examine changes in coverage and outcomes by demographic group. The reform increased coverage most among young adults and the near elderly, men, people from the lowest income zip codes, and people identified as black and Hispanic. Since changes in outcomes are not generally proportional to changes in coverage across demographic groups, there are likely spillover effects from gains in coverage in one group to outcomes in another.

This paper is the first to examine the effect of Massachusetts health insurance reform on hospital outcomes. In other research, we aim to answer other economic questions using variation induced by health insurance reform in Massachusetts. In Kolstad and Kowalski (2010), we examine the impact of individually mandated health insurance coverage on the labor market.

Appendix 1: Block Bootstrap

Since our main unit of observation is the state, we follow Bertrand, Duflo, and Mullainathan (2004), and block bootstrap our confidence intervals by state. Doing so allows for an arbitrary variancecovariance matrix within states across time. In this section, we discuss some implementation details that are not discussed by Bertrand et al. (2004).

First, the empirical simulation results using the CPS presented by Bertrand et al. (2004) assume that half of the states are treated and the other half are untreated. Since we have only one treated state, it is only sampled in approximately one third of block bootstrap draws. In these draws, the difference-in-difference coefficient on *MA*After* cannot be estimated. In practice, we include these replications in the bootstrap sample to estimate the confidence intervals on the other coefficients without bias. We use a large number of bootstrap replications – 1,000 – so that the confidence intervals on our coefficient of interest are still based on a large number of bootstrap replications.

Second, all of our regressions are weighted. To address weighting within our block bootstrap procedure, we sample states with replacement. Within a state, the sum of the weights does not change because all observations from a given state are drawn at once. However, the sum of the weights varies across regressions because not all states are sampled and some states are sampled more than once.

Appendix 2: Risk Adjustment

A. Risk Adjusters as Covariates

Selection into hospitals after the reform in Massachusetts is an outcome of interest in its own right, which we address in the next section, but to examine causal changes in other outcomes, we control for characteristics of the patient pool. To do so, we use seven sets of risk adjustment variables: demographic characteristics, the number of diagnoses on the discharge record, individual components of the Charlson Score measure of comorbidities, AHRQ comorbidity measures, disease staging measures All-Patient Refined Diagnosis Related Groups (APR-DRG)s, and All-Payer Severity-adjusted (APS)-DRGs. Each risk adjustment measure was developed with a slightly different purpose, but the correlation among them is high. Since our focus is on controlling for selection and not on investigating the relative merit each measure, we include all outcome-relevant measures in the same specification. We run all of our regressions on the individual level, as opposed to the hospital level, to capture the interactions between measures on the individual level.

We construct the first four measures ourselves from the information in the discharge records.

First, we include the same demographic measures that we include in specification 3 of Table 1 – saturated controls in race, gender, and income, as well as age and age squared. Second, following Kleiner and Gruber (2010), we control for the number of diagnoses on the hospital discharge record. The number of diagnoses in our nonelderly sample varies from 0 to 15, the average is 5, and the maximum potentially reported is 15. Third, using data on the composition of diagnoses, we calculate and control for the individual components of the Charlson Score.[28] The Charlson score, which includes 18 components, has been shown to have a strong relationship with mortality (Quan et al. 2005). The average number of components in our nonelderly data is 0.36, and the maximum is 9. Fourth, we control for the 29 AHRQ comorbidity measures. The nonelderly mean in our data is 1 and the maximum is 14.

The remaining three measures were constructed in our data using proprietary algorithms. The disease staging measures, developed by Medstat, are intended to measure disease progression without reference to performed treatments. There are three separate disease staging measures: one for length of stay, one for resource demand, and one for mortality. The length of stay and resource demand disease staging measures take on integer values from 1 to 5 with means in our nonelderly sample of 2.9 and 2.8, respectively. The mortality disease staging measure takes on integer values from 0 to 5 with a mean of 2.1. In each specification, we include only the disease staging measure that is most relevant to the outcome of interest. The APR-DRG measures and the APS-DRG measures, developed by 3M, were developed as refinements to the existing Refined DRGs (R-DRGs) and All-Patient DRGs (AP-DRGs) systems, which expanded the DRG system beyond the Medicare population. The APR-DRG and APS-DRG measures include further adjustments for severity and for neonatal discharges. There are two separate APR-DRG measures: a mortality-specific measure and a general severity measure. We include only the relevant measure in each specification. The mortality measure takes on integer values from 0 to 4 with a mean in our data of 1.2, and the severity measure takes on the same values with a mean of 1.7. There are three APS-DRG measures: one charge-specific measure, one length of stay-specific measure, and one mortality-specific measure.

Each outcome-specific APS-DRG measure was developed using the 2000 NIS data, and is standardized to have a mean of 1 in that sample. (HSS, Inc. (2003)) Multiplying the outcome-specific measure by the mean of that outcome in the 2000 NIS gives a prediction of the expected value

[28] See Charlson, Pompei, Ales, and MacKensie (1987) for the origin of the Charlson Score measures. We draw our Charlson Score code from http://healthservices.cancer.gov/seermedicare/program/charlson.comorbidity.macro.txt.

of that outcome. For example, the mean charge in the 2000 NIS was $13,241.41, and the largest value of the charge-specific APS-DRG for a discharge in our sample was 26.21, so predicted charges for that discharge would be $347,057 in the year 2000. To interpret the other magnitudes, the mean LOS in the 2000 NIS was 4.5096, and mean mortality in the 2000 NIS was 0.0247 deaths per discharge. In our sample including all ages, the means of each measure are slightly than the standardized value of 1.0, reflecting real changes in since the year 2000 and sampling changes from the 2000 NIS to 2004-2007 NIS. The charges measure takes on values from 0.09 to 26.21, with a mean of 1.03 (0.88 in the nonelderly sample); the length of stay measure takes on values from 0.22 to 19.02, with a mean of 1.02 (0.92 in the nonelderly sample); and the mortality measure takes on values from 0 to 41.5, with a mean of 1.10 (0.67 in the nonelderly sample). We include only the relevant measure in each specification.

B. Risk Adjusters as an Outcome of the Reform

As mentioned above, changes in the underling risk of the patients that we observe in hospitals – the underlying hospital case mix – is an interesting outcome of reform. To examine changes in case mix directly, we estimate our preferred specification with each of the three outcome-specific APS-DRG measures as the dependent variable. The coefficient on *MA*After* from the charges-specific regression in the nonelderly sample is -0.0125 with an asymptotic 95% confidence interval of [-.0222,-0.0028]**. This suggests that the reform decreased predicted charges by roughly 1.36 percent, relative to the mean in Massachusetts before the reform of 0.92. This percentage change in predicted charges is not large enough to explain our baseline results in Table 3. Furthermore, relative to this exercise, which relies exclusively on the APS-DRG measure because it is the most straightforward to interpret, we favor the results controlling for seven sets of risk adjusters, reported in Table 3.

The coefficients on *MA*After* from the length of stay regression is not statistically significant. The coefficient from the length of stay-specific regression suggests a decrease in predicted length of stay of -0.0013 [-0.0099,0.0074], which is a 0.1 percentage decrease in length of stay on a base of 0.95 in Massachusetts before the reform. The sign of this prediction runs counter to the length of stay selection story discussed in the paper, but it is not statistically significant. The lack of statistical significance of this result is further evidence that changes in the underlying patient pool do not drive our results.

Appendix 3: Robustness of Insurance Results

One potential concern about the external validity of our results is that Massachusetts could differ from the remainder of the country in ways that we do not observe, and these differences could lead to changes in coverage at the same time that the reform was implemented. Such differences could be due, for example, to factors that affected the entire Northeast. Alternately, it could be due to factors that affected states with relatively low levels of uninsurance. To account for potential unobserved differences that are correlated with observed differences in uninsurance, we estimate a series of specifications in our nonelderly sample, in which we restrict the comparison groups to states most similar to Massachusetts. We present these results in Appendix Table 6.

We first examine changes in uninsurance. When we restrict the comparison group of hospitals to hospitals in the Northeast Census division in the second panel, the impact of the reform on uninsurance is larger than it is in the preferred specification, reproduced in the first panel. As reported in the means at the bottom of the column, the initial level of insurance coverage in the comparison states in the Northeast Census division was similar to the initial level of insurance coverage in the comparison states in the national sample. However, in the third panel, when we restrict the comparison group to include only New England states, which had an initial lower but not statistically lower rate of uninsurance than Massachusetts, the estimated impact of the reform falls to a 1.14 percentage point reduction in uninsurance. This is statistically smaller than the baseline effect, but it remains statistically significantly different from zero at the 1% level despite the much smaller sample size. To investigate the possibility that this change in magnitude is due to the limited health reforms that occurred in 2006 in Maine and Vermont, in column 4, we estimate the main specification in the full sample but we exclude three states that had limited health reforms in the same period – Maine, Vermont, and California. Reassuringly, our point estimate remains unchanged from our main specification. As an alternative specification check, we estimate the same specification on the sample of the 25 states with the highest levels of nonelderly insurance coverage before the reform in the CPS. Our point estimate is not statistically different.

In the remaining columns of Appendix Table 6, we present similar specifications for each of our main outcomes of interest. In general, our quantitative conclusions as well as our qualitative conclusions are unchanged when we change the group of comparison states. Our emergency admission result is particularly robust.

An additional issue in extrapolating from our Massachusetts results is that Massachusetts had a relatively smaller potential increase in insurance due to its high baseline level. If the cost of expanding coverage is convex, we expect larger reductions in uninsurance from the same policies in locations with higher baseline levels of uninsurance. We could test this proposition if another state with a different baseline level of uninsurance enacted the same policies. In the absence of such a natural experiment, we look for suggestive evidence in support of this hypothesis by examining the effect of the reform by baseline levels of insurance coverage within Massachusetts on the hospital level.

In the bottom panel of Appendix Table 7, we divide hospitals based on their initial level of insurance coverage in the pre-reform period. Because not all hospitals in the sample were in the pre-reform data, we first restrict the sample to hospitals that appear at least once in the sample in the before period. These results are presented in specification 4. Estimating the average impact of the reform in this subsample, we find that impact of the reform remained virtually unchanged, 1.19 percentage points compared to 1.25. We then divide the sample into two groups based on whether the hospital had below- or above-median levels of uninsurance in the before period. Mean uninsurance in the sample of discharges from below-median hospitals was 3.03 percentage points, as compared to 6.35 percentage points in the above-median hospitals.

Estimates from columns (2) and (3) show that the impact of the reform was greater in the hospitals with higher initial rates of uninsurance (a 1.68 percentage point increase in coverage) and smaller in the hospitals with lower initial rates of uninsurance (a 1.10 percentage point increase in coverage). In the second panel, which restricts the sample to the 658 hospitals that provide the most identification in the main specification because they are in the sample before and after the reform, the pattern of results is similar. In the third panel, with restricts the sample to the balanced panel of the 52 hospitals that are in the sample in all possible quarters (2004 Q1 to 2007 Q4, with 2006 Q4 and 2007 Q4 not required for Massachusetts), the pattern of results is also similar. Although these results are not statistically different from each other, they suggest that hospitals with highest initial levels of uninsurance saw larger reductions in uninsurance than those with relatively less uninsurance prior to the reform. Without recovering the structural parameters that determine the cost of coverage expansion it is difficult to make precise out of sample predictions. However, these findings are consistent with convex costs of coverage expansion, suggesting the results we find in Massachusetts could be lower than the impact of the national reform.

Appendix 4: Quality Indicators

We use software from AHRQ to calculate these measures in our NIS data (AHRQ Quality Indicators Software Download). For each set of indicators, we use the most recent set of code that does not include a windows executable file. We use the December 2009 Version 4.1 of the Prevention Quality Indicators (Prevention Quality Indicators Download), the March 2008 Version 3.2a of the Inpatient Quality Indicators with results of the DRG grouper software merged on from the NIS Severity file (Inpatient Quality Indicators Download), the January 2009 Version 3.2a of the Patient Safety Indicators (Patient Safety Indicators Download), and the December 2009 Version 4.1 of the Pediatric Quality Indicators (Pediatric Quality Indicators Download).

All prevention quality indicators include, excluding PQI 02, which is not included in either measure, are included in the overall PQI index. The overall pediatric quality indicator is an indicator for any one of PDI 14 (Chronic), 15 (Chronic), 16 (Acute), or 17 (Acute), set to zero for age less than six.

Appendix 5: Robustness to Annual Cost to Charge Ratios

To account for the fact that cost to charge ratios are computed on an annual basis, we re-estimate our base model with three alternate specifications, presented in Appendix Table 5. In all specifications, we redefine the *During* and *After* periods to be the entirety of 2006 and 2007 respectively. Thus, our cost regression estimates are based on a cost to charge ratio computed for the entire *During* or *After* period (i.e. the 2007 cost estimates are based on the total costs in 2007 and total charges in 2007). Our findings are largely unchanged by this alternate definition. The impact on our charge measures is still negative and significant, and we do not find any impact of the reform on cost. The third row of Appendix Table 7 presents estimates that aggregate charge and cost outcomes by year. One difficulty in doing this is the fact that we are missing data from Q4 2006 and 2007 in Massachusetts. We thus aggregate the total charges and costs for each hospital for

Q1-Q3 in each year. These results are consistent with our primary results as well as the other specifications in Appendix Table 5.

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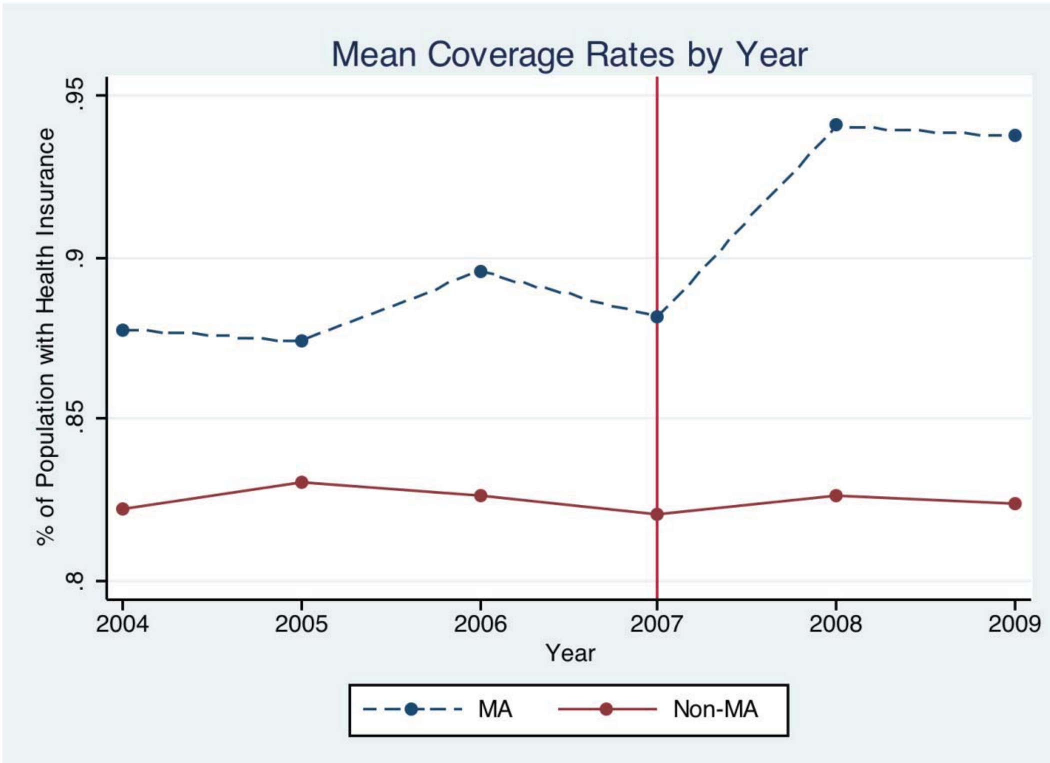
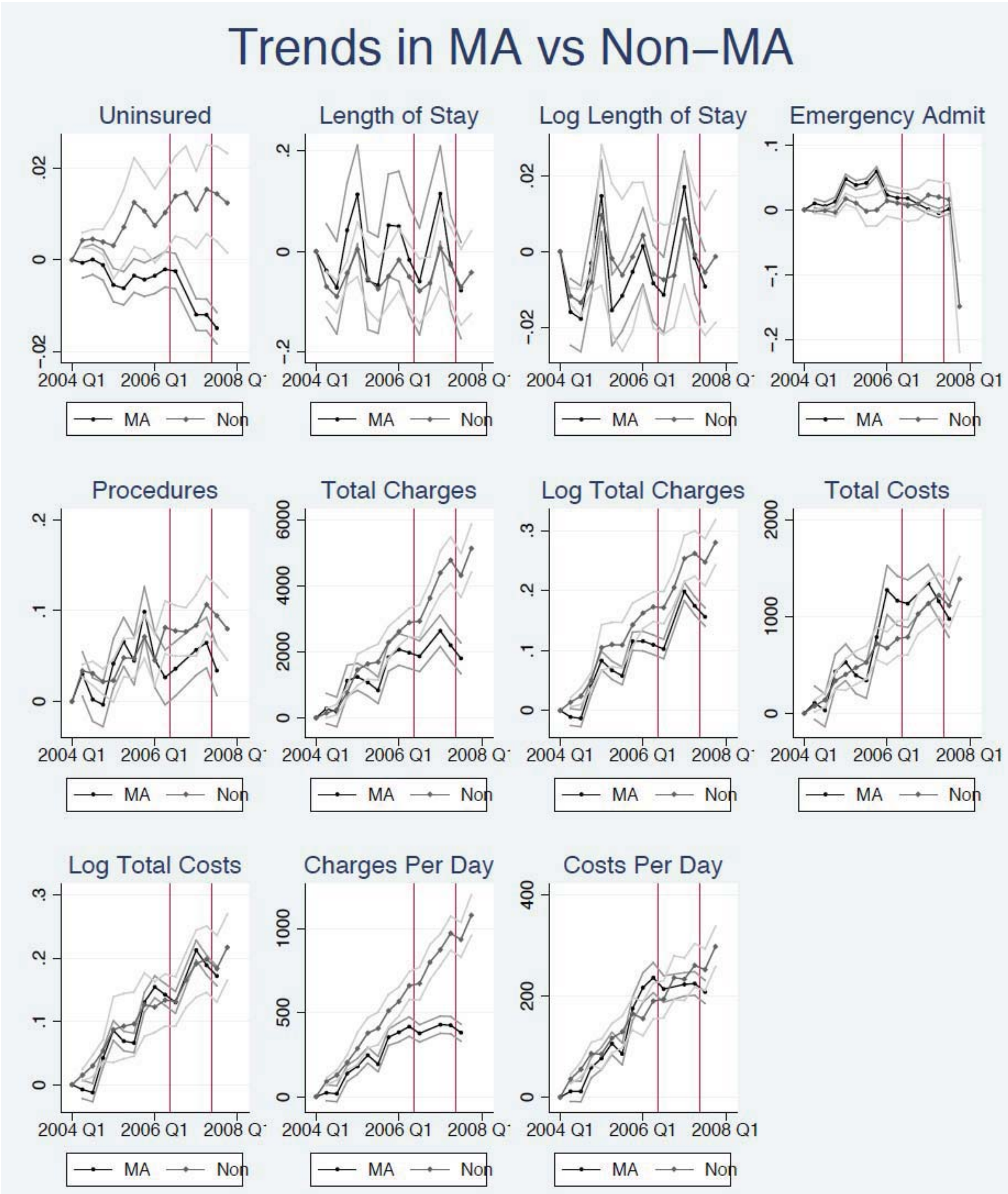
Figure 1



Source: 2004-2009 Supplements to the Current Population Survey, authors' calculations.

Figure 2



Source: NIS authors' calculations.
Trends obtained from regressions including hospital fixed effects.
95% asympototic confidence intervals shown.
Confidence intervals clustered by state for Non–MA and robust for MA.
No data available in MA 2006 Q4 or MA 2007 Q4.
Vertical lines separate During and After periods.

Table 1: Effect of Reform on Uninsurance

	(1) (simple)	(2) (preferred)	(3) (covariates)
Dependent Variable:	**Uninsured**	**Uninsured**	**Uninsured**
Ma*After	-0.0286	-0.0182	-0.0185
	[-0.0374,-0.0199]***	[-0.0246,-0.0119]***	[-0.0251,-0.0120]***
	[-0.0367,-0.0194]+++	[-0.0235,-0.0107]+++	[-0.0233,-0.0105]+++
MA*During	-0.0194	-0.0117	-0.0127
	[-0.0265,-0.0124]***	[-0.0159,-0.0076]***	[-0.0169,-0.0086]***
	[-0.0266,-0.0115]+++	[-0.0154,-0.0067]+++	[-0.0165,-0.0075]+++
MA	-0.0148		
	[-0.0308,0.0011]*		
	[-0.0285,0.0004]+		
After	0.0074		
	[-0.0013,0.0162]*		
	[-0.0016,0.0157]+		
During	0.0077		
	[0.0006,0.0147]**		
	[0.0005,0.0147]++		
Constant	0.0791	0.0727	0.0388
	[0.0632,0.0951]***	[0.0664,0.0790]***	[0.0205,0.0571]***
	[0.0645,0.0935]+++	[0.0593,0.0868]+++	[0.0287,0.0505]+++
Hospital Indicators	No	Yes	Yes
Quarter Indicators	No	Yes	Yes
Covariates	No	No	Yes
N (Nonelderly)	21,181,718	21,181,718	21,181,718
R Squared	0.0004	0.0704	0.0853

95% asymptotic CI clustered by state shown, followed by 95% CI obtained through block bootstrap by state, 1000 reps.
***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.
+++99% bootrapped CI does not include zero, ++95% bootstrapped CI does not include zero, +90% bootstrapped CI does not include zero.
All specifications are weighted using discharge weights. Bootstrapped CI based on 100 replications for (3). Otherwise 1000.
(1) MA*Before and Before omitted. During includes 2006 Q3 through 2007 Q2. After includes 2007 Q3 and later.
(4,5,6) MA*Before and 2004 Q1 omitted. See Appendix 2 for coefficients on selected covariates.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Table 2: Results by Type of Insurance Coverage In NIS

	Mutually Exclusive Types of Coverage				
	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	**Uninsured**	**Medicaid**	**Private**	**Medicare**	**Other**
MA*After	-0.0182	0.0470	-0.0354	0.0004	0.0063
	[-0.0246,-0.0119]***	[0.0366,0.0573]***	[-0.0424,-0.0284]***	[-0.0067,0.0075]	[0.0025,0.0100]***
	[-0.0235,-0.0107]+++	[0.0382,0.0575]+++	[-0.0438,-0.0292]+++	[-0.0071,0.0055]	[0.0026,0.0096]+++
MA*During	-0.0117	0.0408	-0.0279	-0.0005	-0.0006
	[-0.0159,-0.0076]***	[0.0349,0.0467]***	[-0.0329,-0.0230]***	[-0.0032,0.0022]	[-0.0036,0.0024]
	[-0.0154,-0.0067]+++	[0.0354,0.0471]+++	[-0.0334,-0.0231]+++	[-0.0033,0.0018]	[-0.0039,0.0019]
N (Nonelderly)	21,181,718	21,181,718	21,181,718	21,181,718	21,181,718
R Squared	0.0704	0.1184	0.1571	0.0354	0.0745
Mean MA Before	0.06430	0.24600	0.56310	0.10730	0.01930
Mean Non-MA Before	0.07910	0.28760	0.49780	0.09280	0.04270
Mean MA After	0.04310	0.25210	0.55370	0.12340	0.02770
Mean Non-MA After	0.08660	0.28830	0.47920	0.09750	0.04840

	(6)	(7)
Dependent Variable:	**CommCare**	**No Coverage Info**
MA*After	0.0092	0.0008
	[0.0092,0.0092]***	[-0.0011,0.0028]
	[0.0092,0.0092]+++	[-0.0009,0.0028]
MA*During	0.0032	-0.0016
	[0.0032,0.0033]***	[-0.0064,0.0032]
	[0.0032,0.0033]+++	[-0.0069,0.0015]
N (Nonelderly)	21,181,718	21,228,683
R Squared	0.0094	0.0692
Mean MA Before	0.0000	0.0002
Mean Non-MA Before	0.0000	0.0020
Mean MA After	0.0084	0.0001
Mean Non-MA After	0.0000	0.0014

95% asymptotic CI clustered by state shown, followed by 95% CI obtained through block bootstrap by state, 1000 reps.
***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.
+++99% bootrapped CI does not include zero, ++95% bootstrapped CI does not include zero, +90% bootstrapped CI does not include zero.
All specifications and means are weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Table 3: Outcomes Beyond Insurance Coverage

	(1)	(2)	(3)	(4)
Dependent Variable:	**Length of Stay**	**Log Length of Stay**	**Emergency Admit**	**Procedures**
MA*After	-0.0577	-0.0019	-0.0324	-0.0568
	[-0.1037,-0.0117]**	[-0.0091,0.0053]	[-0.0465,-0.0182]***	[-0.0811,-0.0325]***
	[-0.1093,-0.0211]+++	[-0.0100,0.0035]+	[-0.0444,-0.0175]+++	[-0.0841,-0.0352]+++
N (Nonelderly)	21,228,009	21,228,009	21,228,683	21,228,683
R Squared	0.0325	0.0454	0.1127	0.0792
Mean MA Before	5.4256	1.4267	0.3868	1.6186
Mean Non-MA Before	5.0770	1.3552	0.3591	1.4761
Mean MA After	5.2344	1.4180	0.3604	1.7236
Mean Non-MA After	5.0762	1.3563	0.2780	1.5777
MA*After	-0.1044	-0.0086	-0.0315	-0.0626
with risk adjusters	[-0.1441,-0.0647]***	[-0.0138,-0.0035]***	[-0.0456,-0.0174]***	[-0.0811,-0.0441]***

	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable:	**Total Charges**	**Log Total Charges**	**Total Costs**	**Log Total Costs**	**Charges Per Day**	**Costs Per Day**
	-1,985	-0.0559	-210	-0.0050	-387	-38
	[-2493,-1477]***	[-0.0801,-0.0318]***	[-386,-33]**	[-0.0319,0.0220]	[-462,-313]***	[-65,-10]***
	[-2409,-1424]+++	[-0.0797,-0.0334]+++	[-391,-35]++	[-0.0326,0.0198]+	[-452,-306]+++	[-68,-15]+++
N (Nonelderly)	20,813,027	20,813,027	17,622,662	17,622,662	20,812,808	17,622,498
R Squared	0.0750	0.1661	0.0490	0.0918	0.1617	0.0809
Mean MA Before	15,862	9.0142	7,479	8.3195	2,950	1,412
Mean Non-MA Before	18,706	9.1275	6,635	8.1536	3,600	1,281
Mean MA After	19,056	9.2059	8,191	8.4323	3,555	1,555
Mean Non-MA After	22,793	9.3162	7,433	8.2507	4,419	1,449
MA*After	-2,057	-0.0588	-251	-0.0116	-350	-25
with risk adjusters	[-2533,-1580]***	[-0.0767,-0.0409]***	[-352,-150]***	[-0.0300,0.0067]	[-420,-281]***	[-47,-3]**

95% asymptotic CI clustered by state shown, followed by 95% CI obtained through block bootstrap by state, 1000 reps.
***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.
+++99% bootstrapped CI does not include zero, ++95% bootstrapped CI does not include zero, +90% bootstrapped CI does not include zero.
All specifications and means are weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.
Length of Stay is calculated as one plus the discharge date minus the admission date. The smallest possible value is one day.
Total Charges never takes on a value of zero or less. Emergency Admit indicates that the source of admission was an emergency room.
Risk adjusters include seven sets of risk adjustment variables: demographic characteristics, the number of diagnoses on the discharge record, individual components of the Charlson Score measure of comorbidities, AHRQ comorbidity measures, disease staging, All-Patient Refined (APR)-DRGs, and All-Payer Severity-adjusted (APS)-DRGs. See text for more details.

Table 4: Regressions on the Hospital-Quarter Level

	(1) Total Discharges	(2) Nonelderly Dischg.	(3) Elderly Dischg.	(4) Hospital Bedsize	(5) Total Discharges	(6) Cost to Charge Ratio
MA* After	-76	-58	-18	0.0246	-74	0.0153
	[-263,110]	[-234,118]	[-37,1]*	[0.0079,0.0412]***	[-265,118]	[0.0099,0.0207]***
Controls for Bedsize	No	No	No	No	Yes	No
N (All Ages)	16,461	16,461	16,461	16,441	16,461	11,861
Mean MA Before	5,616	3,592	2,023	0.6350	5,616	0.5007
Mean Non-MA Before	5,029	3,454	1,569	0.7529	5,029	0.4079
Mean MA After	7,057	4,721	2,336	0.7206	7,057	0.4716
Mean Non-MA After	5,168	3,686	1,478	0.7582	5,168	0.3726
	(7)	(8)	(9)	(10)	(11)	(12)
	Total Charges, $Mill	**Log Total Charges**	**Total Costs, $Mill**	**Log Total Costs**	**Charges Per Day**	**Costs Per Day**
MA* After	-13.466	-0.0655	-2.269	-0.0251	-366	-24
	[-22.389,-4.544]***	[-0.0980,-0.0329]***	[-4.718,0.181]*	[-0.0620,0.0118]	[-452,-280]***	[-48,-1]**
N (All Ages)	16,341	16,341	16,341	11,861	16,341	11,861
Mean MA Before	112.932	18.0708	49.791	17.3748	2,889	1,387
Mean MA After	180.097	18.3782	74.645	17.6902	3,577	1,589
	(13)	(14)	(15)	(16)	(17)	(18)
	SD. Charges	**SD/Mean Charges**	**SD Costs**	**SD/Mean Costs**	**SD Length of Stay**	**SD/Mean LOS**
MA* After	-4,137	-0.0448	-820	-0.0505	-0.3426	-0.0397
	[-5741,-2534]***	[-0.0770,-0.0125]***	[-1265,-374]***	[-0.0850,-0.0160]***	[-0.4694,-0.2157]***	[-0.0556,-0.0238]***
N (All Ages)	16,337	16,337	11,859	11,859	16,457	16,457
Mean MA Before	24,585	1.3485	11,488	1.3444	6.3783	1.0671
Mean MA After	29,403	1.3160	12,737	1.3330	5.8920	1.0295

95% asymptotic CI clustered by state shown.

***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.

All specifications and means are on hospital-quarter level, weighted by sum of the discharge weights. Total hospital-quarters=16,461, Total hospitals=2,697.

All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Discharges with missing age included in "Total Discharges" specifications but not in "Nonelderly" or "Elderly" specifications.

Table 5: Prevention Quality Indicators

Prevention Quality Indicators	Improvement?	MA* After		N,	Mean MA Before
PQI 90 Overall PQI	Y	-0.0015	[-0.0029,-0.0001]**	15,618,170	0.0838
PQI 01 Diabetes Short-term Comp. Admission	Y	-0.0002	[-0.0004,-0.0000]**	15,618,170	0.0058
PQI 02 Perforated Appendix Admission Rate		-0.0090	[-0.0201,0.0022]	166,279	0.2457
PQI 03 Diabetes Long-Term Comp. Admission		0.0002	[-0.0002,0.0005]	15,618,170	0.0083
PQI 05 COPD Admission Rate	Y	-0.0015	[-0.0018,-0.0011]***	15,618,170	0.0097
PQI 07 Hypertension Admission rate	Y	-0.0002	[-0.0005,0.0001]	15,618,170	0.002
PQI 08 CHF Admission Rate		-0.0002	[-0.0007,0.0003]	15,618,170	0.0109
PQI 10 Dehydration Admission Rate		0.0002	[-0.0000,0.0005]	15,618,170	0.0054
PQI 11 Bacterial Pneumonia Admission Rate	N	0.0006	[-0.0000,0.0013]*	15,618,170	0.0172
PQI 12 Urinary Tract Infection Admission Rate	Y	-0.0005	[-0.0008,-0.0002]***	15,618,170	0.007
PQI 13 Angina without Procedure Admission Rate	N	0.0006	[0.0004,0.0008]***	15,618,170	0.0014
PQI 14 Uncontrolled Diabetes Admission rate	N	0.0001	[-0.0000,0.0003]*	15,618,170	0.0007
PQI 15 Adult Asthma Admission Rate	Y	-0.0006	[-0.0009,-0.0002]***	15,618,170	0.0146
PQI 16 Rate of Lower-extremity Amputation	Y	-0.0006	[-0.0008,-0.0004]***	15,618,170	0.0023

95% asymptotic CI clustered by state shown. All specifications and means are weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.
Regressions in this table estimated in the sample of nonelderly discharges.

Table 6: Insurance by Age

	(1) Uninsured	(2) Medicaid	(3) Private	(4) Medicare	(5) Other	(6) CommCare
All Ages (100% of sample)						
Ma*After	-0.0125	0.0276	-0.0168	-0.0019	0.0035	0.0055
	[-0.0174,-0.0076]***	[0.0209,0.0344]***	[-0.0229,-0.0107]***	[-0.0075,0.0037]	[0.0006,0.0065]**	[0.0055,0.0055]***
Mean MA Before	0.0409	0.1536	0.3693	0.4214	0.0148	0.0000
Nonelderly (age<65) (66% of sample)						
Ma*After	-0.0182	0.0470	-0.0354	0.0004	0.0063	0.0092
	[-0.0246,-0.0119]***	[0.0366,0.0573]***	[-0.0424,-0.0284]***	[-0.0067,0.0075]	[0.0025,0.0100]***	[0.0092,0.0092]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Elderly (age 65+) (34% of sample)						
Ma*After	-0.0018	0.0021	0.0050	-0.0066	0.0013	0.0000
	[-0.0039,0.0002]*	[0.0011,0.0030]***	[-0.0061,0.0162]	[-0.0190,0.0057]	[-0.0009,0.0036]	[-0.0000,-0.0000]***
Mean MA Before	0.0045	0.0096	0.0671	0.9112	0.0076	0.0000
Age<19 (18% of sample)						
Ma*After	-0.0130	0.0825	-0.0675	-0.0022	0.0002	0.0012
	[-0.0244,-0.0016]**	[0.0671,0.0978]***	[-0.0782,-0.0569]***	[-0.0097,0.0053]	[-0.0029,0.0034]	[0.0012,0.0012]***
Mean MA Before	0.0199	0.3122	0.6515	0.0004	0.0160	0.0000
Age 19-26 (8% of sample)						
Ma*After	-0.0395	0.0692	-0.0419	-0.0029	0.0150	0.0154
	[-0.0498,-0.0292]***	[0.0544,0.0839]***	[-0.0522,-0.0316]***	[-0.0107,0.0049]	[0.0108,0.0192]***	[0.0154,0.0154]***
Mean MA Before	0.1266	0.4298	0.3880	0.0339	0.0216	0.0000
Age 27-30 (4% of sample)						
Ma*After	-0.0135	0.0407	-0.0379	-0.0009	0.0115	0.0089
	[-0.0232,-0.0038]***	[0.0239,0.0575]***	[-0.0465,-0.0292]***	[-0.0117,0.0099]	[0.0079,0.0152]***	[0.0089,0.0089]***
Mean MA Before	0.0825	0.2659	0.5805	0.0519	0.0192	0.0000
Age 31-40 (10% of sample)						
Ma*After	-0.0268	0.0404	-0.0188	0.0019	0.0033	0.0090
	[-0.0342,-0.0195]***	[0.0304,0.0504]***	[-0.0269,-0.0107]***	[-0.0068,0.0107]	[-0.0019,0.0085]	[0.0089,0.0090]***
Mean MA Before	0.0841	0.2118	0.5962	0.0883	0.0196	0.0000
Age 41-54 (15% of sample)						
Ma*After	-0.0191	0.0257	-0.0261	0.0111	0.0083	0.0118
	[-0.0281,-0.0102]***	[0.0198,0.0317]***	[-0.0329,-0.0192]***	[0.0037,0.0186]***	[0.0022,0.0145]***	[0.0117,0.0118]***
Mean MA Before	0.0855	0.1975	0.5110	0.1842	0.0220	0.0000
Age 55-64 (12% of sample)						
Ma*After	-0.0063	0.0219	-0.0132	-0.0080	0.0056	0.0119
	[-0.0116,-0.0009]**	[0.0165,0.0273]***	[-0.0183,-0.0082]***	[-0.0133,-0.0027]***	[0.0009,0.0103]**	[0.0119,0.0120]***
Mean MA Before	0.0512	0.1344	0.5511	0.2436	0.0197	0.0000
Age 65-74 (13% of sample)						
Ma*After	-0.0035	0.0031	0.0026	-0.0017	-0.0006	-0.0001
	[-0.0058,-0.0011]***	[0.0018,0.0043]***	[-0.0099,0.0151]	[-0.0158,0.0124]	[-0.0036,0.0024]	[-0.0001,-0.0001]***
Mean MA Before	0.0078	0.0174	0.1131	0.8518	0.0099	0.0000
Age 75+ (21% of sample)						
Ma*After	-0.0010	0.0016	0.0067	-0.0097	0.0023	0.0000
	[-0.0028,0.0008]	[0.0008,0.0024]***	[-0.0036,0.0171]	[-0.0209,0.0016]*	[0.0005,0.0042]**	[0.0000,0.0000]***
Mean MA Before	0.0029	0.0059	0.0449	0.9398	0.0065	0.0000

95% asymptotic CI clustered by state shown under each estimate.
***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.
All specifications and means weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.
Results for missing age category not shown. 8,493 elderly individuals, including 4,893 individuals 75+ have CommCare as primary payer in MA After.

Table 7: Outcomes by Age

	(1) Length of Stay	(2) Log Length of Stay	(3) Emergency Admit	(4) Procedures	(5) Total Charges	(6) Log Total Charges	(7) Total Costs	(8) Log Total Costs	(9) Charges per Day	(10) Costs Per Day
All Ages (100% of sample)										
Ma*After	-0.1222	-0.007	-0.0495	-0.0512	-2,062	-0.0495	-235	-0.0028	-397	-34
	[-0.1650,-0.0793]***	[-0.0135,-0.0005]**	[-0.0646,-0.0344]***	[-0.0774,-0.0250]***	[-2523,-1602]***	[-0.0697,-0.0292]***	[-410,-60]***	[-0.0273,0.0217]	[-470,-324]***	[-63,-4]**
Mean MA Before	5.8833	1.5099	0.4789	1.5513	17,235	9.1828	8,178	8.4939	3,056	1,469
Nonelderly (age<65) (66% of sample)										
Ma*After	-0.0577	-0.0019	-0.0324	-0.0568	-1,985	-0.0559	-210	-0.0050	-387	-38
	[-0.1037,-0.0117]**	[-0.0091,0.0053]	[-0.0465,-0.0182]***	[-0.0811,-0.0325]***	[-2,493,-1,477]***	[-0.0801,-0.0318]***	[-386,-33]**	[-0.0319,0.0220]	[-462,-313]***	[-65,-10]***
Mean MA Before	4.4256	1.4267	0.3868	1.6186	15,900	9.0142	7,479	8.3195	2,950	1,412
Elderly (age 65+) (34% of sample)										
Ma*After	-0.1959	-0.0133	-0.0809	-0.0499	-2,649	-0.0467	-364	-0.0081	-483	-45
	[-0.2511,-0.1408]***	[-0.0190,-0.0075]***	[-0.0999,-0.0619]***	[-0.0847,-0.0151]***	[-3,3308,-1,991]***	[-0.0612,-0.0323]***	[-561,-167]***	[-0.0263,0.0102]	[-599,-367]***	[-82,-8]**
Mean MA Before	6.5975	1.6396	0.6226	1.4463	19,377	9.4458	9,234	8.7577	3,221	1,555
Age<19 (18% of sample)										
Ma*After	-0.0572	-0.0015	0.0107	-0.0892	-1,331	-0.0509	-51	0.0023	-180	4
	[-0.1240,0.0097]*	[-0.0093,0.0062]	[0.0060,0.0153]***	[-0.1230,-0.0554]***	[-1925,-736]***	[-0.0792,-0.0226]***	[-246,145]	[-0.0266,0.0312]	[-246,-114]***	[-16,24]
Mean MA Before	5.2987	1.3623	0.1287	1.4452	13,364	8.3790	7,013	7.7639	2,032	1,069
Age 19-26 (8% of sample)										
Ma*After	0.0200	0.0097	-0.0361	-0.0955	-959	-0.0428	2	0.0068	-326	-32
	[-0.0389,0.0788]	[-0.0006,0.0199]*	[-0.0490,-0.0231]***	[-0.1279,-0.0631]***	[-1558,-361]***	[-0.0711,-0.0145]***	[-177,181]	[-0.0211,0.0347]	[-421,-231]***	[-58,-6]**
Mean MA Before	4.6897	1.3344	0.3859	1.7688	12,325	8.9362	5,845	8.2533	2,572	1,234
Age 27-30 (4% of sample)										
Ma*After	0.1054	0.0088	-0.0042	-0.0668	-121	-0.0060	429	0.0517	-216	21
	[0.0515,0.1593]***	[0.0009,0.0166]**	[-0.0167,0.0083]	[-0.0983,-0.0354]***	[-566,325]	[-0.0289,0.0169]	[267,591]***	[0.0275,0.0758]***	[-300,-132]***	[-3,46]*
Mean MA Before	4.5374	1.3464	0.3144	1.8374	10,961	8.9116	5,019	8.2169	2,432	1,156
Age 31-40 (10% of sample)										
Ma*After	-0.0344	-0.0038	-0.0406	-0.0537	-1,860	-0.0583	-168	-0.0025	-364	-16
	[-0.0947,0.0259]	[-0.0121,0.0044]	[-0.0574,-0.0238]***	[-0.0763,-0.0310]***	[-2309,-1412]***	[-0.0794,-0.0371]***	[-309,-28]**	[-0.0264,0.0214]	[-462,-267]***	[-40,8]
Mean MA Before	4.8814	1.3839	0.3914	1.6715	12,985	9.0599	5,892	8.3455	2,773	1,296
Age 41-54 (15% of sample)										
Ma*After	0.0108	0.0041	-0.0641	0.0190	-2,764	-0.0670	-393	-0.0233	-605	-101
	[-0.0409,0.0624]	[-0.0033,0.0115]	[-0.0867,-0.0415]***	[-0.0130,0.0510]	[-3322,-2205]***	[-0.0830,-0.0510]***	[-595,-190]***	[-0.0487,0.0021]*	[-706,-505]***	[-139,-64]***
Mean MA Before	5.7564	1.4839	0.5807	1.5449	18,432	9.3822	8,313	8.6472	3,601	1,676
Age 55-64 (12% of sample)										
Ma*After	-0.2127	-0.0142	-0.0570	-0.0497	-3,562	-0.0725	-640	-0.0257	-598	-83
	[-0.2686,-0.1568]***	[-0.0204,-0.0080]***	[-0.0767,-0.0373]***	[-0.0920,-0.0073]**	[-4211,-2914]***	[-0.0887,-0.0563]***	[-896,-385]***	[-0.0472,-0.0041]**	[-704,-491]***	[-131,-34]***
Mean MA Before	6.3052	1.5602	0.5541	1.7846	22,137	9.5492	10,058	8.8217	4,027	1,883
Age 65-74 (13% of sample)										
Ma*After	-0.0633	-0.0017	-0.0728	-0.0174	-2,610	-0.0418	-213	0.0016	-588	-63
	[-0.1115,-0.0150]**	[-0.0080,0.0047]	[-0.0912,-0.0544]***	[-0.0590,0.0243]	[-3300,-1919]***	[-0.0578,-0.0258]***	[-431,5]*	[-0.0184,0.0216]	[-715,-461]***	[-107,-19]***
Mean MA Before	6.4894	1.6001	0.5693	1.7335	22,117	9.5488	10,280	8.8393	3,847	1,822
Age 75+ (21% of sample)										
Ma*After	-0.2537	-0.0182	-0.0849	-0.0622	-2,597	-0.0477	-418	-0.0117	-416	-32
	[-0.3245,-0.1830]***	[-0.0247,-0.0116]***	[-0.1045,-0.0654]***	[-0.0938,-0.0305]***	[-3300,-1894]***	[-0.0622,-0.0333]***	[-620,-215]***	[-0.0297,0.0063]	[-527,-305]***	[-66,1]*
Mean MA Before	6.6487	1.6585	0.6481	1.3082	18,057	9.3961	8,743	8.7193	2,920	1,430

95% asymptotic CI clustered by state shown under each estimate.***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.

All specifications and means weighted using discharge weights. All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Sample sizes vary across specifications based on availability of dependent variable. Results for missing age category not shown. There are 8,525 elderly discharges with CommCare.

Table 8: Insurance by Gender and Income

	(1) Uninsured	(2) Medicaid	(3) Private	(4) Medicare	(5) Other	(6) Compare
Female (59% of sample)						
Ma*After	-0.0148	0.0491	-0.0392	0.0006	0.0044	0.0072
	[-0.0208,-0.0087]***	[0.0375,0.0607]***	[-0.0461,-0.0324]***	[-0.0076,0.0087]	[0.0011,0.0076]***	[0.0072,0.0072]***
Mean MA Before	0.0443	0.2707	0.5814	0.0905	0.0131	0.0000
Male (41% of sample)						
Ma*After	-0.0231	0.0448	-0.0308	0.0002	0.0089	0.0119
	[-0.0311,-0.0150]***	[0.0362,0.0535]***	[-0.0384,-0.0233]***	[-0.0056,0.0059]	[0.0041,0.0138]***	[0.0119,0.0119]***
Mean MA Before	0.0908	0.2133	0.5388	0.1297	0.0275	0.0000
Patient's Zip Code in First (Lowest) Income Quartile (30% of sample)						
Ma*After	-0.0248	0.1026	-0.0801	-0.0004	0.0027	0.0083
	[-0.0313,-0.0183]***	[0.0876,0.1176]***	[-0.0880,-0.0722]***	[-0.0070,0.0062]	[-0.0042,0.0097]	[0.0083,0.0083]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Patient's Zip Code in Second Income Quartile (25% of sample)						
Ma*After	-0.0162	0.0431	-0.0504	0.0115	0.0121	0.0125
	[-0.0262,-0.0063]***	[0.0314,0.0547]***	[-0.0613,-0.0395]***	[0.0052,0.0178]***	[0.0076,0.0166]***	[0.0125,0.0125]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Patient's Zip Code in Third Income Quartile (23% of sample)						
Ma*After	-0.0215	0.0316	-0.0208	0.0017	0.0090	0.0091
	[-0.0270,-0.0160]***	[0.0221,0.0411]***	[-0.0284,-0.0133]***	[-0.0053,0.0088]	[0.0062,0.0117]***	[0.0091,0.0091]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Patient's Zip Code in Fourth Income Quartile (22% of sample)						
Ma*After	-0.0062	0.0077	0.0013	-0.0089	0.0061	0.0083
	[-0.0127,0.0003]*	[-0.0008,0.0162]*	[-0.0138,0.0164]	[-0.0205,0.0026]	[0.0025,0.0098]***	[0.0083,0.0083]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000

95% asymptotic CI clustered by state shown under each estimate.

***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.

All specifications and means weighted using discharge weights. All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Regressions in this table estimated on the sample of nonelderly discharges. Results for missing gender and income categories not shown.

Table 9: Outcomes by Gender and Income

	(1) Length of Stay	(2) Log Length of Stay	(3) Emergency Admit	(4) Procedures	(5) Total Charges	(6) Log Total Charges	(7) Total Costs	(8) Log Total Costs	(9) Charges per Day	(10) Costs Per Day
Female (59% of sample)										
Ma*After	-0.0063	0.0020	-0.0234	-0.0775	-1485	-0.0505	-84	0.0006	-349	-28
	[-0.0488,0.0362]	[-0.0043,0.0084]	[-0.0359,-0.0109]***	[-0.1010,-0.0540]***	[-1899,-1072]***	[-0.0715,-0.0294]***	[-228,60]	[-0.0225,0.0236]	[-421,-278]***	[-52,-4]**
Mean MA Before	5.1450	1.4050	0.3320	1.6085	13807	8.9484	6605	8.2648	2678	1293
Male (41% of sample)										
Ma*After	-0.1313	-0.0078	-0.0452	-0.0228	-2654	-0.0629	-370	-0.0118	-432	-48
	[-0.1847,-0.0779]***	[-0.0166,0.0011]*	[-0.0617,-0.0287]***	[-0.0509,0.0052]	[-3301,-2007]***	[-0.0919,-0.0339]***	[-595,-145]***	[-0.0449,0.0213]	[-517,-348]***	[-81,-14]***
Mean MA Before	5.7970	1.4555	0.4595	1.6320	18584	9.1016	8648	8.3927	3311	1571
Patient's Zip Code in First (Lowest) Income Quartile (30% of sample)										
Ma*After	0.1084	0.0164	-0.0844	-0.0151	-1273	-0.0540	53	0.0037	-405	-59
	[0.0487,0.1681]***	[0.0077,0.0252]***	[-0.0969,-0.0718]***	[-0.0435,0.0133]	[-2264,-281]**	[-0.0955,-0.0126]**	[-258,364]	[-0.0429,0.0504]	[-519,-291]***	[-109,-9]**
Mean MA Before	5.6074	1.4402	0.4665	1.6493	15654	9.0038	7138	8.2539	2756	1264
Patient's Zip Code in Second Income Quartile (25% of sample)										
Ma*After	-0.1805	-0.0162	-0.0317	-0.1151	-2438	-0.0665	-401	-0.0125	-400	-34
	[-0.2387,-0.1223]***	[-0.0265,-0.0059]***	[-0.0444,-0.0189]***	[-0.1389,-0.0912]***	[-2985,-1891]***	[-0.0945,-0.0386]***	[-533,-269]***	[-0.0374,0.0124]	[-480,-321]***	[-54,-15]***
Mean MA Before	5.5681	1.4451	0.4437	1.5685	15339	8.9722	7471	8.3176	2787	1390
Patient's Zip Code in Third Income Quartile (23% of sample)										
Ma*After	-0.0865	-0.0015	-0.0028	-0.0466	-2183	-0.0527	-291	-0.0031	-378	-35
	[-0.1486,-0.0245]***	[-0.0098,0.0068]	[-0.0187,0.0131]	[-0.0776,-0.0156]***	[-2712,-1655]***	[-0.0806,-0.0248]***	[-489,-92]***	[-0.0297,0.0235]	[-468,-288]***	[-66,-4]**
Mean MA Before	5.3885	1.4240	0.3671	1.6815	16267	9.0349	7681	8.3393	3057	1466
Patient's Zip Code in Fourth Income Quartile (22% of sample)										
Ma*After	-0.0879	-0.0077	-0.0105	-0.0756	-2162	-0.0609	-207	-0.0209	-408	-28
	[-0.1558,-0.0200]**	[-0.0167,0.0013]*	[-0.0349,0.0139]	[-0.1050,-0.0462]***	[-2965,-1358]***	[-0.0856,-0.0362]***	[-350,-64]***	[-0.0456,0.0037]*	[-550,-266]***	[-51,-5]**
Mean MA Before	5.2333	1.4067	0.3189	1.5753	15716	9.0147	7407	8.3306	3050	1456

95% asymptotic CI clustered by state shown under each estimate.***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.

All specifications and means weighted using discharge weights. All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Regressions in this table estimated in the sample of nonelderly discharges. Sample sizes vary across specifications based on availability of dependent variable. Results for missing gender and income categories not shown.

Table 10: Insurance by Race

	(1) Uninsured	(2) Medicaid	(3) Private	(4) Medicare	(5) Other	(6) CommCare
White (35% of sample)						
MA*After	-0.0129	0.0252	-0.0215	0.0020	0.0072	0.0089
	[-0.0190,-0.0069]***	[0.0193,0.0311]***	[-0.0282,-0.0147]***	[-0.0044,0.0084]	[0.0031,0.0113]***	[0.0089,0.0090]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Black (35% of sample)						
MA*After	-0.0229	0.0575	-0.0502	0.0054	0.0102	0.0116
	[-0.0389,-0.0070]***	[0.0432,0.0717]***	[-0.0661,-0.0344]***	[-0.0122,0.0231]	[0.0025,0.0180]**	[0.0115,0.0116]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Hispanic (7% of sample)						
MA*After	-0.0395	0.1149	-0.0754	-0.0081	0.0081	0.0091
	[-0.0678,-0.0111]***	[0.0725,0.1572]***	[-0.0839,-0.0670]***	[-0.0225,0.0063]	[0.0048,0.0114]***	[0.0091,0.0091]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Asian (1% of sample)						
MA*After	-0.0118	-0.0008	-0.0030	-0.0026	0.0182	0.0117
	[-0.0217,-0.0018]**	[-0.0107,0.0091]	[-0.0112,0.0052]	[-0.0099,0.0046]	[0.0074,0.0290]***	[0.0117,0.0118]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Native American (<1% of sample)						
MA*After	-0.0491	-0.0446	0.1885	-0.0468	-0.0481	0.0013
	[-0.0645,-0.0336]***	[-0.0638,-0.0254]***	[0.1753,0.2018]***	[-0.0597,-0.0339]***	[-0.0554,-0.0407]***	[0.0013,0.0014]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Other Race (2% of sample)						
MA*After	-0.0174	0.0392	-0.0359	-0.0062	0.0204	0.0111
	[-0.0457,0.0110]	[0.0289,0.0494]***	[-0.0641,-0.0077]**	[-0.0184,0.0060]	[0.0113,0.0294]***	[0.0109,0.0113]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000
Unknown Race (19% of sample)						
MA*After	-0.0323	0.0989	-0.0407	-0.0195	-0.0065	0.0049
	[-0.0456,-0.0190]***	[0.0913,0.1066]***	[-0.0545,-0.0269]***	[-0.0243,-0.0146]***	[-0.0128,-0.0002]**	[0.0049,0.0049]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000

95% asymptotic CI clustered by state shown under each estimate.

***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.

All specifications and means weighted using discharge weights. All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Regressions in this table estimated on the sample of nonelderly discharges.

Table 11: Outcomes by Race

	(1) Length of Stay	(2) Log Length of Stay	(3) Emergency Admit	(4) Procedures	(5) Total Charges	(6) Log Total Charges	(7) Total Costs	(8) Log Total Costs	(9) Charges per Day	(10) Costs Per Day
White (35% of sample)										
MA*After	-0.0787	-0.0031	-0.0372	-0.0708	-2,296	-0.0450	-299	0.0011	-415	-36
	[-0.1353,-0.0221]***	[-0.0105,0.0043]	[-0.0540,-0.0205]***	[-0.0933,-0.0484]***	[-2884,-1709]***	[-0.0732,-0.0169]***	[-529,-70]**	[-0.0298,0.0321]	[-511,-320]***	[-71,-2]**
Mean MA Before	5.3834	1.4270	0.3786	1.6602	16,154	9.0585	7,557	8.3562	3,091	1,465
Black (35% of sample)										
MA*After	0.1437	-0.0005	0.0070	-0.0720	-1,085	-0.0425	334	0.0232	-234	25
	[0.0540,0.2333]***	[-0.0184,0.0174]	[-0.0331,0.0471]	[-0.1260,-0.0181]**	[-2335,164]*	[-0.0962,0.0113]	[-53,721]*	[-0.0444,0.0907]	[-375,-93]***	[-15,65]
Mean MA Before	5.9492	1.4840	0.5226	1.4081	15,026	8.9749	7,308	8.3223	2,493	1,239
Hispanic (7% of sample)										
MA*After	-0.0659	-0.0101	-0.0222	-0.0288	-1,997	-0.0498	-82	0.0129	-368	3
	[-0.1350,0.0032]*	[-0.0186,-0.0016]**	[-0.0343,-0.0101]***	[-0.0504,-0.0072]**	[-2810,-1183]***	[-0.0949,-0.0047]**	[-249,85]	[-0.0202,0.0460]	[-497,-239]***	[-21,26]
Mean MA Before	5.1288	1.3786	0.4179	1.5033	13,799	8.8508	6,420	8.1369	2,568	1,198
Asian (1% of sample)										
MA*After	0.5983	0.0495	0.0006	-0.0032	2,511	0.0875	1,766	0.1312	51	150
	[0.4588,0.7378]***	[0.0395,0.0595]***	[-0.0212,0.0223]	[-0.0646,0.0582]	[1871,3151]***	[0.0533,0.1218]***	[1500,2032]***	[0.0943,0.1680]***	[-28,130]	[128,173]***
Mean MA Before	5.0414	1.3643	0.2626	1.5186	13,739	8.7000	6,444	8.0084	2,403	1,145
Native American (<1% of sample)										
MA*After	0.2895	-0.0084	-0.0887	0.0188	-4,200	0.0431	-522	0.0629	-116	-91
	[-0.3692,0.9483]	[-0.0349,0.0182]	[-0.1283,-0.0490]***	[-0.1076,0.1453]	[-7421,-978]**	[-0.0221,0.1083]	[-1741,697]	[-0.0098,0.1356]*	[-372,140]	[-186,4]*
Mean MA Before	5.3411	1.4772	0.2774	1.5372	16,549	9.0043	7,909	8.3879	3,047	1,568
Other Race (2% of sample)										
MA*After	-0.2641	-0.0335	-0.0261	-0.0109	-1,281	-0.1743	269	-0.1319	-374	-41
	[-0.5595,0.0314]*	[-0.0787,0.0117]	[-0.0484,-0.0038]**	[-0.0724,0.0506]	[-2658,97]*	[-0.2626,-0.0861]***	[-414,953]	[-0.2410,-0.0228]**	[-536,-211]***	[-95,12]
Mean MA Before	5.7428	1.4287	0.2211	1.8729	20,396	9.0246	10,841	8.4002	3,115	1,621
Unknown Race (19% of sample)										
MA*After	-0.6176	-0.0154	-0.0898	-0.1801	-6,822	-0.4744	-2,734	-0.4544	-1,273	-513
	[-0.6770,-0.5583]***	[-0.0244,-0.0065]***	[-0.1031,-0.0765]***	[-0.2460,-0.1143]***	[-7498,-6146]***	[-0.5040,-0.4448]***	[-3022,-2445]***	[-0.4918,-0.4171]***	[-1411,-1136]***	[-574,-453]***
Mean MA Before	5.6372	1.4452	0.2554	1.5649	15,700	8.9126	6,770	8.1840	2,833	1,256

95% asymptotic CI clustered by state shown under each estimate.***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.

All specifications and means weighted using discharge weights. All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

Regressions in this table estimated in the sample of nonelderly discharges. Sample sizes vary across specifications based on availability of dependent variable.

Appendix Table 1: Results by Type of Insurance Coverage in CPS

	Mutually Exclusive Types of Coverage					
	(1)	(2)	(3)	(4)	(5)	(6)
	Uninsured	**ESHI**	**Private Insurance Unrelated to Employment**	**Medicaid**	**Medicare**	**VA/Military**
MA*After	-0.0571	0.0345	-0.0086	0.0351	-0.0004	-0.0036
	[-0.0605,-0.0537]***	[0.0283,0.0408]***	[-0.0106,-0.0066]***	[0.0317,0.0386]***	[-0.0012,0.0004]	[-0.0050,-0.0023]***
	[-0.0604,-0.0536]+++	[0.0286,0.0403]+++	[-0.0105,-0.0066]+++	[0.0317,0.0385]+++	[-0.0011,0.0004]+	[-0.0051,-0.0025]+++
During*MA	-0.0049	0.0024	0.0066	-0.0005	0.0011	-0.0047
	[-0.0093,-0.0005]**	[-0.0016,0.0064]	[0.0036,0.0096]***	[-0.0036,0.0025]	[0.0004,0.0019]***	[-0.0061,-0.0034]***
	[-0.0097,-0.0005]++	[-0.0020,0.0060]	[0.0041,0.0102]+++	[-0.0036,0.0022]	[0.0004,0.0019]+++	[-0.006,-0.0032]+++
After	0.0007	-0.0134	-0.0006	0.0101	0.0023	0.0009
	[-0.0027,0.0042]	[-0.0197,-0.0072]***	[-0.0026,0.0015]	[0.0066,0.0136]***	[0.0015,0.0031]***	[-0.0004,0.0022]
	[-0.0027,0.0037]+	[-0.0192,-0.0077]	[-0.0025,0.0013]+	[0.0068,0.0135]+++	[0.0016,0.003]+++	[-0.0003,0.0023]+
During	0.0058	-0.0077	-0.0004	0.0023	0.001	-0.0009
	[0.0014,0.0101]**	[-0.0117,-0.0037]***	[-0.0034,0.0025]	[-0.0008,0.0053]	[0.0002,0.0018]**	[-0.0022,0.0004]
	[0.0015,0.0105]+++	[-0.0113,-0.0033]+++	[-0.0039,0.0020]+	[-0.0004,0.0053]	[0.0003,0.0018]+++	[-0.0024,0.0003]+
MA	-0.0335	0.0513	0.0075	-0.0082	-0.0114	-0.0056
	[-0.0352,-0.0319]***	[0.0490,0.0536]***	[0.0067,0.0083]***	[-0.0098,-0.0067]***	[-0.0118,-0.0111]***	[-0.0061,-0.0051]***
	[-0.0718,0.0013]+	[0.0144,0.1127]+	[-0.0606,0.0084]+	[-0.0683,0.0444]	[-0.0117,0.0016]+	[-0.0633,0.0012]+
Constant	0.1511	0.6531	0.0459	0.1195	0.0172	0.0132
	[0.1494,0.1527]***	[0.6508,0.6555]***	[0.0450,0.0467]***	[0.1180,0.1210]***	[0.0169,0.0175]***	[0.0127,0.0137]***
	[0.1164,0.1889]+++	[0.5922,0.6898]+++	[0.0449,0.1138]+++	[0.067,0.1796]+++	[0.0041,0.0175]+++	[0.0065,0.0708]+++
N (Nonelderly)	1,129,221	1,129,221	1,129,221	1,129,221	1,129,221	1,129,221
R Squared	0.0152	0.0160	0.0030	0.0071	0.0013	0.0066
MA Before	0.1176	0.7044	0.0534	0.1113	0.0057	0.0076
Non-MA Before	0.1732	0.6358	0.0607	0.1066	0.0090	0.0147
MA After	0.0612	0.7255	0.0442	0.1565	0.0077	0.0049
Non-MA After	0.1747	0.6214	0.0602	0.1167	0.0113	0.0157

95% asymptotic CI clustered by state shown, followed by 95% CI obtained through block bootstrap by state, 1000 reps.
***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.
+++99% bootstrapped CI does not include zero, ++95% bootstrapped CI does not include zero, +90% bootstrapped CI does not include zero.
All specifications and means are weighted using discharge weights.
All specifications include state fixed effects.
Source: CPS March Supplement 2004-2007 authors' calculations. See text for more details.

Appendix Table 2: Coefficients on Selected Covariates

Dependent Variable	(1) (preferred) **Uninsured**	(2) (covariates) **Uninsured**		(2) (covariates cont.)
Ma*After	-0.0182	-0.0185	Female	-0.0473
	[-0.0246,-0.0119]***	[-0.0251,-0.0120]***		[-0.0549,-0.0398]***
MA*During	-0.0117	-0.0127	Gender Unknown	-0.0042
	[-0.0159,-0.0076]***	[-0.0169,-0.0086]***		[-0.0298,0.0213]
2004 Q2	0.0041	0.0034	Black	0.0063
	[0.0024,0.0057]***	[0.0020,0.0048]***		[0.0011,0.0114]**
2004 Q3	0.0044	0.0034	Hispanic	0.0361
	[0.0024,0.0064]***	[0.0014,0.0053]***		[0.0136,0.0586]***
2004 Q4	0.0037	0.0032	Asian	0.0187
	[0.0011,0.0064]***	[0.0005,0.0058]**		[0.0130,0.0243]***
2005 Q1	0.0027	0.0026	Native American	0.0056
	[-0.0042,0.0096]	[-0.0038,0.0091]		[-0.0081,0.0194]
2005 Q2	0.0066	0.0061	Other Race	0.0376
	[-0.0013,0.0145]	[-0.0012,0.0135]*		[0.0163,0.0589]***
2005 Q3	0.0119	0.0109	Unknown Race	0.0113
	[0.0025,0.0213]**	[0.0020,0.0198]**		[-0.0059,0.0286]
2005 Q4	0.0100	0.0095	Zip in First (Lowest)	0.0225
	[0.0019,0.0182]**	[0.0017,0.0173]**	Income Quartile	[0.0146,0.0304]***
2006 Q1	0.0071	0.0069	Zip in Second Income	0.0172
	[-0.0007,0.0149]*	[-0.0006,0.0144]*	Quartile	[0.0106,0.0238]***
2006 Q2	0.0099	0.0092	Zip in Third Income	0.0094
	[0.0017,0.0181]**	[0.0014,0.0170]**	Quartile	[0.0046,0.0141]***
2006 Q3	0.0137	0.0129	Zip in Unknown	0.0445
	[0.0052,0.0223]***	[0.0047,0.0210]***	Income Quartile	[0.0013,0.0876]**
2006 Q4	0.0143	0.0139	Age	0.0042
	[0.0043,0.0242]***	[0.0043,0.0235]***		[0.0032,0.0053]***
2007 Q1	0.0105	0.0101	Age Squared	-0.0001
	[0.0023,0.0188]**	[0.0022,0.0181]**		[-0.0001,-0.0001]***
2007 Q2	0.0147	0.0139		
	[0.0052,0.0243]***	[0.0048,0.0229]***		
2007 Q3	0.0140	0.0126		
	[0.0037,0.0242]***	[0.0023,0.0229]**		
2007 Q4	0.0120	0.0112		
	[0.0013,0.0227]**	[0.0006,0.0218]**		
Constant	0.0727	0.0388		
	[0.0664,0.0790]***	[0.0205,0.0571]***		
Hospital Indicators	Yes	Yes		
Covariates	No	Yes, cont. next col.		
N (Nonelderly)	21,181,718	21,181,718		
R Squared	0.0704	0.0853		

95% asymptotic CI clustered by state shown directly under each estimate.
***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.
All specifications and means are weighted using discharge weights.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.
Omitted categories: MA*Before, 2004 Q1, Male, White, Zip in Fourth Income Quartile, Age Less Than One Year.
Coefficients on other ages in increments of one year included but not reported. Age 100+ is the highest age category.

Appendix Table 3: Inpatient Quality Indicators

Inpatient Quality Indicators	Improvement?	MA* After		N,	Mean MA Before
IQI 1 Esophageal Resection Volume	Y	-0.0001	[-0.0001,-0.0000]***	21,168,715	0.0002
IQI 2 Pancreatic Resection Volume	Y	-0.0001	[-0.0001,-0.0001]***	21,168,715	0.0002
IQI 4 AAA Repair Volume	N	0.0001	[0.0001,0.0002]***	21,168,715	0.0004
IQI 6 Percut. Transluminal Cor. Angioplasty, Volume	Y	-0.0012	[-0.0021,-0.0002]**	21,168,715	0.0162
IQI 7 Carotid Endarterectomy, Volume	Y	-0.0003	[-0.0004,-0.0003]***	21,168,715	0.0013
IQI 8 Esophageal Resection Mortality		-0.0649	[-0.2253,0.0956]	829	0.0357
IQI 9 Pancreatic Resection Mortality		0.0156	[-0.0383,0.0695]	1,913	0.0257
IQI 11 AAA Repair Mortality		-0.0055	[-0.0296,0.0187]	5,741	0.0731
IQI 12 CABG Mortality		-0.0010	[-0.0053,0.0033]	97,354	0.0132
IQI 13 Craniotomy Mortality		0.0062	[-0.0038,0.0163]	61,811	0.0423
IQI 14 Hip Replacement Mortality	Y	-0.0009	[-0.0019,0.0001]*	76,083	0.0000
IQI 15 AMI Mortality	N	0.0047	[0.0000,0.0093]**	191,575	0.0210
IQI 16 Congestive Heart Failure (CHF) Mortality	Y	-0.0043	[-0.0063,-0.0024]***	241,624	0.0162
IQI 17 Acute Stroke Mortality		-0.0013	[-0.0090,0.0065]	130,352	0.0813
IQI 18 Gastrointestinal Hemorrhage Mortality		-0.0004	[-0.0033,0.0024]	139,608	0.0133
IQI 20 Pneumonia Mortality		-0.0001	[-0.0026,0.0024]	292,364	0.0221
IQI 21 Cesarean Delivery Rate	N	0.0147	[0.0115,0.0179]***	3,107,324	0.2653
IQI 22 VBAC Rate Uncomplicated	Y	-0.0268	[-0.0316,-0.0220]***	456,701	0.1397
IQI 23 Laparoscopic Cholecystectomy Rate	Y	-0.0165	[-0.0275,-0.0056]***	193,528	0.8138
IQI 25 Bilateral Cardiac Catheterization Rate	Y	-0.0244	[-0.0291,-0.0196]***	507,691	0.1372
IQI 26 CABG Area Rate		-0.0001	[-0.0004,0.0001]	21,168,715	0.0043
IQI 27 PCTA Area Rate	Y	-0.0012	[-0.0021,-0.0003]***	21,168,715	0.0155
IQI 28 Hysterectomy Area Rate		0.0005	[-0.0003,0.0012]	21,168,715	0.0147
IQI 29 Laminectomy or Spinal Fusion Area Rate		-0.0004	[-0.0011,0.0003]	21,168,715	0.0145
IQI 30 Percut. Transluminal Cor. Angioplasty, Mort.	N	0.0065	[0.0046,0.0084]***	309,166	0.0062
IQI 31 Carotid Endarterectomy, Mortality		0.0014	[-0.0020,0.0047]	24,372	0.0037
IQI 32 AMI Mortality WO Transfer	N	0.0103	[0.0048,0.0158]***	144,443	0.0267
IQI 33 Primary Cesarean Delivery Rate	N	0.0172	[0.0140,0.0204]***	2,650,623	0.1710
IQI 34 VBAC Rate All	Y	-0.0254	[-0.0298,-0.0211]***	521,244	0.1350

95% asymptotic CI clustered by state shown. All specifications and means are weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.
Regressions in this table estimated in the sample of nonelderly discharges.
IQI 19 Hip Fracture Mortality and IQI 24 Incidental Appendectomy in the Elderly Rate did not vary for nonelderly.

Appendix Table 4: Patient Safety Indicators and Pediatric Quality Indicators

Patient Safety Indicators	Improvement?	MA* After		N, Mean MA Before	
PSI 1 Complications of Anesthesia	Y	-0.0004	[-0.0005,-0.0003]***	5,152,148	0.0008
PSI 2 Death in Low-Mortality DRGs		0.0000	[-0.0000,0.0001]	7,326,404	0.0003
PSI 3 Pressure Ulcer	Y	-0.0016	[-0.0023,-0.0008]***	3,076,205	0.0110
PSI 4 Failure to Rescue	N	0.0500	[0.0400,0.0599]***	93,977	0.0955
PSI 6 Iatrogenic Pneumothorax, Provider Level		0.0000	[-0.0001,0.0001]	11,203,786	0.0005
PSI 7 Selected Infections Due to Medical Care	Y	-0.0006	[-0.0008,-0.0005]***	10,896,538	0.0021
PSI 8 Postoperative Hip Fracture		0.0000	[-0.0000,0.0001]	2,605,440	0.0002
PSI 9 Postoperative Hemorrhage or Hematoma	N	0.0003	[0.0000,0.0005]**	3,886,326	0.0024
PSI 10 Postop. Physio. and Metab. Derangement	N	0.0009	[0.0006,0.0011]***	1,941,529	0.0008
PSI 11 Postop. Respiratory Failure		-0.0004	[-0.0013,0.0004]	1,667,885	0.0058
PSI 12 Postop. Pul. Embolism or Deep Vein Thromb.	Y	-0.0036	[-0.0043,-0.0029]***	3,876,513	0.0068
PSI 13 Postoperative Sepsis	Y	-0.0046	[-0.0065,-0.0026]***	380,106	0.0053
PSI 14 Postoperative Would Dehiscence	Y	-0.0005	[-0.0011,0.0001]*	846,179	0.0022
PSI 15 Accident. Puncture or Laceration, Provider	Y	-0.0005	[-0.0008,-0.0003]***	11,498,298	0.0034
PSI 17 Birth Trauma - Injury to Neonate	N	0.0003	[0.0000,0.0006]**	3,551,885	0.0032
PSI 18 Ob. Trauma - Vag. Deliv. with Instrument	N	0.0326	[0.0237,0.0415]***	214,112	0.1740
PSI 19 Ob. Trauma - Vag. Deliv. without Instrument	Y	-0.0047	[-0.0065,-0.0029]***	2,203,832	0.0365
PSI 20 Obstetric Trauma - Cesarean Delivery	Y	-0.0030	[-0.0035,-0.0024]***	1,110,668	0.0067
PSI 22 Iatrogenic Pneumothorax, Area Level	Y	-0.0001	[-0.0002,-0.0000]**	11,205,344	0.0007
PSI 23 Selected Infections Due to Medical Care, Area	Y	-0.0009	[-0.0010,-0.0007]***	13,745,803	0.0029
PSI 24 Postoperative Wound Dehiscence, Area	Y	-0.0001	[-0.0002,-0.0001]***	10,912,283	0.0003
PSI 25 Accidental Puncture or Laceration, Area	Y	-0.0008	[-0.0010,-0.0005]***	11,501,735	0.0038
PSI 27 Postoperative Hemorrhage or Hemat., Area	Y	-0.0002	[-0.0003,-0.0001]***	11,789,036	0.0015
Pediatric Quality Indicators					
PDI 90 Overall PDI	N	0.0048	[0.0027,0.0069]***	2,779,077	0.0228
PDI 91 Acute PDI	N	0.0027	[0.0020,0.0034]***	2,779,077	0.0077
PDI 92 Chronic PDI	N	0.0021	[0.0001,0.0041]**	2,779,077	0.0151
PDI 1 Accidental Puncture or Laceration	Y	-0.0005	[-0.0007,-0.0003]***	2,779,077	0.0014
PDI 2 Pressure Ulcer	N	0.0046	[0.0029,0.0063]***	286,267	0.0027
PDI 5 Iatrogenic Pneumothorax	Y	-0.0002	[-0.0003,-0.0001]***	2,492,484	0.0003
PDI 6 Pediatric Heart Surgery Mortality		0.0102	[-0.0191,0.0395]	18,034	0.0301
PDI 7 Pediatric Heart Surgery Volume	N	0.0011	[0.0005,0.0017]***	2,779,077	0.0191
PDI 8 Postoperative Hemorrhage or Hematoma	Y	-0.0010	[-0.0022,0.0002]*	102,620	0.0014
PDI 9 Postoperative Respiratory Failure	Y	-0.0080	[-0.0105,-0.0054]***	83,722	0.0099
PDI 10 Postoperative Sepsis	N	0.0168	[0.0123,0.0213]***	80,976	0.0221
PDI 11 Postoperative Wound Dehiscence		0.0000	[-0.0016,0.0016]	75,842	0.0004
PDI 12 Cent. Venous Catheter-Related Blood. Infect.	Y	-0.0017	[-0.0021,-0.0013]***	2,154,980	0.0065
PDI 13 Transfusion Reaction	N	0.0000	[-0.0000,-0.0000]**	2,779,077	0.0000
PDI 14 Asthma Admission Rate	N	0.0046	[0.0017,0.0075]***	2,779,077	0.0241
PDI 15 Diabetes Short-term Comp. Admissions		0.0000	[-0.0004,0.0005]	2,779,077	0.0023
PDI 16 Gastroenteritis Admission Rate		0.0008	[-0.0030,0.0046]	2,779,077	0.0320
PDI 17 Perforated Appendix Admission Rate	Y	-0.0566	[-0.0845,-0.0286]***	62,942	0.2609
PDI 18 Urinary Tract Admission Rate	N	0.0052	[0.0043,0.0061]***	2,779,077	0.0099
NQI 1 Iatrogenic Pneumothorax in Neonates		-0.0001	[-0.0005,0.0002]	221,375	0.0000
NQI 2 Neonatal Mortality	N	0.0045	[0.0034,0.0055]***	929,939	0.0053
NQI 3 Neonatal Blood Stream Infection	N	0.0253	[0.0174,0.0332]***	98,512	0.0173
PSI 17 Birth Trauma - Injury to Neonate		0.0006	[-0.0005,0.0016]	840,356	0.0126
PQI 9 Low Birth Weight	Y	-0.0236	[-0.0301,-0.0172]***	935,899	0.1855

95% asymptotic CI clustered by state shown. All specifications and means are weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2007, quarterly.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.
Regressions in this table estimated in the sample of nonelderly discharges.

Appendix Table 5: Robustness to Annual Definition of During and After Periods

	(1) Cost to Charge Ratio	(2) Total Charges[1]	(3) Log Total Charges	(4) Total Costs[1]	(5) Log Total Costs	(6) Charges per Day	(7) Costs Per Day
Individual Level (Compare to Table 3)							
MA*After		-2,015	-0.0492	14	0.0158	-401	-12
		[-2631,-1400]***	[-0.0748,-0.0235]***	[-177,204]	[-0.0131,0.0446]	[-487,-314]***	[-44,20]
N		20,813,027	20,813,027	17,622,662	17,622,662	20,812,808	17,622,498
Mean MA Before		15,648	9.0067	7,143	8.2925	2,923	1,362
Hospital-Quarter Level (Compare to Table 4)							
MA*After	0.0192	-14.789	-0.0555	-1.203	-0.0048	-381	-3
	[0.0128,0.0257]***	[-26.361,-3.217]**	[-0.0888,-0.0222]***	[-4.218,1.813]	[-0.0393,0.0298]	[-490,-272]***	[-30,25]
N	11,861	16,341	16,341	11,861	11,861	16,341	11,861
Mean MA Before	0.4998	119.189	18.1020	51.290	17.4010	2,880	1,352
Hospital-Year Level, Excluding Q4 in All Years							
MA*After	0.0195	-43.176	-0.0527	-2.781	-0.0012	-382	2
	[0.0090,0.0300]***	[-95.715,9.363]	[-0.1016,-0.0037]**	[-17.174,11.611]	[-0.0540,0.0517]	[-550,-213]***	[-41,45]
N	2,981	4,116	4,116	2,981	2,981	4,116	2,981
Mean MA Before	0.4998	353.211	19.1896	152.251	18.4904	2,842	1,334

95% asymptotic CI clustered by state shown, followed by 95% CI obtained through block bootstrap by state, 1000 reps.

***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.

All specifications and means are weighted using discharge weights. Hospital-Quarter Level and Hospital-Year Level weighted by sum of discharge weights in each time period.

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

In this table only, During period is defined as 2006 Q1 to 2006 Q4, and After period is defined as 2007 Q1 - 2006 Q4.

Individual Level differs from Table 3 and Hospital-Quarter level differs from Table 4 only by definition of During and After.

Hospital-Year Level Excludes Q4 in all years to account for missing Q4 data in MA in 2006 and 2007.

Otherwise, Hospital-Year Level specification is analogous to Hospital-Quarter Level specification with year instead of quarter fixed effects.

[1]Total Charges and Total Costs are reported in millions in the Hospital-Quarter Level and Hospital-Year Level specifications.

Appendix Table 6: Robustness to Included States

	(1) Uninsured	(2) Length of Stay	(3) Log Length of Stay	(4) Emergency Admit	(5) Procedures	(6) Total Charges	(7) Log Total Charges	(8) Total Costs	(9) Log Total Costs	(10) Charges per Day	(11) Costs Per Day
All States											
MA*After	-0.0182	-0.0577	-0.0019	-0.0324	-0.0568	-1,985	-0.0559	-210	-0.0050	-387	-38
	[-0.0246,-0.0119]***	[-0.1037,-0.0117]**	[-0.0091,0.0053]	[-0.0465,-0.0182]***	[-0.0811,-0.0325]***	[-2493,-1477]***	[-0.0801,-0.0318]***	[-386,-33]**	[-0.0319,0.0220]	[-462,-313]***	[-65,-10]***
N (Nonelderly)	21,181,718	21,228,009	21,228,009	21,228,683	21,228,683	20,813,027	20,813,027	17,622,662	17,622,662	20,812,808	17,622,498
Mean MA Before	0.0643	5.4256	1.4267	0.3868	1.6186	15,862	9.0142	7,479	8.3195	2,950	1,412
Mean Non-MA Before	0.0791	5.0770	1.3552	0.3591	1.4761	18,706	9.1275	6,635	8.1536	3,600	1,281
Mean MA After	0.0431	5.2344	1.4180	0.3604	1.7236	19,056	9.2059	8,191	8.4323	3,555	1,555
Mean Non-MA After	0.0866	5.0762	1.3563	0.2780	1.5777	22,793	9.3162	7,433	8.2507	4,419	1,449
Northeast											
MA*After	-0.0228	-0.0086	-0.0013	-0.0451	-0.0397	-2,574	-0.1044	-425	-0.0541	-459	-67
	[-0.0330,-0.0126]***	[-0.0622,0.0451]	[-0.0071,0.0045]	[-0.0747,-0.0155]***	[-0.0715,-0.0078]**	[-3468,-1681]***	[-0.1395,-0.0692]***	[-753,-97]**	[-0.1106,0.0024]*	[-626,-292]***	[-110,-24]***
N (Nonelderly)	3,936,868	3,937,168	3,937,168	3,937,247	3,937,247	3,932,659	3,932,659	3,300,241	3,300,241	3,932,613	3,300,201
Mean Non-MA Before	0.0790	5.6529	1.4163	0.4281	1.6249	20,860	9.2616	7,068	8.2361	3,736	1,251
Mean Non-MA After	0.1035	5.5949	1.4162	0.3155	1.7617	24,365	9.4722	8,495	8.4293	4,409	1,483
New England											
MA*After	-0.0114	-0.0088	0.0058	-0.0390	-0.0296	-1,655	-0.0497	-471	-0.0191	-330	-97
	[-0.0163,-0.0066]***	[-0.2569,0.2393]	[-0.0081,0.0197]	[-0.0580,-0.0200]***	[-0.1016,0.0425]	[-3673,364]*	[-0.1312,0.0319]	[-1855,912]	[-0.1236,0.0853]	[-541,-119]**	[-251,58]
N (Nonelderly)	1,193,121	1,193,462	1,193,462	1,193,474	1,193,474	1,190,961	1,190,961	1,099,489	1,099,489	1,190,960	1,099,489
Mean Non-MA Before	0.0450	5.3462	1.3890	0.3337	1.4786	16,573	9.0701	7,488	8.3148	3,050	1,402
Mean Non-MA After	0.0511	5.2521	1.3891	0.2719	1.5366	18,608	9.2198	8,675	8.4791	3,607	1,660
All, No ME, VT, CA											
MA*After	-0.0182	-0.0599	-0.0028	-0.0331	-0.0592	-1,989	-0.0605	-196	-0.0044	-387	-34
	[-0.0253,-0.0110]***	[-0.1105,-0.0093]**	[-0.0107,0.0051]	[-0.0488,-0.0174]***	[-0.0859,-0.0325]***	[-2550,-1428]***	[-0.0854,-0.0356]***	[-384,-9]**	[-0.0338,0.0250]	[-470,-305]***	[-63,-6]**
N (Nonelderly)	18,663,012	18,708,546	18,708,546	18,708,896	18,708,896	18,628,998	18,628,998	15,723,760	15,723,760	18,628,779	15,723,596
Mean Non-MA Before	0.0837	5.0926	1.3606	0.3653	1.4660	17,690	9.0886	6,542	8.1499	3,409	1,264
Mean Non-MA After	0.0912	5.0758	1.3597	0.2707	1.5554	21,215	9.2712	7,318	8.2459	4,144	1,428
25 Most Insured											
MA*After	-0.0182	-0.0516	0.0031	-0.0304	-0.0791	-879	-0.0222	-81	0.0205	-242	-30
	[-0.0253,-0.0110]***	[-0.1036,0.0004]*	[-0.0043,0.0105]	[-0.0680,0.0071]	[-0.1313,-0.0270]***	[-2025,268]	[-0.0701,0.0257]	[-499,338]	[-0.0347,0.0756]	[-393,-90]***	[-77,17]
N (Nonelderly)	18,663,012	6,925,862	6,925,862	6,925,953	6,925,953	6,867,629	6,867,629	6,356,972	6,356,972	6,867,594	6,356,944
Mean Non-MA Before	0.0837	4.9274	1.3424	0.3300	1.4804	15,410	8.9738	6,582	8.1711	3,054	1,309
Mean Non-MA After	0.0912	4.8143	1.3217	0.2449	1.5021	16,672	9.0699	6,829	8.2360	3,512	1,456

95% asymptotic CI clustered by state shown, followed by 95% CI obtained through block bootstrap by state, 1000 reps. ***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zerc

All specifications and means are weighted using discharge weights. Specifications are analogous to the preferred specification from Table 1

Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.

"All States" sample includes all states in the 2004-2007 NIS. Does not include AL, AK, DE, ID, LA, MS, MT, ND, NM, PA. Data is available on all 40 states each year from 2004-2007 except ME (2007 only), OK (not 2004), VA (not 2005), and WY (2007 only)

"New England" sample includes MA, CT, NH, VT, ME, RI. "Northeast" sample includes New England, NY, NJ, and PA (not in NIS).

"25 Most Insured" includes the top half of states in terms of initial levels of insurance from the CPS. In decreasing order of insurance: MN, HI, IA, NH, ME, WI, MA, VT, KS, RI, NE, MI, ND (not in NIS), CT, PA (not in NIS), OH, M0, SD, DE (not in NIS), VA, MD, TN, KY, UT

Appendix Table 7: Gains in Uninsurance By Initial Levels of Uninsurance

	All Hospitals	Low Uninsured Hospitals	High Uninsured Hospitals
	(1)	(2)	(3)
Dependent Variable:	**Uninsured**	**Uninsured**	**Uninsured**
Hospitals in Sample Before Reform Only			
Ma*After	-0.0119	-0.0110	-0.0168
	[-0.0172,-0.0066]***	[-0.0143,-0.0076]***	[-0.0253,-0.0083]***
N (All Ages)	29,099,589	13,757,672	15,341,917
N Hospitals	2,315	1,157	1,158
N Hospitals in MA	40	29	11
R Squared	0.0623	0.0075	0.0529
Mean MA Before	0.0409	0.0303	0.0635
Mean Non-MA Before	0.0543	0.0210	0.0848
Mean MA After	0.0291	0.0273	0.0344
Mean Non-MA After	0.0654	0.0263	0.0952
Hospitals in Sample Before and After Reform Only			
Ma*After	-0.0122	-0.0113	-0.0172
	[-0.0174,-0.0071]***	[-0.0149,-0.0078]***	[-0.0254,-0.0091]***
N (All Ages)	12,324,325	5,432,872	6,891,453
N Hospitals	658	315	343
N Hospitals in MA	18	12	6
R Squared	0.0506	0.0086	0.0373
Mean MA Before	0.0466	0.0308	0.0466
Mean Non-MA Before	0.0873	0.0206	0.0873
Mean MA After	0.0344	0.0273	0.0344
Mean Non-MA After	0.0952	0.0263	0.0952
Hospitals in Sample In All Possible Quarters Only			
Ma*After	-0.0164	-0.0101	-0.0239
	[-0.0351,0.0024]*	[-0.0192,-0.0010]**	[-0.0497,0.0019]*
N (All Ages)	1,649,854	565,925	1,083,929
N Hospitals	52	21	31
N Hospitals in MA	6	3	3
R Squared	0.0131	0.002	0.0101
Mean MA Before	0.0356	0.0301	0.0469
Mean Non-MA Before	0.0526	0.0298	0.0609
Mean MA After	0.0273	0.0247	0.0338
Mean Non-MA After	0.0609	0.0346	0.0702

95% asymptotic CI clustered by state shown, followed by 95% CI obtained through block bootstrap by state, 1000 reps.
***99% asymptotic CI does not include zero, **95% asymptotic CI does not include zero, *90% asymptotic CI does not include zero.
All specifications and means are weighted using discharge weights.
Specifications are analogous to the preferred specification from Table 1.
Source: HCUP NIS 2004-2007 authors' calculations. See text for more details.
Low Uninsured Hospitals have less than median uninsurance of 0.039812 before reform.