Insurance Expansions and Hospital Utilization: Relabeling and Reabling?

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The 2010 Patient Protection & Affordable Care Act (ACA) significantly expanded access to private and public health insurance for low-income individuals through incomebased subsidies and income-based eligibility expansions, respectively. In this paper, we use the universe of hospitals from 2009-2015 to characterize how these expansions affected the financing of hospital visits, along with price, utilization, and potential spillovers in the quality of care. The insurance coverage expansions generated a shift in the composition of payers and a modest increase in the utilization of hospital outpatient services. While concerns have been raised that these shifts in utilization could cause negative spillovers to the already insured population (e.g., Medicare enrollees), we find no significant change in the quality of care experienced by those already insured. The primary result of both federally funded insurance expansions was to increase the profits generated and prices charged by the hospitals providing such services.

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1 Introduction

Upon the passage and subsequent implementation of the Patient Protection & Affordable Care Act (ACA) in 2010, nearly 20 million Americans gained access to health insurance for the first time (Garrett and Gangopadhyaya, 2016). The effects of such a momentous influx of new patients onto this uniquely important market have been the subject of relentless political and academic debate. Although considerable attention has been devoted to the insurance gains begotten by the Medicaid expansion component of the ACA, the establishment of private market insurance subsidies has accounted for no less than 40% of the overall coverage gains resulting from the law's implementation (Frean et al., 2017). However, markedly less focus has been placed upon how these gains may have impacted utilization behaviors and the prices faced by patients, both new and old, as well as the financial health of hospitals. In this paper, we chip away at these questions by studying how the dual insurance expansions have affected the provision of hospital-based care along the price and quantity dimensions. In light of the many changes occurring as a result of the law, we also investigate potential spillovers to the quality of care received by the patient population that was already insured pre-ACA. A better understanding of how this surge in newly insured patients affects the hospital industry is critical for gauging the impact of the new law as hospitals account for approximately 47 percent of health care services consumed.¹

Many papers studying the effects of the ACA often focus on the impact in a particular state (e.g., Duggan et al. (2019a) examining Calfornia, or Baicker and Finkelstein (2011); Finkelstein et al. (2012); Taubman et al. (2014), who study the Oregon Health Insurance Experiment) or rely on state-level variation in Medicaid expansion decisions (Courtemanche et al., 2016; Pines et al., 2016; Kaestner et al., 2017). In contrast, our paper focuses measurement on hospitals across the entire U.S. and identifies the impact of the law by exploiting county-level variation in the degree to which the population is exposed to the Medicaid expansion and to private, non-group premium subsidies.² To gain an even more precise measure of the reform's impact on individual hospitals, we combine these exposure variables with patient flow data to estimate the fraction of a *hospital's* patient population that gains insurance through the ACA reforms. Using these precisely targeted measures, we examine the effects of the insurance expansions on utilization, the financing of care, and the potential for spillovers in the quality of care received by the already insured population. Our

¹It accounts for roughly 37 percent of total health care consumption expenditures, including prescription drugs and other medical care goods.

²Our approach overlaps most completely with that of Courtemanche et al. (2019a), who exploit local variation in the pretreatment uninsured rate to identify the effect of ACA insurance expansions on risky health behaviors.

paper adds to the substantial yet growing literature on the effects of health insurance expansions on hospital utilization, and on how such care is financed (Manning et al., 1987; Miller, 2012a,b; DeLeire et al., 2014; Rudowitz and Garfield, 2015; Anderson et al., 2016; Blavin, 2016; Kaufman et al., 2016; Nikpay et al., 2016). We also contribute to the more recent literature examining potential spillover effects of insurance expansions on the outcomes of patient populations that are not directly affected by the ACA (McInerney et al., 2017; Glied and Hong, 2018; Carey et al., 2020).

We find that the ACA insurance expansions, which lowered the cost of care for millions of newly insured individuals, led to a modest increase in outpatient hospital visits following the passage of the new law. The effect on utilization is consistent with a large body of literature on the effects of insurance expansions, including evidence from the Oregon Health Insurance Experiment (Baicker and Finkelstein, 2011; Finkelstein et al., 2012; Taubman et al., 2014) and prior ACA evidence (Pines et al., 2016; Duggan et al., 2019a). The outpatient utilization effects are also consistent with Garthwaite et al. (2019), who examine hospital claims level data from 20 states and show that even the most recent round of Medicaid expansions generated heterogeneous utilization effects across states, varying positively with the size of the group gaining coverage and negatively with the pre-expansion level of uncompensated care costs.³ Unlike Garthwaite et al. (2019), however, we do not find convincing evidence that the recent round of insurance expansions increased the use of inpatient care.

The effects on emergency department care are ambiguous. While the cost of emergency care falls for newly insured individuals, the use of primary care services could potentially offset this effect. We find no evidence of a change in emergency department visits, which is consistent with the Oregon Health Insurance (Baicker and Finkelstein, 2011; Finkelstein et al., 2012; Taubman et al., 2014) and prior ACA evidence (Pines et al., 2016). Our results contrast with Miller (2012a,b), who find that insurance provision decreased the incidence of emergency department visits without sacrificing health outcomes, an apparent efficiency gain. Our results also differ from that of Duggan et al. (2019a), who find evidence of increases in emergency department visits caused by the Medicaid insurance expansion in California.

One potential reason for the divergence of our results with that of Duggan et al. (2019a) is that unlike in our paper, they use individual claims data, which allows them to observe outcomes specifically for the 21-64 year-old age group targeted by the reforms. By contrast, our hospital-level results necessarily include outcomes for unaffected groups, and so our estimated effects are a percent increase relative to a larger base. The magnitudes of our

 $^{^{3}}$ Our identification strategy, indeed, also exploits this former source of heterogeneity to produce our main estimates. While we use variation in exposure to the ACA across counties, Garthwaite et al. (2019) use even finer variation at the zip-code level.

percent change estimates accordingly fall below those of Duggan et al. (2019a).

While we estimate that the effects on total hospital utilization are modest, we find that the law's primary impact was to produce both a shift in how hospital care has been financed-from uncompensated care to Medicaid and private payers-and an increase in prices and profits as reimbursement rates have become relatively more lucrative. The positive effects on prices, revenues, and profits are primarily driven by the Medicaid expansion, which matches the results from Duggan et al. (2019a) in the case of California. The concordance of our results with Duggan et al. (2019a) related to the pricing and financing of hospital services suggests the effects they find in California may be more generalizable to the effects of the ACA more broadly. In fact, we find nearly identical results, even after excluding California from our sample. Our results also corroborate and complement earlier evidence of the ACA effects on hospital finances (Blavin, 2016; Pines et al., 2016; Freedman et al., 2017).

In addition to altering patterns of utilization and hospital financing, the changes created by the ACA insurance expansions have raised concerns of potential spillover effects that could impact the quality of care for the population already insured prior to the ACA. As discussed in Carey et al. (2020), the potential negative spillover effects could arise if the expansion overburdened the health care system and affected outcomes for those already insured. Carey et al. (2020) examine the potential negative spillovers on utilization for the Medicare population and find no effects on primary care services. On the other hand, McInerney et al. (2017) find evidence of spillover effects from Medicaid expansions in the 2000s that reduced spending within the Medicare population but find no effects on the quality of care. Glied and Hong (2018) also find that prior insurance expansions generate negative spillover effects on the utilization for the Medicare population. Although not directly concerned with spillover effects, Kolstad and Kowalski (2012) find that the Massachusetts Health Reform insurance expansion reduced the length of hospital inpatient stays.

While this literature focuses on negative spillovers, it is also possible for positive spillovers to arise from the ACA, as hospitals may improve quality to compete for a newly-insured and more profitable patient population. We examine potential spillover effects on hospital services by primarily focusing on the already insured Medicare population. We find that the influx of newly-insured patients has no effect on Medicare inpatient utilization and no effect on a diverse set of quality indicators including measures of mortality and readmissions for the Medicare patient population. We also find no effects on quality for the full patient population based on patient surveys and process of care measures (i.e., does hospital follow standard medical care protocols?). Thus, this paper contributes to the spillover literature by showing that the public and private insurance expansions have no effect on the quality of care for those already insured, as is consistent with McInerney et al. (2017). It should be emphasized that these quality measures do not capture changes in the health of the newly-insured population, but only changes in the quality of the treatment provided. Indeed, recent research by Sommers et al. (2012); Sommers (2017); Goldin et al. (2021); Miller et al. (forthcoming) detect significant decreases in mortality for the Medicaid population following state Medicaid expansions.

The remainder of the paper proceeds as follows. The next section briefly summarizes the reforms. Section 3 describes the data sources from which the study sample is extracted, Section 4 lays out the identification assumptions and strategy, and Section 5 details how the insurance expansions affected healthcare utilization, prices, and finances. Section 6 ties together the study's findings by directly linking federal dollars provided for insurance expansions with hospital revenues, and then decomposing them into utilization and price effects. Finally, Section 7 provides some closing remarks and illuminates promising avenues for future research on these and related topics.

2 Background

The ACA was a major legislative reform intended to transform several aspects of the U.S. health care system. Facing an uninsurance rate of 16.3 percent in 2010,⁴ one of the primary goals of the law was to increase coverage by expanding access to both private and public health insurance.

As originally written, the law set out to increase public coverage through the expansion of Medicaid eligibility for all non-Medicare eligible individuals under the age of 65 with incomes up to 138 percent of the federal poverty line (FPL). A 2012 Supreme Court ruling upheld the constitutionality of the expansion, but limited the ability of the U.S. Department of Health and Human Services agency to enforce it, leaving the decision of whether to expand Medicaid coverage to the states.⁵ As of January 2014, when the Medicaid expansion initially took effect, 25 states opted to expand Medicaid eligibility (including the District of Columbia).⁶ The geographic differences in adoption of the Medicaid expansion generated differential effects of the law on coverage rates across areas.

A separate provision of the law expanded private insurance coverage by providing meanstested subsidies to eligible individuals to be applied toward purchasing insurance plans on the incipient exchanges. Specifically, individuals and families earning between 100 percent to 400 percent of the FPL, and who are ineligible for Medicaid coverage, would qualify for

 $^{^4{\}rm This}$ is based on the 2011 Current Population Survey, which indicates that there were 49.9 million uninsured individuals out of a population of 306.1 million.

⁵See National Federation of Independent Business v. Sebelius 567 U.S. 519 (2012).

⁶However, several states, such as CA, DC, MN, and WA, had elected to expand earlier than 2014.

these government-provided subsidies. The subsidy amount is determined according to a sliding scale, so that as an individual's income rises, the subsidy amount falls. For example, individuals and families earning up to 133 percent of the FPL would receive a subsidy limiting the amount they would need to pay toward premiums to no more than 2.5 percent of their income. Those whose incomes are between 300 and 400 percent of the FPL, by contrast, are required to pay no more than around 9.5 percent of their income toward premiums.⁷ When income rises above 400 percent of the FPL, the subsidy is eliminated completely. While private insurance subsidies were made available across the country, the law will tend to affect more dramatically those areas with a higher share of individuals in the targeted income range, and who would have remained uninsured absent the insurance subsidies.

The size and scope of the ACA insurance expansions may have also engendered changes to different facets of hospital operations. The expansions could have affected the provision of services, prices, and the finances of hospitals in numerous ways. The increase in insurance eligibility and coverage may have increased the use of medical care services at hospitals, as the out-of-pocket price of treatment would have fallen for newly-insured individuals. Given that Medicaid payments are lower than the private sector payments, but higher than uncompensated care, the effect of the Medicaid expansion on hospital profits should be positive. The average prices paid by private insurers tend to be even higher and so an expansion of these types of payers should have also increased hospital prices and profits. Moreover, unlike with Medicare and Medicaid, the prices paid by non-public payers are affected by market forces, as the short-term capacity of hospitals is limited and insurers must compete for access to hospital services.

All of these changes have potential implications for spillovers on the quantity and quality of treatment for the already insured population, but the expected direction of any effect is unclear. The greater utilization of hospital and physician services could strain resources and reduce the quality of care, affecting the quality of care for Medicare enrollees and other already privately-insured patients. The lower payment rates received from Medicaid patients, relative to those received from the privately insured, could also drive the quality of treatment down as hospitals attempt to control costs. Alternatively, the ACA could lead to greater incentives to improve quality to attract a newly-insured and more profitable patient population.

⁷More precisely, the subsidy amount, in turn, is determined by the premium of the second lowest cost silver plan in the area.

3 Data and Sample Construction

3.1 American Community Survey Data

Though Medicaid expansion decisions were made at the state-level, we construct countylevel measures of exposure to the Medicaid expansion by calculating, for each county, the fraction of the population comprised of low-income individuals who would have become newly eligible for Medicaid; that is, those earning less than their state's Medicaid income eligibility limit (typically 138% of the Federal Poverty Line) but more than their state's pre-ACA Medicaid income eligibility limit and who were uninsured during the last pre-reform year. In states that opted out of the Medicaid expansion in 2014-2015, this treatment variable equals 0. Figure 1 shows the geographic distribution of this exposure measure across the United States. Constructing the treatment in this way allows us to exploit both across and withinstate heterogeneity in the expected impact of the public insurance expansion. To capture variation in exposure to private market subsidies, we similarly construct a county-based measure of the fraction of the population made up of individuals in this intermediate income range (those earning between one and four times the Federal Poverty Level who are also ineligible to receive insurance through their state's Medicaid program) who are uninsured just prior to the ACA's private market exchange rollout. The geographic distribution of this latter treatment, in turn, is depicted in Figure 2. Combining state expansion decisions with county-level data on pre-reform income levels, Duggan et al. (2019b) employ a similar strategy for examining the effects of insurance expansions, though they focus primarily on labor market outcomes.

We use American Community Survey (ACS) data from the 2009-2013 five-year sample to construct our baseline controls and eligibility measures in the pre-reform period. We construct our main treatment variables–exposure to Medicaid eligibility and exposure to private market subsidies–by feeding 2013 county demographic data through 2014 and 2015 insurance eligibility rules for the uninsured population, similarly to how Courtemanche et al. (2019a) construct uninsured rates for MSAs in 2013, the last pre-reform year. We assess future eligibility for both Medicaid and private subsidies in this way, rather than using contemporaneous measures of eligibility at the time of ACA adoption. We use data from prior years because previous research has uncovered mixed evidence on how labor supply responds to altering the availability of public insurance, which in turn directly impacts the very income measures used to assess eligibility (Garthwaite et al., 2014; Shi, 2016; Kaestner et al., 2017). Thus, our approach avoids conflating our eligibility measures with policyinduced changes in income that might otherwise affect the size of the group who meets the relevant standards. In Appendix A.1, we further incorporate 2014 and 2015 ACS data on

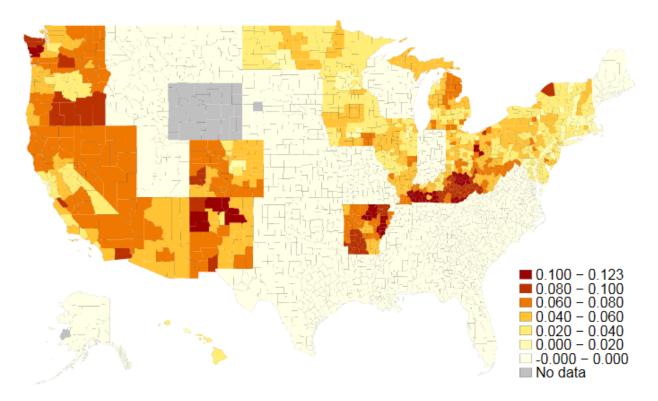


Figure 1: The above map shows county-level variation in the level of exposure to the Medicaid expansion by 2015. That is, each county is shaded in proportion to the fraction of individuals who are earning less than their state's 2015 Medicaid eligibility threshold and are uninsured.

Medicaid and private insurance participation in order to assess both the predictive power of our treatment variables and the extent to which any crowd-out may have occurred.

For descriptive purposes, we further partition our sample of hospitals according to the intensity of treatment of the counties in which they are located on both the public and private subsidy eligibility margins. Specifically, we split counties (and associated hospitals) according to whether the population lies above or below the median level of eligibility for participation in ACA-commissioned Medicaid expansions, which equals approximately 5.1% in the sample. Doing so allows for a comparison of how the outcome variables change over time for three distinct groups: a 'no exposure' group (counties in a non-expansion state), a 'low exposure' group (counties in expansion states but fall below the median level of Medicaid eligibility), and a 'high exposure' group (counties that exceed the median level of Medicaid eligibility). We use a similar procedure to characterize counties that are less or more exposed to exchange subsidies; here, the median fraction of the population eligible to receive such subsidies is 6.5%.

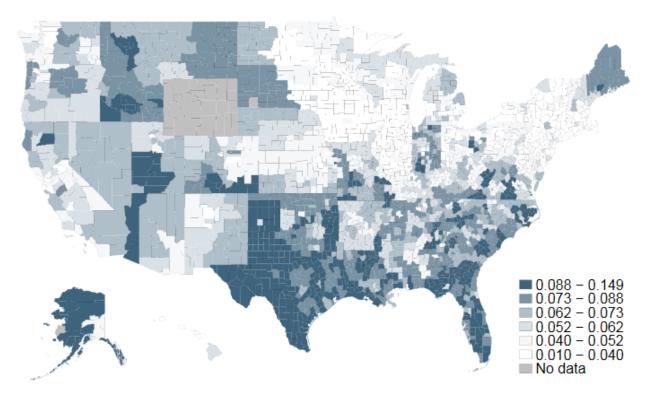


Figure 2: The above map shows county-level variation in the level of exposure to the private non-group insurance expansion by 2015. That is, each county is shaded in proportion to the fraction of individuals who are earning between one and four times the federal poverty level, are ineligible for Medicaid, and are uninsured.

3.2 American Hospital Association Survey and Center for Medicare and Medicaid Services Healthcare Cost Report Data

The study's primary outcome measures are extracted from two separate but interconnected sources of U.S. hospital data; the American Hospital Association (AHA) Survey and the Center for Medicare and Medicaid Services Healthcare Cost Reporting Information System (HCRIS). Each data source includes approximately 4,000 hospitals covering the periods between 2009 and 2015. The AHA Survey captures a variety of hospital utilization measures while the HCRIS data include summary measures from hospital financial reports such as costs, revenues, and profits–some of which are aggregated by payer. Both are national samples that include the full population of hospitals residing in the U.S. and its territories, although AHA survey response rates are missing for some small, rural hospitals.⁸

The AHA Survey includes hospital identifiers alongside a plethora of institutional details,

⁸Perez and Friedman (2017), who use the HCRIS data to the exclusion of the AHA Surveys, cite previously documented evidence of reporting biases in the AHA survey. In particular, survey compliance rates among small, rural hospitals are relatively low, which artificially suppresses their prevalence in the data (Mullner and Chung, 2002; Schrag et al., 2002).

such as total annual expenditures, the components of those expenditures (wages, facility expenses, depreciation, etc.), staffing levels by occupation, hospital type (non-profit, for-profit, and federal), bed counts, and the location of each hospital. Additionally, these data include utilization measures such as yearly discharges, emergency department visits, inpatient days and visits, and outpatient visits. We construct our primary dependent variables from these measures.

Specifically, we track the annual number of inpatient visits by payer type, including the annual number of inpatient visits financed by private (non-public) payers, those financed by Medicaid, and those financed by Medicare, the residual category. By aggregating across payer types, we are also able to track the total annual number of inpatient visits. While we are able to separate inpatient visits by payer type, the AHA Survey does not disaggregate other types of utilization by payer. Thus, we track aggregated measures of outpatient visits, emergency department visits, non-emergent visits, and total utilization, which combines outpatient and inpatient visits in the manner detailed in Section 4.

The HCRIS data are comprised of a national census of hospital financial reports. These data include a detailed set of annual revenue and cost measures, such as the cost of uncompensated care, the cost of Medicaid patients, total operating expenses, and net patient revenue. Our primary set of outcomes include annual measures of total patient revenue, operating expenses, and the profit margin, which is the ratio of total hospital revenues to costs. These two data sources, in turn, are combined to produce price per episode of care, the details of which also appear in Section 4.

Table 1 presents summary statistics on hospital utilization and finances before the rollout of the 2014 Medicaid expansions and private health insurance exchanges according to the intensity of the private subsidy availability treatment in each county. It also displays analogous summary statistics for hospitals based on their enveloping counties' exposure to ACA-generated increases in public insurance availability. While there exists substantial differences in pre-reform levels across most utilization and finances measures by level of exposure to the law, our difference-in-differences strategy requires only that the trends in these outcome variables do not differ prior to the implementation of the treatment. Assessment of the validity of this parallel trends assumption is deferred until Section 5.

3.3 Hospital Compare data

Several hospital quality measures are derived from the CMS Hospital Compare Database. The database includes more than 4,000 Medicare-certified hospitals, from which we cull the quality measures available surrounding the reform, from 2011 to 2016. The reported quality

	Med	icaid expo	Private exposure			
	none low high			low high		
	(1)	(2)	(3)	(4)	(5)	
Inpatient	. /				. /	
Non-public Visits	12,477	15,406	12,419	14,832	11,527	
	[20, 514]	[21, 866]	[17, 225]	[20, 698]	[19, 454]	
Medicaid Visits	8,571	11,458	9,801	10,542	8,650	
	[15, 163]	[21, 555]	[16, 662]	[19, 129]	[16, 376]	
Non-public Share IP Days	0.296	0.313	0.289	0.305	0.292	
-	[0.161]	[0.181]	[0.135]	[0.143]	[0.178]	
Medicaid Share IP Days	0.179	0.193	0.206	0.187	0.192	
· ·	[0.163]	[0.192]	[0.166]	[0.160]	[0.184]	
Total Visits	38,968	47,914	39,478	46,145	36,429	
	[53, 860]	[62, 271]	[48,009]	[58, 307]	[50, 753]	
General	. /]	. /]	. /]	. /]	. /]	
Total OP Visits	135,422	185,594	149,516	186,131	115,883	
	[204,868]	[250, 031]	[246, 595]	[260, 532]	[183,041]	
ED Visits	28,469	30,833	29,189	31,062	27,363	
	[32, 271]	[33,082]	[29,097]	[31,604]	[31, 795]	
Total Admissions	7,380	8,629	7,886	8,669	6,943	
	[10,034]	[10, 863]	[9,282]	[10, 532]	[9,529]	
Days per Admission	9.339	8.857	8.099	8.786	9.040	
, I	[27.711]	[16.435]	[25.199]	[22.009]	[27.122]	
Total Utilization ψ	77,422	100,615	81,934	98,998	69,335	
	[102, 327]	[121,784]	[108, 654]	[121, 747]	[92,877]	
Finances	L / J	L / J	L / J		L / J	
Patient Revenue ϕ	152	206	177	202	141	
	[235]	[310]	[265]	[298]	[221]	
Operating Expenses ϕ	147	210	174	202	137	
	[236]	[327]	[264]	[306]	[222]	
Profit Margin	1.120	1.102	1.109	1.114	1.111	
0	[0.528]	[0.459]	[0.542]	[0.690]	[0.209]	
Uncompensated Care Cost ϕ	7.373	6.414	7.980	6.657	7.923	
1	[22.0]	[13.2]	[62.4]	[15.4]	[47.9]	
Uncompensated Care Share	0.048	0.034	0.040	0.033	0.052	
1	[0.048]	[0.053]	[0.063]	[0.036]	[0.065]	
Total Medicaid Costs $^{\phi}$	15.1	24.4	24.5	22.7	21.4	
	[35.4]	[53.5]	[130]	[48.2]	[37.6]	
Hospitals	2,138	1,120	1,116	2,227	2,147	
Counties	1,330	642	571	1,157	1,250	

Table 1: Pre-ACA Summary Statistics, by Exposure to each Insurance Expansion

Each cell reports the average pre-2014 annual values [standard deviations] of the variable indicated in the first column. Column (1) reflects values for the average hospital in a county with no Medicaid expansion, Column (2) does so for those with belowmedian Medicaid expansion exposure, and Column (3) for those with above-median Medicaid expansion exposure. Columns (4) and (5) report corresponding values for hospitals with below and above median-level exposure to the private expansions. All values are derived from the 2009-2013 HCRIS and AHA databases.

 ψ Total Utilization is calculated by taking the revenue-weighted sum of inpatient and outpatient visits, where outpatient visits are weighted by the ratio between revenue generated by one outpatient day to that generated by one inpatient day.

 $^{\phi}$ Values listed in millions of dollars.

measures are all built from retrospective data and are typically lagged one year, so that the quality measures contained in the 2011 to 2016 Hospital Compare Database pertain to the time period from 2010 to 2015. Since quality effects may propagate relatively slowly and because some of the quality measures use multiple years of data, we remove the data that covers the 2014 period and focus on quality measures from 2015.

There are a large variety of measure types available through the Hospital Compare Database. The quality measures fall into four categories: (1) mortality; (2) process; (3) readmission; and (4) patient surveys. Both the readmission and mortality measures are constructed from the Medicare Fee-for-Service population claims, while the process-based measures (i.e., are hospitals following best practices?) and patient surveys cover the entire hospital patient population. Notably, other ACA reforms enacted simultaneously with the insurance expansions—such as the Hospital Readmissions Reduction Program (HRRP), Hospital Value Based Purchasing Program, and Bundled payments program—also targeted the same readmissions and mortality outcomes measured in the Hospital Compare data. Thus, we think it prudent to place less weight on all results pertaining to the mortality and readmissions measures because it is not possible to disentangle insurance expansion effects from those yielded by these other ACA quality-based initiatives.⁹

Even still, viewing any of the quality measures in isolation is potentially problematic, either because of small sample sizes or because the focus of the measure is overly narrow (e.g., a patient survey question about nurse communication). We circumvent this issue by constructing quality indices that capture these four different dimensions of quality. The indexes are constructed in a way that retains the maximum number of indicators for each hospital, where higher measures correspond to poorer quality for mortality, process measures, and readmissions but reflect superior quality for the patient survey-based index.¹⁰ Sections A.2 and A.3 of the Appendix contains a more detailed discussion of how the study's quality indices are formed along with the complete list of the variables used to construct them. Also in Appendix Section A.4 is Table A5, which shows the summary statistics for each of the four main Hospital Compare quality indices. Note that the process measures are slightly skewed toward higher-quality scores and that both the range and standard deviation are smaller than for the other quality indices. This suggests that there is less variation in hospital quality along the process measure dimension than for the patient survey, readmission, or mortality measures.

As we do not have information solely on individuals that gained coverage through the

⁹We thank an anonymous referee for bringing to our attention this important caveat.

¹⁰Since the available measures for mortality, process measures, and readmission measures fluctuate over time, we construct a second set of indices for 2013 forward that reflect a wider range of indicators, sacrificing the length of the panel for the breadth of outcomes.

ACA, all of these quality measures capture spillover effects. The readmission and mortality measures are based on Medicare data, so they are limited to spillover effects on the Medicare population. While the other measures are based on the broader hospital patient population, most of this population was already insured prior to the ACA, so these should also be considered spillover measures as well.

3.4 Quality Measures from Medicare Claims Data

As a final check on spillover effects on quality, we incorporate quality measures from a 5% national sample of CMS Medicare claims data. Specifically, we construct from the 2010-2015 sample of claims a 30-day, 60-day, and 90-day mortality index for those patients suffering from 18 different "non-deferrable" acute conditions for which mortality is relatively common among the Medicare population, such as intracerebral hemorrhages, cerebral artery occlusions, convulsions, head injury, and fractures of the neck (Card et al., 2009; Ballard et al., 2010; Garthwaite et al., 2017).¹¹ Because other quality-based ACA initiatives targeted mortality for acute myocardial infarction (AMI), heart failure, and pneumonia, we are careful to avoid estimating effects on outcomes for patients suffering from these afflictions as including them may lead us to misattribute the changes in health to the insurance expansion, rather than the quality-based initiatives. This is especially likely as hospitals in lower income markets are also the ones that experienced the largest gains in coverage and were also more vulnerable to penalization under the Hospital Value Based Purchasing (HVBP) program, for example.¹² After pooling the sample across these five years, we retain mortality outcomes for approximately 390,000 Medicare patients distributed across nearly 4,000 hospitals.¹³

These data offer several advantages over much of the Hospital Compare data. First, the outcomes are relatively easy to interpret because they reflect simple mortality rates. Second, by using individual level data, we can exclude conditions affected by other quality-based ACA initiatives (i.e., AMI, heart failure, and pneumonia) and we can control for demographics and health conditions of the individuals with the observed health events. Third, we can customize the outcome measure to different lengths (e.g., 30, 60 and 90-day measures). Fourth, similar to the mortality and readmission indices in the Hospital Compare data, the measure is focused on the Medicare population rather than that full patient population. To the extent that the insurance expansions produce changes in the composition of the patient population, focusing on Medicare outcomes allows us to focus on the spillover effect, and

 $^{^{11}\}mathrm{The}$ full list of conditions used are listed in Appendix A.3

¹²We are grateful to anonymous referee to pointing out the set of conditions targeted by other ACA reforms that were enacted simultaneously with the insurance expansions.

¹³These data were originally constructed for a paper by Dauda, Dunn, and Hall (2018).

circumvent the thorny issue of disentangling true changes in hospital quality from changes in a hospital's patient population as a result of the ACA.¹⁴

Again, it should be emphasized that this measure is centered on the spillover effects of the reform rather than its direct effect on the outcomes of those patients whose insurance status changed.

4 Regression Framework and Identification

To assess how increased access to both public and private non-group insurance affects hospital utilization, prices, finances, and quality spillovers, we utilize a difference-in-differences framework with a continuous treatment variable. As mentioned earlier, we construct our treatment variables using 2013 ACS data to calculate the fraction of the county's population that is uninsured and will have become newly eligible for either insurance expansion in 2014 and 2015. While the ACS provides geographic identifiers at the public use microdata area (PUMA) level, we use the PUMA to county mapping files to estimate treatment sizes for these more recognizable geographic units. Given that the outcomes are measured at the hospital level while the treatments-i.e., exposure to the ACA-induced expansions of Medicaid eligibility and private insurance subsidies-are county-specific, one possible identification assumption is that individuals tend to seek care at hospitals within their county. This assumption is consistent with the well-established empirical feature of health care markets (McGuirk and Porell, 1984; Buchmueller et al., 2006; Baker et al., 2016) that patients tend to choose medical care facilities that minimize travel time and distance.

However, there is also substantial evidence that county borders do not pose a meaningful barrier to access as consumers often cross county and state borders to seek care (Garnick et al., 1987; Radany and Luft, 1993; Yip and Luft, 1993). Instead of imposing this strong assumption, we use Medicare claims data with information on hospital and patient locations to estimate the expected hospital patient population emanating from each county. The estimated patient flows are used to construct hospital-specific weights that relate the exposure to the ACA for individuals residing in a county to a hospital-specific patient population.¹⁵

¹⁴It should be noted that the Hospital Compare data also include mortality information that is based on the full sample of Medicare fee-for-service claims data. The key disadvantage of the Hospital Compare mortality data is that individual-level controls cannot be included and the data is pooled over multiple years, as is discussed in Appendix A.2.

¹⁵Here, we describe this approach in greater detail. Let $S_{c,h}^{MDR}$ be the share of inpatient Medicare patients residing in county c and receiving service in hospital h, where this share is averaged over the 2010-2013 period to reduce year-to-year variance. This share is then scaled to the total population in each county to produce an expected number of patients coming from county c and traveling to hospital h. That is, the expected patient count coming from county c is $EP_{c,h} = P_c \cdot S_{c,h}^{MDR}$ where P_c is the total population count for county c. The calculated weight for county c for hospital h is the expected share of patients coming

We explicitly weight all treatments and controls by our constructed hospital-specific weights. These weights, when applied to the covariates, attempt to re-create the characteristics of the patient population actually seen at each hospital in the sample. This procedure creates a sensible mapping between the extent to which a county is treated by the ACA's insurance expansions and the extent to which a hospital is treated by the ACA's insurance expansions. For example, suppose hospital 1 is located in county B but receives 50% of its patients from county A, where 10% of the population is newly eligible for Medicaid under the ACA; and the remaining 50% from county B, where no Medicaid expansion takes place. Under this weighting strategy, hospital 1 will receive a Medicaid treatment of 0.05 whereas it would have instead received a treatment of 0 under the initial simplifying assumption that all patients seek care only in the hospital that reside within their county. Thus, we use the county-weighting approach in all of our analysis, as it is theoretically superior to the assumption that all individuals seek care in hospitals within their county. This approach is consistent with much of the literature that applies hospital-specific flows and distance, rather than pre-defined geographic boundaries, to create relevant patient populations (Luft et al., 1990; Town and Vistnes, 2001; Gaynor and Vogt, 2003; Tay, 2003; Romley and Goldman, 2011; Dranove and Ody, 2016). It also captures the spirit of how the Dartmouth Atlas Project constructs Hospital Referral Regions (HRRs) based on patient flows from Medicare data.

We apply the weights just described to all of the associated covariates, including countylevel measures of exposure, to make all of the variables hospital-specific. We then estimate the following equation:

$$log(Y_{ht}) = \beta_0 + \beta_1(post_t \times Medicaid Newly Eligibles_h) + \beta_2(post_t \times Private Subsidy Eligible_h) + \beta_3(X_{ht}) + year_t + hospital_h + \beta_{state}(year_t \times I(state_h)) + \epsilon_{ht},$$
(1)

where subscripts h denote the hospital, and t the year (2009, 2010, 2011, 2012, 2013,

$$ES_{c,h} = \frac{EP_{c,h}}{\sum_{\forall c} EP_{c,h}}$$

The implicit assumption is that conditional on the county of residence, the travel patterns for the Medicare population is similar to that of the broader population. We eliminate county patient flows that account for less than 1 percent of the total flows, as these flows may introduce noise into the estimates.

from county c, which is equal to:

2014, or 2015).^{16,17}

First, we enumerate our outcome variables, log (Y_{ht}) , all of which capture measures of hospital utilization and finances. Our utilization measures are multifarious, recording separately the logged annual number of inpatient visits financed by Medicaid, Medicare, or private sources. They also include logged annual visits aggregated across all payers for each of the following categories: inpatient stays, outpatient visits, emergency department visits, non-emergency department visits, and a price-weighted measure of inpatient and outpatient visits, which we define explicitly in the subsequent paragraph.

The remaining dependent variables each capture at least one element of hospital revenues or costs. These include logged annual measures of hospital episode prices, revenues, operating expenses, profits, along with the total cost of uncompensated care and Medicaid visits. While these latter five measures are simply lifted from hospital income statements, the price measure is constructed in two steps, following Melnick et al. (2011); Trish and Herring (2015); Ho and Lee (2017); Roberts et al. (2017). First, we create an aggregated measure of total utilization by summing the median revenue-weighted number of inpatient and outpatient days at a particular hospital. Next, we divide the total patient revenue generated at the hospital by this adjusted utilization measure. We also run an alternative specification in which we replace the dependent variable with logged annual patient revenue and control for this utilization measure directly in the regression.

The coefficients of interest are β_1 and β_2 , which tell us the effect of a 100 percentage point increase in the fraction newly eligible for each insurance expansion on the percentage point change in our hospital outcomes. To determine the average effect of a Medicaid expansion or private insurance expansion on each outcome measure, one can simply scale these coefficients by the average percent of the population served by each hospital that has become eligible for either program. These fractions are 5.1% and 6.5%, respectively, (the same as the median fractions) and the associated implied effects are included in bold below the coefficient estimates in each regression table.

As is typical in all difference-in-differences studies, the core assumption underlying our identification strategy is one of parallel trends. That is to say, the growth rate in all of the

 $^{^{16}}$ In approximately 40% of hospitals, the fiscal year does not coincide exactly with the calendar year. In these cases, we modulate the treatment effect by the 'dose' received in a given calendar year. For example, a hospital whose fiscal year ends 09/31/14 but that expands Medicaid on 01/01/14 would receive a 3/4ths treatment dose in 2014 and a full dose in 2015.

¹⁷Following Frean et al. (2017), we had initially controlled for the fraction of individuals eligible for Medicaid coverage prior to the ACA in order to capture the so-called 'woodwork effect,' whereby some individuals who had been eligible for Medicaid prior to the ACA enrolled only after the ACA rollout in 2014 due to an enhanced awareness of their eligibility for the program. The main results were invariant to the inclusion of such a control and so-in the spirit of parsimony-we exclude it from the model.

outcome variables would have evolved similarly in the absence of the Medicaid or privateinsurance expansions. We also add hospital fixed effects, which absorb any hospital-specific heterogeneity in patient populations, hospital quality, and hospital practices that could affect utilization and finances. Beyond controlling for the standard main effects in the interaction term (year fixed effects), we also allow each state to have its own time trend, (*year*_t × $I(state_h)$). We further control for X_{ht} , a vector of time-varying demographic characteristics that could otherwise affect the demand for health care and the channel through which it is provided, such as the age distribution in the county and the county-specific unemployment rate. In the robustness section of the paper we address potential violations of the parallel trend assumption.

Another potential confound is heterogeneity in the market structure in which each hospital operates. It would be problematic if the effects of the insurance expansions are likewise correlated with changes in the hospital industry structure. This could be of particular importance when considering the effect of Medicaid and commercial insurance expansions on hospital prices and profits. To address this possibility, we also add controls for the hospital Herfindahl-Hirschman Index (HHI) using hospital system information available in the AHA data. Additionally, we include fixed effects for the network in which each hospital operates, as well as for the hospital system to which it belongs.

As previous scholarship has recognized, 5 states (California, Connecticut, Minnesota, New Jersey, and Washington) and the District of Columbia opted to boost Medicaid income eligibility limits early, between 2011 and 2013 (Sommers et al., 2013; Golberstein et al., 2015; Frean et al., 2017). By applying state-specific Medicaid and Childrens' Health Insurance Plan (CHIP) income eligibility limits¹⁸ to their appropriate demographic group in the ACS data (children, non-disabled adults, and parents), we explicitly control for such early Medicaid expansions in our regressions. That is, the primary treatment variable of interest captures only the size of the groups attaining eligibility for Medicaid in 2014 and 2015 who would have been ineligible previously. Nonetheless, we flexibly control for the possibility that outcomes may have evolved differently in these early expansion states (for example, to allow for the possibility that the effects of insurance expansion grow over time) by interacting an indicator for whether that county resides in a state that had expanded early with the traditional post-ACA sample period of 2014-2015. We fold this interaction term into our vector of hospital market characteristics, X_{ht} . Additionally, we assess the possibility that our results are being driven by these early expansion states by re-doing all analyses-in Appendix A.5-after having removed either all six early expansion states or California from the sample.

One other concern is the possibility that county-level differences in income distributions

¹⁸See https://www.kff.org/state-category/medicaid-chip/.

may be correlated with the expansion decisions. Here we rely on the results of a paper by Frean et al. (2017), which constructs a "simulated eligibility" instrument for both Medicaid expansions and private insurance subsidies based on a randomly selected national sample of families from the ACS. This approach, pioneered by Currie and Gruber (1996a,b) and Cutler and Gruber (1996), purges the estimates of any lingering association between the policy decision and the characteristics of the underlying population. Reassuringly, Frean et al. (2017), who estimate whether and how the ACA insurance expansions affected insurance coverage, find identical results when using either the reduced form or instrumental variable specifications. Therefore, we too need not worry about policy endogeneity when interpreting our estimates.

We also supplement our analyses with a test on inpatient utilization by Medicare patients. Given the recent work by Carey et al. (2020) that finds no spillover effects on the utilization of primary care services by Medicare enrollees using state-level variation in ACA expansion, we do not anticipate an effect on the Medicare inpatient sample. However, given our more targeted hospital-specific treatment analysis and the work by Kolstad and Kowalski (2012) that finds effects on inpatient length of stay, we view this as an important check. To the extent that there are no spillover effects, the specification is still a useful placebo test. If we were, for example, to observe that Medicare-financed hospital care is correlated with either the public or private insurance expansion treatment variables, this would raise the spectre that our results are being driven, at least in part, by some heretofore neglected source of unobserved heterogeneity, potentially related to spillover effects.

Lastly, when estimating ACA insurance expansion effects on quality by way of the Hospital Compare data or Medicare claims data, we are careful to control for the hospital penalty rate or status under each of the three major ACA quality initiatives (HRRP, HVBP, and Bundled payments).¹⁹ Doing so will reduce the bias introduced by the simultaneous enactment of these quality initiatives–which should independently affect our quality measures–with the ACA insurance expansions of 2014 and 2015.

¹⁹The Hospital Value-Based Purchasing Program withholds 2% of Medicare payments from hospitals and then either imposes a penalty or awards a bonus based on a total performance score. This score reflects the achievement of quality outcomes relative to both past performance and that of peer hospitals. The Hospital Readmissions Reduction Program calculates an aggregate excess readmission ratio (ERR) for six conditions—AMI, HF, pneumonia, COPD, CABG, and hip replacement or total knee arthroplasty procedures. Hospitals were penalized according to their ERR, with payment reductions having been capped at 1% in 2013, 2% in 2014, and 3% in 2015. Finally, the AHA database indicates whether each hospital participated in a bundled payment program "where the hospital receives a single payment from a payer for a package of services and then distributes payments to participating providers of care (such as a single fee for hospital and physician services for a specific procedure, e.g. hip replacement, CABG)." We control for these year-specific payment adjustment factors, provided by CMS, and the Bundled Payment program participation indicator in both our Hospital Compare and Medicare mortality regressions.

5 Results

Before presenting our main results, we demonstrate that our estimates of the ACA expansion effects are measured appropriately. Specifically, we replicate the analysis in the literature by applying a difference-in-differences estimator to examine the effects of insurance expansion on the number of newly insured individuals in the population. Our results, reported in Table A1 of Appendix Section A.1, shows that our measures of expanded Medicaid and private subsidy eligibility both positively and significantly contribute to the number of newly insured individuals. We find no evidence of crowd out.

As we discuss in greater detail in the appendix, our results match previous findings in the literature (Long et al., 2014; Smith and Medalia, 2014; Sommers et al., 2014; Alker and Chester, 2015; Cohen and Martinez, 2015; McMorrow et al., 2015; Sommers et al., 2015; Buchmueller et al., 2016; Leung and Mas, 2016; Levy et al., 2016; Courtemanche et al., 2017; Frean et al., 2017; Kaestner et al., 2017; Duggan et al., 2019b). In the remainder of the paper, however, we focus on the hospital effects of these expansions.

5.1 ACA Effects on Hospital Utilization and Finances

We begin with an event-study approach in which we interact dummies for {no, low, high} levels of exposure to the Medicaid expansion with each year of the data. Likewise, we interact dummies for {low, high} exposure to private insurance subsidies with each year of the sample. The parallel trends assumption would stand if it were the case that the interaction terms in the pre-period are indistinguishable from 0. That is to say, there were no differential pre-trends in the outcome variables that were correlated with future treatment.

Figures 3 and 4 succinctly summarize the results of the analysis. In general, the absence of differential pre-trends by treatment status lends credibility to the parallel trends assumption that had been employed in the difference-in-differences analysis. Moreover, with the notable exception of emergency department (ED) visits, the ACA effects on the outcomes emerge only beginning in 2014 and persist thereafter. Namely, Figures 3a and 3b reveal that uncompensated care costs decrease by nearly 50 percentage points in hospitals whose patients were exposed to the Medicaid expansion, while the extent of Medicaid inpatient care usage increases monotonically in the degree to which a hospital is exposed to the Medicaid expansion, topping out at 20 percentage points for these most intensively treated hospitals. This monotonic relationship extends also to outpatient visits, though weakly, and to hospital revenues, prices, and profits: hospitals with above-median exposure to the Medicaid treatment experienced increases in outpatient visits, revenues, and prices by approximately 4 percentage points each, and increases in profits by 2 percentage points. Likewise, the remaining figures show that above-median exposure to the private insurance expansion produces marginal, if any, gains in hospital utilization along with modest 2 percentage point gains in prices, revenues, and profits.

Columns (1)-(4) of Table 2 document formally how the Medicaid and private insurance expansions separately affected the (logged) number of inpatient visits financed by Medicaid, Medicare, or a combination of self-pay and private insurance, respectively. We find that a 1 percentage point increase in Medicaid eligibility simultaneously raised the number of Medicaid-financed inpatient visits by 2.8 percentage points, which implies an average Medicaid treatment effect of 14.3 percentage points. The relatively large magnitudes uncovered appear to have resulted from the influx of previously uninsured patients onto the Medicaid rolls, which is supported by a corresponding decline in uncompensated care costs by 48 percentage points in Table A10 of Appendix Section A.6. Such a shift in payer mix from uncompensated care to Medicaid without an overall change in inpatient utilization is consistent with other recent work (Freedman et al., 2017).

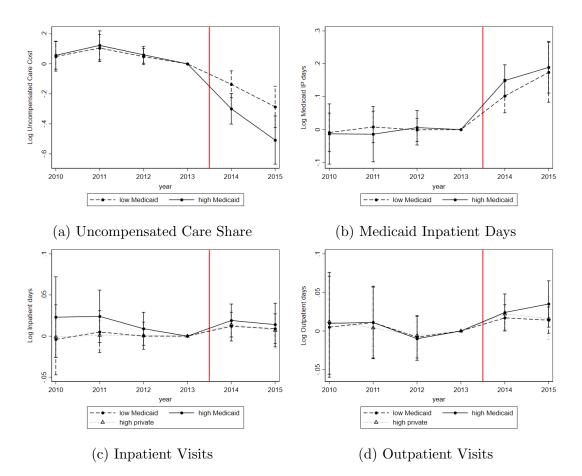
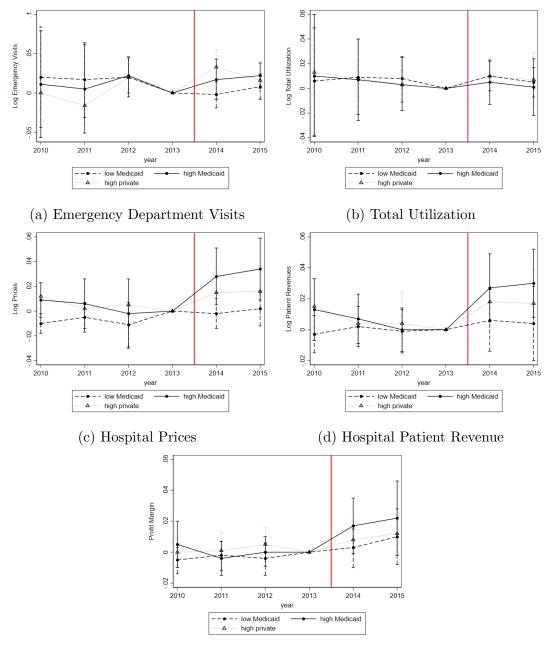


Figure 3: Hospital Outcomes, by exposure to ACA insurance expansions

These event-study plots illustrate how hospital outcomes evolve according to their county-weighted level of exposure to the ACA Medicaid and private insurance expansions. The connecting points are the point estimates on the interaction between each group and the indicated year dummy. Each dependent variable is measured in logs, and so the coefficients should be interpreted as percentage point changes. The dashed and solid lines with circle markers, and lightly dotted lines with triangle markers represent hospitals with below and above-median levels of exposure to Medicaid, and with above-median exposure to the private insurance expansions, respectively. The leave-out or 'control' group for the Medicaid treatment is made up of hospitals with 0 exposure to Medicaid expansions, and hospitals with below-median levels of exposure for the private treatment. Each set of vertical bars covers the associated 95% confidence intervals.



(e) Hospital Profits

Figure 4: Hospital Outcomes, by exposure to ACA insurance expansions (ctd.) See footnotes to Figure 3.

We further broaden the scope of the analysis to aggregate utilization effects, separately characterizing the relationship between insurance expansions and overall inpatient visits, outpatient visits, and emergency department visits. Starting with the results in column (5), each 1 percentage point increase in Medicaid eligibility induces a 0.0092 percentage point increase (p<0.05) in outpatient visits, the vast majority of which are not emergency care episodes. This translates to a 4.7 percentage point increase for hospitals with an average level of exposure to the Medicaid treatment. Each 1 percentage point increase in private insurance subsidy availability, however, produces no statistically significant increase in inpatient or outpatient utilization. Additionally, neither the Medicaid nor the private insurance expansion has a significant impact on inpatient visits. Limited changes in inpatient utilization is consistent with the literature showing relatively inelastic demand for more acute conditions (Duarte, 2012).

	IP Visits			OP Visits	ED visits	Non-ED OP visits	$OP + IP^{\dagger}$	
	Total	Non-public	Medicaid	Medicare				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$post \times Medicaid$ Eligibility	0.36	-1.79^{***}	2.82^{***}	0.31	0.92^{**}	0.20	0.68	0.54^{*}
	(0.37)	(0.44)	(0.72)	(0.54)	(0.41)	(0.38)	(0.55)	(0.30)
<i>post</i> ×Subsidy Eligibility	0.35	1.59^{***}	-0.58	0.43	0.14	0.78^{*}	-0.60	0.11
	(0.25)	(0.58)	(0.63)	(0.30)	(0.49)	(0.39)	(0.60)	(0.26)
ACA Medicaid effect	0.018	-0.090	0.143	0.015	0.047	0.010	0.034	0.027
				0.013 0.028	0.047 0.009	$0.010 \\ 0.051$	-0.039	0.027
ACA Subsidy effect	0.023	0.104	-0.043					
mean(dependent variable)	9.74	8.43	7.75	8.96	11.23	9.78	10.89	10.68
Observations	27,181	26,746	26,583	27,049	26,799	25,415	$26,\!692$	27,132
Hospitals	4,370	4,370	4,336	4,361	4,336	4,114	4,370	4,336
Counties	2,396	2,397	2,393	2,403	2,405	2,336	2,401	2,403
R^2	0.990	0.963	0.964	0.977	0.979	0.986	0.959	0.989
Unemp. Rate	Y	Y	Υ	Υ	Y	Y	Y	Y
Year FEs	Υ	Υ	Υ	Υ	Υ	Υ	Υ	Υ
Hospital FEs	Υ	Υ	Υ	Υ	Υ	Υ	Υ	Υ
State Time Trends	Υ	Υ	Υ	Υ	Υ	Υ	Υ	Υ
Demographic Controls	Υ	Υ	Υ	Υ	Υ	Υ	Υ	Υ

Table 2: Effect of Program Eligibility on Overall Utilization

Estimates are from fixed-effect regressions of the logged dependent variable (indicated in column heading) on independent variables (listed as row headings). ACA Medicaid effect scales the $post \times$ Medicaid Eligibility coefficient by the average change in the fraction of the hospital's patient population that became newly eligible for Medicaid (5.1%). ACA Subsidy effect scales the $post \times$ Subsidy Eligibility coefficient by the average change in the fraction of the hospital's patient population that became newly eligible for private subsidies in the non-group market (6.5%). Mean logged values of the dependent variable are reported below these implied effects. Robust standard errors are clustered at the state level. The study sample is from 2009 through 2015 while the program eligibility variables are constructed from 2013 ACS data.

* p < 0.10, ** p < 0.05, *** p < 0.01

[†] Outpatient visits are weighted by the median price ratio between outpatient and inpatient visits.

We do, however, find that hospitals exposed to the private insurance expansion experienced a 5.1 percentage point average increase in the use of emergency services, though with limited precision. This is consistent with recent work by Courtemanche et al. (2019b), which finds that the private and Medicaid expansions under the ACA slowed ambulance times by 24% by inducing individuals who otherwise would not have utilized emergency services to do so. However, event study Figure 4a shows that the increase in ED visits pre-dated the ACA private insurance expansions and so we should be cautious in interpreting this coefficient too literally. Combining inpatient and outpatient visits, we find a marginally significant positive 2.7 percentage point effect of the Medicaid insurance expansions on overall hospital utilization; although this effect is positive, it all but vanishes in the event-study plot. Duggan et al. (2019a) find positive effects for overall hospital utilization in certain specifications but not for those in which outcomes are aggregated at the hospital level, as in our study.

We next examine whether prices per stay increase, as might be expected when uninsured individuals gain insurance coverage and private insurers compete for access to hospitals. To investigate this possible effect, we explore the potential price effects associated with the insurance eligibility expansions. Table 3 shows that a 1 percentage point increase in public insurance and private subsidy eligibility increased average hospital episode prices by approximately 0.006 percentage points (p<0.01) and 0.0016 percentage points each, respectively, though the latter result is imprecise. Given that the average level of exposure to these expansions are 5.1% and 6.5%, respectively, these translate to 3 and 1 percentage point increases. While the reimbursement rate for private insurance is influenced by market forces, the Medicaid reimbursement rate is still more lucrative than is the self-pay rate extracted from the uninsured population who may have previously received uncompensated care. Thus, we see that the Medicaid expansion also resulted in a robust increase in revenues in both the event study plot 4c and in the difference-in-differences analysis (Column 3 of Table 3). The implied average increase in this latter analysis is 2.9 percentage points.

Given the changes in both compositional and aggregate utilization behavior, one would similarly expect to find changes in hospital finances in ways that reflect the underlying shifts in payers.²⁰ As expected, Appendix Table A10 shows that these Medicaid expansions lowered the cost of uncompensated care while increasing Medicaid costs. Moreover, columns (2) and (4) show that these declines in uncompensated care costs fell more in hospitals with a greater burden of uncompensated care prior to the reforms. These results align with those of Dranove et al. (2016), who find that uncompensated care costs fell from 4.1 to 3.1 percentage points of operating costs in expansion states and that reductions were larger for states with larger uncompensated care caseloads pre-ACA

That the Medicaid expansions reduced uncompensated care costs (though by less than they raised Medicaid revenues) is also borne out in column (5) of Table 3, which shows

²⁰While it had been anticipated that the ACA would dramatically reduce disproportionate hospital share (DSH) payments–which compensate facilities based on the amount of "charity care" provided–DSH payment reductions had been repeatedly delayed by Congress well beyond the sample period.

		1			
	Price	$\operatorname{Price}^{\phi}$	Pt. Revenue	Oper. Expenses	Profit Margins
	(1)	(2)	(3)	(4)	(5)
$post \times Medicaid$ eligibility	0.60^{***}	0.57^{***}	0.58^{***}	0.08	0.44^{**}
	(0.20)	(0.19)	(0.20)	(0.22)	(0.19)
$post \times Subsidy$ eligibility	0.16	0.17	0.32^{*}	0.25^{*}	0.13
	(0.19)	(0.17)	(0.19)	(0.14)	(0.10)
	0.090	0.000	0.000	0.004	0.000
ACA Medicaid effect	0.030	0.029	0.029	0.004	0.022
ACA Subsidy effect	0.010	0.011	0.021	0.016	0.008
mean(dependent variable)	7.47	18.12	18.12	18.14	1.11
Observations	25,211	25,211	26,372	26,372	26,372
Hospitals	4,097	4,097	4,401	4,401	4,401
Counties	2,324	2,324	2,346	2,346	2,346
R^2	0.964	0.994	0.994	0.996	0.769
Utilization Controls	Y	Y			
Unemp. Rate	Υ	Υ	Υ	Υ	Υ
Year FEs	Υ	Υ	Υ	Υ	Υ
Hospital FEs	Υ	Υ	Υ	Υ	Υ
State Time Trends	Υ	Y	Υ	Υ	Υ
Demographic Controls	Υ	Υ	Υ	Y	Y

Table 3: Effect of Program Eligibility on Hospital Finances

See first footnote to Table 2.

 $^{\phi} \rm Price$ is calculated slightly differently here. The dependent variable is net revenue and quantity controls are added.

* p < 0.10, ** p < 0.05, *** p < 0.01

that each one percentage point increase in the Medicaid expansion-eligible population raised profit margins by over 0.004 percentage points (p<0.05). This translates to an implied increase of 2.2 percentage points for hospitals with an average level of exposure to the Medicaid expansion treatment. The key takeaway is that hospitals have been able to capture a significant share of the rents associated with ACA-induced insurance coverage increases. Given the non-results on inpatient utilization, we conclude, like Duggan et al. (2019a), that the increase in profits is attributable entirely to increases in revenue per bed, rather than increases in patient volume. Importantly, we find similar results when California is excluded from the analysis, implying the results from Duggan et al. (2019a) for the case of California can be generalized across the rest of the country.

5.2 ACA Effects on Measures of Quality Spillovers

To fully understand the impact of the insurance expansions on the provision of care at hospitals, we further assess whether and how the policy affected the quality of care provided for the already insured population. Table 4 shows the impact on each of the constructed quality indexes based on the Hospital Compare data, which are all small in magnitude and opposite in sign, confounding our ability to infer any uniform changes in quality. The mortality index derived from the hospital compare database takes on a median value of around 13, indicating an average 13 percent 30-day mortality rate across the three conditions included in the index. Taking the point estimates at face value, then, the typical impact of the reform in an expansion county is equal to the estimated coefficient -3.1 times the median affected population of 0.051 and divided by the mean mortality rate of 12.8%, which equals a statistically insignificant 1.2 percentage point decrease in the mortality rate. The process measures, on the other hand, show some deterioration following the ACA, while the patient survey and readmissions measures neither improve nor worsen.

	Η	Iospital Co	ompare Measure	Mortality from Medicare Claims			
	mortality	$\mathrm{process}^{\dagger}$	readmissions	pt survey	30-day	60-day	90-day
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post×Medicaid Eligibility	-3.10	0.03	1.56	-1.27	-0.156	-0.106	0.026
	(2.91)	(0.68)	(4.95)	(1.24)	(0.144)	(0.166)	(0.151)
$post \times Subsidy$ Eligibility	-4.92**	1.69**	-4.74	0.24	0.200^{*}	0.191	0.151
	(2.33)	(0.63)	(3.23)	(1.19)	(0.110)	(0.127)	(0.129)
ACA Medicaid effect	-0.158	0.001	0.080	-0.065	-0.008	-0.005	0.001
ACA Subsidy effect	-0.320	0.110	-0.308	0.016	0.013	0.012	0.010
mean(dependent variable)	12.81	-0.07	19.79	0.03	0.094	0.128	0.151
Observations	12,669	9,156	15,430	13,496	389,580	389,580	389,580
Hospitals	3,225	3,184	3,231	2,767	4,089	4,089	4,089
Counties	1,105	1,070	1,105	961	1,789	1,789	1,789
R^2	0.862	0.885	0.890	0.891	0.127	0.133	0.137
Unemp. Rate	Y	Y	Y	Y	Y	Y	Y
Hospital FEs	Υ	Υ	Υ	Υ	Υ	Υ	Υ
Demographic Controls	Υ	Υ	Υ	Υ	Υ	Υ	Υ
Year FEs	Υ	Υ	Υ	Υ	Υ	Υ	Υ
State Time Trends	Υ	Υ	Υ	Υ	Υ	Υ	Υ
ACA Quality Programs Penalty Rate	Υ	Υ	Υ	Υ	Υ	Υ	Υ

Table 4: Effect of Program Eligibility on Quality Measures

Estimates are from fixed-effect regressions of the dependent variable (indicated in column heading) on independent variables (listed as row headings). **ACA Medicaid effect** scales the $post \times Medicaid$ Eligibility coefficient by the average change in the fraction of the hospital's patient population that became newly eligible for Medicaid (5.1%). **ACA Subsidy effect** scales the $post \times Subsidy$ Eligibility coefficient by the average change in the fraction of the hospital's patient population that became newly eligible for private subsidies in the non-group market (6.5%). Mean values of the dependent variable are reported below these implied effects. Robust standard errors are clustered at the state level. Note that the Hospital Compare variables are constructed as indices and that higher measures correspond to quality reductions for mortality, process measures, and readmissions but correspond to quality improvements for the survey-based measures. Robust standard errors are clustered at the state level. The study sample is from 2010 through 2015 unless otherwise indicated.

[†] Sample is missing 2010-2011 measures

* p < 0.10, ** p < 0.05, *** p < 0.01

The three measures (mortality, process, and readmissions) shown in Appendix Table

A6 are created using additional variables for 2013 onward, which permits inclusion of a broader set of mortality and quality measures in the index (e.g., the mortality rate for two additional conditions, COPD and stroke). However, this benefit must be measured against the shortcoming of having eliminated part of the pre-reform period from what is already a short panel. Moreover, because state-time trends are included, identification in this analysis is based on intra-state differences in expansion exposure. Here, consistent quality effects are detectable in neither the Medicaid nor the commercial insurance treatments.²¹

Lastly, we turn to the effects of the insurance expansions on Medicare mortality. Microlevel Medicare claims data allow us to validate mortality declines observed in the Hospital Compare data, and also focus on conditions that are not influenced by other ACA incentive programs (i.e., excluding AMI, heart failure, and pneumonia). Unlike the mortality results using Hospital Compare data, columns (5)-(7) of Table 4 show modest but imprecise increases in mortality for those hospitals residing in counties that are highly exposed to the commercial expansion. They do show equally noisy mortality reductions in response to increased exposure to the Medicaid expansions though. Nonetheless, both results dissipate as the mortality window increases from 30 to 90 days.

Overall, we find limited evidence of any change in hospital quality spilling over to the already insured population owing to the health insurance expansions. Importantly, our analysis focuses on the impact of the ACA expansions on the quality of treatment, primarily for the already insured Medicare population. This does not necessarily reflect the impact of the insurance expansions on the health of the population. In fact, as mentioned previously, several recent papers that study the ACA provide evidence that access to care leads to improvements in population health for those gaining coverage.

5.3 Robustness Checks

One additional concern is that the effects could be correlated with changes in the hospital industry structure, which may be particularly important for the effects we find on private-sector prices. As mentioned previously, we include measures of the hospital HHI, which has no impact on our main findings, including our estimated effect on prices. All of the results are additionally robust to the inclusion of hospital network and system fixed effects. As an alternative specification for our pricing regression, we exclude Maryland, which imposes some price controls. We find no change in our estimates there either.²²

²¹We further investigated whether the expansions may have had an effect specific to capacity-constrained hospitals that may be particularly sensitive to changes in utilization, but we find limited evidence of any effect on quality for these hospitals.

²²These latter two classes of robustness checks are available upon request due to space limitations.

Finally, Appendix A.5 displays the results of the analyses after having removed early expansion states from the sample. The estimated effects on inpatient and overall utilization in Table A7 are nearly identical. Likewise, prices and operating margins (Table A8) and uncompensated care (Table A9) appear not to evolve differently when the sample of expansion states is restricted to those expanding Medicaid no sooner than 2014.

6 Discussion

In an attempt to tie neatly together all of the results we have uncovered, we seek to estimate the amount by which a \$1 increase in federal spending on both the public and private insurance expansions increased hospital revenues. We then decompose this increase in hospital revenues into increases into P and Q, approximated by our measures of hospital prices and total utilization. Given the general weakness of the utilization effects uncovered in this study, we caution against interpreting the P x Q decomposition as ironclad. Rather, we view this as a simple accounting exercise that attempts to trace out the ramifications of an increase in subsidized insurance coverage on the market for hospital services.

According to the Congressional Budget Office (CBO), in 2014-2015, the true combined cost to the federal government of the Medicaid expansion and the subsidies and cost-sharing reductions made available to those purchasing insurance on the new private exchanges was approximately \$123 billion.²³ Based on Table 1 and Table 3, we estimate that the private insurance expansion increased hospital revenues by \$15.80 billion (\$172 million average revenue/hospital \times 4,374 hospitals \times 2.1 percentage point increase in hospital revenues). Analogously, the Medicaid expansion increased hospital revenues by \$12.45 billion (\$192 million average revenue/hospital \times 2,236 hospitals \times 2.9 percentage point increase in hospital revenues). Thus, the total increase in hospital revenues generated by the federal expansions was \$28.25 billion. In other words, each \$1 increase in federal funding for the ACA insurance expansions increased hospital revenues by 23 cents.

Furthermore, the Medicaid expansion led to a 3.0 percentage point increase in hospital prices and a 2.7 percentage point increase in total utilization, while the private expansion triggered a price and utilization increase of 1 and 0.7 percentage points, respectively. After weighting by the number of hospitals affected by either type of expansion, the average total increase in hospital prices and utilization due to the ACA was 2.5 percentage points and 2.0 percentage points, respectively. Thus, price increases were responsible for 55% of the \$28.25 billion increase in ACA-induced increase in hospital revenues, with the remaining 45% having come from (mostly outpatient) utilization effects. Alternatively, of the 23 cents in hospital

²³See https://www.cbo.gov/system/files/115th-congress-2017-2018/reports/53094-acaprojections.pdf.

revenues generated for each federal dollar spent on the ACA expansions, 12.5 are due to increases in price and 10.5 arise from quantity growth. This calculation, however, masks the extent to which price and quantity evolved differently for inpatient versus outpatient care. Because neither the cost report nor AHA data allow us to calculate separate prices for inpatient and outpatient care, we are unable to decompose revenue by type of care. However, it is worth noting that the percentage point increase in outpatient care attributable to the Medicaid expansion was nearly 3 times the analogous increase in inpatient care (4.7 versus 1.8 percentage points).

We also estimate by how much the increase in Federal Medicaid spending reduced uncompensated care, which could have contributed to both the estimated increase in hospital prices and utilization. Average uncompensated care for a hospital that was treated with a Medicaid expansion was \$7.2 million, and there were 2,236 such hospitals. That is, the total cost of uncompensated care provided by expansion hospitals pre-reform was \$16.1 billion. The Medicaid expansions, in turn, reduced uncompensated care by 48% (9.43 percentage points/1 percentage point increase in the Medicaid expansion \times 5.07 percentage point increase in Medicaid eligibility), which suggests that \$7.73 billion of the \$28.25 billion increase in total hospital revenues was generated through a shift from uncompensated care to the treatment of newly insured patients.

7 Conclusion

In this paper, we provide a full accounting of the impacts of the ACA-sanctioned private and public health insurance expansions on hospital utilization, prices, profits, and quality spillovers to the already insured population. We find evidence of an increase in prices and profits associated with the expansions, which is sensible in light of the fact that private insurance and Medicaid pay higher reimbursement rates than do uninsured patients, who often receive uncompensated care and pay a price of zero. To this end, the Medicaid expansions were associated with substantial declines in uncompensated care costs that largely offset concomitant increases in Medicaid costs, which further propped up the profits of hospitals residing in expansion states. As with Duggan et al. (2019a), in fact, we find that profit increases resulting from inpatient care are entirely attributable to increases in revenue per bed-rather than increases in patient volume. Our paper contributes to the literature by corroborating that the California-specific effects of the ACA on hospital finances, as estimated by Duggan et al. (2019a), tend to generalize across the rest of the country.

The strongest evidence of an impact on utilization corresponds to the use of outpatient services, though it is undercut somewhat by more marginal event-study results. This is consistent with prior evidence that finds an increase in the utilization of outpatient services for preventative care (Kirby and Vistnes, 2016). Using data from the Behavioral Risk Factor Surveillance System (BRFSS), Simon et al. (2017) document qualitatively congruent trends in preventive care uptake among newly eligible Medicaid enrollees, such as breast exams, mammograms, and routine checkups. Wherry and Miller (2016) find that those residing in expansion states experienced relatively larger increases in visits with general physicians, while Shartzer et al. (2015) uncover a 4.9 percentage point increase in the fraction of adults with a usual source of care in expansion states. Furthermore, Sommers et al. (2015) and Collins et al. (2016) document a decline in the share of low-income adults lacking both easy access to medicine and to a personal physician in expansion states relative to non-expansion states.

This channel may even partially explain why we find no significant impact on inpatient or emergency department utilization for Medicaid. Increases in the early detection of diseaseswhich, left untreated, might necessitate inpatient stays-is one such possible mechanism that could account for the change in observed utilization patterns. For example, Kaufman et al. (2016) find a 23% increase in Medicaid patients with newly identified diabetes in expansion states, as compared to only a 0.4% increase in non-expansion states. Early detection of diabetes reduces risk of infection and subsequent amputation, which would accordingly reduce the incidence of inpatient care episodes. Because increases in preventive care occurred simultaneously with the increasing prevalence of outpatient visits, we cannot rule out a preventive care story in explaining our main result. Indeed, Wen et al. (2019) find that preventable hospitalizations for ambulatory-care sensitive conditions fell more dramatically in Medicaid expansion states. This should have exerted a negative effect on inpatient visits, and thus may contribute to our overall null result in the current study.

Alternatively, there may simply be greater challenges in detecting utilization for emergency and inpatient utilization services, which tend to be less responsive to cost sharing and may be more difficult to detect in aggregate hospital data. Duggan et al. (2019a) find micro data to be particularly important for identifying hospital utilization effects in California, where large and precise overall utilization effects emerge. We reconcile these results with our estimates, which are much smaller for outpatient care and insignificant for inpatient and emergency care, by recognizing that our hospital-level analysis is not able to exclude those unaffected by the reforms, i.e., those under the age of 21 or over the age of 65. That is, we estimate percent changes off a base that reflects combined utilization across treated and untreated age groups. The incorporation of micro-level hospital data that include utilization and pricing information for all payers may be a useful avenue for future research that could help parse out the alternative explanations for the effects that we observe in this study. Finally, we examine whether there are any spillover effects on the quality of care for the already insured population for all U.S. states. We analyze a variety of quality measures, including mortality outcomes, readmissions, process measures based on best treatment practices, and measures from patient surveys. Despite policy-induced shocks to patient insurance statuses, utilization of care, and hospital profitability, we fail to find that the ACA resulted in any material improvements or declines in the quality of treatment received at hospital facilities for the population already insured, similar to McInerney et al. (2017). This evidence suggests that, conditional on already being insured, the ACA had little spillover effect on the quality of treatment. Although we find no spillover effects on the population already insured, it is important to highlight that several recent papers looking at the direct effects of the ACA on health find reductions in mortality rates for the population gaining coverage through the ACA (Sommers et al., 2012; Sommers, 2017; Goldin et al., 2021; Miller et al., forthcoming).²⁴

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²⁴Although Duggan et al. (2019a) fails to reject the null of no improvement for in-hospital mortality among patients in California experiencing an acute condition (e.g., heart attack or pneumonia) following the passage of the ACA, they do so because their point estimates are imprecisely estimated. The point estimates, however, are consistent with a meaningful decline in mortality post-ACA, consistent with the findings of other papers examining the health effects on the newly insured population.

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A Appendix

A.1 Insurance Eligibility and Takeup Results

Table A1 shows the relationship between eligibility and takeup for both Medicaid and nongroup private insurance. The first row of column (1) indicates that each 1 percentage point increase in Medicaid eligibility increases Medicaid coverage by 0.89 percentage points. Given that average Medicaid eligibility rates increased by 5.1 percentage points in expansion state counties due to the ACA, these eligibility gains imply Medicaid coverage expansions of approximately 4.5 percentage points, which falls within the range of prior estimates in the literature (Long et al., 2014; Sommers et al., 2014; Smith and Medalia, 2014; Alker and Chester, 2015; Cohen and Martinez, 2015; McMorrow et al., 2015; Sommers et al., 2015; Buchmueller et al., 2016; Leung and Mas, 2016; Levy et al., 2016; Courtemanche et al., 2017; Frean et al., 2017; Kaestner et al., 2017). The estimate in row 2 of column (2) suggests that each percentage point increase in the fraction of individuals eligible for an exchangebased subsidy increased private coverage by approximately 0.45 percentage points. Given a 6.5 percentage point increase in average subsidy eligibility, the overall increase in private coverage attributable to subsidy availability is 3 percentage points. These results match Frean et al. (2017) exactly, as we too attribute 60% of the coverage gains to the Medicaid expansion and the remaining 40% to the private subsidy component of the law. Examining effects over the 2014-2015 period, Duggan et al. (2019b) estimate that insurance increases by 2.6 and 4.2 percentage points in non-expansion and expansion states, respectively, while our analysis produces larger but still comparable estimates of 3 and 7.5 percentage points.

In light of historical evidence of public insurance crowd-out rates ranging between 20% and 60% (Cutler and Gruber, 1996; Sasso and Buchmueller, 2004; Gruber and Simon, 2008), it is worth measuring the magnitude of crowd-out induced by the most recent wave of public insurance expansions commissioned by the ACA. In spite of the relatively large crowdout effects detected in these earlier seminal studies, others have detected only minimal crowdout during similar expansions (Thorpe and Florence, 1998; Hamersma and Kim, 2013). Importantly, Kronick and Gilmer (2002) show that crowdout increases with increasing Medicaid income eligibility limits so that not all expansions should be expected to yield comparable degrees of crowdout. Though the ACA Medicaid expansions are historically among the most generous with respect to income eligibility limits, Frean et al. (2017) find no evidence of private coverage crowd-out owing to Medicaid expansions. They reconcile such a result with previous evidence of ACA-induced crowdout by arguing that private insurance takeup is higher in non-expansion states as individuals earning between 1 and 1.38 times the Federal Poverty Level in non-expansion states receive generous exchange subsidies that are not

	Medicaid Coverage	Private Coverage	Any Coverage
	(1)	(2)	(3)
		0.000	
$post \times Medicaid$ eligibility	0.891^{***}	-0.036	0.783^{***}
	(0.114)	(0.058)	(0.081)
$post \times Subsidy$ eligibility	-0.094	0.455***	0.385***
	(0.126)	(0.085)	(0.073)
mean Δ eligibility	0.051	0.065	0.116
Implied ACA coverage effect	0.045	0.030	0.075
mean(dependent variable)	0.183	0.653	0.868
Observations	21,502	21,502	21,502
Counties	$3,\!118$	$3,\!118$	$3,\!118$
States	51	51	51
R^2	0.955	0.971	0.941
Unemp. Rate	Y	Y	Y
Year FEs	Υ	Υ	Y
County FEs	Υ	Υ	Υ
Demographic controls	Υ	Υ	Υ

Table A1: Effect of Program Eligibility on Insurance Coverage

Estimates are from fixed-effect regressions of the dependent variable (indicated in column heading) on independent variables (listed as row headings). The *post*×Medicaid Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a county population's exposure to the Medicaid treatment. The *post*×Subsidy Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a county population's exposure to the private subsidy treatment. The **Implied ACA coverage effect** multiplies these coefficients by the mean Δ eligibility estimates to generate average effects on the change in insurance coverage. Robust standard errors are clustered at the state level. The study sample is from 2009 through 2015 while the program eligibility variables are constructed from 2013 ACS data.

available to their counterparts in expansion states due to the latter group's access to public insurance. Such a difference in access is accordingly manifested in the form of a negative correlation between Medicaid expansions and private non-group coverage takeup. In constructing our measure of county-level exposure to private non-group subsidies, we explicitly handle this issue by accounting for those individuals earning between 1 and 1.38 times the FPL in non-expansion states. We too detect no evidence of crowd-out rate as is consistent with much of this earlier work (Courtemanche et al., 2017; Frean et al., 2017; Kaestner et al., 2017; Soni et al., 2017).

A.2 Construction of Quality Measures

The first set of measures are the 30-day mortality measures reported for a select set of acute conditions. The 30-day mortality rates observed for the full sample include those corresponding to heart failure, acute myocardial infraction (AMI), and pneumonia. Also included in the database are 30-day mortality measures for stroke and COPD, but these measures only become available one year prior to ACA implementation. Each mortality measure is constructed using three years of mortality data so that even the post-reform measures that include mortality outcomes for 2015 also cover part of the pre-reform period, such as 2013. Importantly, these 30-day mortality estimates are derived entirely from the Medicare population. Such a population of patients has desirable properties for the purposes of measuring quality effects as it remains constant pre- and post- reform and would not be directly affected by the changing mix of patients due to the reform. Any observed effects on this population are indicative of spillover effects from the reform.

Another set of quality metrics are process measures that track how effectively and timely are hospitals are providing care. These measures include variables indicating whether patients receive recommended treatment, such as the likelihood that individuals in an outpatient setting with chest pain or a possible heart attack are administered an anti-clotting drug within 30 minutes of arrival. Also provided are measures of timeliness (e.g., the average time a patient spends in the emergency department before being seen by a healthcare professional); as well as measures of overtreatment (e.g., how often outpatients with lower back pain who had an MRI without trying recommended treatments first, such as physical therapy). It is possible that these process measures could be affected by selection bias, as they are constructed from the claims data for all of the hospital's patients, rather than the Medicare population exclusively. However, these process measures focus on basic standards of care that should not be influenced by the patient mix.

The two remaining categories of quality measures are readmission rates and measures

from patient surveys, which we view as potentially less reliable indicators of quality. The readmission rates are constructed from several of the same conditions for which we observe the mortality rates, including heart failure, acute myocardial infraction (AMI), and pneumonia. Readmission data is also available for stroke, chronic obstructive pulmonary disease (COPD), and hip/knee surgery patients for 2013 forward. While readmissions are a potentially useful measure of quality, the literature has expressed concern that its connection to the quality of treatment at hospital may be tenuous, as they are more likely to reflect patient frailty and the frequency of chronic disease flare-ups (Benbassat and Taragin, 2000; Fischer et al., 2014).

The hospital compare data also report answers from patient surveys designed to isolate the quality of their experience at the hospital. They are asked to report the quality of the communication with doctors, the quality of communication with nursing staff, whether they were provided adequate information upon discharge, and can also rate their overall hospital experience. There are two concerns arising from the use of hospital surveys. First, the insurance expansion may have potentially altered the mix of patients filling out the survey. Those areas that experience a greater expansion will necessarily have a larger share of newly-insured individuals filling out the survey, for example. Second, the survey may capture general impressions of hospital quality that may not accurately reflect treatment quality, such as newness of its furniture and amenities.

The indexes used in the analysis are constructed to include the maximum amount of information on quality for each hospital. However, not all quality measures are available for all hospitals. If a particular measure is not observed for a hospital or is missing for a year, then that measure is excluded. For instance, for hospital A we may observe pneumonia and heart failure mortality rates for all years, but not the AMI mortality rate. In this case, the mortality index for hospital A would include pneumonia and heart failure, but AMI would be excluded. To account for the fact that distinct measures of quality may have different trends, we include indicator-specific trends interacted with each measure included in the index. For example, for hospital A, we include specific trends for pneumonia and heart failure, but not a specific trend for AMI. While we view these measure-specific trend values as important controls, our main findings are not sensitive to the inclusion of these additional trend variables. To ensure that there is adequate comparison across hospitals, we only include quality measures that are available for more than one thousand hospitals.

The patient survey data asks qualitative multiple choice questions. In the hospital compare data, the information is reported as a percentage, such as the share of patients who reported that their doctors always communicated well, usually communicated well, and sometimes/never communicated well. To incorporate this information into our analysis we assign numeric values to each response (E.g., 10, 5 and 0) and then compute a weighted average of those responses to derive a single measure for each question.

Another issue that arises for both our procedure measures and survey measures, is that they are not directly comparable. For instance, "average time patients who came to the emergency department with broken bones had to wait before receiving pain medication" is very different from "outpatients with low back pain who had an MRI without trying recommended treatments first, such as physical therapy". Therefore, before averaging across measures, we first standardize each measure by subtracting the mean and dividing by the standard deviation of the variable (i.e., for variable x we could calculate "standardized x" = (x-mean(x))/sd(x)).

Another feature of the procedure-based measures is that a higher value could indicate a higher level of quality or a lower level of quality, depending on the particular measure. Prior to averaging over these measures, we further normalize these indexes, so that higher numbers always indicate lower quality. (By default, this is how most of the process measures of quality are constructed.) We do so by multiplying the standardized quality measure by negative one for those measures where a higher value indicates a higher level of quality.

The list of variables used for each of the indexes we constructed is shown in Tables A2 and A3. Id. A wider set of procedure measures become available in 2012 (a total of 15 measures in 2012, compared to just 6 possible measures available for all prior years), so our key procedure index covers the period that begins with 2012.

In 2013 there are four additional process measures available, along with two additional mortality measures and three additional readmission measures. Given the availability of these additional measures in 2013, we also conduct analysis using 2013 as the only prereform year.

Variable	Index Category	Availability
Acute Myocardial Infarction (AMI) Pneumonia Heart Failure (HF) Chronic Obstructive Pulmonary Disease (COPD) Acute Ischemic Stroke (STK)	30-Day Mortality Rate	all years all years all years 2013- 2013-
Acute Myocardial Infarction (AMI) Pneumonia Heart Failure (HF) Chronic Obstructive Pulmonary Disease (COPD) Acute Ischemic Stroke (STK) Hip/Knee surgery	30-Day Readmission Rate	all years all years all years 2013- 2013- 2013-
Outpatients with chest pain or possible heart attack who got aspirin within 24 hours of arrival Average number of minutes before outpatients with chest pain or possible heart attack got an ECG	Procedural	all years all years
Outpatients with low back pain who had an MRI without trying recommended treatments first, such as physical therapy		all years
Outpatients who had a follow-up mammogram, ultrasound, or MRI of the breast within 45 days after a screening mammogram		all years
Outpatient CT scans of the abdomen that were "combination" (double) scans Outpatient CT scans of the chest that were		all years all years
"combination" (double) scans Outpatients who got cardiac imaging stress tests before low-risk outpatient surgery		2011-
Outpatients with brain CT scans who got a sinus CT scan at the same time (Higher number suggests overuse) Average time patients spent in the emergency department		2011-
before being sent home Average time patients spent in the emergency department before they were seen by a healthcare professional		2012- 2012-

Table A2: Hospital Compare Index Components

These variables are constructed based on the hospital compare database available from the Centers for Medicare & Medicaid Services. The archived data is available at: https://data.medicare.gov/data/archives/hospital-compare.

Variable	Index Category	Availabilit
Average time petients who same to the emergency		
Average time patients who came to the emergency department with broken bones had to wait before	Procedural	2012-
receiving pain medication	Fiocedurar	2012-
Percentage of patients who left the emergency department		
before being seen		2012-
Percentage of patients who came to the emergency		
lepartment with stroke symptoms who received brain		2012-
scan results within 45 minutes		2012
Average (median) time patients spent in the emergency		
lepartment, before they were admitted to the hospital as		2012-
an inpatient		2012
Average (median) time patients spent in the emergency		
department, after the doctor decided to admit them as an		
inpatient before leaving the emergency department for		2012-
their inpatient room		
Percent of mothers whose deliveries were scheduled too		
early (1-2 weeks early), when a scheduled delivery was not		2012-
medically necessary		
Ischemic stroke patients who got medicine to break up a		9019
blood clot within 3 hours after symptoms started		2012-
Patients with blood clots who were discharged on a blood		
thinner medicine and received written instructions about		2012-
that medicine		
Patients who developed a blood clot while in the hospital		2012-
who did not get treatment that could have prevented it		2012-
Patient score on nurse communication	Patient Survey	all years
Patient score on doctor communication	i autono parvoj	all years
Patient score on staff responsiveness		all years
Patients score on pain control		all years
Patient score on communication of medication		all years
Patient score on communication for home recovery upon		
lischarge		all years
Patient score on understood their care when they left the		11
nospital		all years
Patient overall ranking of hospital score		all years
Patient score on recommending the hospital		all years

Table A3: Hospital Compare Index Components (Continued)

These variables are constructed based on the hospital compare database available from the Centers for Medicare & Medicaid Services. The archived data is available at: https://data.medicare.gov/data/archives/hospital-compare.

A.3 Conditions used in Medicare Mortality Analysis

Citation	Condition	ICD-9 code
Card et al. (2009)		
0.000 0.000 (2000)	Respiratory Failure	518.81
	Intracerebral hemmorhage	431
	Chronic airway obstruction	496
	Fracture of neck	820.21
	Cerebral artery occlusion	434.9
	Convulsions, unknown cause	780.39
	Asthma, with asthmaticus	493.91
Garthwaite et al. (2019)		
	Intracerebral hemmorhage	431
Garthwaite et al. (2017)		
	Cardiac dysrhythmia	427
Ballard et al. (2010)		
	Chest pain, unspecified	786.50
	Multiple open wounds	879.8
	Asthma, unspecified	493.90
	Syncope and collapse	780.2
	Other general symptoms	780.9
	Laceration	870
	Fracture/dislocation	800-839
	Head injury, unspecified	959.01
	Otitis media	382.9
	Urinary Tract Infection	595, 595.9, 597.8, 599, 771.82
	Allergic reaction	995.3

Table A4: Patient Conditions selected from Medicare Claims Data

The above conditions were selected from the 5% Medicare sample for the purposes of estimating ACA effects on Medicare mortality.

A.4 Quality Effects

	Observations	Mean	Std. Dev.	1st % ile	median	99 th %ile
mortality index	12,669	12.81	1.75	9.47	12.61	17.72
process measures $\mathrm{index}\phi$	9,156	-0.07	0.53	-1.23	-0.11	1.56
readmissions index	$15,\!430$	19.79	1.83	16	19.68	24.5
survey measures index	13,496	0.03	0.79	-2.21	0.05	1.87

Table A5: Hospital Compare Indices Summary Statistics

 ϕ Measures are unavailable for 2010-2011.

Note that all hospital compare variables are constructed as indices and that higher measures correspond to quality reductions for mortality, process measures, and readmissions but correspond to quality improvements for the survey-based measures.

	mortality	process	readmissions
	(1)	(2)	(3)
$post \times Medicaid$ Eligibility	-3.47	0.15	-1.01
	(2.55)	(0.84)	(3.73)
$post \times Subsidy$ Eligibility	-5.67**	1.25^{*}	-1.91
I the second of the second sec	(2.28)	(0.70)	(2.54)
ACA Medicaid effect	-0.177	0.008	-0.051
ACA Subsidy effect	-0.368	0.081	-0.124
mean(dependent variable)	12.50	-0.12	16.74
Observations	6,400	5,716	6,454
Hospitals	3,261	2,962	$3,\!351$
Counties	$1,\!118$	1,046	$1,\!122$
R^2	0.934	0.895	0.987
Missing Years	2010-12	2010-12	2010-12
Unemp. Rate	Y	Y	Y
Hospital FEs	Υ	Υ	Υ
Demographic Controls	Υ	Υ	Υ
Year FEs	Υ	Υ	Υ
State FEs	Υ	Υ	Υ
State Time Trends	Υ	Υ	Υ
ACA Quality Programs Penalty Rate	Υ	Υ	Υ

Table A6: Effect of Program Eligibility on Alternative Hospital Compare Measures

See first footnote to Table 4.

A.5 Robustness Checks- Early adopter states removed

		Removed st	ates: CA,	CT, DC,	MN, NJ, a	nd WA		
	IP Visits				OP Visits	ED visits	Non-ED OP visits	$OP + IP^{\dagger}$
	Total (1)	Non-public (2)	Medicaid (3)	Medicare (4)	(5)	(6)	(7)	(8)
$post \times Medicaid$ Eligibility	0.27 (0.38)	-1.70^{***} (0.49)	2.34^{***} (0.77)	-0.00 (0.54)	0.61 (0.37)	0.01 (0.37)	0.43 (0.53)	0.29 (0.23)
$post \times Subsidy$ Eligibility	(0.36) 0.46^{*} (0.26)	(0.43) 1.74^{**} (0.67)	-0.39 (0.63)	(0.34) (0.35)	(0.01) -0.05 (0.51)	(0.51) (0.52) (0.32)	-0.49 (0.66)	(0.23) 0.04 (0.28)
Observations Hospitals	21,859 3,665	21,484 3.670	21,366 3.649	21,797 3.669	21,672 3,646	20,696 3,451	21,604 3,647	27,853 3,669
Counties	2,139	2,142	2,142	2,142	2,142	2,135	2,141	2,143
R^2	0.988	0.963	0.965	0.980	0.976	0.986	0.954	0.987
			Remov	ed states:	CA			
			Visits		OP Visits	ED visits	Non-ED OP visits	$\mathrm{OP} + \mathrm{IP}^\dagger$
	$\begin{array}{c} \text{Total} \\ (1) \end{array}$	Non-public (2)	Medicaid (3)	Medicare (4)	(5)	(6)	(7)	(8)
$post \times Medicaid$ Eligibility	0.25 (0.37)	-1.72^{***} (0.48)	2.45^{***} (0.76)	-0.08 (0.52)	0.65^{*} (0.36)	0.16 (0.38)	0.37 (0.53)	0.30 (0.22)
$post \times Subsidy$ Eligibility	0.34 (0.26)	1.58^{**} (0.62)	-0.33 (0.57)	0.43 (0.33)	0.07 (0.50)	0.57^{*} (0.32)	-0.45 (0.63)	0.08 (0.26)
Observations	23,540	23,142	23,034	23,466	23,331	22,268	23,270	23,531
Hospitals	3,958	3,963	3,941	$3,\!959$	3,939	3,726	3,942	3,963
$\frac{\text{Counties}}{R^2}$	$\frac{2,246}{0.987}$	2,249 0.963	2,250 0.965	2,249 0.979	2,249 0.975	2,243 0.986	2,249 0.954	2,250
11	0.901	0.905	0.905	0.979	0.975	0.900	0.994	0.901

Table A7: Effect of Program Eligibility on Overall Utilization, non-early adopters

Estimates are from fixed-effect regressions of the logged dependent variable (indicated in column heading) on independent variables (listed as row headings). The $post \times$ Medicaid Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a hospital patient population's exposure to the Medicaid treatment. The $post \times$ Subsidy Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase by how many percentage points the dependent variable changes for a 1 percentage point increase in a hospital patient population's exposure to the private subsidy treatment. Robust standard errors are clustered at the state level. The study sample is from 2009 through 2015 while the program eligibility variables are constructed from 2013 ACS data. All controls in Table 2 included.

[†] Outpatient visits are weighted by the median price ratio between outpatient and inpatient visits.

* p < 0.10,** p < 0.05,**
**p < 0.01

	Price (1)	$\frac{\text{Price}^{\phi}}{(2)}$	Pt. Revenue (3)	Oper. Expenses (4)	Profit Margins (5)
	(1)	(2)	(0)	(т)	(0)
$post \times Medicaid$ eligibility	0.58**	0.56**	0.53**	0.01	0.40*
	(0.22)	(0.21)	(0.22)	(0.25)	(0.21)
<i>post</i> ×Subsidy eligibility	0.23	0.21	0.34	0.24	0.13
	(0.21)	(0.20)	(0.21)	(0.16)	(0.11)
Observations	20,788	20,788	22,191	22,191	22,191
Hospitals	$3,\!467$	$3,\!467$	$3,\!683$	3,683	3,683
Counties	2,141	2,141	2,149	2,149	2,149
R^2	0.966	0.995	0.994	0.996	0.743
		Remov	red states: CA	Α	
	Price	$\operatorname{Price}^{\phi}$	Pt. Revenue	Oper. Expenses	Profit Margins
	(1)	(2)	(3)	(4)	(5)
$post \times Medicaid$ eligibility	0.65***	0.61***	0.60***	0.04	0.46**
post Another englosing	(0.22)	(0.21)	(0.21)	(0.24)	(0.20)
<i>post</i> ×Subsidy eligibility	0.20	0.21	0.35^{*}	0.24	0.13
r	(0.20)	(0.18)	(0.20)	(0.15)	(0.10)
Observations	22,201	22,201	23,896	23,896	23,896
Hospitals	3,734	3,734	3,978	3,978	3,978
Counties	2,249	2,249	$2,\!257$	2,257	$2,\!257$
R^2	0.966	0.995	0.994	0.996	0.737

Table A8: Effect of Program Eligibility on Hospital Finances, non-early adoptors

Removed states: CA, CT, DC, MN, NJ, and WA

Estimates are from fixed-effect regressions of the logged dependent variable (indicated in column heading) on independent variables (listed as row headings). The $post \times Medicaid$ Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a hospital patient population's exposure to the Medicaid treatment. The $post \times Subsidy$ Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a hospital patient population's exposure to the private subsidy treatment. Robust standard errors are clustered at the state level. The study sample is from 2009 through 2015 while the program eligibility variables are constructed from 2013 ACS data. All controls and variable constructions are identical to those detailed in Table 3.

 $^{\phi}$ Price is calculated slightly differently here. The dependent variable is net revenue and quantity controls are added.

Removed states: CA, CT, DC, MN, NJ, and WA									
	Cost of Uncomp Care		Share of	Uncomp Care [†]	Medicaid Cos				
	(1)	(2)	(3)	(4)	(5)				
<i>post</i> ×Medicaid Eligibility	-4.68***	-4.18***	-0.08	0.46^{*}	2.89***				
poor incarcara Engloritoj	(1.09)	(1.56)	(0.13)	(0.24)	(1.04)				
<i>post</i> ×Subsidy Eligibility	2.73***	1.77^{*}	0.28^{*}	-0.34	-0.48				
	(0.77)	(0.99)	(0.17)	(0.28)	(0.61)				
Medicaid Eligibility×Share $Uncomp_{pre}$		-6.70		-7.96**					
r		(27.26)		(3.72)					
Subsidy Eligibility×Share $Uncomp_{me}$		14.70		7.92^{*}					
		(12.80)		(4.58)					
Observations	18,600	18,600	19,953	19,953	19,035				
Hospitals	3,423	3,423	$3,\!609$	3,609	3,434				
Counties	2,102	2,102	2,135	2,135	2,103				
R^2	0.942	0.942	0.538	0.538	0.973				
	Remove	d states: CA	4						
	Cost of U	Incomp Care	Share of [Uncomp Care †	Medicaid Cos				
	(1)	(2)	(3)	(4)	(5)				
<i>post</i> ×Medicaid Eligibility	-4.50***	-4.07**	-0.08	0.43^{*}	2.69***				
F	(1.06)	(1.54)	(0.16)	(0.22)	(1.00)				
<i>post</i> ×Subsidy Eligibility	2.81***	1.83**	0.28^{*}	-0.33	-0.56				
	(0.74)	(0.90)	(0.16)	(0.28)	(0.65)				
Medicaid Eligibility×Share $Uncomp_{pre}$		-5.62		-7.87**					
r		(26.98)		(3.67)					
Subsidy Eligibility×Share $Uncomp_{pre}$		15.32		7.89					
- pro		(11.89)		(4.63)					
Observations	20,001	20,001	21,340	21,340	21,344				
Hospitals	3,692	3,692	3,892	$3,\!892$	3,706				
Counties	2,208	2,208	2,243	2,243	2,210				
R^2	0.941	0.941	0.544	0.579	0.973				

Table A9: Effect of Program Eligibility on Uncompensated Care and Medicaid Cost, nonearly adoptors

[†] Dependent variable is not logged.

Estimates are from fixed-effect regressions of the logged dependent variable (indicated in column heading) on independent variables (listed as row headings). The $post \times Medicaid$ Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a hospital patient population's exposure to the Medicaid treatment. The $post \times Subsidy$ Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a hospital patient population's exposure to the Medicaid treatment. The $post \times Subsidy$ Eligibility coefficient indicates by how many percentage points the dependent variable changes for a 1 percentage point increase in a hospital patient population's exposure to the private subsidy treatment. Robust standard errors are clustered at the state level. The study sample is from 2009 through 2015 while the program eligibility variables are constructed from 2013 ACS data. All controls and variable constructions are identical to those detailed in Table A10.

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	Cost of Uncomp Care Share of Uncomp Care [†]			Medicaid Cost	
	(1)	(2)	(3)	(4)	(5)
$post \times Medicaid$ Eligibility	-9.43***	-8.55***	-0.27***	0.56	5.46***
	(0.99)	(1.66)	(0.09)	(0.42)	(0.86)
<i>post</i> ×Subsidy Eligibility	5.49***	4.68***	0.37**	-0.21	-1.85***
	(0.77)	(0.99)	(0.15)	(0.20)	(0.57)
Medicaid Eligibility×Share $Uncomp_{pre}$		-15.14		-14.06**	
		(27.07)		(6.96)	
Subsidy Eligibility×Share $Uncomp_{pre}$		12.22		7.13^{*}	
		(12.63)		(3.55)	
ACA Medicaid effect	-0.481		-0.014		0.028
ACA Subsidy effect	0.356		0.024		-0.012
mean(dependent variable)	7.21	7.21	0.056	0.056	15.84
Observations	22,347	22,347	22,347	22,347	21,344
Hospitals	4,038	4,038	4,038	4,038	3,966
Counties	2,290	2,290	2,290	2,290	2,201
R^2	0.931	0.931	0.503	0.550	0.971
Unemp. Rate	Y	Y	Y	Y	Y
Year FEs	Υ	Υ	Υ	Υ	Y
DSH FEs	Υ	Υ	Υ	Υ	Y
Hospital FEs	Υ	Υ	Υ	Υ	Υ
State FEs	Υ	Υ	Υ	Υ	Υ
State Time Trends	Υ	Υ	Υ	Υ	Υ
Demographic Controls	Υ	Υ	Υ	Υ	Υ

Table A10: Effect of Program Eligibility on Uncompensated Care and Medicaid Cost

See first footnote to Table 2.

 † Dependent variable is not logged. * p < 0.10, ** p < 0.05, *** p < 0.01