NBER WORKING PAPER SERIES

THE IMPACT OF HEALTH CARE REFORM ON HOSPITAL AND PREVENTIVE CARE: EVIDENCE FROM MASSACHUSETTS

Jonathan T. Kolstad
Amanda E. Kowalski

Working Paper 16012
http://www.nber.org/papers/w16012

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2010

This paper has been updated to include newly-available data that extend our analysis for an additional 15 months after the reform. Andrew Maleki, Doug Norton, Aditi Sen and Swati Yanamadala provided excellent research assistance. C. Lanier Benkard, Joseph Doyle, Amy Finkelstein, Sherry Glied, Dana Goldman, Jonathan Gruber, Kate Ho, Jill Horwitz, Rob Huckman, Kosali Simon, Jonathan Skinner, Erin Strumpf, Ebonya Washington, and Heidi Williams provided helpful feedback. We thank the Schaeffer Center at USC. We also thank seminar participants at the University of Connecticut, Columbia University, the University of Illinois, the University of Lausanne, Case Western, Rice/University of Houston, the NBER Summer Institute, the Louis and Myrtle Moskowitz Workshop on Empirical Health Law and Business at the University of Michigan, and the 2nd HEC Montreal IO and Health Conference. Mohan Ramanujan and Jean Roth provided invaluable support at the NBER. Remaining errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The Impact of Health Care Reform On Hospital and Preventive Care: Evidence from Massachusetts
Jonathan T. Kolstad and Amanda E. Kowalski
NBER Working Paper No. 16012
May 2010
JEL No. H22,H51,I11,I28,I38

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. Although previous researchers have studied the impact of expansions in health insurance coverage among the indigent, children, and the elderly, the Massachusetts reform gives us a novel opportunity to examine the impact of expansion to 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, 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 utilization patterns. Using the Current Population Survey, we find that the reform increased insurance coverage among the general Massachusetts population. Our main source of data is a nationally-representative sample of approximately 20% of hospitals in the United States. Among the population of hospital discharges in Massachusetts, the reform decreased uninsurance by 36% relative to its initial level. We also find that the reform affected utilization patterns by decreasing length of stay and the number of inpatient admissions originating from the emergency room. Using new measures of preventive care, we find some evidence that hospitalizations for preventable conditions were reduced. The reform affected nearly all age, gender, income, and race categories. We also examine costs on the hospital level and find that hospital cost growth did not increase after the reform in Massachusetts relative to other states.

Jonathan T. Kolstad
The Wharton School
University of Pennsylvania
108 Colonial Penn Center
3641 Locust Walk
Philadelphia, PA 19104-6218
jkolstad@wharton.upenn.edu

Amanda E. Kowalski
Department of Economics
Yale University
37 Hillhouse Avenue
Room 32, Box 208264
New Haven, CT 06520
and NBER
amanda.kowalski@yale.edu

1 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. In light of both reforms, it is of great policy importance to understand the impact of a growth in coverage to near-universal levels, unprecedented in the United States. In theory, insurance coverage could increase or decrease the intensity and cost of health care, depending on the underlying demand for care and its impact on health care delivery. Which effects dominate in practice is an empirical question.

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. Currie and Gruber (1996); Card et al. (2008); Finkelstein (2007)). The Massachusetts reform gives us a novel opportunity to examine the impact of a policy that achieved near-universal health insurance coverage among the entire state population. Furthermore, the magnitude of the expansion in coverage after the Massachusetts reform is similar to the predicted magnitude of the coverage expansion in the national reform. 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.

The first question we address is whether the Massachusetts reform resulted in reductions in uninsurance. 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.

After estimating changes in the presence and composition of coverage, we turn to the impact of the reform on hospital and preventive care. We first study the intensity of care provided. Because health insurance lowers the price of health care services to consumers, a large-scale expansion in coverage has the potential to increase demand for health care services, the intensity of treatment, and cost. Potentially magnifying this effect are general equilibrium shifts in the way care is supplied due to the large magnitude of the expansion (Finkelstein (2007)). Countervailing this effect is the monopsonistic role of insurance plans in setting prices and quantities for hospital services. To the extent that health reform altered the negotiating position of insurers vis a vis hospitals, expansions in coverage could actually reduce total demand, intensity of treatment, or costs. The existence of insurance itself can also alter the provision of care in the hospital directly (e.g. substitution towards services that are reimbursed). Achieving near-universal insurance could alter length of stay and other measures of services intensity though 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 pool of patients with more homogeneous coverage (Glied and Zivin (2002)). Given these competing hypotheses, expanded insurance coverage could raise or lower the intensity of care provided.

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 must provide at least some care, without regard to insurance status, the ER is a potentially important point of access to hospital care for the uninsured.[1] When the ER is the primary point of entry into the hospital, changes in admissions from the ER can 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 or as drug prescriptions); 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 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.

We also study the impact of insurance on access to care outside of the inpatient setting. Using a

[1] Under the Emergency Medical Treatment and Labor Act (EMTALA), hospitals must provide stabilizing care and examination to people who arrive in the ER for an emergency condition without considering whether a person is insured or their ability to pay.

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. We augment this analysis with data on direct measures of access to and use of outpatient and preventive care.

Finally, we turn to the impact of the reform on the cost of hospital care. We examine hospital-level measures of operating costs (e.g. overhead, salaries, and equipment) that include both fixed and variable costs. This allows us to jointly measure direct effects of insurance on cost – the relative effect of changing the out of pocket price – as well as the potential for quality competition at the hospital level. In the latter case, hospitals facing consumers who are relatively 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 lead to 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 (Robinson and Luft (1987)).[2] The impact of all of these effects would be increased hospital costs as coverage approaches near-universal levels.

Our analysis relies on three 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). In addition, we augment our study of access and preventive care using data from the Behavioral Risk Factor Surveillance System (BRFSS).

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

[2] The resulting equilibrium is akin to the socially excessive level of fixed costs associated with free entry (Cutler et al. (2010); Mankiw and Whinston (1986)) though information asymmetries and insurance can exacerbate these problems.

section, we present the difference-in-difference results for the impact of the reform on insurance coverage and hospital and preventive care. 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.

2 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 et al. (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.[3] 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 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 Mas-

[3] 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)). In April 2010, Governor Deval Patrick's administration used existing regulatory authority for the first time to deny premium increases in the individual / small group market. In August 2010, additional legislation established open seasons in this market.

sachusetts 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 et al. (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.[4] Our main results, however, focus on administrative data from hospitals.

3 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 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,

[4]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.

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.[5] 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 2008. Our full sample includes a total of 36,362,108 discharges for individuals of all ages. An 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 quarterly instead of annually. Relative to the original version of this paper (Kolstad and Kowalski (2010)), we extend our analysis by an additional 15 months after the reform using data that recently became available. The reform was passed in April 2006, and our data now begin at the start of 2004 and extend through the end of 2008.

Because some aspects of the reform, 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. To be conservative, we define the *After* reform period to include 2007 Q3 and later. The *After* period represents the time after July 1, 2007, when one of the most salient features of the reform, the individual health insurance

[5] "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" (Agency for Healthcare Research and Quality (2004 – 2007b)). 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.

mandate, took effect. 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. The *Before* period includes 2004 Q1 through 2006 Q2.

Unfortunately, Massachusetts did not provide Q4 data to the NIS in 2006 or 2007.[6] To address this limitation, we drop all data from all states in 2006 Q4 and 2007 Q4.[7] We account for seasonal trends by including a fixed effect for each quarterly time period.

In total, from 2004-2008, the data cover 42 states – Alabama, Alaska, Delaware, Idaho, Mississippi, Montana, North Dakota, and New Mexico 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 3,090 unique hospitals, with 48 in Massachusetts.[8] 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. Although it would be interesting to examine the reform controlling for individual fixed effects, individual identifiers are not available.

4 Difference-in-Difference Empirical Results

4.1 Impact on Uninsurance

4.1.1 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. 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

[6] Potential users of these data should note that to address this limitation, the NIS relabeled some data from the first three quarters of the year in 2006 and 2007 MA as Q4 data. Using information provided by NIS, we recovered the unaltered data for use here.

[7] In Kolstad and Kowalski (2010), we did not drop data from other states in 2006 Q4 and 2007 Q4, but we do here so that we can measure hospital fixed effects equally well in Massachusetts and other states. This change accounts for the very small discrepancy between the $MA^{*}During$ coefficients in this paper and in the previous version.

[8] With few exceptions, if a hospital is in the data in a given year, it is in the data for all available quarters of that year.

plans, but they were eligible for Medicaid expansions if they met the income eligibility criteria).[9]

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%.[10] This increased to a mean coverage level of 93.8% in the 2008-2009 CPS.[11] 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. In this time period, Massachusetts had the seventh highest level of coverage among the nonelderly in the US. It was 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 this comparison of 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. The estimated reduction in nonelderly uninsurance

[9]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 (n.d.), Gray et al. (2006)).

[10]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.

[11]Results from 2007 are difficult to interpret because the reform was in the midst of being implemented in March, when the CPS survey was taken. Medicaid expansions had occurred at that point but the individual mandate was not implemented until July 2007. We thus focus on the period that was clearly before the full reform – CPS March supplement answers from 2004-2006 – compared to 2008-2009. Note that we use more precise definitions of the periods before, during, and after the reform in the NIS, as described below. We have made these definitions as comparable as possible across all data sets.

of 5.7 percentage points represents a 48% reduction relative to the pre-reform rate of nonelderly uninsurance in Massachusetts. In Appendix Tables 2-4 we present estimates of the decline in uninsurance for each age, gender, income, and race category using the CPS. We discuss these results when we consider the incidence of the reform.

To some, the decrease in uninsurance experienced by Massachusetts may appear small. To put this in perspective, the national reform targets a reduction in uninsurance of a similar magnitude. The Centers for Medicare and Medicaid Services, Office of the Actuary, National Health Statistics Group, predicts a decrease in uninsurance of 7.1 percentage points nationally from 2009 to 2019 (Truffer et al. (2010)).

4.1.2 Regression Results on the Impact on Uninsurance

Using the NIS data, we begin by estimating a simple difference-in-difference specification. Our primary estimating equation is:

$$\begin{aligned} Y_{dht} = {} & \alpha + \beta(MA^{*}After)_{ht} + \gamma(MA^{*}During)_{ht} \\ & + \sum_{h} \rho_h (Hosp = h)_h + \sum_{t} \phi_t \, YearQuarter_t + X'_{dht}\delta + \epsilon_{dht} \end{aligned} \quad (1)$$

where Y is an outcome variable 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 also include hospital and quarterly time fixed effects. Thus, identification comes from comparing hospitals to themselves over time in Massachusetts compared to other states, after flexibly allowing for seasonality and trends over time. We include hospital fixed effects to account for the fact that the NIS is an unbalanced panel of hospitals. Without hospital fixed effects, we are concerned that change in outcomes could be driven by changes in the sample of hospitals in either Massachusetts or control states (primarily the prior since the sample is nationally representative but is not necessarily representative within each state) after the reform.[12] Our preferred specification, which includes time and hospital fixed effects, is the most conservative model in our view.[13]

For each outcome variable of interest we also estimate models that incorporate a vector X of patient demographics and other risk adjustment variables. We do not control for these variables in our main specifications because we are interested in measuring the impact of the 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. Beyond our main specifications, the impact of the reform on outcomes holding the patient population fixed is also highly relevant. For this reason, in other specifications, we incorporate state-of-the-art risk adjusters, and we present a number of specifications focused on understanding changes in the patient composition. We return to this in more detail below. In general, however, we find that though there is some evidence of selection, it is not large enough to alter the robustness of our findings with respect to coverage or most outcomes.

We use linear probability models for all of our binary outcomes. Because our specifications are nearly saturated in the independent variables, our coefficients are very similar to unreported average marginal effects from probit models.[14] Under each coefficient, we report asymptotic 95% confidence intervals, clustered to allow for arbitrary correlations between observations within a state. Following Bertrand et al. (2004), we also report 95% confidence intervals obtained by block bootstrap by state.[15]

In addition to the specifications we present here, we consider a number of robustness checks to investigate the internal 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

[12]Restricting the sample to the balanced panel of the 52 hospitals that are in the sample in all possible quarters (2004 Q1 to 2008 Q4, excluding 2006 Q4 and 2007 Q4) eliminates approximately 98% of hospitals and 97% of discharges, likely making the sample less representative, so we do not make this restriction in our main specifications. However, in the last panel of Appendix Table 7, we present our main specifications using only the balanced panel, and the results are not statistically different from the main results.

[13]It is possible that insurance coverage changes *which* hospital people visit, in which case the bias from the use of hospital fixed effects would be of ambiguous sign. However, we are not able to investigate this claim since we do not have longitudinal patient identifiers. Absent differential selection into specific hospitals based on insurance status, we believe that the model with hospital fixed effects is the most conservative.

[14]This is consistent with a general phenomenon. See Angrist and Pischke (2009).

[15]See Appendix A 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.

these results in Appendix B. In Figures 2 and 3, 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.[16] 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. When we formalize this visual analysis in results not reported, we find slightly different trends in Massachusetts, some with statistical significance. However, the magnitude of these effects is generally small relative to the *MA*After* coefficients for each outcome. Taken together, this evidence suggests that our estimates are unlikely to be driven by differential pre-reform trends in Massachusetts.

4.1.3 Effects on the Composition of Insurance Coverage among Hospital Discharges

In this section, we investigate the effect of the Massachusetts reform on the level and composition of health insurance coverage in the sample of hospital discharges. 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 equation (1) separately for each coverage type and report the results in columns 1 through 5 of Table 1. 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 full sample and for the elderly only in Table 6.

Column 1 presents the estimated effect of the reform on the overall level of uninsurance. We find that the reform led to a 2.31 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. Since the model with fixed effects obscures the main effects of *MA* and *After*, we also report mean coverage rates in Massachusetts and other states before and after the

[16]This normalization obscures initial level differences in Massachusetts and other states that could alter the visual comparison of trends.

reform. The estimated impact of Chapter 58 represents an economically significant reduction in uninsured discharges of roughly 36% (2.31/6.43) of the Massachusetts pre-reform mean. We present coefficients on selected covariates from this regression in column 1 of Appendix Table 5.

We see from the difference-in-difference results in column 2 of Table 1 that among the nonelderly hospitalized population, the expansion in Medicaid coverage was larger than the overall reduction in uninsurance. Medicaid coverage expanded by 3.89 percentage points, and uninsurance decreased by 2.31 percentage points. Consistent with the timing of the initial Medicaid expansion, the coefficient on $MA^{*}During$ suggests that a 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 crowded out private coverage in the hospital, which decreased by 3.06 percentage points. The risk-adjusted coefficient in the last row of column 2 suggests that even after controlling for selection into the hospital, our finding of crowd out persists. All of these effects are statistically significant at the 1% level.

To further understand crowd out and the incidence of the reform on the hospitalized population relative to the general population, we compare the estimates from Table 1 – coverage among those who were hospitalized – with results from the CPS – coverage in the overall population. 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."[17] Thus the estimated impact on Medicaid is actually the combined effect of expansions in traditional Medicaid with increases in CommCare and CommChoice.[18] Medicaid expansions are larger among the hospital discharge population than they are in the CPS – a 3.89 percentage point increase vs. a 3.50 percentage point increase, respectively. Furthermore, the CPS

[17]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.

[18]Since the CPS coded CommChoice plans as Medicaid, we are concerned that estimated increases in Medicaid coverage in the CPS should could lead to overestimates of crowd out because the estimated Medicaid expansion could include includes individuals who transitioned from private market unsubsidized care to CommChoice unsubsidized care. To investigate this possibility, in unreported regressions, we divide the sample by income to exclude individuals who are not eligible for subsidized care. 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.

coefficient is statistically lower than the NIS coefficient. It is not surprising to see larger gains in coverage in the hospital because hospitals often retroactively cover Medicaid-eligible individuals who had not signed up for coverage. Furthermore, the hospitalized population disproportionately represents poor individuals, and these individuals could have multiple discharges.[19]

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 by a similar amount 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 is in marked contrast with the 3.54 percentage point *decrease* in private coverage that we observe in the NIS.

Returning to the NIS, we consider changes in coverage beyond private and Medicaid. Our results also indicate a statistically significant change in the number of non-elderly covered by Medicare. The magnitude of the effect, however, is quite small both in level of coverage and in change relative to the baseline share of non-elderly inpatient admissions covered by Medicare. *Other* coverage, the general category that includes other types of government coverage including CommCare, increased by a statistically significant 1.06 percentage points. We restrict the dependent variable to include only CommCare in specification 6. By definition, CommCare coverage is zero outside of Massachusetts and before the reform. CommCare increased by 1.24 percentage points. The coefficient is larger than the overall increase in *Other* coverage, though the difference in the coefficients is not statistically significant.[20] As reported in specification 7, the probability of having missing coverage information

[19] As shown in Table 7, people from the poorest 25% of zip codes account for 28% of hospital discharges. 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. In contrast, 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 care, instead of retroactively taking up Medicaid after a hospitalization.

[20] We would have liked to have used Massachusetts residents that were prohibited from obtaining coverage through

also increased after the reform, but this increase was small relative to the observed increases in coverage.

4.2 Impacts on Health Care Provision

Having established the impact of the Massachusetts reform on coverage, we next turn to our primary focus: understanding the impact of achieving near-universal health insurance coverage on health care delivery and cost. To do so, we estimate equation (1) with a set of dependent variables that capture changes in the way in which health care is delivered and consumed. This section is divided into four subsections, each intended to address key areas of health care delivery that might be affected by health insurance coverage. In our next section, 4.2.1, we consider the impact of health insurance expansion on the extensive margin decision to seek care. In section 4.2.2, we study changes in the intensive margin choice of intensity of services provided, conditional on admission to the hospital. In the third subsection, we study the impact of health insurance coverage on outpatient and preventive care. Finally, we turn to studying the impact of the Massachusetts reform on hospital costs.

The relevant results for this section are contained in Tables 1 through 5. Tables 1, 3, and 4 report results on the discharge level, following directly from equation (1). Tables 2 and 5, on the other hand, report results on the hospital-time period level. Table 2 presents results that are aggregated to the hospital-quarter level because we need to aggregate individual discharges to examine total discharges. Table 5 presents hospital cost results on the hospital-year level. Our cost measures, which we describe in more detail below, are based on data collected on an annual basis, allowing us only to identify the model from changes from year to year within a hospital.

4.2.1 Impact on Hospital Volume and Patient Composition

One potential impact of the reform could be to increase the use of inpatient hospital services. Whether more people accessed health insurance after the reform is of intrinsic interest as this implies a change in welfare due to the policy (e.g. an increase in moral hazard through insurance

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. Furthermore, the individual mandate could still have had an impact on those prohibited from obtaining coverage through CommCare, for example if it encouraged enrollment of people previously eligible but not enrolled in CHAMPUS.

or a decrease in *ex ante* barriers to the hospital through insurance). Beyond this, changes in the composition of patients present an important empirical hurdle to estimating the causal 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 differed in underlying health status, changes in treatment intensity could reflect this, rather than actual changes in the way care is delivered. 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 and compare our results to specifications without controls.

In Table 2, 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 2, which includes hospital and quarterly fixed effects to mitigate the impact of changes in sample composition, the coefficient on $MA^{*}After$ in column 1 indicates that the number of quarterly discharges for hospitals in Massachusetts was unchanged relative to other states following the reform. The coefficient estimate of 19 is small relative to the pre-reform quarterly discharge level of 5,616, and it is not statistically significant. In column 2, we re-estimate the model with the log of total discharges as the dependent variable to account for any skewness in hospital size. The coefficient on $MA^{*}After$ in this specification also indicates that the reform had no impact on the total volume of discharges. Columns 3 to 6 break down changes in discharges by age relative to 65. Among these subgroups we find no statistically significant change in total elderly or nonelderly discharges in either levels or logs. 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.

To deal with changes in the patient population directly, we control for observable changes in the health of the patient pool using six 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, All-Patient Refined (APR)-DRGs, and All-Payer Severity-adjusted (APS)-DRGs. We discuss these measures in depth in Appendix C. 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 specifications 8 to 13 of Table 2 we estimate our model with a subset of our measures of patient severity as the dependent variable. For this exercise, we focus on the six sets of severity measures that are simplest to specify as outcome variables. This allows us to observe some direct changes in the population severity in Massachusetts after the reform. These results present a mixed picture of the underlying patient severity. For four of the six measures, we find no significant change in severity. Two of the severity measures, however, saw statistically significant changes after the reform. The results in specification 8 suggest that the average severity, measured by the Charlson score, *increased* following the reform. The model of APS-DRG charge weights suggests the opposite. Taken together, these results and the lack of any change in total discharges are not indicative of a consistent pattern of changes in the patient population within a given hospital in Massachusetts after the reform relative to before relative to other states.

Despite this general picture, and in light of the results in columns 8 and 13, for all of our outcome variables we estimate the same model incorporating the vector of covariates X, which flexibly controls for all risk adjusters simultaneously. In general, our results are unchanged by the inclusion of these controls – consistent with the small estimated impact of the reform on the individual severity measures. If anything, we find that the main results are strengthened by the inclusion of covariates as we would expect with increased severity after the reform.

In column 7 of Table 2, we investigate the possibility that either hospital size increased or sampling variation led to an observed larger size of hospitals after the reform relative to before the reform by 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. This 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.

4.2.2 Impact on Resource Utilization and Length of Stay

Moving beyond the question of the extensive margin decision to go to the hospital or to admit a patient to the hospital, 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 – that is, if moral hazard or income effects dominate. Alternately, 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 – that is, if insurer bargaining effects dominate. Length of stay may also decline if insurance alters treatment decisions, potentially allowing substitution between inpatient and outpatient care or drugs that would not have been feasible without coverage. To investigate these two effects, we estimate models of length of stay following equation (1) with both levels and logs of the dependent variable.

The results in specifications 8 and 9 Table 1 show that length of stay decreased by 0.05 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 0.12 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 unreported results, we also estimate models of the probability a patient exceeds specific length of stay cutoffs. The results validate the findings that compare level and log outcomes (column 1 and 2). Patients were statistically significantly roughly 10 percent less likely to stay beyond 13 and 30 days in Massachusetts after the reform. The probabilities of staying beyond shorter cutoffs (2, 5 and 9 days) were unchanged. The results suggest that patients were significantly more likely to stay at least 3 days though the magnitude of the coefficient suggests an increase of only 1 percent relative to the baseline share.

To address the concern that our estimated reduction in length of stay was driven by differential selection of healthier patients into the hospital after the reform in MA, we report results controlling for risk adjustment variables in the last row of all specifications in Table 1. 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.

One plausible mechanism for the decline in length of stay is limited hospital capacity. As with patient severity, capacity constraints are interesting in their own right, but they could bias our estimates of other reform impacts. Capacity itself is endogenous and may have 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 (1980) 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).[21] Because beds are a fixed cost, capacity and utilization are a source of scale economies for hospitals. If hospitals seek to improve efficiency (potentially when faced with 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.

Our results can provide some insight into whether changes in length of stay seem to be related to capacity by comparing the additional capacity that resulted from the decrease 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 7 of Table 6. The new, lower average length of stay is $5.88 - 0.06 = 5.82$ days. This would make room for an extra $(0.06 * 5,616)/(5.88 - 0.06) = 58$

[21] Joskow (1980) formally models the stochastic arrival rate as a Poisson process which yields a standard deviation equal to $\sqrt{\text{ADC}}$.

discharges. An extra 58 discharges exceeds the estimated increase of 19 discharges (from column 1 of Table 2). We note, however, that the upper bound of the 95% confidence interval for the estimated change in total discharges is greater than 58. Thus, decreased length of stay could 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 change in the supply-side constraint of 58.[22]

4.2.3 Impact on Access, Prevention, Quality 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 future cost reductions (Chernew et al. (2007)). 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 decrease 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 10 of Table 1, 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 2.02 percentage point reduction in the fraction of admissions from the emergency room. Relative to an initial mean in Massachusetts of 38.7 percent this estimate represents a decline in inpatient admissions originating in the emergency room of 5.2 percent. The risk-adjusted estimate reported in the

[22] 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.

bottom row of specification 10 is very similar.[23]

As a further specification check, we decompose the effect by zip code income quartile in section 5. To the extent that income is a proxy for *ex ante* coverage levels, we expect larger declines in inpatient admissions originating in the ER among relatively poorer populations. The results in section 5 conform to our expectations. We find that the reduction in emergency admissions was particularly pronounced among people from zip codes in the lowest income quartile. As reported in the bottom panel of Table 7, the coefficient estimate suggests a 12.2 percent reduction (significant at the 1 percent level) in inpatient emissions from the emergency room. The effect in the top two income quartiles, on the other hand, is not statistically significantly different from zero (coefficient estimate of -0.0107 and 0.0098 for the 3rd and 4th income quartiles respectively). Taken together, these results suggest that the reform did reduce use of the ER as a point of entry into inpatient care. This effect was driven by expanded coverage, particularly among lower income populations.

In addition to the use of the ER, we are interested in measuring whether providing health insurance directly affects access to and use of preventive care or quality of care provided. 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).[24] See Appendix D for more details on these measures. Each set of quality indicators includes several outcomes developed by doctors and health services researchers 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.

To investigate the impact of the reform on prevention, we use the PQI measures, which were developed as a means to measure the quality of outpatient care using inpatient data, which are more

[23]We note that these results differ somewhat from discussions in media and policy circles (Kowalczyk (2010)). Our analysis differs for a few reasons. First, we are focused solely on inpatient admissions from the ER. While this limits the scope of our results, it allows us to focus on a population of particular importance, the relatively sick and costly populations who, ultimately, receive care in the hospital. A second issue with the existing discussion of Massachusetts ER usage is the lack of a control group. Our results take into account trends in ER usage nationwide that are likely to be changing over time. Using this approach we are better able to account for changes in ER usage unrelated to the reform that affect MA, though we note that our findings do not appear to be driven by differential trends in states other than Massachusetts (see Figure 2).

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

readily available. The appearance of certain preventable conditions in the inpatient setting, such as appendicitis that results in perforation of the appendix, or diabetes that results in lower extremity amputation, 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.[25] One concern in using these measures over a relatively narrow window of time is 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 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.[26] Interpreted with different emphasis, these measures also capture impacts on health through averted hospitalizations. 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 3 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.[27] We find little overall effect in the base specification.[28] 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, 3 exhibit a statistically significant decrease, 9 exhibit no statistically significant change, and 1 exhibits a statistically significant increase. Taken together, these results suggest that there

[25]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 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.

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

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

[28]We note that in previous work with a shorter post-reform period (Kolstad and Kowalski (2010)) we found larger effects on the PQI measures. The decline in the unadjusted estimate may suggest that there was only a short term impact of the reform on preventive care. Future work that incorporates a longer time period and panel data would be better able to model such dynamics.

may have been small impacts on preventive care but little overall effect in reducing the number of preventable admissions.

We also estimate the model including controls for severity. If the impact of insurance or outpatient care on the existence of a PQI varies in patient severity, it is possible that the small estimated effects mask a compositional effect of the inpatient population after the reform. That is, if relatively severe patients are more likely to be hospitalized with a PQI, regardless of the outpatient care they receive, then estimates that hold the patient population fixed provide a better estimate for the impact of the reform on preventive care. These results are contained in the right panel of Table 3. For the overall PQI measure, the coefficient on $MA^{*}After$ is -0.0023 and is statistically significant at the 1 percent level. Compared to the baseline rate of PQIs, this corresponds to a decline of 2.7 percent in preventable admissions. Results for the individual measures tell a similar story. Taken together, these results suggest that there was a small overall effect of the reform on preventable admissions but, holding the severity of the population fixed, there were significant declines. We find that, if anything, the inpatient population was more severe after the reform. Thus comparing the two coefficients – with and without risk adjustment – suggests that the effect of the reform on reducing preventable admissions was largest among relatively less severe patients.

To supplement our analysis of preventive care, we also estimate models of prevention using data from the BRFSS for 2004-2009. The BRFSS is a state-based system of health surveys that collects information on health risk behaviors, preventive practices, and health care access.[29] The results are presented in Table 4. Column 1 presents the differences-in-differences estimate for the impact of reform on those reporting they have health insurance coverage. Consistent with the results from the CPS, we find an increase in coverage in Massachusetts after the reform relative to before relative to other states of roughly 5 percent. The remaining seven columns present results that are relevant to outpatient and preventive care. In column 2, we see a significant increase of 1.26 percent in individuals reporting they had a personal doctor. The reform also led to a decline in individuals reporting they could not access care due to cost by 3.06 percentage points. Columns 4-8 present

[29] The BRFSS is a cross-sectional telephone survey conducted by state health departments on a monthly basis with technical and methodological assistance provided by the Centers for Disease Control (CDC). Sampling follows a multistage design based on random-digit-dialing in order to select a representative sample from the non-institutionalized adult population in each state and territory. Our sample from BRFSS data contain 2,293,672 observations; in the analysis, data are weighted for the probability of selection of a telephone number, the number of adults in a household, and the number of telephones in a household; post-stratification adjustments are also made for non-response and non-coverage of households without telephones.

diff-in-diff estimates for the impact of the reform on a set of direct measures of preventive care. We find little overall impact in the population. The only statistically significant estimate is for the impact of the reform on receiving a flu vaccination.

We next return to the NIS and focus on Patient Safety Indicators – measures of the quality of care provided in the inpatient setting. If achieving near-universal coverage altered the way care was delivered in the hospital setting, we expect to see shifts in patient safety indicators – outcomes that should not be observed if appropriate care is provided. The lower panel of Table 3 presents a full set of PSI measures with and without risk adjusters. One issue in estimating models using the PSIs is the relevant population at risk. Unlike most PQI measures, PSI outcomes are only relevant for a subset of the populations (e.g. obstetric trauma can only occur if a woman is pregnant). Thus, we do not have a single pooled measure of PQIs but instead present the full sample estimated separately among the relative population at risk for each.

The outcomes in Table 3 suggest that, overall, patient safety was improved following the expansion in insurance coverage. Of the 23 PSIs, we find statistically significant improvements for 13, no change for 7 and declines for 3. Not only is the effect of the reform statistically significant on those that improved but they also appear to be economically significant. For example, mortality for low mortality DRGs declined by roughly one third of its baseline level and pressure ulcers was reduced by 36 percent relative to baseline.

In addition to the PQI and PSI measures we also estimate models that represent quality of care for adult and pediatric populations – Inpatient Quality Indicators and Pediatric Quality Indicators. As discussed above, we do not have a strong prior about the impact of coverage expansions on these indicators. Empirical results are correspondingly mixed. Although there were decreases in some measures and increases in others, no general pattern is visible. Appendix Table 6 reports the results for these two measures. The reform was particularly unlikely to improve quality for pediatric patients. 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 as unavoidable pediatric hospitalizations (we find a decrease in preventable hospitalizations among the nonelderly).

4.2.4 Impact on Hospital Costs

In this section, we investigate the impact of the reform on hospital costs. The cost impact, as we discuss above, depends on the relative changes in incentives facing hospitals and physicians in treatment and investment decisions. 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)). Insurers are also able to negotiate lower prices for care and, in the case of managed care plans, address treatment decisions directly through quantity limits (i.e. prior authorization rules that require a physician to get approval from the insurer in order for a procedure to be reimbursed, etc.) (Cutler et al. (2000)). Thus, increases in coverage could lead to a countervailing decrease in cost with insurance coverage. Consequently, it is an empirical question whether increases in health insurance coverage among the hospitalized population will raise or lower cost.

To measure hospital costs directly, 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 the annual total costs of operating the hospital such as overhead costs, salaries, and equipment.[30] 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. Several papers in the economics literature measure total costs at the discharge ratio by deflating total charges by the cost to charge ratio (see e.g. Almond et al. (2010). However, since there is no variation in observed costs at a level finer than the hospital level, estimating such a regression requires the strong assumption that the ratio of costs to charges is the same for all discharges within the hospital. Since we are interested in hospital-level costs, we need not impose this assumption, and we can focus on results

[30] Costs do not include the cost of uncompensated care, which presumably declined with the increase in insurance coverage.

at the hospital level.

Table 5 presents difference-in-difference estimates for the impact of the reform on hospital costs using a variety of specifications. The first column presents estimates in levels. The coefficient estimate of 9.54 is not statistically significantly different from zero. The logarithmic specification in column 2 yields a negative estimate for the impact of the reform on cost, though the coefficient is also insignificant. The difference between the two, though not statistically significant, can be explained in that the logarithmic specification takes into account trends in growth for both treatment and control groups. That is, Massachusetts had a differential trend in cost growth relative to the rest of the country before the reform. The log specification results indicate that this trend was not altered by the expansion in coverage relative to trends before and after the reform in the remainder of the country. The plots in Figure 4, which depict cost trends in Massachusetts and other states for the outcomes in Table 5, show that Massachusetts had a higher rate of cost growth as compared to other states before the reform. After the reform, Massachusetts relative costs appear to be in line with their pre-reform trend.

In the next columns, and in the bottom plots in Figure 4, we model cost per day and discharge, to account for changes in cost that may be due to changes in the intensity or total volume of patients treated. Both results are consistent with the levels regression in column 1 and suggest that cost growth was largely unchanged by the expansion in coverage in Massachusetts.

Decomposing the results by year, however, paints a slightly different picture. In the lower panel of Table 5, we allow the post period effect to differ in 2007 and 2008 (the two years of data we have following the reform). The coefficient for MA*2008 in levels is positive and statistically significant. Given the relatively short period of our study, it is difficult to identify dynamic effects. However, the results do suggest that there may, eventually, have been increased cost levels, despite little rise in the immediate aftermath. The other specifications, however, do not show a significant differential effect on the log, per discharge, or per day cost in 2008 in MA relative to before the reform relative to other states. Thus, taking into account trends or volume and length of stay changes after the reform, we do not find increased cost even a year and a half after the full reform (including the individual mandate) went into effect. Combining the estimates across specifications, our results imply that the Massachusetts reform did not increase the cost of hospital care relative to the baseline trend in cost growth. Thus, the Massachusetts reform did not appear to “bend the cost curve" upward or downward.

5 Direct and Spillover Effects of the Reform

5.1 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. 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 of coverage and not just the expansion in coverage matters. We report the instrumental variable estimates subject to this caveat.

When scaling our reduced form estimates by our first stage coverage estimates, we use two sets of first stage coverage estimates – those from the NIS and those from the CPS. Scaling by the NIS estimate gives the effect of reduced uninsurance *in the hospital* on outcomes, and scaling by the CPS estimate gives the effect of reduced uninsurance *in the population* on hospital outcomes. In a similar context, Anderson et al. (2010) use a bounding exercise to address how changes in population coverage result in changes in hospital coverage because they only observe hospital coverage. Here, rather than using a bounding exercise with its accompanying assumptions, we can simply scale by the population estimate from the CPS to address this issue.

Returning to the estimated coefficients on length of stay and emergency admissions, we combine these results with the estimated coefficient from the NIS uninsurance regression. We find that a one percentage point increase in insurance coverage *in the hospital* decreases length of stay by 0.022 days (0.050/2.31) and decreases emergency admissions by 0.87 percentage points (2.02/2.31).[31] Using the CPS first stage estimates, we find that a one percentage point increase in insurance coverage *in the population* decreases length of stay by 0.009 days (0.050/5.71) and decreases emergency

[31] These estimates are approximate because the two underlying estimates are estimated on slightly different samples due to data availability.

admissions by 0.37 percentage points (2.02/5.71).

5.2 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.

Comparison of the first stage results in the NIS and the CPS for all non-elderly demographic groups suggests that a one percentage point increase in population coverage translated into a 0.4 (2.31/5.71) percentage point increase in coverage in the hospital. If demographic groups gain insurance coverage in the population and in the hospital at the same rate, we expect the ratio of the NIS first stage to the CPS first stage to be the same across demographic groups. However, it is possible that population coverage translates into hospital coverage differently for different demographic groups (e.g. if men have bigger gains in coverage in the hospital than in the population because hospitalized men tend to have lower incomes).[32] Differential changes in hospital coverage relative to population coverage within a demographic group will be reflected in the ratio of the NIS first stage result to the CPS first stage result within that demographic group.

Comparing changes in coverage by age in the NIS in Table 6 and in the CPS in Appendix Table 2, we see that the ratio is generally constant across demographic groups at roughly the population ratio of 0.4. This comparison provides further evidence that the reform itself did not lead to large differential selection into hospitals based on observable characteristics. However, for individuals 27-30 and for individuals 45-54, a one percentage point increase in population coverage translated into a 0.13 (0.022/0.166) and 0.67 (0.028/0.042) percentage point increase in hospital coverage, respectively. Thus, it seems that younger individuals were less likely to gain coverage in the hospital than they were in the population, presumably because younger hospitalized already had higher rates of coverage. The reverse was true for older individuals. To address these differences in the propensity of coverage in the population to translate into coverage in the hospital, we prefer

[32] Differential selection into the hospital by newly insured members of each demographic group could also affect the translation of population coverage into actual coverage. However, we find limited scope for differential selection into the hospital in section 4.2.1.

to use the CPS first stage rather than the NIS first stage to scale our IV estimate within each demographic subgroup.

In Table 6 and Appendix Table 2, we examine changes in coverage by age in the NIS and the CPS, respectively. As expected, increases in coverage are most pronounced in the nonelderly, but the elderly did experience small gains in coverage, mostly through Medicaid. As 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 in MA in 2008 Q4, 808 elderly discharges, including 466 discharges for age 75+, 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 takeup of CommCare is largest among the near elderly hospitalized population, aged 55-64. All nonelderly age groups experienced a statistically significant decline in private coverage, providing further evidence that public coverage crowed out private coverage within the hospitalized population.[33]

When we turn to outcomes by age in specifications 7 to 9 in Table 6, we see some heterogeneity in estimated effects of length of stay across age groups, with some groups experiencing statistically significant decreases and others experiencing statistically significant increases. Since all groups experienced expanded coverage in the NIS and the CPS, 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. The NIS and CPS IV estimates for the effect of coverage on inpatient admissions from the ER are uniformly negative for all individuals over 18 and, generally, increase 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 if this is the primary point of access for those without insurance. The

[33] 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.

CPS IV estimate for the impact of a one percentage point increase in population coverage among individuals 27-30 is a 0.13 percentage point reduction in the probability of an inpatient admission originating in the ER. The estimated impact for those from 31-40, 41-54, and 55-64 is a 0.33, 1.22, and 1.12 percentage point reduction respectively.

In the top panel of Table 7 and Appendix Table 3, we report difference-in-difference results for insurance coverage by gender in the NIS and CPS, respectively. From the mean coverage rate in the lower rows, we can see that Massachusetts males were approximately 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 likely to be uninsured after the reform.[34] 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. The IV estimate for the impact of population coverage on admissions from the ER is relatively uniform across the two groups with a predicted reduction of 0.39 and 0.27 percent for a one percent increase in coverage for men and women respectively.

In the lower panel of Table 7 and Appendix Table 3, we report difference-in-difference results for insurance coverage by the income quartile of the patient's zip code.[35] People from the lowest income zip codes are over-represented in hospital discharges, making up 28% 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 experienced no statistically significant change in coverage overall and the only statistically significant effects were on *Other* and CommCare coverage. 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

[34] 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, coverage is often more valuable for women of child-bearing age because childbirth is an expensive and common medical expense.

[35] Income thresholds that determine the quartile of income for each zip code are different in every year. For 2008, the lowest quartile ranged from $1 to $38,999, the second ranged from $39,000 to $48,999, the third ranged from $49,000 to $63,999, and the fourth was greater than or equal to $64,000. 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.

codes of all income quartiles, consistent with heterogeneity in income within zip codes.

Specifications 7 to 9 of Table 7 show changes in outcomes by zip code income quartile. Interestingly, the impact of the reform on both the level and log of LOS is positive and significant for individuals from the poorest zip codes. Individuals from the second income quartile, however, saw statistically significant declines in LOS in both levels and logs. As discussed above, decreases in emergency admissions are particularly pronounced for individuals in the lowest income zip codes, for which Medicaid expansions were the largest. Other outcomes show minimal heterogeneity across the income categories, though this could be due in part to the fact that estimates for costs are based on hospital level cost to charge ratios that could obscure within hospital heterogeneity in costs by income.

Finally, we examine heterogeneity in coverage by race in the NIS in Table 8 and in the CPS in Appendix Table 4. 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, 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 columns 7 to 9 of Table 8, 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 see decreases in length of stay among White patients, length of stay increased among Black, Hispanic, Asian and Native American patients. Admissions from the emergency room declined across most races with the exception of 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 NIS and CPS IV estimates are not proportional across subgroups. This could be evidence for spillovers from insurance coverage in the hospital production function or it could reflect heterogeneity in the underlying impact of insurance on outcomes by subgroup, perhaps by type of coverage. One plausible mechanism for the former 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 an individual's choice to purchase health insurance. Among all of our outcome measures, we find the least 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.

6 Conclusion

In this paper, we show that the Massachusetts health insurance reform expanded coverage among the inpatient hospital population by approximately 36 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 by health status.

We show declines in length of stay and admissions from the emergency room following the reform. Our results also suggest that prevention increased outside of hospitals, resulting in a decline in inpatient admissions for some preventable conditions, reflecting a likely health impact for individuals susceptible to these conditions. In the midst of these gains, we find no evidence that hospital cost growth 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.

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.

References

Agency for Healthcare Research and Quality, *HCUP Nationwide Inpatient Sample (NIS). Healthcare Cost and Utilization Project (HCUP)* April 2004 – 2007. www.hcup-us.ahrq.gov/nisoverview.jsp.

_ , *AHRQ Quality Indicators Software Download.* April 2007. http://www.qualityindicators.ahrq.gov/software.htm.

_ , *Inpatient Quality Indicators Download. AHRQ Quality Indicators.* March 2007. http://www.qualityindicators.ahrq.gov/iqi_download.htm.

_ , *Patient Safety Indicators Download. AHRQ Quality Indicators.* March 2007. http://www.qualityindicators.ahrq.gov/psi_download.htm.

_ , *Pediatric Safety Indicators Download. AHRQ Quality Indicators.* March 2007. http://www.qualityindicators.ahrq.gov/pqi_download.htm.

_ , *Pediatric Safety Indicators Download. AHRQ Quality Indicators.* March 2007. http://www.qualityindicators.ahrq.gov/pdi_download.htm.

Almond, Douglas, Jr. Joseph J. Doyle, Amanda E. Kowalski, and Heidi Williams, "Estimating Marginal Returns to Medical Care: Evidence from At-Risk Newborns," *Quarterly Journal of Economics*, 2010, *125* (2), 591–634.

Anderson, Michael, Carlos Dobkin, and Tal Gross, "The Effect of Health Insurance Coverage on the Use of Medical Services," *NBER Working Paper 15823*, 2010.

Angrist, Joshua D. and Jörn-Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press, 2009.

Bebinger, Martha, "Mission Not Yet Accomplished? Massachusetts Contemplates Major Moves On Cost Containment," *Health Affairs*, 2009, *28* (5), 1373.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How Much Should We Trust Differences-in-Differences Estimates?*," *Quarterly Journal of Economics*, 2004, *119* (1), 249–275.

Birnbaum, Michael and Elizabeth Patchias, "Medicare Coverage for Seniors: How Universal Is It and What Are the Implications?," Paper presented at the annual meeting of the APSA 2008 Annual Meeting, Hynes Convention Center, Boston, Massachusetts, August 28, 2008. http://www.allacademic.com/meta/p281081_index.html.

Card, David, Carlos Dobkin, and Nicole Maestas, "The Impact of Nearly Universal Insurance Coverage on Health Care Utilization: Evidence from Medicare," *American Economic Review*, 2008, *98* (5), 2242–2258.

Charlson, Mary E., Peter Pompei, Kathy L. Ales, and C. Ronald MacKenzie, "A New Method of Classifying Prognostic Comorbidity in Longitudinal Studies: Development and Validation," *Journal of Chronic Diseases*, 1987, *40* (5), 373–383.

Chernew, Michael E., Allison B. Rosen, and A. Mark Fendrick, "Value-Based Insurance Design," *Health Affairs*, 2007, *26* (2), w195.

Currie, Janet and Jonathan Gruber, "Health Insurance Eligibility, Utilization of Medical Care, and Child Health," *The Quarterly Journal of Economics*, 1996, *111* (2), 431–466.

Cutler, David M. and Jonathan Gruber, "Does Public Insurance Crowd Out Private Insurance," *The Quarterly Journal of Economics*, 1996, *111* (2), 391–430.

_ , Mark McClellan, and Joseph P. Newhouse, "How Does Managed Care Do It?," *The RAND Journal of Economics*, 2000, *31* (3), 526–548.

—, Robert S. Huckman, and Jonathan T. Kolstad, "Input Constraints and the Efficiency of Entry: Lessons from Cardiac Surgery," *American Economic Journal: Economic Policy*, 2010, *2* (1), 51–76.

Dafny, Leemore and Jonathan Gruber, "Public Insurance and Child Hospitalizations: Access and Efficiency Effects," *Journal of Public Economics*, 2005, *89* (1), 109–129.

Dranove, David and Mark A. Satterthwaite, "Monopolistic Competition When Price and Quality are Imperfectly Observable," *RAND Journal of Economics*, 1992, *23* (4), 518–534.

Finkelstein, Amy, "The Aggregate Effects of Health Insurance: Evidence from the Introduction of Medicare," *The Quarterly Journal of Economics*, 2007, *122* (1), 1–37.

Glied, Sherry and Joshua Zivin, "How Do Doctors Behave When Some (but Not All) of Their Patients are in Managed Care?," *Journal of Health Economics*, 2002, *21* (2), 337–353.

Gray, Bradford H., Roberta Scheinmann, Peri Rosenfeld, and Ruth Finkelstein, "Aging without Medicare? Evidence from New York City," *Inquiry*, 2006, *43* (3), 211–221.

Gruber, Jonathan and Samuel A. Kleiner, "Do Strikes Kill? Evidence from New York State," *NBER Working Paper 15855*, 2010.

HSS, Inc., *Definitions Manual for All-Payer Severity-adjusted DRG (APS-DRGs) Assignment.* 2003. http://www.hcup-us.ahrq.gov/db/nation/nis/APS_DRGsDefManualV26Public.pdf.

Joskow, Paul L., "The Effects of Competition and Regulation on Hospital Bed Supply and the Reservation Quality of the Hospital," *The Bell Journal of Economics*, 1980, pp. 421–447.

Kaiser Family Foundation, "Side-by-side Comparison of Major Health Care Reform Proposals," March 2010. http://www.kff.org/healthreform/upload/housesenatebill_final.pdf.

Kingsdale, Jon, "Implementing Health Care Reform in Massachusetts: Strategic Lessons Learned," *Health Affairs*, 2009, *28* (4), w588.

Kolstad, Jonathan T. and Amanda E. Kowalski, "The Impact of an Individual Health Insurance Mandate On Hospital and Preventive Care: Evidence from Massachusetts," *NBER Working Paper 16012*, 2010.

Kowalczyk, Liz, "Emergency Room Visits Grow in Mass," *The Boston Globe*, July 2010.

Kowalski, Amanda, "Censored Quantile Instrumental Variable Estimates of the Price Elasticity of Expenditure on Medical Care," *NBER Working Paper 15085*, 2009.

Long, Sharon, "On the Road to Universal Coverage: Impacts of Reform in Massachusetts at One Year," *Health Affairs*, 2008, *27* (4), w270.

Long, Sharon K., Karen Stockley, and Alshadye Yemane, "Another Look at the Impacts of Health Reform in Massachusetts: Evidence Using New Data and a Stronger Model," *American Economic Review*, 2009, *99* (2), 508.

Mankiw, N. Gregory and Michael D. Whinston, "Free Entry and Social Inefficiency," *The RAND Journal of Economics*, 1986, pp. 48–58.

Manning, Willard G., Joseph P. Newhouse, Naihua Duan, Emmett Keeler, Bernadette Benjamin, Arleen Leibowitz, M. Susan Marquis, and Jack Zwanziger, "Health Insurance and the Demand for Medical Care: Evidence from a Randomized Experiment," *The American Economic Review*, 1987, *77* (3), 251–277.

McDonough, John E., Brian Rosman, Fawn Phelps, and Melissa Shannon, "The Third Wave of Massachusetts Health Care Access Reform," *Health Affairs*, 2006, *25* (6), w420.

Pauly, Mark V., "The Economics of Moral Hazard: Comment," *The American Economic Review*, 1968, pp. 531–537.

Quan, Hude, Vijaya Sundararajan, Patricia Halfon, Andrew Fong, Bernard Burnand, Jean-Christophe Luthi, Duncan L. Saunders, Cynthia A. Beck, Thomas E. Feasby, and William A. Ghali, "Coding Algorithms for Defining Comorbidities in ICD-9-CM and ICD-10 Administrative Data."

Robinson, James C. and Harold S. Luft, "Competition and the Cost of Hospital Care, 1972 to 1982," *Journal of the American Medical Association*, 1987, *257* (23), 3241.

The Massachusetts Health Insurance Connector Authority, "Report to the Massachusetts Legislature: Implementation of the Health Care Reform Law, Chapter 58, 2006–2008," 2008. Jon Kingsdale, Executive Director. Available at www.mahealthconnector.org.

Truffer, Christopher J., Sean Keehan, Sheila Smith, Jonathan Cylus, Andrea Sisko, John A. Poisal, Joseph Lizonitz, and M. Kent Clemens, "Health Spending Projections through 2019: The Recession's Impact Continues," *Health Affairs*, 2010, *29* (3), 522.

Yelowitz, Aaron and Michael F. Cannon, "The Massachusetts Health Plan: Much Pain, Little Gain," *Policy Analysis*, January 2010. http://www.cato.org/pub_display.php?pub_id=11115.

Appendices

A Block Bootstrap

Since our main identifying variation is at the state-time level, we follow Bertrand et al. (2004), and block bootstrap our confidence intervals by state. Doing so allows for an arbitrary variance-covariance 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.[36]

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

[36] Difficulties in the block bootstrap procedure aside, we acknowledge that having only one treated state potentially limits the external validity of our results. To address this limitation, we consider level and trend differences between MA and other states, which we discuss in Section 4.1.2.

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.

B 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 7.

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 smaller 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. Similarly, 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 1.59 percentage point reduction in uninsurance. This is statistically smaller than the baseline effect, but it remains 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, Vermont and San Francisco, California, in column 4, we estimate the main specification in the full sample but we exclude 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 from that in the main specification.

In the remaining columns of Appendix Table 7, 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 Appendix Table 8, 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 the second panel. Estimating the average impact of the reform in this subsample, we find that impact of the reform remained virtually unchanged, 1.52 percentage points compared to 1.53. 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. In among hospitals Massachusetts in the sample in the *Before* period, 29 hospitals had uninsurance below the national median, and the remaining 11 hospitals had uninsurance above the national median. We are able to estimates statistically significant impacts on uninsurance in both groups of hospitals, but estimates based on hospitals with below-median uninsurance are more precise.

Estimates from the first column of the third and fourth panels show that the impact of the reform was statistically indistinguishable in hospitals with lower initial rates of uninsurance (a 1.78 percentage point increase in coverage) and hospitals with higher initial rates of uninsurance (a 1.65 percentage point increase in coverage). Although we might have expected that convex cost of coverage expansion would have led to greater reductions in uninsurance in hospitals with initially higher rates of uninsurance, the results are not statistically different from each other. As shown in the second to fourth columns, impacts on length of stay and admissions from the ER were also similar across both groups of hospitals. 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 suggest the results we find in Massachusetts, which had lower initial uninsurance than other states, could be similar to the impact of the national reform.

C Risk Adjustment

Selection into hospitals after the reform in Massachusetts is an outcome of interest in its own right, which we address as a complement to our analysis, but to examine causal changes in other outcomes, we control for characteristics of the patient pool. To do so, we use six[37] 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, 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 of each measure, we include all risk adjustment variables in the same specification. We run all of our regressions on the discharge level, as opposed to the hospital level, to capture the interactions between measures on the discharge 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 Gruber and Kleiner (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.[38] The Charlson score, which includes 18 components, has been shown to have a strong relationship with mortality (Quan et al. (n.d.)). The average number of components in our nonelderly data is 0.37, and the maximum

[37]In Kolstad and Kowalski (2010), we included a seventh set of risk adjusters, Medstat disease staging measures, but we must omit them here because they are no longer available in 2008.

[38]See Charlson et al. (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.

is 9. Fourth, we control for the 29 AHRQ comorbidity measures. The nonelderly mean in our data is 1 and the maximum is 14.

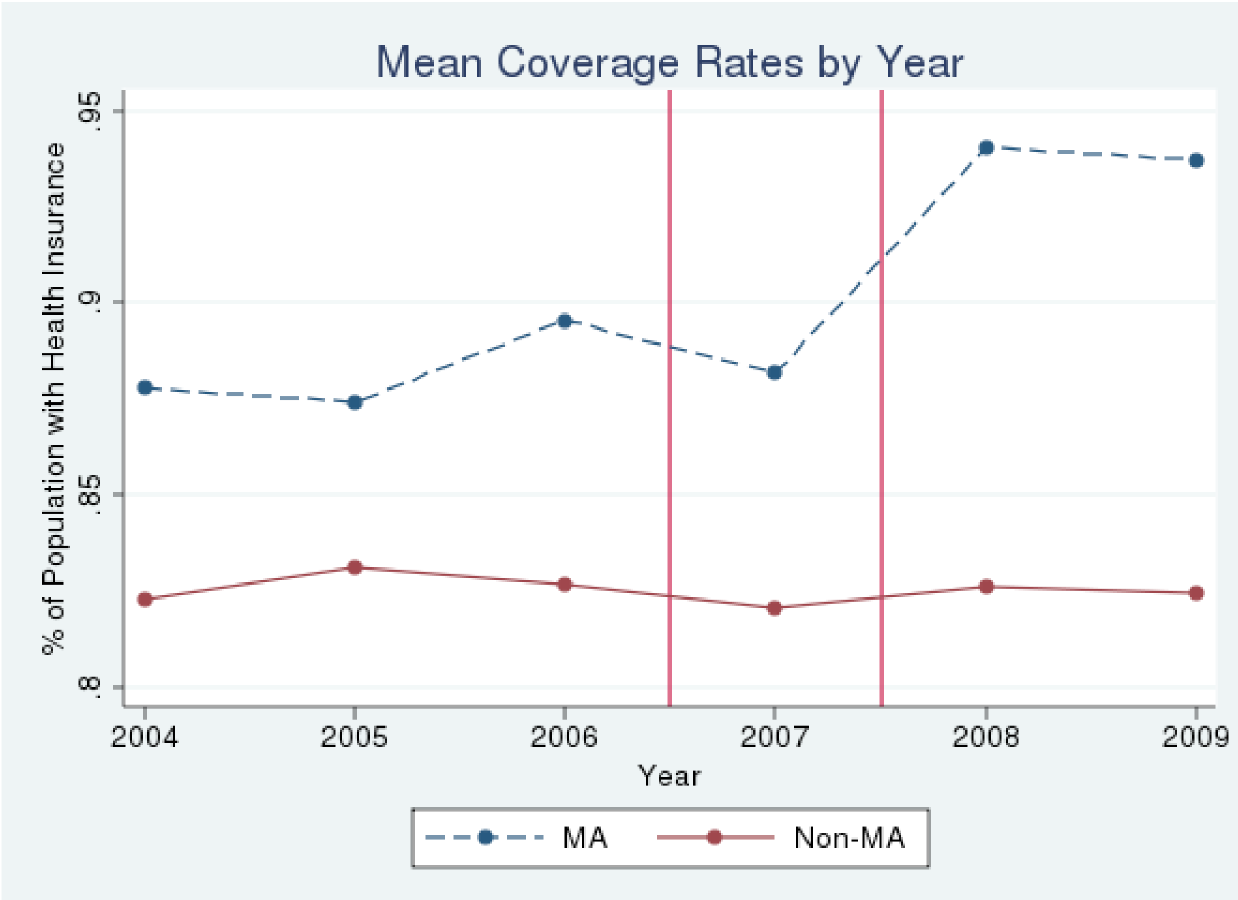
The remaining two measures were constructed in our data using proprietary algorithms. The APR-DRG measures and the APS-DRG measures, developed by 3M, were developed as refinements to the existing Refined DRGs (R-DRGs) and All-Patient DRGs (AP-DRGs) systems, which expanded the DRG system beyond the Medicare 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 both measures in each specification. The mortality measure takes on integer values from 0 to 4 with a mean in our data of 1.3, 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 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 nonelderly 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 0.88 in the nonelderly sample; the length of stay measure takes on values from 0.21 to 19.02, with a mean of 0.93 in the nonelderly sample; and the mortality measure takes on values from 0 to 46.74, with a mean of 0.69 in the nonelderly sample. We include all three measures in each specification. The second specification of Appendix Table 5 reports the coefficients on all risk adjustment variables in the risk-adjusted uninsurance regression reported in the last row of the first specification in Table 1.

D Quality Indicators

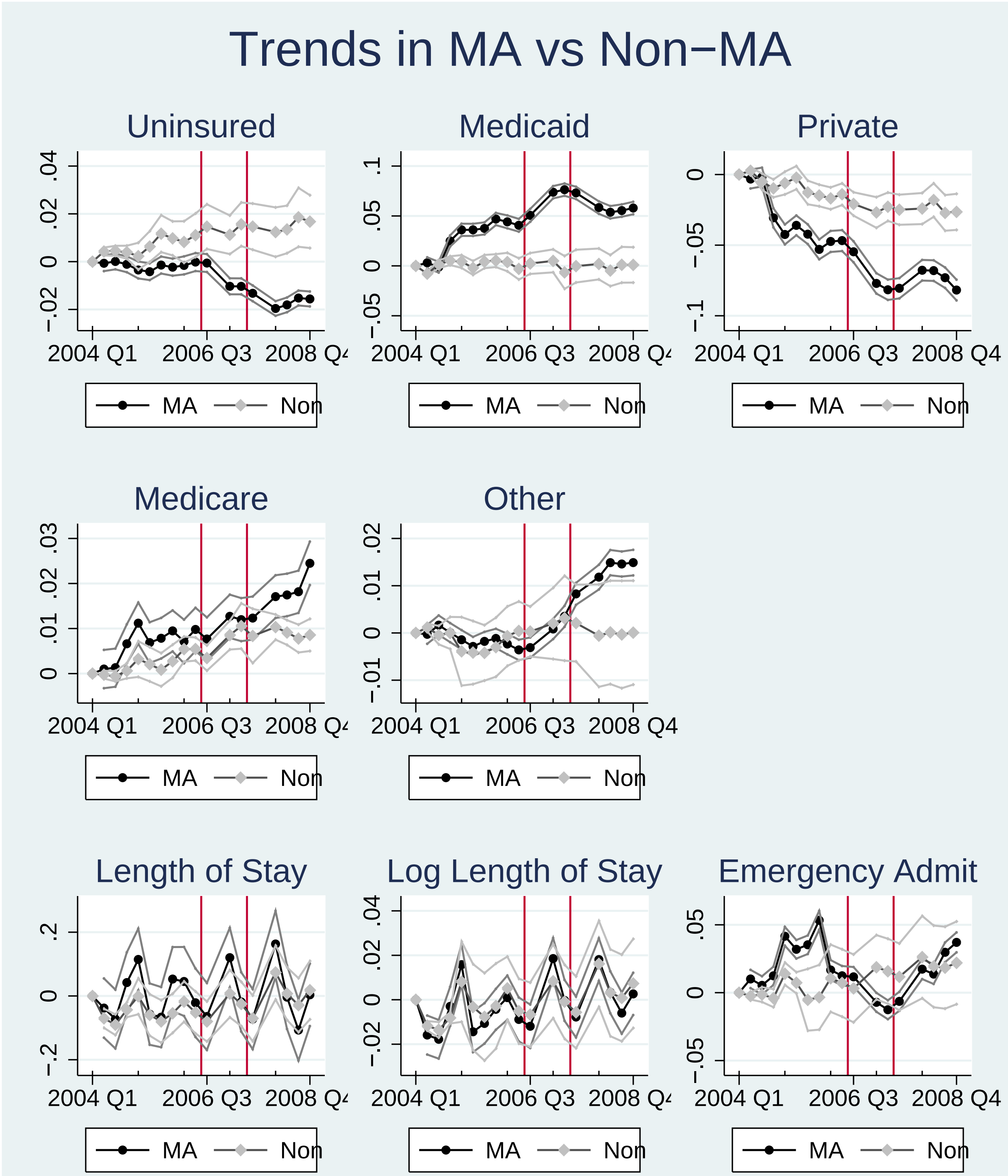
We use software from AHRQ to calculate these measures in our NIS data (Agency for Healthcare Research and Quality (2007a)). For each set of indicators, we use the most recent set of code that does not include a windows executable file. We use the December 2009 Version 4.1 of the Prevention Quality Indicators (Agency for Healthcare Research and Quality (2007e)), 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 (Agency for Healthcare Research and Quality (2007c)), the January 2009 Version 3.2a of the Patient Safety Indicators (Agency for Healthcare Research and Quality (2007d)), and the December 2009 Version 4.1 of the Pediatric Quality Indicators (Agency for Healthcare Research and Quality (2007f)). 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.

Figure 1



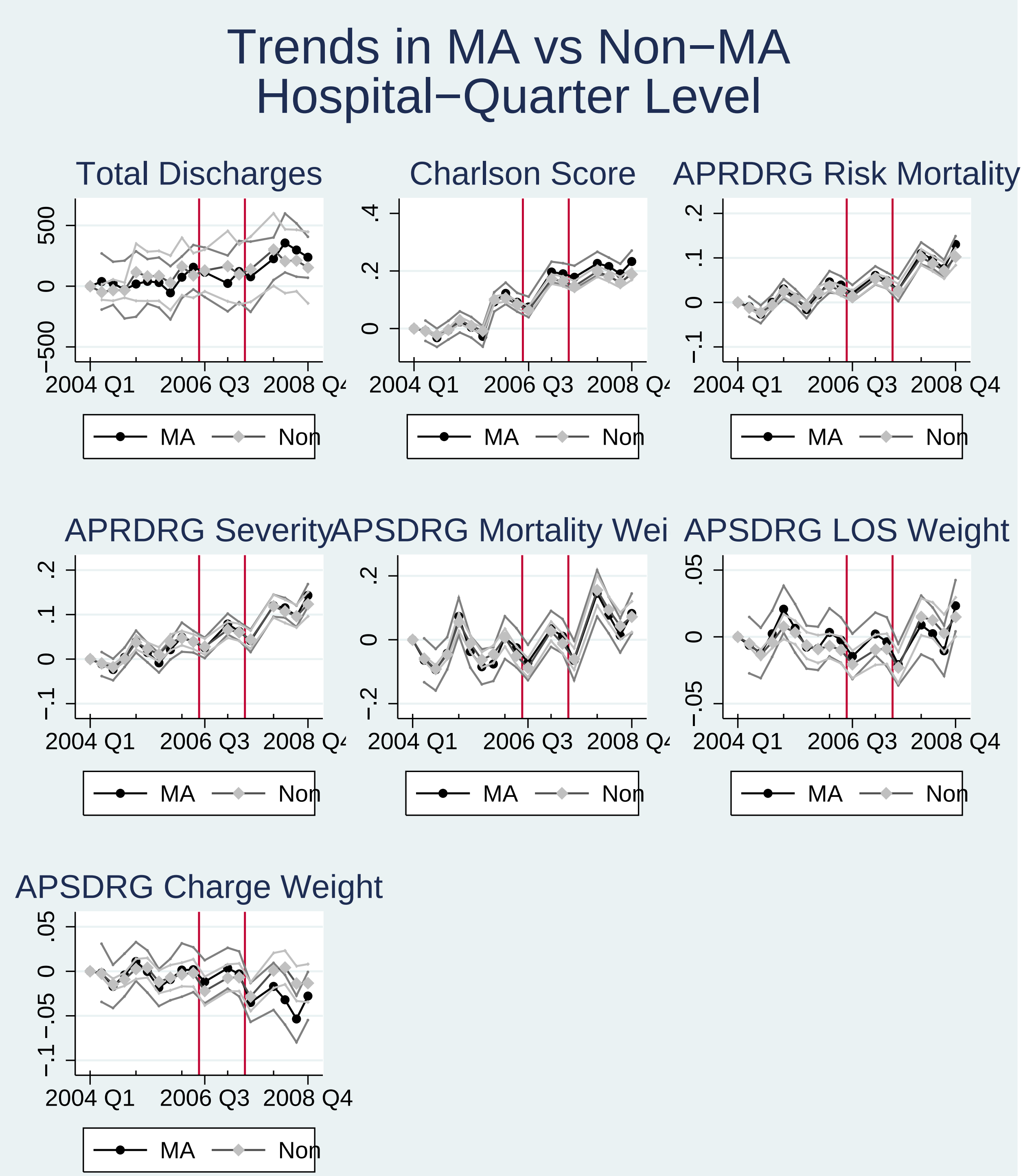
Source: CPS March Supplement 2004-2009, authors' calculations.
Vertical lines separate *During* and *After* periods.

Figure 2



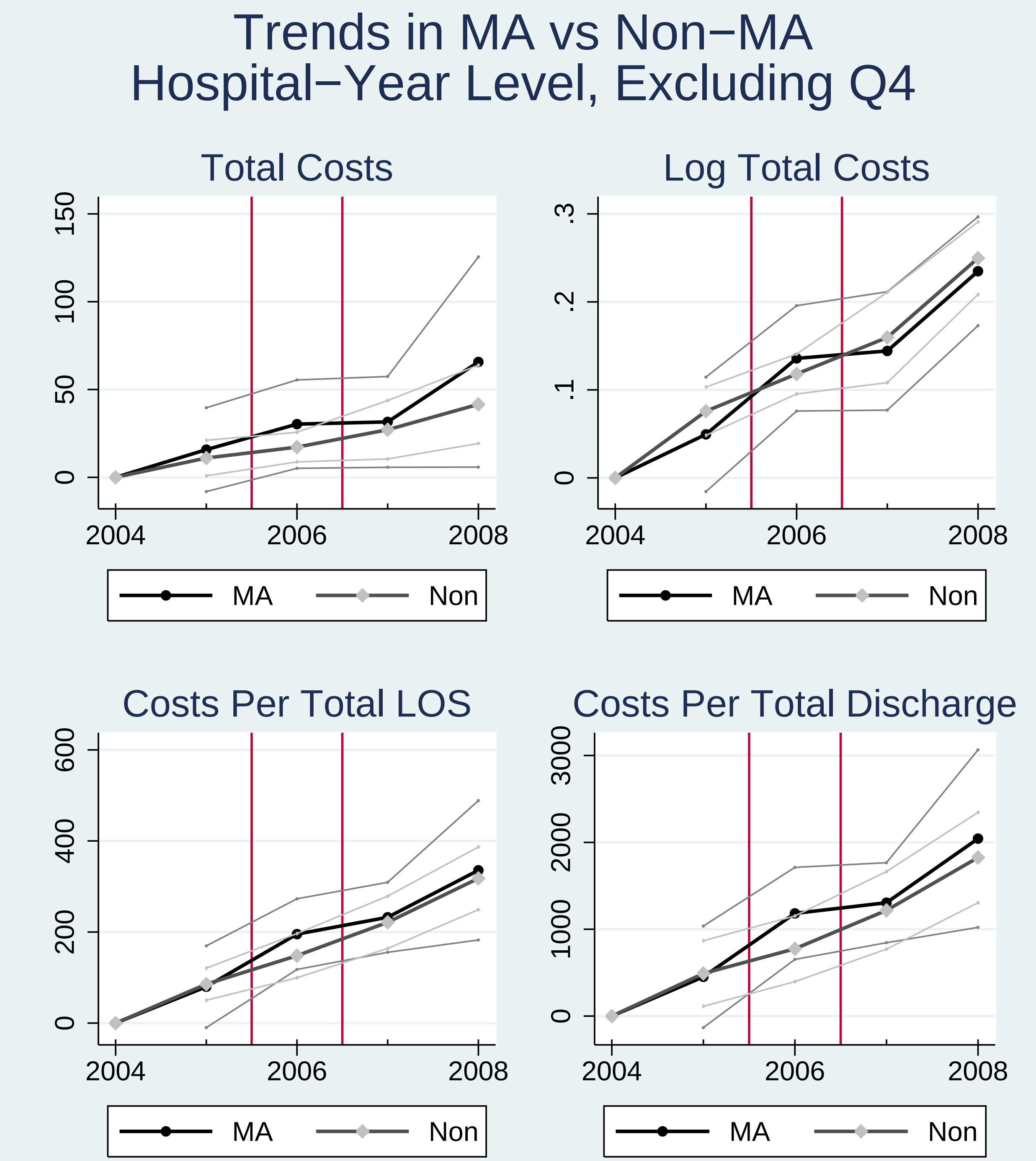
Source: NIS authors' calculations.
Trends obtained from regressions including hospital fixed effects.
95% asympototic confidence intervals shown.
Confidence intervals clustered by state for Non−MA and robust for MA.
All data omitted in 2006 Q4 and 2007 Q4 because of MA data availability.
Vertical lines separate During and After periods.

Figure 3



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.
All data omitted in 2006 Q4 and 2007 Q4 because of MA data availability.
Vertical lines separate During and After periods.

Figure 4



Source: NIS authors' calculations.
For this figure, data collapsed annually by hospital, excluding Q4.
All Q4 data omitted because of MA data availability.
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.
Vertical lines separate 2006 from other years.

Table 1: Insurance and Outcomes in NIS

	Mutually Exclusive Types of Coverage						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	**Uninsured**	**Medicaid**	**Private**	**Medicare**	**Other**	**CommCare**	**No Coverage Info**
MA*After	-0.0231	0.0389	-0.0306	0.0042	0.0106	0.0124	0.0015
	[-0.0300,-0.0162]***	[0.0265,0.0512]***	[-0.0378,-0.0233]***	[0.0013,0.0070]***	[0.0041,0.0171]***	[0.0123,0.0124]***	[0.0000,0.0030]**
	[-0.0299,-0.0166]+++	[0.0293,0.051]+++	[-0.0385,-0.0236]+++	[0.0014,0.0068]+++	[0.0050,0.0181]+++	[0.0124,0.0125]+++	[0.0001,0.0029]++
MA*During	-0.0129	0.0365	-0.0224	-0.0003	-0.0009	0.0029	-0.0017
	[-0.0176,-0.0083]***	[0.0293,0.0437]***	[-0.0274,-0.0173]***	[-0.0024,0.0017]	[-0.0043,0.0026]	[0.0029,0.0029]***	[-0.0065,0.0031]
	[-0.0177,-0.0084]+++	[0.0302,0.0438]+++	[-0.0277,-0.0168]+++	[-0.0025,0.0018]*	[-0.0049,0.0026]*	[0.0029,0.0029]+++	[-0.0076,0.0014]*
N (Nonelderly)	23,860,930	23,860,930	23,860,930	23,860,930	23,860,930	23,860,930	23,913,983
R Squared	0.0659	0.1148	0.1502	0.0341	0.0689	0.0249	0.0662
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000	0.0002
Mean Non-MA Before	0.0791	0.2876	0.4978	0.0928	0.0427	0.0000	0.0020
Mean MA After	0.0352	0.2594	0.5518	0.1177	0.0360	0.0165	0.0040
Mean Non-MA After	0.0817	0.2790	0.4923	0.1020	0.0450	0.0000	0.0017
MA*After	-0.0228	0.0374	-0.0275	0.0021	0.0107	0.0124	0.0014
with risk adjusters	[-0.0297,-0.0158]***	[0.0235,0.0514]***	[-0.0361,-0.0190]***	[-0.0007,0.0050]	[0.0047,0.0168]***	[0.0123,0.0124]***	[0.0000,0.0028]**
R Squared	0.0939	0.2232	0.2381	0.2006	0.0761	0.0249	0.0666

	Outcomes		
	(8)	(9)	(10)
Dependent Variable:	**Length of Stay**	**Log Length of Stay**	**Emergency Admit**
MA*After	-0.0504	-0.0012	-0.0202
	[-0.0999,-0.0008]**	[-0.0111,0.0086]	[-0.0397,-0.0007]**
	[-0.1026,-0.0065]++	[-0.0113,0.0066]*	[-0.0351,0.0011]+
MA*During	-0.0037	0.0037	-0.0317***
	[-0.0369,0.0294]	[-0.0022,0.0095]	[-0.0449,-0.0184]***
	[-0.0367,0.0238]*	[-0.0026,0.0084]*	[-0.0409,-0.0166]+++
N (Nonelderly)	23,913,183	23,913,183	23,913,983
R Squared	0.0335	0.0458	0.1088
Mean MA Before	5.4256	1.4267	0.3868
Mean Non-MA Before	5.0770	1.3552	0.3591
Mean MA After	5.3717	1.4355	0.4058
Mean Non-MA After	5.0958	1.3596	0.3745
MA*After	-0.1037	-0.0105	-0.0220
with risk adjusters	[-0.1471,-0.0603]***	[-0.0186,-0.0023]**	[-0.0427,-0.0012]**
R Squared	0.3801	0.4038	0.2907

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10
95% bootstrapped CI, blocks by state, 1000 reps: +++ Significant at .01, ++ Significant at .05, + Significant at .10
MA*During always included but coefficient not always reported. All specifications and means weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.
Risk adjusters include six 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, All-Patient Refined (APR)-DRGs, and All-Payer Severity-adjusted (APS)-DRGs. See Appendix 2.
CommCare included in Other. No Coverage Info not included in any other specifications.
Length of Stay is calculated as one plus the discharge date minus the admission date. The smallest possible value is one day. Emergency Admit indicates emergency room source of admission.
Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Table 2: Regressions on the Hospital-Quarter Level in NIS

	(1) Total Discharges	(2) Log Total Disch.	(3) Nonelderly Dischg.	(4) Log Noneld. Dischg.	(5) Elderly Dischg.	(6) Log Elderly Dischg.	(7) Hospital Bedsize
MA* After	19	0.0037	-7	0.0029	26	0.0115	-0.0032
	[-183,220]	[-0.0133,0.0206]	[-174,160]	[-0.0192,0.0250]	[-19,70]	[-0.0059,0.0289]	[-0.0165,0.0100]
N (All Ages)	18,622	18,622	18,622	18,590	18,622	18,327	18,595
Mean MA Before	5616	8.4125	3,592	7.8958	2,023	7.4190	0.6350
Mean Non-MA Before	5029	8.1894	3,454	7.7512	1,569	7.0394	0.7529
Mean MA After	6769	8.5300	4,433	8.0369	2,336	7.5456	0.6914
Mean Non-MA After	5389	8.2327	3,712	7.7908	1,672	7.0949	0.7602
	(8) Charlson Score	(9) APRDRG Risk Mortality	(10) APRDRG Severity	(11) APSDRG Mortality Weight	(12) APSDRG LOS Weight	(13) APSDRG Charge Weight	
MA* After	0.0340	0.0057	0.0062	-0.0059	-0.006	-0.0195	
	[0.0205,0.0476]***	[-0.0050,0.0164]	[-0.0097,0.0220]	[-0.0323,0.0206]	[-0.0141,0.0022]	[-0.0310,-0.0080]***	
N (All Ages)	18,622	18,622	18,622	18,622	18,622	18,622	
Mean MA Before	0.5750	1.5467	1.8892	1.1504	1.0660	1.0623	
Mean Non-MA Before	0.5058	1.4919	1.8413	1.0992	1.0313	1.0356	
Mean MA After	0.7325	1.6144	1.9672	1.2324	1.0758	1.0488	
Mean Non-MA After	0.6518	1.5700	1.9289	1.2167	1.0457	1.0417	

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

MA*During always included but coefficient not always reported. All specifications and means are on hospital-quarter level, weighted by sum of discharge weights.

All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.

Hospital Bedsize takes on values of 0 for small, .5 for medium, and 1 for large. See text for more details.

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

Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Table 3: Prevention Quality Indicators in NIS

Prevention Quality Indicators	**Improvement?**	**MA* After**		**Improvement?**	**MA* After, Risk Adjusted**		**N,**	**Mean MA Before**
PQI 90 Overall PQI		-0.0002	[-0.0016,0.0011]	Y	-0.0023	[-0.0036,-0.0009]***	17,674,454	0.0838
PQI 01 Diabetes Short-term Comp. Admission		-0.0001	[-0.0002,0.0001]		-0.0002	[-0.0005,0.0001]	17,674,454	0.0058
PQI 02 Perforated Appendix Admission Rate	Y	-0.0463	[-0.0557,-0.0368]***	Y	-0.0072	[-0.0133,-0.0012]**	189,588	0.2457
PQI 03 Diabetes Long-Term Comp. Admission		0.0000	[-0.0002,0.0003]	Y	-0.0009	[-0.0011,-0.0007]***	17,674,454	0.0083
PQI 05 COPD Admission Rate		-0.0002	[-0.0005,0.0001]	Y	-0.0005	[-0.0008,-0.0002]***	17,674,454	0.0097
PQI 07 Hypertension Admission rate		0.0001	[-0.0002,0.0004]		0.0001	[-0.0002,0.0004]	17,674,454	0.0020
PQI 08 CHF Admission Rate		0.0000	[-0.0005,0.0005]		-0.0003	[-0.0006,0.0001]	17,674,454	0.0109
PQI 10 Dehydration Admission Rate		0.0000	[-0.0002,0.0002]	Y	-0.0005	[-0.0007,-0.0003]***	17,674,454	0.0054
PQI 11 Bacterial Pneumonia Admission Rate		0.0001	[-0.0005,0.0006]		0.0004	[-0.0002,0.0009]	17,674,454	0.0172
PQI 12 Urinary Tract Infection Admission Rate		-0.0001	[-0.0004,0.0002]		0.0000	[-0.0003,0.0003]	17,674,454	0.0070
PQI 13 Angina without Procedure Admission Rate	N	0.0005	[0.0004,0.0007]***	N	0.0005	[0.0004,0.0007]***	17,674,454	0.0014
PQI 14 Uncontrolled Diabetes Admission rate		0.0001	[-0.0001,0.0003]		-0.0001	[-0.0003,0.0001]	17,674,454	0.0007
PQI 15 Adult Asthma Admission Rate	Y	-0.0006	[-0.0009,-0.0002]***	Y	-0.0006	[-0.0009,-0.0003]***	17,674,454	0.0146
PQI 16 Rate of Lower-extremity Amputation	Y	-0.0005	[-0.0006,-0.0004]***	Y	-0.0006	[-0.0007,-0.0005]***	17,674,454	0.0023
Patient Safety Indicators								
PSI 1 Complications of Anesthesia		0.0000	[-0.0001,0.0002]		-0.0001	[-0.0002,0.0001]	5,822,429	0.0008
PSI 2 Death in Low-Mortality DRGs	Y	-0.0001	[-0.0001,-0.0000]**	Y	-0.0002	[-0.0002,-0.0001]***	8,223,877	0.0003
PSI 3 Pressure Ulcer	Y	-0.0040	[-0.0051,-0.0029]***	Y	-0.0037	[-0.0049,-0.0024]***	3,518,425	0.0110
PSI 4 Failure to Rescue		0.0106	[0.0041,0.0171]***	N	0.0073	[0.0008,0.0138]**	108,149	0.0955
PSI 6 Iatrogenic Pneumothorax, Provider Level		0.0000	[-0.0000,0.0001]		0.0000	[-0.0000,0.0001]	12,720,445	0.0005
PSI 7 Selected Infections Due to Medical Care	Y	-0.0005	[-0.0007,-0.0004]***	Y	-0.0006	[-0.0008,-0.0004]***	12,310,324	0.0021
PSI 8 Postoperative Hip Fracture	N	0.0000	[-0.0001,-0.0000]**	N	0.0000	[-0.0001,-0.0000]**	2,944,801	0.0002
PSI 9 Postoperative Hemorrhage or Hematoma	N	0.0002	[0.0001,0.0004]**		0.0000	[-0.0002,0.0002]	4,403,970	0.0024
PSI 10 Postop. Physio. and Metab. Derangement	Y	-0.0002	[-0.0004,-0.0000]**	Y	-0.0002	[-0.0003,0.0000]*	2,200,834	0.0008
PSI 11 Postop. Respiratory Failure	Y	-0.0008	[-0.0017,0.0001]*		0.0002	[-0.0006,0.0010]	1,897,941	0.0058
PSI 12 Postop. Pul. Embolism or Deep Vein Thromb.	Y	-0.0010	[-0.0017,-0.0004]***	Y	-0.0009	[-0.0015,-0.0004]***	4,392,033	0.0068
PSI 13 Postoperative Sepsis	Y	-0.0023	[-0.0036,-0.0009]***	Y	-0.0026	[-0.0036,-0.0016]***	424,437	0.0053
PSI 14 Postoperative Would Dehiscence	Y	-0.0014	[-0.0017,-0.0010]***	Y	-0.0015	[-0.0019,-0.0012]***	939,696	0.0022
PSI 15 Accident. Puncture or Laceration, Provider	Y	-0.0007	[-0.0009,-0.0005]***	Y	-0.0010	[-0.0012,-0.0008]***	13,049,551	0.0034
PSI 17 Birth Trauma - Injury to Neonate		-0.0002	[-0.0006,0.0003]		0.0001	[-0.0003,0.0006]	3,976,866	0.0032
PSI 18 Ob. Trauma - Vag. Deliv. with Instrument		0.0008	[-0.0057,0.0073]	N	0.0076	[0.0011,0.0140]**	236,512	0.1740
PSI 19 Ob. Trauma - Vag. Deliv. without Instrument	N	0.0026	[0.0010,0.0041]***	N	0.0022	[0.0006,0.0038]***	2,463,695	0.0365
PSI 20 Obstetric Trauma - Cesarean Delivery	Y	-0.0025	[-0.0030,-0.0019]***	Y	-0.0025	[-0.0030,-0.0020]***	1,248,050	0.0067
PSI 22 Iatrogenic Pneumothorax, Area Level		0.0000	[-0.0000,0.0001]		0.0000	[-0.0000,0.0001]	12,722,192	0.0007
PSI 23 Selected Infections Due to Medical Care, Area	Y	-0.0007	[-0.0008,-0.0005]***	Y	-0.0007	[-0.0009,-0.0006]***	15,515,612	0.0029
PSI 24 Postoperative Wound Dehiscence, Area	Y	-0.0001	[-0.0002,-0.0001]***	Y	-0.0001	[-0.0002,-0.0001]***	12,398,272	0.0003
PSI 25 Accidental Puncture or Laceration, Area	Y	-0.0008	[-0.0009,-0.0006]***	Y	-0.0011	[-0.0013,-0.0009]***	13,053,510	0.0038
PSI 27 Postoperative Hemorrhage or Hemat., Area		0.0000	[-0.0001,0.0001]		-0.0001	[-0.0001,0.0000]	13,393,334	0.0015

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

Y and N indicate statistically significant gains and losses, respectively.

MA*During always included but coefficient not always reported. All specifications and means are weighted using discharge weights

All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.

Risk adjusters include six 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, All-Patient Refined (APR)-DRGs, and All-Payer Severity-adjusted (APS)-DRGs. See Appendix 2.

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

Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Table 4. Insurance and outcomes in BRFSS

	Access and Preventive Outcomes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any Health Plan	**Any Personal Doctor**	**Could not Access Care due to Cost**	**On BP Medication**	**Cholesterol Check in Last Year**	**Flu Shot in Last Year**	**Mammogram in Last Year**	**PSA in Last Year**
MA*After	0.0496	0.0126	-0.0306	0.0304	-0.0152	0.0168	-0.0136	0.0048
	[0.0399, 0.0593]***	[0.0010, 0.0241]**	[-0.0395, -0.0216]***	[-0.0144, 0.0752]	[-0.0357, 0.0052]	[0.0015, 0.0320]**	[-0.0406, 0.0133]	[-0.0431, 0.0527]
MA*During	0.0137	0.0078	-0.0168	0.0704	0.0050	0.0071	-0.0277	-0.0331
	[0.0012, 0.0262]**	[-0.0067, 0.0223]	[-0.0274, -0.0063]***	[0.0187, 0.1222]***	[-0.0201, 0.0302]	[-0.0105, 0.0246]	[-0.0595, 0.0041] *	[-0.0908, 0.0246]
After	0.0065	0.0115	0.0115	0.0205	0.0175	0.1060	0.0317	0.0169
	[0.0032, 0.0097]***	[0.0080, 0.0149]***	[0.0086, 0.0143]***	[0.0109, 0.0300]***	[0.0125, 0.0225]***	[0.1027, 0.1099]***	[0.0245, 0.0388]***	[0.0041, 0.0296]***
During	0.0060	0.0064	0.0002	0.0340	0.0112	0.0649	0.0214	0.0120
	[0.0019, 0.0101]***	[0.0020, 0.0109]***	[-0.0034, 0.0038]	[0.0200, 0.0480]***	[0.0036, 0.0187]***	[0.0607, 0.0692]***	[0.0127, 0.0301]***	[-0.0037, 0.0276]
MA	0.0814	0.0765	-0.0862	-0.1390	-0.0230	0.0204	0.1200	-0.0047
	[0.0698, 0.0930]***	[0.0636, 0.0894]***	[-0.0967, -0.0756]***	[-0.1816, -0.0971]***	[-0.0442, -0.0018]**	[0.0049, 0.0359]***	[0.0923, 0.1471]***	[-0.0528, 0.0434]
Constant	0.8200	0.7860	0.1720	0.7790	0.7130	0.2100	0.5980	0.6960
	[0.8099, 0.8293]***	[0.7753, 0.7966]***	[0.1631, 0.1806]***	[0.7536, 0.8044]***	[0.6974, 0.7291]***	[0.1994, 0.2205]***	[0.5766, 0.6188]***	[0.6607, 0.7305]***
N (Nonelderly)	1,658,293	1,658,784	1,659,567	243,497	724,819	1,410,193	373,036	114,573
R Squared	0.018	0.021	0.008	0.016	0.008	0.017	0.006	0.005
MA Before	0.8877	0.8556	0.0937	0.6358	0.6960	0.2246	0.7155	0.6851
Non-MA Before	0.8119	0.7651	0.1527	0.6820	0.6756	0.1988	0.6130	0.6461
MA After	0.9432	0.8796	0.0750	0.6897	0.6998	0.3452	0.7328	0.7061
Non-MA After	0.8167	0.7751	0.1647	0.7017	0.6954	0.3029	0.6441	0.6640

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

All specifications and means are weighted using population weights

All specifications include state fixed effects and time fixed effects for 2004 to 2009, monthly

Source: BRFSS 2004-2009 authors' calculations. See text for more details

Table 5: Cost Regressions on the Hospital-Year Level, Excluding Q3, in NIS

	(1)	(2)	(3)	(4)
	Total Costs, $Mill	**Log Total Costs**	**Total Costs/LOS**	**Total Costs/Disch.**
MA* After 2006	9.544	-0.003	16.123	154.602
	[-4.669,23.758]	[-0.043,0.037]	[-32.972,65.218]	[-181.444,490.648]
N (All Ages)	3,869	3,869	3,869	3,869
Mean MA Before 2006	152.292	18.491	1,328.481	7,844.672
Mean Non-MA Before 2006	121.030	18.178	1,318.436	7,451.341
Mean MA After 2006	230.174	18.798	1,629.457	9,576.684
Mean Non-MA After 2006	152.690	18.381	1,557.693	8,728.628
MA*After 2006 Divided by Year to Investigate Dynamics				
MA* 2008	22.441	-0.006	19.677	231.422
	[3.310,41.572]**	[-0.043,0.032]	[-41.922,81.276]	[-199.535,662.380]
MA*2007	1.869	-0.001	14.008	108.884
	[-10.152,13.890]	[-0.052,0.050]	[-36.777,64.793]	[-231.317,449.085]

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10
MA*During always included but coefficient not always reported.
All specifications and means are on hospital-year level, weighted by sum of the discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, annually.
Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Table 6: Insurance and Outcomes by Age in NIS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Uninsured	**Medicaid**	**Private**	**Medicare**	**Other**	**CommCare**	**Length of Stay**	**Log Length of Stay**	**Emergency Admit**
All Ages (100% of sample, including missing ages)									
Ma*After	-0.0153	0.0238	-0.0183	0.0039	0.0059	0.0076	-0.0626	0.0006	-0.0367
	[-0.0201,-0.0104]***	[0.0161,0.0315]***	[-0.0248,-0.0119]***	[-0.0003,0.0081]*	[0.0010,0.0109]**	[0.0076,0.0077]***	[-0.1102,-0.0150]**	[-0.0090,0.0103]	[-0.0585,-0.0149]***
Mean MA Before	0.0409	0.1536	0.3693	0.4214	0.0148	0.0000	5.8833	1.5099	0.4789
Nonelderly (age<65) (66% of sample)									
Ma*After	-0.0231	0.0389	-0.0306	0.0042	0.0106	0.0124	-0.0504	-0.0012	-0.0202
	[-0.0300,-0.0162]***	[0.0265,0.0512]***	[-0.0378,-0.0233]***	[0.0013,0.0070]***	[0.0041,0.0171]***	[0.0123,0.0124]***	[-0.0999,-0.0008]**	[-0.0111,0.0086]	[-0.0397,-0.0007]**
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000	5.4256	1.4267	0.3868
Elderly (age 65+) (34% of sample)									
Ma*After	-0.0016	0.0020	-0.0015	0.0021	-0.0011	0.0001	-0.0640	0.0049	-0.0675
	[-0.0025,-0.0006]***	[0.0006,0.0034]***	[-0.0122,0.0092]	[-0.0098,0.0140]	[-0.0054,0.0033]	[0.0001,0.0001]***	[-0.1211,-0.0070]**	[-0.0049,0.0148]	[-0.0951,-0.0398]***
Mean MA Before	0.0045	0.0096	0.0671	0.9112	0.0076	0.0000	6.5975	1.6396	0.6226
Age<19 (18% of sample)									
Ma*After	-0.0154	0.0720	-0.0575	-0.0010	0.0019	0.0026	-0.0672	-0.0024	0.0192
	[-0.0361,0.0053]	[0.0454,0.0987]***	[-0.0680,-0.0470]***	[-0.0050,0.0029]	[-0.0025,0.0062]	[0.0026,0.0026]***	[-0.1192,-0.0153]**	[-0.0104,0.0056]	[0.0132,0.0252]***
Mean MA Before	0.0199	0.3122	0.6515	0.0004	0.0160	0.0000	5.2987	1.3623	0.1287
Age 19-26 (7% of sample)									
Ma*After	-0.0407	0.0449	-0.0210	0.0013	0.0155	0.0204	-0.0134	0.0008	-0.0252
	[-0.0491,-0.0323]***	[0.0297,0.0600]***	[-0.0306,-0.0113]***	[-0.0034,0.0061]	[0.0092,0.0218]***	[0.0204,0.0205]***	[-0.0646,0.0377]	[-0.0079,0.0096]	[-0.0423,-0.0082]***
Mean MA Before	0.1266	0.4298	0.3880	0.0339	0.0216	0.0000	4.6897	1.3344	0.3859
Age 27-30 (4% of sample)									
Ma*After	-0.0221	0.0433	-0.0372	0.0019	0.0141	0.0137	0.0899	0.0103	-0.0209
	[-0.0302,-0.0139]***	[0.0272,0.0593]***	[-0.0461,-0.0283]***	[-0.0025,0.0062]	[0.0074,0.0208]***	[0.0136,0.0137]***	[0.0376,0.1423]***	[0.0022,0.0184]**	[-0.0366,-0.0051]**
Mean MA Before	0.0825	0.2659	0.5805	0.0519	0.0192	0.0000	4.5374	1.3464	0.3144
Age 31-40 (10% of sample)									
Ma*After	-0.0304	0.0313	-0.0130	0.0026	0.0095	0.0121	-0.1187	-0.0041	-0.0240
	[-0.0374,-0.0234]***	[0.0215,0.0411]***	[-0.0206,-0.0054]***	[-0.0016,0.0068]	[0.0012,0.0179]**	[0.0120,0.0122]***	[-0.1819,-0.0555]***	[-0.0142,0.0060]	[-0.0459,-0.0021]**
Mean MA Before	0.0841	0.2118	0.5962	0.0883	0.0196	0.0000	4.8814	1.3839	0.3914
Age 41-54 (15% of sample)									
Ma*After	-0.0283	0.0230	-0.0247	0.0155	0.0146	0.0163	0.0504	0.0068	-0.0513
	[-0.0377,-0.0189]***	[0.0154,0.0306]***	[-0.0323,-0.0171]***	[0.0119,0.0191]***	[0.0055,0.0237]***	[0.0162,0.0163]***	[-0.0104,0.1111]	[-0.0057,0.0194]	[-0.0830,-0.0195]***
Mean MA Before	0.0855	0.1975	0.5110	0.1842	0.0220	0.0000	5.7564	1.4839	0.5807
Age 55-64 (12% of sample)									
Ma*After	-0.0120	0.0181	-0.0145	-0.0036	0.0120	0.0147	-0.1480	-0.0090	-0.0462
	[-0.0176,-0.0063]***	[0.0110,0.0251]***	[-0.0221,-0.0069]***	[-0.0074,0.0001]*	[0.0049,0.0192]***	[0.0146,0.0147]***	[-0.2012,-0.0948]***	[-0.0213,0.0034]	[-0.0759,-0.0166]***
Mean MA Before	0.0512	0.1344	0.5511	0.2436	0.0197	0.0000	6.3052	1.5602	0.5541
Age 65-74 (13% of sample)									
Ma*After	-0.0028	0.0017	-0.0006	0.0048	-0.0031	0.0001	-0.0136	0.0086	-0.0572
	[-0.0040,-0.0017]***	[-0.0001,0.0035]*	[-0.0131,0.0119]	[-0.0095,0.0190]	[-0.0084,0.0023]	[0.0001,0.0001]***	[-0.0623,0.0351]	[-0.0018,0.0189]	[-0.0844,-0.0300]***
Mean MA Before	0.0078	0.0174	0.1131	0.8518	0.0099	0.0000	6.4894	1.6001	0.5693
Age 75+ (21% of sample)									
Ma*After	-0.0009	0.0022	-0.0010	-0.0003	0.0001	0.0000	-0.0833	0.0038	-0.0733
	[-0.0018,-0.0000]**	[0.0010,0.0034]***	[-0.0108,0.0087]	[-0.0109,0.0103]	[-0.0037,0.0039]	[0.0000,0.0000]***	[-0.1530,-0.0136]**	[-0.0057,0.0133]	[-0.1016,-0.0451]***
Mean MA Before	0.0029	0.0059	0.0449	0.9398	0.0065	0.0000	6.6496	1.6587	0.6484

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

MA*During always included but coefficient not always reported. All specifications and means weighted using discharge weights.

All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.

Sample sizes vary across specifications based on availability of dependent variable. 808 elderly discharges, including 466 discharges for age 75+ have CommCare in MA 2008 Q4.

Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Table 7: Insurance and Outcomes by Gender and Income in NIS

	(1) Uninsured	(2) Medicaid	(3) Private	(4) Medicare	(5) Other	(6) CommCare	(7) Length of Stay	(8) Log Length of Stay	(9) Emergency Admit
Female (59% of sample)									
Ma*After	-0.0182	0.0399	-0.0357	0.0046	0.0095	0.0109	0.0035	0.0046	-0.0147
	[-0.0256,-0.0108]***	[0.0264,0.0534]***	[-0.0426,-0.0288]***	[0.0014,0.0079]***	[0.0040,0.0149]***	[0.0109,0.0110]***	[-0.0398,0.0468]	[-0.0038,0.0129]	[-0.0320,0.0026]*
Mean MA Before	0.0443	0.2707	0.5814	0.0905	0.0131	0.0000	5.1450	1.4050	0.3320
Male (41% of sample)									
Ma*After	-0.0300	0.0381	-0.0238	0.0035	0.0122	0.0143	-0.1277	-0.0095	-0.0282
	[-0.0374,-0.0226]***	[0.0273,0.0490]***	[-0.0319,-0.0158]***	[0.0010,0.0059]***	[0.0040,0.0204]***	[0.0143,0.0144]***	[-0.1893,-0.0662]***	[-0.0216,0.0027]	[-0.0513,-0.0051]**
Mean MA Before	0.0908	0.2133	0.5388	0.1297	0.0360	0.0165	5.7970	1.4555	0.4595
Patient's Zip Code in First (Lowest) Income Quartile (28% of sample)									
Ma*After	-0.0359	0.1050	-0.0899	0.0059	0.0148	0.0130	0.0535	0.0158	-0.0570
	[-0.0440,-0.0277]***	[0.0873,0.1227]***	[-0.1004,-0.0793]***	[0.0025,0.0093]***	[0.0035,0.0260]**	[0.0130,0.0130]***	[-0.0050,0.1119]*	[0.0071,0.0244]***	[-0.0703,-0.0436]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000	5.6074	1.4402	0.4665
Patient's Zip Code in Second Income Quartile (26% of sample)									
Ma*After	-0.0226	0.0447	-0.0495	0.0148	0.0126	0.0165	-0.0941	-0.0081	-0.0190
	[-0.0309,-0.0143]***	[0.0305,0.0590]***	[-0.0595,-0.0396]***	[0.0113,0.0184]***	[0.0053,0.0199]***	[0.0164,0.0165]***	[-0.1310,-0.0572]***	[-0.0153,-0.0009]**	[-0.0297,-0.0083]***
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000	5.5681	1.4451	0.4437
Patient's Zip Code in Third Income Quartile (23% of sample)									
Ma*After	-0.0234	0.0217	-0.0106	0.0020	0.0103	0.0130	-0.0890	0.0017	-0.0107
	[-0.0295,-0.0174]***	[0.0098,0.0336]***	[-0.0187,-0.0026]**	[-0.0010,0.0050]	[0.0069,0.0138]***	[0.0130,0.0130]***	[-0.1417,-0.0363]***	[-0.0061,0.0095]	[-0.0392,0.0178]
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000	5.3885	1.4240	0.3671
Patient's Zip Code in Fourth (Highest) Income Quartile (21% of sample)									
Ma*After	-0.0059	0.0006	0.0010	-0.0007	0.0050	0.0090	-0.0911	-0.0185	0.0098
	[-0.0159,0.0042]	[-0.0080,0.0093]	[-0.0116,0.0135]	[-0.0066,0.0051]	[0.0011,0.0089]**	[0.0090,0.0090]***	[-0.1957,0.0136]*	[-0.0478,0.0108]	[-0.0324,0.0519]
Mean MA Before	0.0643	0.2460	0.5631	0.1073	0.0193	0.0000	5.2333	1.4067	0.3189

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

MA*During always included but coefficient not always reported. All specifications and means weighted using discharge weights.

All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.

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.

Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Table 8: Insurance and Outcomes by Race in NIS

	(1) Uninsured	(2) Medicaid	(3) Private	(4) Medicare	(5) Other	(6) CommCare	(7) Length of Stay	(8) Log Length of Stay	(9) Emergency Admit
White (45% of sample)									
MA*After	-0.0156	0.0192	-0.0193	0.0071	0.0086	0.0125	-0.0665	-0.0050	-0.0234
	[-0.0218,-0.0095]***	[0.0105,0.0279]***	[-0.0266,-0.0121]***	[0.0042,0.0101]***	[0.0053,0.0120]***	[0.0124,0.0126]***	[-0.1282,-0.0048]**	[-0.0191,0.0091]	[-0.0480,0.0011]*
Mean MA Before	0.0524	0.1647	0.6475	0.1147	0.0207	0.0000	5.3834	1.4270	0.3786
Black (12% of sample)									
MA*After	-0.0287	0.0616	-0.0474	0.0043	0.0103	0.0138	0.1013	0.0107	-0.0019
	[-0.0383,-0.0192]***	[0.0476,0.0755]***	[-0.0573,-0.0375]***	[-0.0032,0.0118]	[0.0026,0.0179]***	[0.0138,0.0139]***	[0.0163,0.1862]**	[-0.0026,0.0240]	[-0.0516,0.0477]
Mean MA Before	0.1012	0.4469	0.3008	0.1363	0.0149	0.0000	5.9492	1.4840	0.5226
Hispanic (12% of sample)									
MA*After	-0.0491	0.1058	-0.0627	-0.0002	0.0062	0.0122	0.0203	0.0023	-0.0200
	[-0.0943,-0.0039]**	[0.0476,0.1639]***	[-0.0756,-0.0498]***	[-0.0054,0.0051]	[-0.0036,0.0160]	[0.0121,0.0122]***	[-0.0368,0.0774]	[-0.0091,0.0138]	[-0.0320,-0.0079]***
Mean MA Before	0.0903	0.5390	0.2773	0.0801	0.0134	0.0000	5.1288	1.3786	0.4179
Asian (2% of sample)									
MA*After	-0.0223	-0.0175	0.0222	0.0025	0.0152	0.0141	0.1930	0.0289	0.0037
	[-0.0300,-0.0147]***	[-0.0275,-0.0075]***	[0.0125,0.0319]***	[-0.0020,0.0070]	[0.0091,0.0213]***	[0.0141,0.0142]***	[0.0706,0.3153]***	[0.0128,0.0451]***	[-0.0068,0.0143]
Mean MA Before	0.0661	0.2911	0.6070	0.0250	0.0109	0.0000	5.0414	1.3643	0.2626
Native American (1% of sample)									
MA*After	0.0042	0.0542	0.0694	-0.0538	-0.0739	-0.0009	0.4699	0.0201	-0.0905
	[-0.0091,0.0175]	[0.0211,0.0872]***	[0.0538,0.0850]***	[-0.0630,-0.0447]***	[-0.1255,-0.0223]***	[-0.0009,-0.0009]***	[0.1538,0.7860]***	[-0.0033,0.0436]*	[-0.1333,-0.0478]***
Mean MA Before	0.0514	0.3330	0.4700	0.1305	0.0152	0.0000	5.3411	1.4772	0.2774
Other Race (3% of sample)									
MA*After	-0.0248	0.0048	0.0032	-0.0017	0.0185	0.0146	-0.1673	-0.0212	-0.0260
	[-0.0454,-0.0041]**	[-0.0054,0.0149]	[-0.0154,0.0218]	[-0.0087,0.0053]	[0.0103,0.0267]***	[0.0143,0.0148]***	[-0.3827,0.0480]	[-0.0563,0.0138]	[-0.0421,-0.0100]***
Mean MA Before	0.1107	0.3108	0.5238	0.0331	0.0217	0.0000	5.7428	1.4287	0.2211
Unknown Race (25% of sample)									
MA*After	-0.0430	0.0657	0.0120	-0.0098	-0.0250	0.0095	-0.5677	-0.0246	-0.0951
	[-0.0590,-0.0269]***	[0.0536,0.0779]***	[-0.0066,0.0305]	[-0.0145,-0.0050]***	[-0.0336,-0.0164]***	[0.0095,0.0095]***	[-0.6256,-0.5098]***	[-0.0347,-0.0146]***	[-0.1065,-0.0836]***
Mean MA Before	0.0596	0.2316	0.6044	0.0756	0.0289	0.0000	5.6372	1.4452	0.2554

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10
MA*During always included but coefficient not always reported. All specifications and means weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.
Regressions in this table estimated in the sample of nonelderly discharges. Sample sizes vary across specifications based on availability of dependent variable
Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Appendix Table 1: Insurance 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]+++
MA*During	-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	0.0010	-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: *** Significant at .01, ** Significant at .05, * Significant at .10

95% bootstrapped CI, blocks by state, 1000 reps: +++ Significant at .01, ++ Significant at .05, + Significant at .10

MA*During always included but coefficient not always reported. All specifications and means weighted using sample weights.

All specifications include state fixed effects and time fixed effects for 2004 to 2009, annually. Omitted state: Alabama.

Source: CPS March Supplement 2004-2009 authors' calculations. See text for more details.

Appendix Table 2: Insurance by Age in CPS

	(1) Uninsured	(2) ESHI	(3) Medicaid	(4) Medicare	(5) Military	(6) Private
Age<19 (32% of sample)						
Ma*After	-0.0351	0.0210	0.0416	-0.0002	-0.0046	-0.0227
	[-0.0518,-0.0183]***	[-0.0223,0.0644]	[0.0047,0.0784]**	[-0.0018,0.0013]	[-0.0070,-0.0022]***	[-0.0380,-0.0074]***
Mean MA Before	0.0728	0.7116	0.1587	0.0006	0.0055	0.0508
Age 19-26 (9% of sample)						
Ma*After	-0.1410	0.1000	0.0454	0.0011	0.0037	-0.0091
	[-0.1622,-0.1198]***	[0.0585,0.1415]***	[0.0086,0.0823]**	[-0.0015,0.0037]	[-0.0038,0.0111]	[-0.0320,0.0137]
Mean MA Before	0.2654	0.5321	0.0977	0.0000	0.0018	0.1030
Age 27-30 (5% of sample)						
Ma*After	-0.1662	0.1269	0.0329	0.0085	-0.0026	0.0006
	[-0.1927,-0.1397]***	[0.0859,0.1679]***	[0.0195,0.0463]***	[0.0063,0.0107]***	[-0.0063,0.0010]	[-0.0459,0.0471]
Mean MA Before	0.2691	0.5984	0.0934	0.0000	0.0019	0.0372
Age 31-40 (14% of sample)						
Ma*After	-0.0711	0.0372	0.0506	0.0066	-0.0038	-0.0195
	[-0.1039,-0.0382]***	[0.0084,0.0659]**	[0.0305,0.0708]***	[-0.0079,0.0211]	[-0.0067,-0.0010]***	[-0.0372,-0.0018]**
Mean MA Before	0.1417	0.7050	0.0959	0.0032	0.0049	0.0492
Age 41-54 (21% of sample)						
Ma*After	-0.0424	0.0129	0.0295	0.0006	-0.0029	0.0023
	[-0.0649,-0.0198]***	[-0.0360,0.0618]	[0.0058,0.0531]**	[-0.0071,0.0084]	[-0.0055,-0.0003]**	[-0.0096,0.0141]
Mean MA Before	0.0872	0.7693	0.0857	0.0041	0.0074	0.0464
Age 55-64 (9% of sample)						
Ma*After	-0.0408	0.0292	0.0396	-0.0123	-0.0114	-0.0042
	[-0.0541,-0.0276]***	[-0.0428,0.1012]	[0.0101,0.0690]***	[-0.0302,0.0056]	[-0.0151,-0.0078]***	[-0.0312,0.0228]
Mean MA Before	0.0806	0.7594	0.0525	0.0298	0.0243	0.0534
Age 65-74 (5% of sample)						
Ma*After	-0.0150	0.0734	0.0343	-0.0665	0.0165	-0.0427
	[-0.0040,-0.0259]***	[0.0011,0.1458]**	[0.0175,0.0511]***	[-0.1340,0.0009]*	[-0.0172,0.0503]	[-0.1280,0.0426]
Mean MA Before	0.0159	0.3945	0.0565	0.2466	0.0144	0.2721
Age 75+ (4% of sample)						
Ma*After	-0.0071	0.0798	0.0065	-0.0028	-0.0025	-0.0092
	[-0.0096,-0.0046]***	[0.0180,0.1416]**	[-0.0171,0.0300]	[-0.0044,-0.0011]***	[-0.0055,0.0005]	[-0.0274,0.0090]
Mean MA Before	0.0062	0.3178	0.0406	0.3493	0.0083	0.2778

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

MA*During always included but coefficient not always reported. All specifications and means weighted using sample weights.

All specifications include state fixed effects and time fixed effects for 2004 to 2009, annually. Omitted state: Alabama.

Source: CPS March Supplement 2004-2009 authors' calculations. See text for more details.

Appendix Table 3: Insurance by Gender and Income in CPS

	(1) **Uninsured**	(2) **ESHI**	(3) **Medicaid**	(4) **Medicare**	(5) **Military**	(6) **Private**
Female (51% of sample)						
Ma*After	-0.0547	0.0284	0.0419	-0.0009	-0.0018	-0.0128
	[-0.0720,-0.0374]***	[0.0008,0.0560]**	[0.0233,0.0605]***	[-0.0045,0.0026]	[-0.0032,-0.0005]***	[-0.0212,-0.0043]***
Mean MA Before	0.0937	0.6496	0.1037	0.0513	0.0059	0.0958
Male (49% of sample)						
Ma*After	-0.0719	0.0476	0.0367	0.0014	-0.0057	-0.0081
	[-0.0787,-0.0652]***	[0.0081,0.0871]**	[0.0156,0.0579]***	[-0.0005,0.0033]	[-0.0081,-0.0034]***	[-0.0298,0.0137]
Mean MA Before	0.1274	0.6713	0.0950	0.0303	0.0098	0.0663
Patient's Zip Code in First (Lowest) Income Quartile (15% of sample)						
Ma*After	-0.1232	-0.0206	0.1396	-0.0007	-0.0043	0.0092
	[-0.1612,-0.0852]***	[-0.0440,0.0027]*	[0.1172,0.1620]***	[-0.0178,0.0165]	[-0.0090,0.0004]*	[-0.0375,0.0559]
Mean MA Before	0.2188	0.2154	0.4621	0.0205	0.0086	0.0746
Patient's Zip Code in Second Income Quartile (22% of sample)						
Ma*After	-0.1222	0.0635	0.0595	0.0059	0.0087	-0.0155
	[-0.1917,-0.0526]***	[-0.0203,0.1473]	[0.0231,0.0959]***	[0.0022,0.0097]***	[-0.0070,0.0245]	[-0.0457,0.0148]
Mean MA Before	0.2108	0.5354	0.1600	0.0039	0.0051	0.0848
Patient's Zip Code in Third Income Quartile (29% of sample)						
Ma*After	-0.0796	0.0777	0.0067	-0.0026	-0.0100	0.0078
	[-0.0912,-0.0679]***	[0.0400,0.1154]***	[-0.0146,0.0281]	[-0.0111,0.0059]	[-0.0141,-0.0060]***	[-0.0195,0.0351]
Mean MA Before	0.1410	0.7130	0.0778	0.0053	0.0122	0.0507
Patient's Zip Code in Fourth Income Quartile (33% of sample)						
Ma*After	-0.0160	0.0334	0.0109	0.0007	-0.0043	-0.0247
	[-0.0214,-0.0107]***	[0.0062,0.0606]**	[-0.0020,0.0238]*	[-0.0007,0.0021]	[-0.0075,-0.0010]**	[-0.0364,-0.0131]***
Mean MA Before	0.060	0.8799	0.0120	0.0016	0.0051	0.0417

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

MA*During always included but coefficient not always reported. All specifications and means weighted using sample weights.

All specifications include state fixed effects and time fixed effects for 2004 to 2009, annually. Omitted state: Alabama.

Source: CPS March Supplement 2004-2009 authors' calculations. See text for more details.

Appendix Table 4: Insurance by Race in CPS

	(1) **Uninsured**	(2) **ESHI**	(3) **Medicaid**	(4) **Medicare**	(5) **Military**	(6) **Private**
White (78% of sample)						
MA*After	-0.0568	0.0473	0.0261	0.0002	-0.0047	-0.0121
	[-0.0691,-0.0444]***	[0.0120,0.0827]***	[0.0000,0.0522]**	[-0.0026,0.0030]	[-0.0065,-0.0029]***	[-0.0224,-0.0019]**
Mean MA Before	0.1149	0.7190	0.0963	0.0049	0.0078	0.0571
Black (11% of sample)						
MA*After	-0.1493	-0.0094	0.1636	-0.0079	0.0085	-0.0056
	[-0.1774,-0.1213]***	[-0.0484,0.0296]	[0.1170,0.2103]***	[-0.0138,-0.0019]**	[0.0036,0.0134]***	[-0.0551,0.0440]
Mean MA Before	0.2045	0.5402	0.2080	0.0102	0.0000	0.0371
Hispanic (11% of sample)						
MA*After	-0.3239	0.4516	-0.1174	-0.0007	-0.0029	-0.0067
	[-0.6683,0.0205]*	[0.3552,0.5479]***	[-0.4071,0.1724]	[-0.0023,0.0009]	[-0.0053,-0.0005]**	[-0.0627,0.0493]
Mean MA Before	0.4027	0.2030	0.3661	0.0000	0.0000	0.0283
Asian (4% of sample)						
MA*After	-0.0754	-0.0029	0.0630	0.0122	-0.0080	0.0111
	[-0.0999,-0.0508]***	[-0.0606,0.0549]	[0.0277,0.0983]***	[0.0061,0.0182]***	[-0.0142,-0.0019]**	[-0.0373,0.0595]
Mean MA Before	0.1796	0.6308	0.1275	0.0027	0.0113	0.0481
Native American (1% of sample)						
MA*After	-0.1316	-0.1769	0.3094	-0.0008	0.0025	-0.0026
	[-0.2802,0.0170]*	[-0.4387,0.0849]	[-0.0859,0.7047]	[-0.0084,0.0068]	[-0.0082,0.0131]	[-0.0159,0.0107]
Mean MA Before	0.1741	0.7337	0.0921	0.0000	0.0000	0.0000

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

MA*During always included but coefficient not always reported. All specifications and means weighted using sample weights.

All specifications include state fixed effects and time fixed effects for 2004 to 2009, annually. Omitted state: Alabama.

Source: CPS March Supplement 2004-2009 authors' calculations. See text for more details.

Appendix Table 5: Coefficients on Selected Covariates in NIS

	(1) (preferred)	(2) (covariates)		(2) (covariates cont.)		(2) (covariates cont.)		(2) (covariates cont.)
Dependent Variable	**Uninsured**	**Uninsured**						
Ma*After	-0.0231	-0.0228	Female	-0.0394	AHRQ Comorb.	-0.0220	Charlson 6	0.0255
	[-0.0300,-0.0162]***	[-0.0297,-0.0158]***		[-0.0453,-0.0336]***	cm_dmcx	[-0.0279,-0.0161]***		[0.0201,0.0308]***
MA*During	-0.0129	-0.0131	Gender Unknown	-0.0058	AHRQ Comorb.	0.0720	Charlson 7	0.0086
	[-0.0176,-0.0083]***	[-0.0177,-0.0085]***		[-0.0256,0.0139]	cm_drug	[0.0506,0.0933]***		[0.0047,0.0124]***
2004 Q2	0.0041	0.0036	Black	0.0067	AHRQ Comorb.	-0.0009	Charlson 8	-0.0333
	[0.0025,0.0058]***	[0.0022,0.0050]***		[0.0019,0.0115]***	cm_htn_c	[-0.0028,0.0010]		[-0.0407,-0.0258]***
2004 Q3	0.0044	0.0037	Hispanic	0.0362	AHRQ Comorb.	-0.0115	Charlson 9	0.0063
	[0.0024,0.0065]***	[0.0018,0.0056]***		[0.0159,0.0565]***	cm_hypothy	[-0.0142,-0.0087]***		[0.0020,0.0107]***
2004 Q4	0.0038	0.0037	Asian	0.0187	AHRQ Comorb.	0.0044	Charlson 10	0.0558
	[0.0011,0.0065]***	[0.0010,0.0064]***		[0.0134,0.0240]***	cm_liver	[0.0004,0.0084]**		[0.0427,0.0689]***
2005 Q1	0.0020	0.0025	Native American	0.0008	AHRQ Comorb.	-0.0332	Charlson 11	0.0130
	[-0.0034,0.0074]	[-0.0027,0.0077]		[-0.0120,0.0137]	cm_lymph	[-0.0408,-0.0256]***		[0.0071,0.0189]***
2005 Q2	0.0059	0.0062	Other Race	0.0353	AHRQ Comorb.	0.0245	Charlson 12	-0.0036
	[-0.0004,0.0121]*	[0.0002,0.0122]**		[0.0167,0.0539]***	cm_lytes	[0.0203,0.0287]***		[-0.0060,-0.0012]***
2005 Q3	0.0112	0.0109	Unknown Race	0.0084	AHRQ Comorb.	-0.0244	Charlson 13	-0.0193
	[0.0037,0.0187]***	[0.0036,0.0182]***		[-0.0043,0.0211]	cm_mets	[-0.0293,-0.0195]***		[-0.0244,-0.0142]***
2005 Q4	0.0093	0.0102	Zip in First (Lowest)	0.0220	AHRQ Comorb.	-0.0144	Charlson 14	-0.0066
	[0.0024,0.0162]***	[0.0034,0.0170]***	Income Quartile	[0.0149,0.0291]***	cm_neuro	[-0.0176,-0.0111]***		[-0.0111,-0.0021]***
2006 Q1	0.0079	0.0095	Zip in Second Income	0.0170	AHRQ Comorb.	0.0027	Charlson 15	0.0248
	[-0.0005,0.0163]*	[0.0013,0.0176]**	Quartile	[0.0107,0.0234]***	cm_obese	[0.0002,0.0053]**		[0.0188,0.0309]***
2006 Q2	0.0107	0.0118	Zip in Third Income	0.0092	AHRQ Comorb.	-0.0439	Charlson 16	0.0582
	[0.0019,0.0195]**	[0.0033,0.0203]***	Quartile	[0.0048,0.0137]***	cm_para	[-0.0529,-0.0349]***		[0.0479,0.0686]***
2006 Q3	0.0146	0.0157	Zip in Unknown	0.0470	AHRQ Comorb.	-0.0140	Charlson 17	-0.0008
	[0.0054,0.0237]***	[0.0067,0.0247]***	Income Quartile	[0.0138,0.0802]***	cm_perivasc	[-0.0176,-0.0105]***		[-0.0043,0.0028]
2006 Q4			Age	0.0039	AHRQ Comorb.	-0.0061	Charlson 18	-0.0353
				[0.0030,0.0048]***	cm_psych	[-0.0105,-0.0018]***		[-0.0488,-0.0219]***
2007 Q1	0.0109	0.0132	Age Squared	-0.0001	AHRQ Comorb.	-0.0043	Diagnosis count	-0.0047
	[0.0030,0.0189]***	[0.0055,0.0210]***		[-0.0001,-0.0000]***	cm_pulmcirc	[-0.0069,-0.0016]***		[-0.0057,-0.0038]***
2007 Q2	0.0151	0.0170	AHRQ Comorb.	-0.0296	AHRQ Comorb.	-0.0430	APRDRG	0.0044
	[0.0061,0.0241]***	[0.0082,0.0258]***	cm_aids	[-0.0385,-0.0207]***	cm_renlfail	[-0.0535,-0.0325]***	Risk mortality	[0.0034,0.0054]***
2007 Q3	0.0145	0.0161	AHRQ Comorb.	0.0901	AHRQ Comorb.	-0.0283	APRDRG	0.0013
	[0.0049,0.0241]***	[0.0064,0.0258]***	cm_alcohol	[0.0725,0.1077]***	cm_tumor	[-0.0334,-0.0231]***	Severity	[-0.0005,0.0030]
2007 Q4			AHRQ Comorb.	0.0063	AHRQ Comorb.	-0.0489	APSDRG	0.0014
			cm_anemdef	[0.0038,0.0089]***	cm_ulcer	[-0.0573,-0.0405]***	Mortality Weight	[0.0009,0.0019]***
2008 Q1	0.0120	0.0154	AHRQ Comorb.	-0.0191	AHRQ Comorb.	-0.0042	APSDRG	0.0043
	[0.0018,0.0222]**	[0.0052,0.0256]***	cm_arth	[-0.0251,-0.0131]***	cm_valve	[-0.0066,-0.0017]***	LOS Weight	[0.0004,0.0083]**
2008 Q2	0.0131	0.0162	AHRQ Comorb.	-0.0188	AHRQ Comorb.	0.0072	APSDRG	-0.0056
	[0.0033,0.0230]**	[0.0063,0.0262]***	cm_bldloss	[-0.0244,-0.0133]***	cm_wghtloss	[0.0037,0.0107]***	Charge Weight	[-0.0083,-0.0028]***
2008 Q3	0.0181	0.0210	AHRQ Comorb.	-0.0316	Charlson 1	0.0422		
	[0.0060,0.0303]***	[0.0088,0.0332]***	cm_chf	[-0.0386,-0.0246]***		[0.0331,0.0512]***		
2008 Q4	0.0164	0.0202	AHRQ Comorb.	-0.0157	Charlson 2	0.0086		
	[0.0055,0.0273]***	[0.0092,0.0313]***	cm_chrnlung	[-0.0195,-0.0119]***		[0.0058,0.0115]***		
Constant	0.0708	0.0373	AHRQ Comorb.	0.0090	Charlson 3	0.0205		
	[0.0645,0.0772]***	[0.0196,0.0551]***	cm_coag	[0.0070,0.0110]***		[0.0139,0.0271]***		
Hospital Indicators	Yes	Yes	AHRQ Comorb.	0.0016	Charlson 4	0.0001		
Covariates	No	Yes, cont. next cols.	cm_depress	[-0.0007,0.0039]		[-0.0021,0.0023]		
N (Nonelderly)	23,860,930	23,860,930	AHRQ Comorb.	-0.0570	Charlson 5	-0.0094		
R Squared	0.0659	0.0939	cm_dm	[-0.0688,-0.0452]***		[-0.0131,-0.0058]***		

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

95% bootstrapped CI, blocks by state, 1000 reps: +++ Significant at .01, ++ Significant at .05, + Significant at .10

MA*During always included but coefficient not always reported. All specifications and means weighted using discharge weights.

Coefficients on hospital fixed effects not reported. Omitted categories: MA*Before, 2004 Q1, Male, White, Zip in Fourth Income Quartile.

Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Appendix Table 6: Inpatient and Pediatric Quality Indicators in NIS

Inpatient Quality Indicators	Improvement?	MA* After		Improvement?	MA* After, Risk Adjusted		N, Mean MA Before	
IQI 1 Esophageal Resection Volume		0.0000	[-0.0000,0.0000]	N	0.0000	[0.0000,0.0000]**	23,847,297	0.0002
IQI 2 Pancreatic Resection Volume	Y	-0.0001	[-0.0001,-0.0000]***	N	0.0000	[-0.0001,-0.0000]***	23,847,297	0.0002
IQI 4 AAA Repair Volume		0.0000	[-0.0000,0.0000]		0.0000	[-0.0000,0.0000]	23,847,297	0.0004
IQI 6 Percut. Transluminal Cor. Angioplasty, Volume	Y	-0.0010	[-0.0019,-0.0002]**		-0.0006	[-0.0013,0.0001]	23,847,297	0.0162
IQI 7 Carotid Endarterectomy, Volume	Y	-0.0001	[-0.0002,-0.0000]***	Y	-0.0002	[-0.0002,-0.0001]***	23,847,297	0.0013
IQI 8 Esophageal Resection Mortality		-0.0010	[-0.1050,0.1031]		-0.0065	[-0.1066,0.0936]	953	0.0357
IQI 9 Pancreatic Resection Mortality		0.0252	[-0.0196,0.0701]		0.0249	[-0.0219,0.0716]	2,327	0.0257
IQI 11 AAA Repair Mortality	Y	-0.0331	[-0.0516,-0.0146]***	Y	-0.0305	[-0.0477,-0.0133]***	6,580	0.0731
IQI 12 CABG Mortality		0.0014	[-0.0017,0.0045]	N	0.0040	[0.0006,0.0073]**	108,373	0.0132
IQI 13 Craniotomy Mortality	Y	-0.0107	[-0.0183,-0.0031]***	Y	-0.0078	[-0.0159,0.0003]*	66,356	0.0423
IQI 14 Hip Replacement Mortality		-0.0004	[-0.0010,0.0003]		-0.0001	[-0.0008,0.0006]	89,199	0.0000
IQI 15 AMI Mortality	N	0.0131	[0.0104,0.0158]***	N	0.0135	[0.0114,0.0157]***	216,870	0.0210
IQI 16 Congestive Heart Failure (CHF) Mortality	N	0.0037	[0.0024,0.0050]***	N	0.0040	[0.0025,0.0054]***	269,453	0.0162
IQI 17 Acute Stroke Mortality		0.0014	[-0.0031,0.0058]	N	0.0153	[0.0104,0.0202]***	149,772	0.0813
IQI 18 Gastrointestinal Hemorrhage Mortality	N	0.0029	[0.0004,0.0054]**		0.0008	[-0.0016,0.0033]	159,381	0.0133
IQI 20 Pneumonia Mortality	N	0.0021	[0.0001,0.0041]**	N	0.0040	[0.0023,0.0057]***	328,652	0.0221
IQI 21 Cesarean Delivery Rate	N	0.0078	[0.0037,0.0120]***	Y	-0.0024	[-0.0044,-0.0003]**	3,477,744	0.2653
IQI 22 VBAC Rate Uncomplicated	Y	-0.0107	[-0.0186,-0.0027]***	N	0.0061	[0.0021,0.0101]***	515,576	0.1397
IQI 23 Laparoscopic Cholecystectomy Rate	Y	-0.0362	[-0.0452,-0.0273]***	Y	-0.0401	[-0.0503,-0.0298]***	219,424	0.8138
IQI 25 Bilateral Cardiac Catheterization Rate	Y	-0.0380	[-0.0416,-0.0344]***	Y	-0.0372	[-0.0407,-0.0338]***	568,498	0.1372
IQI 26 CABG Area Rate	Y	-0.0003	[-0.0005,0.0000]*		0.0000	[-0.0003,0.0002]	23,847,297	0.0043
IQI 27 PCTA Area Rate	Y	-0.0009	[-0.0017,-0.0001]**		-0.0005	[-0.0011,0.0002]	23,847,297	0.0155
IQI 28 Hysterectomy Area Rate		0.0003	[-0.0007,0.0012]		-0.0005	[-0.0013,0.0004]	23,847,297	0.0147
IQI 29 Laminectomy or Spinal Fusion Area Rate	Y	-0.0005	[-0.0009,-0.0001]**		0.0002	[-0.0003,0.0008]	23,847,297	0.0145
IQI 30 Percut. Transluminal Cor. Angioplasty, Mort.		0.0042	[0.0030,0.0053]***	N	0.0046	[0.0035,0.0058]***	345,200	0.0062
IQI 31 Carotid Endarterectomy, Mortality	Y	-0.0026	[-0.0044,-0.0008]***	Y	-0.0023	[-0.0046,0.0000]*	27,336	0.0037
IQI 32 AMI Mortality WO Transfer	N	0.0148	[0.0110,0.0187]***	N	0.0118	[0.0086,0.0149]***	142,418	0.0267
IQI 33 Primary Cesarean Delivery Rate	N	0.0095	[0.0058,0.0132]***	Y	-0.0017	[-0.0036,0.0001]*	2,962,168	0.1710
IQI 34 VBAC Rate All	Y	-0.0089	[-0.0169,-0.0009]**		0.0012	[-0.0033,0.0057]	588,227	0.1350
Pediatric Quality Indicators								
PDI 90 Overall PDI	N	0.0048	[0.0033,0.0062]***		0.0003	[-0.0008,0.0014]	3,085,305	0.0228
PDI 91 Acute PDI	N	0.0007	[0.0000,0.0013]**		0.0000	[-0.0006,0.0005]	3,085,305	0.0077
PDI 92 Chronic PDI	N	0.0041	[0.0030,0.0052]***		0.0003	[-0.0004,0.0011]	3,085,305	0.0151
PDI 1 Accidental Puncture or Laceration	Y	-0.0003	[-0.0004,-0.0001]***	Y	-0.0003	[-0.0004,-0.0001]***	3,085,305	0.0014
PDI 2 Pressure Ulcer	N	0.0011	[0.0002,0.0021]**	N	0.0007	[-0.0001,0.0016]*	319,153	0.0027
PDI 5 Iatrogenic Pneumothorax		0.0000	[-0.0000,0.0001]		0.0000	[-0.0000,0.0001]	2,768,569	0.0003
PDI 6 Pediatric Heart Surgery Mortality	Y	-0.0473	[-0.0623,-0.0323]***	Y	-0.0706	[-0.0819,-0.0593]***	19,902	0.0301
PDI 7 Pediatric Heart Surgery Volume		0.0001	[-0.0004,0.0007]		0.0003	[-0.0003,0.0010]	3,085,305	0.0191
PDI 8 Postoperative Hemorrhage or Hematoma	N	0.0015	[0.0002,0.0028]**		0.0009	[-0.0005,0.0023]	115,041	0.0014
PDI 9 Postoperative Respiratory Failure	N	0.0068	[0.0034,0.0101]***	Y	-0.0049	[-0.0075,-0.0024]***	94,474	0.0099
PDI 10 Postoperative Sepsis	N	0.0056	[0.0009,0.0103]**		-0.0022	[-0.0074,0.0030]	89,923	0.0221
PDI 11 Postoperative Wound Dehiscence	N	0.0024	[0.0016,0.0033]***	N	0.0022	[0.0014,0.0030]***	81,672	0.0004
PDI 12 Cent. Venous Catheter-Related Blood. Infect.	Y	-0.0017	[-0.0021,-0.0013]***	Y	-0.0018	[-0.0022,-0.0015]***	2,397,245	0.0065
PDI 13 Transfusion Reaction		0.0000	[-0.0000,0.0000]		0.0000	[-0.0000,0.0000]	3,085,305	0.0000
PDI 14 Asthma Admission Rate		-0.0006	[-0.0024,0.0012]		-0.0003	[-0.0022,0.0016]	956,902	0.0126
PDI 15 Diabetes Short-term Comp. Admissions		-0.0001	[-0.0004,0.0001]		-0.0002	[-0.0004,0.0001]	245,451	0.0000
PDI 16 Gastroenteritis Admission Rate	N	0.0018	[0.0011,0.0026]***		0.0005	[-0.0004,0.0015]	1,052,379	0.0053
PDI 17 Perforated Appendix Admission Rate	N	0.0117	[0.0043,0.0192]***		-0.0004	[-0.0049,0.0041]	108,432	0.0173
PDI 18 Urinary Tract Admission Rate	N	0.0097	[0.0081,0.0113]***	N	0.0010	[0.0005,0.0014]***	3,085,305	0.0241
NQI 1 Iatrogenic Pneumothorax in Neonates	N	0.0003	[0.0000,0.0006]**	N	0.0009	[0.0007,0.0011]***	3,085,305	0.0023
NQI 2 Neonatal Mortality	N	0.0060	[0.0038,0.0081]***	N	0.0027	[0.0008,0.0047]***	3,085,305	0.0320
NQI 3 Neonatal Blood Stream Infection	Y	-0.0594	[-0.0751,-0.0437]***	Y	-0.0150	[-0.0221,-0.0079]***	69,873	0.2609
PSI 17 Birth Trauma - Injury to Neonate	N	0.0009	[0.0003,0.0015]***		0.0004	[-0.0002,0.0009]	3,085,305	0.0099
PQI 9 Low Birth Weight	Y	-0.0091	[-0.0145,-0.0037]***	Y	-0.0132	[-0.0177,-0.0086]***	1,062,131	0.1855

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10

Y and N indicate statistically significant gains and losses, respectively.

MA*During always included but coefficient not always reported. All specifications and means are weighted using discharge weights.

All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.

Risk adjusters include six 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, All-Patient Refined (APR)-DRGs, and All-Payer Severity-adjusted (APS)-DRGs. See Appendix 2.

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.

Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Appendix Table 7: Robustness to Included States in NIS

	(1) Uninsured	(2) Length of Stay	(3) Log Length of Stay	(4) Emergency Admit
All States				
MA*After	-0.0231	-0.0504	-0.0012	-0.0202
	[-0.0300,-0.0162]***	[-0.0999,-0.0008]**	[-0.0111,0.0086]	[-0.0397,-0.0007]**
N (Nonelderly)	23,860,930	23,913,183	23,913,183	23,913,983
Mean MA Before	0.0643	5.4256	1.4267	0.3868
Mean Non-MA Before	0.0791	5.0770	1.3552	0.3591
Mean MA After	0.0352	5.3717	1.4355	0.4058
Mean Non-MA After	0.0817	5.0958	1.3596	0.3745
Northeast				
MA*After	-0.0160	-0.0293	-0.0070	-0.0293
	[-0.0309,-0.0011]**	[-0.2007,0.1421]	[-0.0551,0.0411]	[-0.0500,-0.0085]**
N (Nonelderly)	4,510,280	4,511,992	4,511,992	4,512,094
Mean Non-MA Before	0.0790	5.6529	1.4163	0.4281
Mean Non-MA After	0.0613	5.6453	1.4203	0.4425
New England				
MA*After	-0.0159	-0.0021	0.0170	-0.0411
	[-0.0193,-0.0125]***	[-0.1276,0.1233]	[-0.0214,0.0554]	[-0.0749,-0.0072]**
N (Nonelderly)	1,369,181	1,370,420	1,370,420	1,370,438
Mean Non-MA Before	0.0450	5.3462	1.3890	0.3337
Mean Non-MA After	0.0408	5.3576	1.3840	0.3324
All, No ME, VT, CA				
MA*After	-0.0236	-0.0537	-0.0022	-0.0202
	[-0.0311,-0.0161]***	[-0.1082,0.0008]*	[-0.0129,0.0085]	[-0.0417,0.0013]*
N (Nonelderly)	21,067,733	21,119,253	21,119,253	21,119,708
Mean Non-MA Before	0.0837	5.0926	1.3606	0.3653
Mean Non-MA After	0.0871	5.0996	1.3644	0.3813
25 Most Insured				
MA*After	-0.0246	-0.0477	0.0056	-0.0066
	[-0.0367,-0.0126]***	[-0.0817,-0.0138]***	[-0.0012,0.0124]	[-0.0623,0.0491]
N (Nonelderly)	8,028,553	8,057,249	8,057,249	8,057,439
Mean Non-MA Before	0.0595	4.9274	1.3424	0.3300
Mean Non-MA After	0.0638	4.9342	1.3438	0.3426
Balanced Panel (Only Hospitals That Appear in Data in Every Possible Quarter)				
MA*After	-0.0273	0.1087	0.0297	0.0244
	[-0.0500,-0.0045]**	[-0.0872,0.3045]	[-0.0038,0.0633]*	[-0.0705,0.1193]
N (Nonelderly)	768,541	770,696	770,696	770,699
Mean MA Before	0.0461	5.4089	1.4104	0.4701
Mean Non-MA Before	0.0714	4.6790	1.3037	0.2769
Mean MA After	0.0279	5.4701	1.4323	0.4902
Mean Non-MA After	0.0800	4.6364	1.2969	0.2720

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10
MA*During always included but coefficient not always reported.
All specifications and means weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.
"All States" sample includes all states in the 2004-2008 NIS. Does not include AL, AK, DE, ID, MS, MT, ND, NM.
"New England" sample includes MA, CT, NH, VT, ME, RI. "Northeast" sample includes New England, NY, NJ.
"25 Most Insured" includes the top half of states in terms of initial levels of insurance from the CPS.
In decreasing order of insurance, "25 Most Insured" includes: MN, HI, IA, NH, ME, WI, MA, VT, KS, RI, NE, MI, ND (not in NIS), CT, PA, OH, M0, SD, DE (not in NIS), VA, MD, TN, KY, UT.
"Balanced Panel" only includes states that appear in every possible quarter of the data. These hospitals are in AZ (2), CA, CO (2), GA, MA (2), MD, MN, NC, NE, OH, TN, TX (4), UT, WA (4), WV (2).
Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.

Appendix Table 8: Insurance and Outcomes in Low vs. High Uninsured Hospitals in NIS

	(1) **Uninsured**	(2) **Length of Stay**	(3) **Log Length of Stay**	(4) **Emergency Admit**
All Hospitals (100% of sample, including missing ages)				
Ma*After	-0.0153	-0.0626	0.0006	-0.0367
	[-0.0201,-0.0104]***	[-0.1102,-0.0150]**	[-0.0090,0.0103]	[-0.0585,-0.0149]***
N (All Ages)	36,282,073	36,343,449	36,343,449	36,345,238
N Hospitals	3,090	3,090	3,090	3,090
N Hospitals in MA	48	48	48	48
Mean MA Before	0.0409	5.8833	1.5099	0.4789
Mean Non-MA Before	0.0543	5.6256	1.4517	0.4291
Mean MA After	0.0226	5.7516	1.5084	0.4912
Mean Non-MA After	0.0557	5.5819	1.4482	0.4491
Only Hospitals that Appear in Data At Least Once Before Reform				
Ma*After	-0.0152	-0.0651	-0.0001	-0.0369
	[-0.0202,-0.0102]***	[-0.1155,-0.0147]**	[-0.0104,0.0102]	[-0.0596,-0.0143]***
N (All Ages)	30,390,240	30,441,321	30,441,321	30,442,760
N Hospitals	2,315	2,315	2,315	2,315
N Hospitals in MA	40	40	40	40
Mean MA Before	0.0409	5.8833	1.5099	0.4789
Mean Non-MA Before	0.0543	5.6256	1.4517	0.4291
Mean MA After	0.0230	5.7678	1.5085	0.4856
Mean Non-MA After	0.0595	5.5803	1.4460	0.4473
Low Uninsured Hospitals				
Ma*After	-0.0178	-0.0895	0.0019	-0.0409
	[-0.0228,-0.0129]***	[-0.1522,-0.0269]***	[-0.0074,0.0112]	[-0.0577,-0.0241]***
N (All Ages)	14,407,631	14,426,624	14,426,624	14,427,194
N Hospitals	1,157	1,157	1,157	1,157
N Hospitals in MA	29	29	29	29
Mean MA Before	0.0303	5.8769	1.5058	0.4431
Mean Non-MA Before	0.0210	5.6142	1.4487	0.4087
Mean MA After	0.0220	5.7938	1.5119	0.4742
Mean Non-MA After	0.0295	5.6036	1.4438	0.4306
High Uninsured Hospitals				
Ma*After	-0.0165	-0.0260	-0.0063	-0.0086
	[-0.0246,-0.0085]***	[-0.0782,0.0263]	[-0.0151,0.0024]	[-0.0312,0.0140]
N (All Ages)	15,982,609	16,015,279	16,015,279	16,016,149
N Hospitals	1,158	1,158	1,158	1,158
N Hospitals in MA	11	11	11	11
Mean MA Before	0.0635	5.8969	1.5186	0.5550
Mean Non-MA Before	0.0848	5.6360	1.4546	0.4478
Mean MA After	0.0314	5.5593	1.4808	0.5769
Mean Non-MA After	0.0845	5.5609	1.4478	0.4612

95% asymptotic CI clustered by state: *** Significant at .01, ** Significant at .05, * Significant at .10
MA*During always included but coefficient not always reported.
All specifications and means weighted using discharge weights.
All specifications include hospital fixed effects and time fixed effects for 2004 to 2008, quarterly.
Low Uninsured Hospitals have less than median uninsurance of 0.0398 before reform.
Sample sizes vary across specifications based on availability of dependent variable.
Source: HCUP NIS 2004-2008 authors' calculations. See text for more details.