

Ippolito, Benedic N.

**Working Paper**

## Financial incentives, hospital care, and health outcomes: Evidence from fair pricing laws

AEI Economics Working Paper, No. 2015-11

**Provided in Cooperation with:**

American Enterprise Institute (AEI), Washington, DC

*Suggested Citation:* Ippolito, Benedic N. (2016) : Financial incentives, hospital care, and health outcomes: Evidence from fair pricing laws, AEI Economics Working Paper, No. 2015-11, American Enterprise Institute (AEI), Washington, DC

This Version is available at:

<https://hdl.handle.net/10419/280532>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

# FINANCIAL INCENTIVES, HOSPITAL CARE, AND HEALTH OUTCOMES: EVIDENCE FROM FAIR PRICING LAWS\*

MICHAEL M. BATTY<sup>†</sup> AND BENEDIC N. IPPOLITO<sup>‡</sup>

July 2016

State laws that limit how much hospitals are paid by uninsured patients provide a unique opportunity to study how financial incentives of healthcare providers affect the care they deliver. We estimate the laws reduce payments from uninsured patients by 25-30 percent. Even though the uninsured represent a small portion of their business, hospitals respond by decreasing the amount of care delivered to these patients, without measurable effects on a broad set of quality metrics. The results show that hospitals can, and do, target care based on financial considerations, and suggest that altering provider financial incentives can generate more efficient care.

---

\*The authors are grateful to J. Michael Collins, Tom DeLeire, Jason Fletcher, Jesse Gregory, Sara Markowitz, John Mullahy, Karl Scholz, Alan Sorensen, Justin Sydnor, Chris Taber, Dave Vanness, Bobbi Wolfe, and numerous seminar participants for their helpful comments. This work was supported by the National Institutes of Health under the Ruth L. Kirschstein National Research Service Award No. T32 MH18029 from the National Institute of Mental Health.

<sup>†</sup> Michael Batty, Federal Reserve Board of Governors, [mike.batty@frb.gov](mailto:mike.batty@frb.gov)

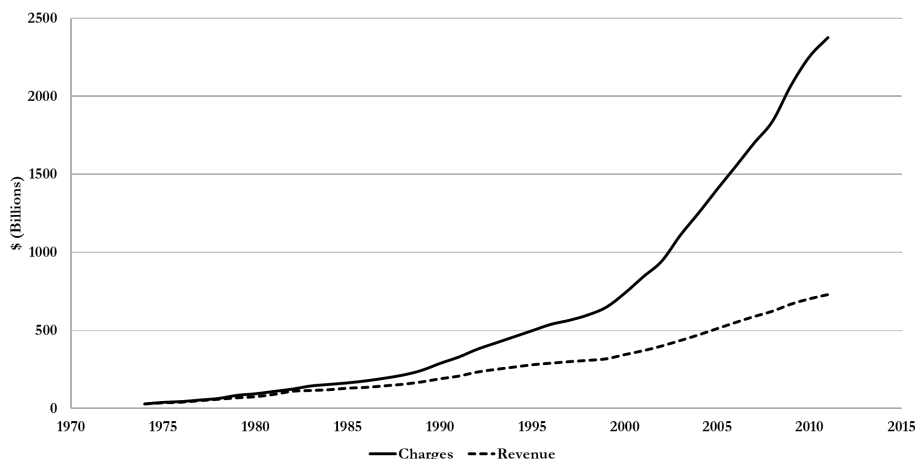
<sup>‡</sup> Benedic Ippolito, American Enterprise Institute, [benedic.ippolito@aei.org](mailto:benedic.ippolito@aei.org)

# 1 Introduction

It is widely believed that the way health care providers are paid affects the care they deliver. Given estimates that suggest 30% of healthcare spending is wasteful (Smith et al., 2013), there is hope that proper incentives can alter provider behavior in ways that improve the efficiency of healthcare. Opportunities to study how provider financial incentives affect care and its efficiency are relatively rare, though. Much of the existing literature relies on comparisons of fundamentally different groups - insured and uninsured patients (Levy and Meltzer, 2008), or combines insurance's effect on payments to providers with the financial protections it affords patients (Finkelstein et al., 2012; Card et al., 2009, 2008; Manning et al., 1987). In this paper we take advantage of an exogenous change in financial incentives created by "fair pricing" laws - which limit how much uninsured patients pay hospitals - to investigate how hospital care and health outcomes respond to financial incentives.

After a hospital visit, patients typically receive a bill showing three different prices for each service: the official list price, the price negotiated by the insurer (if applicable), and the amount remaining for the patient. As recently as the late 1970s, hospitals typically collected the full list price for the services delivered. In the years since, list prices have increased substantially, and now bear little relationship to either hospital expenses or payments made on behalf of insured patients (Tompkins et al., 2006). As depicted in Figure 1, while hospital spending has increased rapidly (9% annually), it has been far exceeded by growth in charges (12.4% annually).

Figure 1: Charges and Revenues for US Hospitals, 1974-2012



*Note:* Charges represent the list price of hospital care delivered, while revenue represents actual prices paid to hospitals. 1974-2003 taken from Tompkins et al. (2006). 2004-2012 constructed from Centers for Medicare and Medicaid Services (CMS) data on hospital revenue, charges, and cost-to-charge ratios. All dollar figures are nominal.

While insured patients benefit from the negotiated discounts, the uninsured are typically billed full list price.<sup>1</sup> Unsurprisingly, these billing practices have been characterized as inequitable. A number of states have responded by enacting “fair pricing” laws (FPLs) that prevent hospitals from collecting more from uninsured patients than they would for the same services from a public or large private insurer. Thus, FPLs create competing incentives for care delivery by reducing both the price to the consumer, and the payment to the provider. This allows us to determine whether overall changes in care are dominated by patient vs. provider responses to the changing financial incentives.

We first use the Medical Expenditure Panel Survey and hospital financial

---

<sup>1</sup>While hospitals often settle for less, they negotiate from a position of strength, because they have the legal authority to sue for the full amount.

data to establish that FPLs do impose binding price ceilings for uninsured patients. We estimate that the price for hospital care for the average uninsured patient falls by 25 to 30 percent. We then use data from the Nationwide Inpatient Sample, in an event study framework, to show that hospitals substantially decrease the amount of inpatient care delivered to uninsured patients in response. The introduction of a FPL leads to a seven to nine percent reduction in the length of stay for uninsured patients, and a similar percentage reduction in billed charges per stay. These changes in treatment patterns are not mirrored in the insured population, adding to growing evidence that hospitals can, and do, treat patients differently based on insurance status (e.g. Doyle, 2005). The effects we observe also illustrate how provider behavior can generate the type of insurance-based care disparities that have been well documented (e.g. Levy and Meltzer, 2008).

Although a reduction in the quantity of care might itself be thought of as a decrease in quality, hospitals may have the ability to produce the same health outcomes more efficiently. Using a battery of metrics, including targeted short-term quality indicators developed by the Agency for Healthcare Research and Quality (AHRQ), and longer-term information on the frequency of hospital readmission, we find no evidence that FPLs lead to worse health outcomes. FPLs are not associated with increases in mortality, medical errors, or readmissions. Nor do we observe changes in the appropriate use of high-cost, high-tech medical procedures. In addition to the consistent pattern of null results, we are generally able to rule out more than modest declines in quality. This may be because within broad types of admissions, hospitals target these reductions at relatively less severe patients. Thus, FPLs appear to do more to generate efficient care, rather than lower quality care.

High and seemingly arbitrary hospital list prices have garnered significant attention in recent years, are often cited as creating considerable financial distress for uninsured patients (Anderson, 2007; Dranove and Millenson, 2006;

Reinhardt, 2006; Tompkins et al., 2006), and FPLs appear to be an increasingly popular solution.<sup>2</sup> Even after full implementation of the Affordable Care Act (ACA), an estimated 30 million Americans will remain uninsured and thus potentially affected by these new regulations.<sup>3</sup> While evidence has shown that hospitals comply with FPLs (Melnick and Fonkych, 2013), ours is the first study of how fair pricing laws affect the amount and quality of health care given to uninsured patients.

In addition, FPLs provide a new and compelling opportunity to study how providers alter care in response to financial incentives, and how this ultimately affects patient outcomes. Our study complements an existing literature that mostly studies Medicare policy changes from the 1980s and 90s. Much of the evidence comes from the 1983 introduction of the Prospective Payment System (PPS), which moved Medicare from reimbursing hospitals for their costs of providing services (plus a modest margin), to almost exclusively reimbursing hospitals a flat rate based on the diagnoses of a patient. Research suggests it led to relatively large reductions in length of stay and the volume of hospital admissions (Coulam and Gaumer, 1991), more patients being treated in outpatient settings (Hodgkin and McGuire, 1994; Ellis and McGuire, 1993), but no substantive reductions in quality of care (Chandra et al., 2011). Another body of work focuses on more targeted Medicare fee changes, and yields mixed results. Recently, Clemens and Gottlieb (2014) show how area-specific price shocks from a 1997 Medicare rule change lead physicians increase care and invest more in medical technology, while leaving

---

<sup>2</sup>Twelve states have enacted FPLs thus far, several others are considering legislation, and courts in several more are adjudicating class action law suits that could ultimately impose similar restrictions.

<sup>3</sup>Updated estimates are available from the Congressional Budget Office. The ACA provides very limited protection from list prices for people who remain uninsured. It includes a fair pricing clause, but it only applies to non-profit hospitals, and does not specify an amount of financial assistance or eligibility rules.

health outcomes largely unaffected.<sup>4</sup> A change like the introduction of PPS is somewhat similar to FPLs, but it was a one-time change to Medicare, meaning it lacks a clear control group since essentially all hospitals were affected at the same time, and the relevant outcomes were not stable prior to implementation. The state and time variation of FPL enactment is advantageous in this regard since it provides a natural control group to help rule out potential confounding effects. Moreover, FPLs offer particularly compelling evidence on the importance of provider financial incentives because they show how even those imposed for a small and often overlooked population such as the uninsured can elicit a strong, targeted response.

## 1.1 Description of Fair Pricing Laws

Although not all fair pricing laws are identical, the typical law includes several essential features. First and foremost, it limits collections from most uninsured patients (below an income cap) to amounts similar to what public or private insurers would pay for the same service. Further, it requires that hospitals provide free care to low to middle income uninsured patients.<sup>5</sup>

We restrict our attention to six states that enacted fair pricing laws in our data window and cover the majority of the uninsured population. They are summarized in Table 1.<sup>6</sup>

---

<sup>4</sup>Other papers in this area, including Rice (1983), Nguyen and Derrick (1997), Yip (1998), and Jacobson et al. (2010), tend to find evidence of backward bending supply curves, where physicians increase utilization of services to offset the lost income from fee reductions.

<sup>5</sup>The law will also require that these discounts be publicized throughout the hospital (and on the bill) so uninsured patients know to apply.

<sup>6</sup>The table captures the most important feature of each law, but the more detailed provisions are discussed here: <http://www.communitycatalyst.org/initiatives-and-issues/initiatives/hospital-accountability-project/free-care>. We exclude six other states that have some form of price restrictions for uninsured patients. Maryland, Maine, Connecticut, and Colorado enacted laws too early or late for our data. Oklahoma is not included because it does not mandate that hospitals publicize their FPLs, and instead requires patients to discover and apply for the discount themselves. Our search for infor-

Table 1: Fair pricing laws by state

State	Year Enacted	Income Limit as Percent of Fed. Poverty Level	Percent of Uninsured Covered
MN	2005	~500%	86%
NY	2007	300%	76%
CA	2007	350%	81%
RI	2007	300%	77%
NJ	2009	500%	87%
IL	2009	~600%	~95%

*Note:* FPLs cover the facility charge rather than those of separately billing doctors. The facility charge is approximately 85% of the average total bill. We estimate percentage of uninsured covered in each state using the Current Population Survey. The income cap for Minnesota’s law is actually \$125,000, which is approximately 500% of poverty for a family of four, and Illinois sets the cap at 300% for rural hospitals.

Although the income limit varies by state, in each case the vast majority of uninsured patients are covered. Thus, for most of our analysis we will not distinguish between these six different laws. There are several substantive differences, such as whether prices are capped relative to public vs. private payers, and how much free care is mandated. Our general findings hold for the FPL in each state, but we investigate these differences in more detail in Appendix A.

## 2 Price Changes Imposed by Fair Pricing Laws

It is not immediately clear that FPLs impose meaningful (i.e., binding) price ceilings. It is well known that outside of these laws, hospitals provide discounted or free “charity care” to certain uninsured patients, and struggle to

---

mation about the Oklahoma law suggests that uninsured patients would have considerable difficulty learning about their eligibility for the discount, and our analysis of hospital behavior in the state suggests this is a critical feature of a FPL. Finally, Tennessee has a law that sets a cap on payments at 175% of cost, which allows considerably higher prices than our other treatment states. Still, our overall results are very similar if we include Oklahoma and/or Tennessee as treatment states.



collect payment from others. If instead of mandating new discounts, FPLs primarily formalize those that are already achieved through these less formal channels, we would expect them to have limited effect on hospital behavior.<sup>7</sup> In this section we analyze several data sources that indicate FPLs do reduce payments by uninsured patients to hospitals on the order of 25 to 30 percent.<sup>8</sup>

## 2.1 Medical Expenditure Panel Survey (MEPS)

We begin by investigating how much uninsured patients actually pay hospitals. Previous research has shown that, on average, hospitals collect a similar percentage of the list price from uninsured and publicly insured patients (Hsia et al., 2008; Melnick and Fonkych, 2008). We are unaware, however, of any existing research that documents the underlying variation in collection rates (percentages of list prices paid) from the uninsured population. Below we show that the similar average collection rates masks wide dispersion in payments from uninsured patients. The results suggest that FPLs are likely to bind for at least a meaningful number of uninsured who pay a large portion of list price.

The Medical Expenditure Panel Survey (MEPS) is a nationally representative survey of health care use and spending in the United States. Critical to our work, it is the most reliable publicly available patient-level data about payments from uninsured patients. To improve the reliability of payment data, the MEPS verifies self-reported payments with health care providers

---

<sup>7</sup>It may be possible for FPLs to affect negotiated prices, and thus hospital behavior, even when the price ceiling is not binding. For example, by restricting the hospital's opening offer, FPLs could reduce the final price reached in negotiations between hospitals and uninsured patients. Even if the final prices are not affected, FPLs may improve the financial well-being of patients through reduced use of debt collectors.

<sup>8</sup>Appendix B describes the passage of California's FPL, which provides alternative evidence that hospitals believe the restrictions are meaningful.

when possible.<sup>9</sup> Our sample includes all patients with either public or no insurance in the MEPS between 2000 and 2004<sup>10</sup> who went to the hospital at least once, resulting in 21,168 patient-year observations. Each individual is interviewed five times over two years, but for our analysis we ignore the panel structure of the data and pool all year-person observations. We split our sample into two groups: those who had public insurance at some point in the year (Medicare or Medicaid), and those who had no insurance at any point in the year.

Table 2 shows the average annual charges and collection rates for publicly insured and uninsured patients. Like previous research, we find that hospitals collect similar percentages of list prices from the two groups. Not surprisingly, patients with public insurance - which includes many relatively expensive patients (Medicare and disabled individuals covered by Medicaid) - have considerably higher average charges.

Table 2: Summarizing hospital charges and collections by payer-type

Insurance Status	Count	Mean Hospital Charges	Mean Percentage of List Price Collected
Public Insurance	17,276	\$13,046	38%
Uninsured	3,892	\$5,035	37%

*Note:* The data are from the Medical Expenditure Panel Survey from 2000-2004.

However, the distributions of payments from these two patient groups show the averages are misleading. Figure 2 presents a histogram of collection rates for uninsured and publicly insured patients.<sup>11</sup> For this exercise we exclude the highest income uninsured patients who are generally not covered

<sup>9</sup>The results in this section do not change if we restrict the sample to only those with verified payment information. Further, we focus on the facility rather than the "separately billing doctor" charges because only facilities charges are typically covered by the FPLs.

<sup>10</sup>The data lack state identifiers so we select this period because it precedes the earliest FPL.

<sup>11</sup>In Appendix C we show that Medicare and Medicaid patients have very similar payment distributions.

by FPLs, but a version of the figure including all uninsured patients is very similar. Collection rates for publicly insured patients are more concentrated around the average rate (38 percent),<sup>12</sup> while payments from uninsured patients are much less centralized, with most of the weight at very low and very high collection amounts. Indeed, the data show that many uninsured patients pay large fractions of their hospital bills. Note that distribution in collection rate occurs both because hospitals charge different prices, and because patients ultimately pay different amounts when facing the same bill. Since reimbursement from public insurers is relatively stable across patients, we believe the distribution of public payers primarily captures variation in prices, and then the excess dispersion of the uninsured represents variation in payments.

It is possible that differences in care received explain the patterns in Figure 2. For example, if bill size and collection rates are negatively correlated, then the high end of the collection rate distribution for uninsured patients may be driven by patients with small bills. To address this concern, we employ quantile regressions of percentage of list price paid against a dummy variable for being uninsured, while holding bill size constant.<sup>13</sup> Table 3 reports the results. Even after adjusting for the size of hospital bill, uninsured patients pay a bit more than public payers at the median, but a large fraction of uninsured patients pay much more.<sup>14</sup>

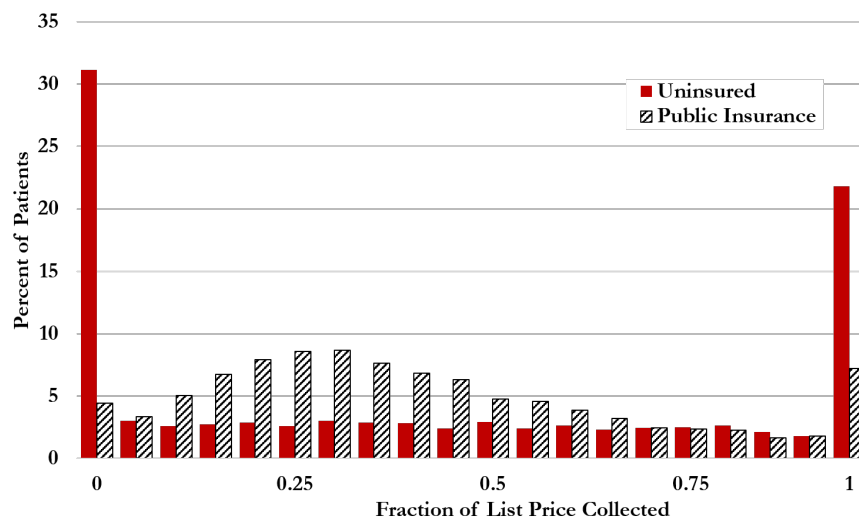
---

<sup>12</sup>Some of the weight in the tails of the distribution for publicly insured patients is likely from patients who had public insurance at some point in the year, but were uninsured at the time of the hospital visit.

<sup>13</sup>We control for bill amount because sample sizes are too small to match uninsured and publicly insured patients on the basis of diagnosis.

<sup>14</sup>Mahoney (2015) finds a stronger relationship between bill size and payments than we do. This is likely because he is only measuring out-of-pocket payments from patients, while we consider any source of payment for an uninsured stay (such as liability or auto insurance, worker's compensation, or other state and local agencies that aid uninsured patients). We focus on total payment because it is what is relevant to the hospital. While collection rates for patients purely paying out of pocket are somewhat lower, they still display the pattern of bunching at very low and very high collection rates.

Figure 2: Distribution of percentage of list price paid for publicly insured and uninsured patients - excluding high income uninsured



*Note:* The data are from the Medical Expenditure Panel Survey from 2000-2004. We exclude uninsured patients with incomes above 400% of poverty (which approximates the group not covered by FPLs).

Ideally, we would use these data to compare payments from uninsured patients before and after FPLs are enacted. Unfortunately, the number of uninsured patients who have hospital expenditures in the MEPS is too small to perform this type of state-level analysis.<sup>15</sup> Instead, we can generate a prediction of how much FPLs would reduce payments by approximating the payment cap. Specifically, we match each uninsured patient in our data (excluding those with high enough incomes to not qualify for FPLs) with a publicly insured patient who has a similar bill size.<sup>16</sup> If the uninsured patient

<sup>15</sup>There are approximately 200 observations per year from the group of FPL states. Given the inherent variability of collection rates, and the subsequent importance of risk-adjustment, this is too small to produce a reliable estimate.

<sup>16</sup>Ideally, this calculation would be based upon capping payments from uninsured at the mean dollar amount a publicly insured patient paid for the same service (since the

Table 3: Quantile regressions of percentage of list price paid by payer type

Collection Ratio	<i>Evaluated at:</i>			
	25th Percentile	50th Percentile	75th Percentile	90th Percentile
Uninsured	-0.234*** (0.00267)	0.0211 (0.0148)	0.213*** (0.0106)	0.084*** (0.00479)
Log(Charges)	-0.004*** (0.000726)	-0.022*** (0.00121)	-0.043*** (0.00167)	-0.036*** (0.00140)

*Note:* Each column is a quantile regression evaluated at the specified point in the distribution of the percentage of list price paid. The regression includes patients with public insurance or no insurance, from MEPS in the years 2000-2004. Standard errors are clustered at the patient level and shown in parentheses. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . The sample size for each regression is 21,168.

in the pair paid a higher percentage of their bill than did the publicly insured patient, we cap collections from the uninsured at the percentage paid by the publicly insured. Although this method may over or underestimate the cap for any given uninsured patient, on average it will reflect payments made with caps that are based upon the typical publicly insured patient (as does the modal FPL). Over five hundred simulations of this exercise, the projected payments from uninsured patients fall by an average of 31%, or \$1,800 per inpatient.<sup>17</sup>

distribution of payments from public patients for a given service should be fairly compact), but the MEPS lacks appropriate diagnosis information (DRGs) to make this type of comparison feasible.

<sup>17</sup>This exercise abstracts from the variety of federal, state, and local programs that pay hospitals for providing uncompensated care. Although a recent estimate finds that in aggregate these programs reimburse two-thirds of uncompensated care (Coughlin, 2014), we believe it is unlikely they will allow hospitals to substantially offset the fall in prices caused by FPLs. Federal programs for Medicare and the VA do not apply to this population, and state/local programs would require dedicated funding increases. Although we cannot comment on each program, Medicaid Disproportionate Share Hospital payments

## 2.2 Hospital Financial Data

In this section we use hospital financial data from our largest treatment state, California, to provide direct evidence on payment reductions caused by FPLs. The California Office of Statewide Health Planning and Development (OSHPD) provides utilization and financial data by payer category from all California hospitals. These data allow us to compare how payments from the uninsured change after the introduction of a FPL relative to other patients.

In order to compare payments for similar amounts of care we focus on payment-to-cost ratios (where cost includes marginal and allocated overhead). This also adjusts for any changes to the amount, and thus the cost, of care provided to uninsured patients as a result of the FPL. Figure 3 shows how the payment-to-cost ratios evolve for uninsured and Medicaid in the years leading to and following the enactment of California's FPL.<sup>18</sup> Prior to the FPL, payments from both groups trend similarly, but diverge markedly after enactment, largely due to a decline in payments from the uninsured. We compare uninsured to Medicaid patients because they are arguably the most similar, however our results are very similar if we instead compare uninsured to either privately insured or Medicare. Pooling the pre and post years, the payments per unit of care from the uninsured have fallen by 26.5% relative to Medicaid patients.

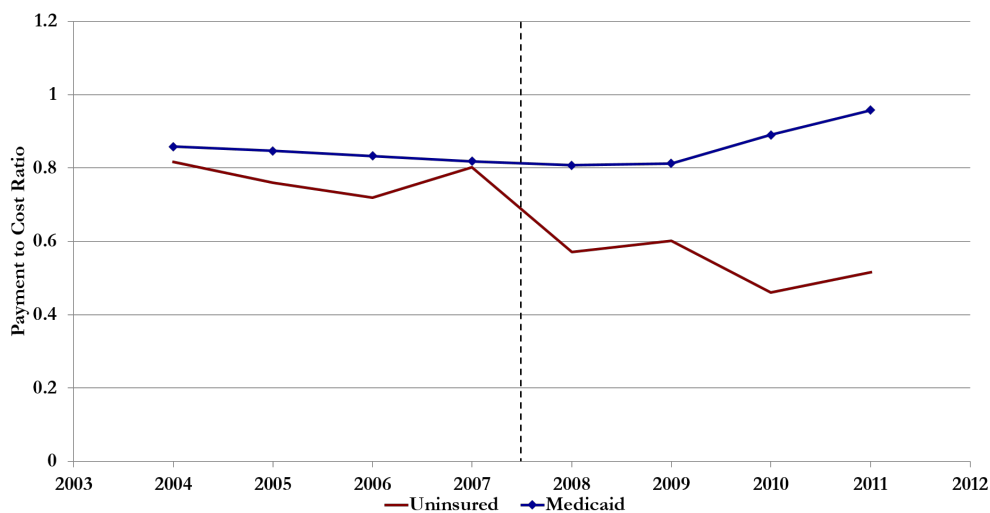
While California provides unusually detailed financial data, some other states do report uncompensated care (charity care and uncollectable bills). A decline in payments from the uninsured should be reflected in an increase in uncompensated care. However, other payer groups also contribute to un-

---

(the largest such program) did not increase. Further, these programs are designed to reimburse hospitals for treating particularly poor patients, rather than those already paying relatively high prices.

<sup>18</sup>A given year's file contains data for fiscal years that ended in that year. As such, the 2008 file is the first data point after the FPL, whereas approximately half of the data in 2007 file comes from before the law was officially in effect.

Figure 3: Payment-to-Cost Ratios by Payer in California



*Note:* Source: California OSHPD financial pivot files. Payments include patient revenue from all sources. Costs include marginal costs and allocated overhead. All dollar figures are nominal.

compensated care, and movements can be further obscured by the rapid increases in charges that we have described previously. Still, compared to Oregon, a neighboring state that did not enact a FPL, California experienced an increase in uncompensated care consistent with Figure 3. This gives us confidence that the change in uninsured prices in California is not driven by factors that affect uninsured patients in non-FPL states, and suggests that FPLs impose meaningful changes to hospital financial incentives.

Notably, the estimate of the price reduction from the MEPS is very similar to the experience of California hospitals revealed by the OSHPD data. Although both methods have limitations, together they provide considerable evidence that FPLs substantially reduce hospital prices for the average uninsured patient. Hospitals in the largest FPL state saw a sharp reduction in payments from the uninsured after enactment, and our analysis using MEPS

shows that the observed payment reductions are very similar to what we would predict using patient-level data.

## 3 Measuring the Impact of Fair Pricing Laws on Hospital Care

### 3.1 Inpatient Records Data

We study the effects of FPLs on treatment patterns and quality using inpatient records. Each inpatient record includes detailed information on diagnoses, procedures, basic demographic information, payer, hospital characteristics, and admission/discharge information. It also reports the charges incurred (based upon list prices), but does not follow up to capture the amounts patients ultimately pay. Thus, the records allow us to study quantity and quality of care, but not the financial effects of FPLs.

Our primary data source is the Nationwide Inpatient Sample (NIS) developed by the Agency for Healthcare Research and Quality. The NIS is the largest all-payer inpatient care database in the United States. In each year, it approximates a stratified 20% random sample of US acute care hospitals (roughly 8 million discharges from 1000 hospitals). If a hospital is sampled in a given year, all inpatient records from that year at that hospital are included in the data. The data contain a hospital, but not person identifier. This allows us to track changes within hospitals over time, but each time the same person visits a hospital he or she will appear as a distinct record. Since roughly 20% of hospitals are sampled each year, each hospital in our data appears an average of 2.3 times between 2003 and 2011. For the bulk of our analysis, we restrict our sample to all inpatient records for uninsured patients from 41 states (including all six states with fair pricing laws).<sup>19</sup> This

---

<sup>19</sup>Thirty-three states are present in each year of our data, with the other 8 beginning to



gives us approximately 3.2 million observations.

## 3.2 Empirical Framework

For our primary analysis, we use the following event-study specification (e.g., Jacobson et al. (1993)). For an inpatient record,  $i$ , in year  $t$ , quarter  $q$ , state  $s$ , and hospital  $h$ :

$$Y_i = \alpha + \sum_{L \in K} \delta_L FPL_{L(i)} + \beta X_i + \mu_{h(i)} + \gamma_{t(i)} + \chi_{q(i)} + \epsilon_i, \quad (1)$$

where  $K = \{-6, -5, -4, -3, -2, 0, 1, 2, 3, 4\}$ .

$Y_i$  is the outcome of interest (such as length of stay, charges, quality of care, or diagnosis),  $X_i$  is vector of patient characteristics,  $\mu_h$ ,  $\gamma_t$ ,  $\chi_q$  are fixed effects for hospital, year, and quarter, respectively, and  $h(i)$ ,  $t(i)$ , and  $q(i)$  denote the hospital, year, and quarter associated with record  $i$ .

The set of  $FPL_{L(i)}$  dummies represent year relative to the enactment of a fair pricing law ( $L = 0$  denotes the first year of enactment). For example,  $FPL_{1(i)} = 1$  if record  $i$  is from a state between one and two years after the enactment of a FPL, and zero otherwise. Each of the  $\delta_L$  coefficients is measured relative to the omitted category: "1 year prior to adoption." Although our primary specification is built upon the  $FPL_{L(i)}$  dummies, at times we will also report more traditional difference-in-differences results using a single indicator variable for the presence of a FPL.

The validity of this research design relies on the assumption that outcomes in the treatment and control states would have behaved similarly in the "post period" absent the introduction of a fair pricing law. Finding  $\delta_L$  coefficients in the "prior" years that are indistinguishable from zero would indicate the

---

participate in the NIS after 2003. As noted earlier, we exclude CT, MD, ME, and WI. We also drop MA because of dramatic changes to their uninsured population after the 2006 health reform. The remaining 4 states do not share data with the NIS as of 2011.

outcome variables were on similar paths before the laws were passed, and is what we would expect to see if this assumption were true. As we will show throughout the results, the pre-trends we observe imply that the non-FPL states are a valid control group.

It is not immediately clear which patient characteristics should be included in  $X_i$ . We are most interested in measuring how FPLs alter the way a hospital would treat a given uninsured patient, which suggests we should include a rich set of demographic and diagnosis control variables. However, FPLs may change the composition of uninsured patients that are admitted. Excluding patient-level controls would capture the effect of FPLs, allowing for changes to the patient population. Moreover, many FPLs link their payment cap to Medicare's PPS, meaning the payment cap is determined by the diagnosis, giving providers a reason to increase the severity (Carter et al., 1990; Dafny, 2005). As a result, we will investigate the effects of FPLs both with and without controlling for patient diagnosis.<sup>20</sup>

We include hospital fixed effects to account for systematic differences in treatment strategies across hospitals. Without hospital fixed effects, we would be concerned that changes in outcomes could be driven by changes in the sample of hospitals selected each year. Including both hospital and year dummies in the model means the identification of our treatment effects comes from repeated observations of hospitals before and after the introduction of fair pricing laws.<sup>21</sup>

To account for potential within-state correlation of outcomes, we cluster standard errors at the state level. However, as outlined in Conley and Taber (2011), this approach still requires the number of treated clusters to grow

---

<sup>20</sup>We test this "upcoding" theory directly in Appendix D. Unlike the studies of upcoding in the Medicare market, we see little evidence that hospitals engage in this kind of strategic coding behavior in response to fair pricing laws.

<sup>21</sup>Approximately 400, or half of the hospitals in FPL states are observed before and after enactment. Appendix G shows that hospitals that are and are not observed on both sides of FPL enactment do not differ systematically.

large in order to produce consistent estimates. This is relevant given that the number of treated clusters in our application is six. In the results that follow, we show that the confidence intervals produced by state-level clustering and the Conley-Taber method of inference are quite similar.

### **Outcome variables**

The main goal of our analysis is to test whether hospitals respond to fair pricing laws by reducing the quantity and/or quality of treatment delivered to uninsured patients.<sup>22</sup> We choose length of stay (LOS) as our primary measure of quantity for several reasons. First, it is an easily measured proxy for resource use that has a consistent interpretation across hospitals and over time. Furthermore, the large reductions in LOS that occurred after the introduction of Medicare’s prospective payment system (which clearly introduced cost-controlling incentives) suggest that hospitals view length of stay as an important margin upon which they can operate to control costs. Also, decreases in LOS are likely indicative of other cost-controlling behavior, like reductions in the amount, or intensity, of treatment. In addition to LOS, we supplement our analysis of care quantity through other metrics, such as total hospital charges, rates of admission, and frequency of patient transfer. As shown in Appendix F, the results for these alternative measures are similar.

Of course, we are ultimately more concerned with how changes in the amount of care translate into changes in health outcomes. To directly measure care quality, we employ a set of short and longer-term quality metrics. For short-term metrics, we use the Inpatient Quality Indicators software package developed by AHRQ. The package calculates a battery of metrics, including in-hospital risk-adjusted mortality from selected conditions and pro-

---

<sup>22</sup>In Appendix E we also investigate whether FPLs have any impact on the way hospitals set list prices.

cedures, utilization of selected procedures that are associated with decreased mortality, and incidence of potentially preventable in-hospital complications. AHRQ selected each metric both because it is an intuitive measure of quality, and because there is significant variation among hospitals. Since we aim to measure aggregate quality, we will combine the individual metrics within each category into composite measures. For instance, instead of estimating changes in mortality from each individual condition or procedure, we will instead measure mortality from any of the conditions or procedures selected by AHRQ. To assess longer-term changes in quality of care, we measure readmission rates at 30, 60, and 90 days after discharge.

### **Risk-adjustment**

Because FPLs may encourage strategic manipulation of diagnoses, we use the Clinical Classifications Software (CCS) categorization scheme provided by HCUP as our primary risk-adjustment method. The CCS collapses the 14,000 ICD-9-CM's diagnosis codes into 274 clinically meaningful categories. For instance, 40 ICD-9-CM codes corresponding to various types of heart attacks are aggregated into a single "Acute myocardial infarction" group. We argue that it is much less likely that strategic diagnosing would move a patient between, as opposed to within, CCS categories. Thus, controlling for CCS still provides meaningful information about the severity of the health condition, while also providing a buffer against the type of strategic diagnosing described above. Admittedly, this risk-adjustment strategy may miss more granular diagnosis information. To compensate, we also look for changes in the characteristics of the patient population that would suggest systematic changes in diagnosis patterns are driven by real changes in patient composition.

## Defining Treatment

Recall that fair pricing laws only apply to uninsured patients with incomes up to some multiple of the poverty line. Since our data do not include individual level income, we cannot identify which uninsured patients are actually covered. Thus, we estimate an intent-to-treat model using all uninsured patients regardless of personal income. By assigning some non-treated patients to the treatment group, our results may underestimate the true effects of the laws. However, we only study states where the percentage of uninsured covered by a FPL is very high (at least 76 percent), meaning our estimates should be close to treatment-on-the-treated estimates. It is also possible that because a patient's income may not be immediately salient, and the vast majority of uninsured patients they encounter are covered, hospitals may treat all uninsured patients as if they are covered by the laws.<sup>23</sup> In this case we would not underestimate the true effect.

## California-Specific Model

For some of our analysis, we will utilize the California State Inpatient Database from 2005 to 2009, which is very similar to the NIS, but covers the universe of California admissions in a year. For analysis using the SID, we estimate the following model for an inpatient record,  $i$ , in year  $t$ , quarter  $q$ , and hospital  $h$ :

$$Y_i = \alpha + \sum_{L \in K} \delta_L FPL_{L(i)} + \beta X_i + \mu_{h(i)} + \gamma_{t(i)} + \chi_{q(i)} + \epsilon_i, \quad (2)$$

where  $K = \{-2, 0, 1, 2, 3\}$ .

---

<sup>23</sup>Under the EMTALA, hospitals may only begin to inquire about ability to pay after it is clear doing so will not compromise patient care. Reports suggest that some hospitals do pull credit reports for patients to inform collections efforts, though some advocates argue this practice may affect provision of care (see "Why Hospitals Want Your Credit Report" in the March 18, 2008 issue of the Wall Street Journal).

$Y_i$  is the outcome of interest,  $X_i$  is vector of patient characteristics which contains the same information as in the NIS,  $\mu_h$ ,  $\gamma_t$ ,  $\chi_q$  are fixed effects for hospital, year, and quarter, respectively, and  $h(i)$ ,  $t(i)$ , and  $q(i)$  denote the hospital, year, and quarter associated with record  $i$ . Equation 2 illustrates the event study specification, though we will often replace the yearly treatment dummies with a single difference-in-difference dummy for the FPL. The most important difference between this specification and the one estimated with the NIS is the control group. Because these data only cover California, we can not compare uninsured in California to uninsured in other states. Instead, we compare uninsured to the most similar insured group in the state: Medicaid patients. Identification of our treatment effects comes from comparing uninsured to Medicaid patients within the same hospitals over time. Finally, standard errors are clustered at the hospital level.

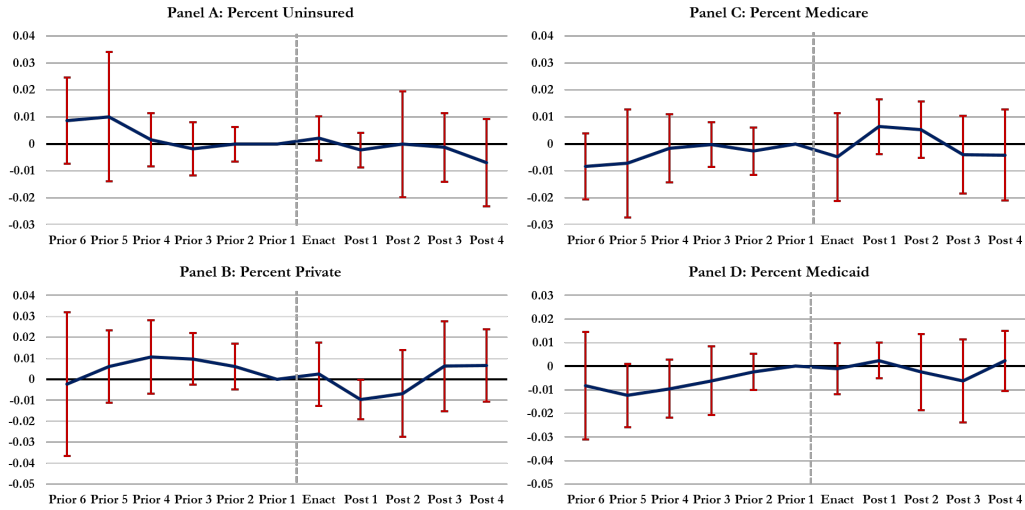
### 3.3 Investigating Changes in Patient Composition

FPLs can be thought of as a type of catastrophic insurance, so they may induce more people to go without insurance and/or more uninsured patients to seek treatment at hospitals. Moreover, the reduced payments could lead hospitals to change admission patterns of the uninsured. Any such changes would be important for interpreting the results of our main analysis regarding the type and amount of care delivered. To investigate this margin we first estimate the impact of FPLs on the payer mix of patients treated at hospitals. Specifically, we estimate an event-study specification at the hospital-year level where the outcome is the fraction of patients with a given insurance type.

The yearly treatment effects are plotted in Figure 4. Most importantly, Panel A illustrates the effect of FPLs on the fraction of patients that are uninsured. The treatment coefficients are small and indistinguishable from

zero, indicating that FPLs are not associated with significant changes in the share of uninsured inpatient stays at hospitals. In the first two years under a FPL we can rule out changes larger than one percentage point. The precision of these estimates is generally lower in later years, though coefficients remain small. In Panels B, C, and D we report estimates for patients with private insurance, Medicare, and Medicaid, respectively. Overall, we see little evidence that FPLs systematically change the payer mix of patients that are admitted to hospitals.

Figure 4: The effect of fair pricing laws on the share of inpatients stays accounted for by insurance type



*Note:* We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. Standard errors are clustered at the state level and are illustrated by the vertical lines. Pre-treatment means: Medicare: 41%, Medicaid: 19%, Priv: 33%, Uninsured: 5%.

While the number of uninsured treated is stable, it is possible that the underlying composition of the uninsured is affected by FPLs. In Figure 5, we show the effect of FPLs on a number of observable characteristics of the

uninsured admitted to hospitals. For context we also include estimates for the insured sample.

Panels A and B show the effect of FPLs on the average age of patients and fraction non-white. In both cases the coefficients for insured and uninsured are generally similar. Moreover, in neither case do we see systematic shifts among the uninsured following enactment. The NIS does not include individual-level income, but does include a categorical variable indicating where the median income of a patient’s home zip code falls in the national distribution (specifically, which quartile). Panel C shows the fraction of patients who are from a zip code with a median income in the top quartile. There is a consistent small increase in patients from higher income zip codes in treated states, though the trend appears to pre-date FPLs and occurs both for insured and uninsured. Particularly with the uninsured, treated states were trending differently prior to enactment. Finally, the fraction of female uninsured in treated states is somewhat noisy. We observe positive coefficients in a few post years, though the same is true of most prior years as well. Overall, we observe some changes in the characteristics of the uninsured in treatment states, though there is little indication that FPLs directly cause these shifts. We will revisit this compositional issue in the next section where we report regression results with and without controls for characteristics of the patient population.

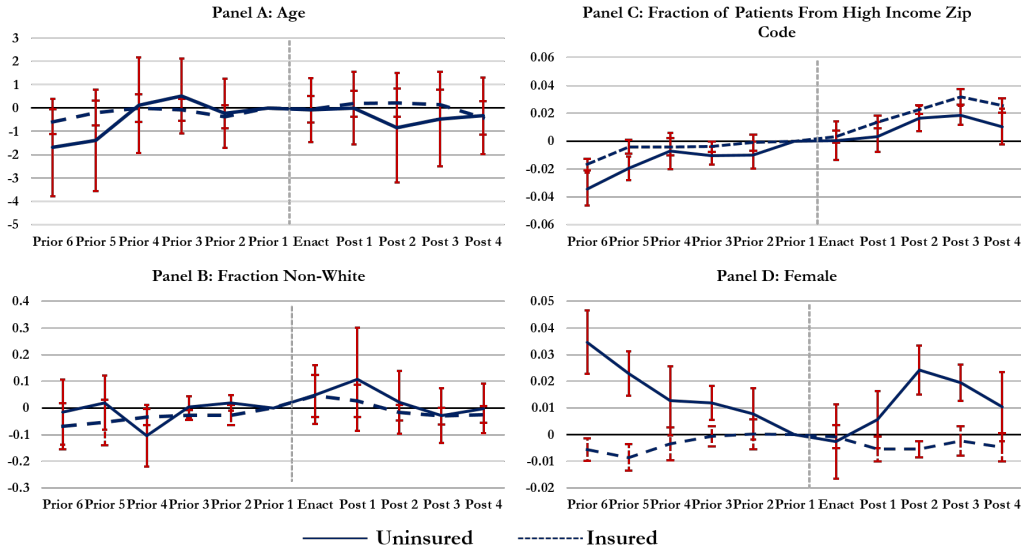
## 4 Results for Quantity of Care

### 4.1 Length of stay

We now test whether FPLs induce hospitals to engage in cost-reducing behavior through shortened lengths of stay for uninsured patients. The results are reported in Table 4. Model (1) reports our yearly treatment effects with



Figure 5: The effect of fair pricing laws on the composition of admitted patients



*Note:* We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. Standard errors are clustered at the state level and are illustrated by the vertical lines. Pre-treatment means: age: 35.1, fraction non-white: 0.448, fraction from high income zip: 0.23, fraction female: 0.48.

no demographic or risk-adjustment. In model (2) we include demographics, while model (3) we include CCS-based risk-adjusters and demographics. Standard errors are clustered at the state level.

By excluding all patient-level controls in model (1) we are measuring how FPLs affect length of stay, without attempting to control for any potential changes in the types of uninsured being admitted. Model (3) offers a more “apples-to-apples” comparison by measuring how hospitals treat observably similar patients before and after a FPL. Comparing results across models reveals the importance of any changes in patient attributes over time.

Across the models we do not see significant effects prior to the enactment

of fair pricing laws, indicating that our treated and control states were trending similarly prior to the introduction of a FPL. In the years post adoption we see clear and systematic evidence of reduced lengths of stay in the treated group. The magnitudes grow in the first years after enactment, which suggests that hospitals may be slow to react to FPLs, and/or hospitals learn tactics to shorten hospital stays over time.

The size of the treatment coefficients typically reduces slightly with the addition of more controls, though the estimates in model (1) fall within the confidence intervals of model (3). This is consistent with the analysis presented in the previous section - changes in composition of the uninsured are unlikely to be driving the results. Focusing on the column (3), towards the end of our sample hospital stays for uninsured patients have fallen around 0.3 days, or about 7.5 percent. It is worth noting that the smallest treatment effect within the confidence interval is approximately four percent, meaning we can conclude with a high degree of certainty that FPLs substantially reduce LOS.

To put the effect sizes we observe in context, it is helpful to revisit the experience from the introduction of Medicare's PPS, which was generally considered to have a large impact on length of stay. In their literature review, Coulam and Gaumer (1991) highlight an example of a nearly 10% drop in length of stay in the year after the Prospective Payment System (PPS). Since stays were falling in the years leading up to the PPS, though at a much lower rate, this appears to be a reasonable upper bound on the effect size. In that light, the effects we see from fair pricing laws are substantial.

In Figure 6, we illustrate the results from the specification including all demographics and CCS-based risk-adjusters. We show confidence intervals generated by state clustering and by the Conley-Taber procedure. The figure shows that the reduction in LOS is robust to the use of either method. This is consistent with our hypothesis that correlation of outcomes within hospitals

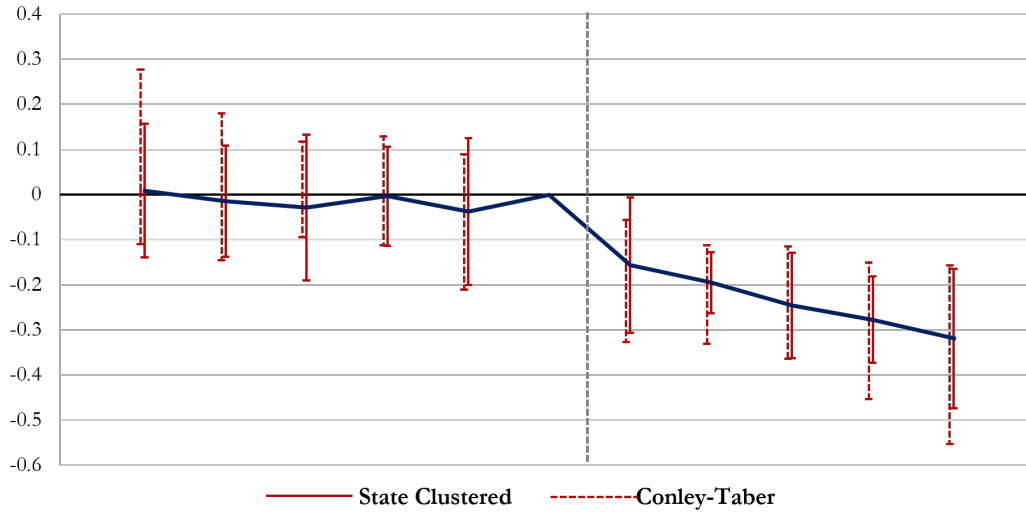
Table 4: The effect of FPLs on length of stay for uninsured patients.

Outcome Variable: Length of Stay			
Pre-treatment mean: 4.08 days			
	(1)	(2)	(3)
	No controls	Demographics	Demographics & Risk-Adjustment
Prior 6	-0.0992 [-0.311,0.113]	-0.0327 [-0.196,0.131]	0.00891 [-0.139,0.157]
Prior 5	-0.102 [-0.336,0.131]	-0.0488 [-0.235,0.137]	-0.0144 [-0.138,0.110]
Prior 4	-0.0571 [-0.281,0.167]	-0.0493 [-0.254,0.155]	-0.0288 [-0.190,0.132]
Prior 3	-0.0323 [-0.166,0.101]	-0.0464 [-0.191,0.0982]	-0.00367 [-0.114,0.107]
Prior 2	-0.0829 [-0.320,0.154]	-0.0798 [-0.291,0.132]	-0.0373 [-0.200,0.125]
Enactment	-0.217* [-0.431,-0.00228]	-0.219* [-0.397,-0.0409]	-0.156* [-0.306,-0.00676]
Post 1	-0.265*** [-0.401,-0.130]	-0.268*** [-0.375,-0.161]	-0.195*** [-0.263,-0.128]
Post 2	-0.362*** [-0.540,-0.185]	-0.333*** [-0.470,-0.196]	-0.246*** [-0.363,-0.129]
Post 3	-0.292*** [-0.433,-0.150]	-0.293*** [-0.417,-0.170]	-0.277*** [-0.373,-0.182]
Post 4	-0.385** [-0.636,-0.134]	-0.372** [-0.591,-0.153]	-0.319*** [-0.473,-0.165]
Observations	3143772	3143772	3143772

*Note:* Estimates are based on Equation 1. Standard errors are clustered at the state level, and 95 percent CIs are reported in brackets. \* p<0.05, \*\* p<0.01, \*\*\* p<0.001. All models include hospital, year, and season fixed effects. Patient demographics included in all regressions: age, age<sup>2</sup>, gender, and median income of patient's home zip code (categorical variable). Risk adjusters include either the DRG weight or the CCS category of a patient's primary diagnosis, whether a stay was elective, and whether a stay occurred on a weekend.

is far more important than within states. This pattern holds for every model we estimate, so for the rest of our results we only show one set of confidence intervals. We choose errors clustered at the state level because they are more robust to small sample sizes in particular states.<sup>24</sup> We also focus on Model (3) for the remainder of our results because it is qualitatively similar to our other models.

Figure 6: The effect of fair pricing laws on length of stay for uninsured patients



*Note:* This figure illustrates the effect of FPLs on length of stay for uninsured patients and is based on model (3) from Table 4. Data are from the Nationwide Inpatient Sample. We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. The solid and dashed vertical lines indicate the 95% confidence interval calculated using state clustering and the Conley-Taber procedure, respectively. The regression includes our full set of fixed effects, patient demographics, and risk-adjusters.

In Appendix F we re-estimate model (3) for each treatment state individually to investigate whether the overall effects are driven by a subset of FPL

<sup>24</sup>For instance, in some simulations in the Conley-Taber procedure a very small control state (like AK) will stand in for, and be given the weight of, a big FPL state (like CA). This makes Conley-Taber more susceptible to outlying observations from hospitals in small states.

states. The reported estimates are predictably noisier, but show similar reductions in length of stay across our treated states. The fact that we observe similar effects across states also helps to reduce the likelihood that the effects are the result of a separate, concurrent state policy. In that section we also report the results of placebo tests where we missassign treatment status to 6 randomly chosen states (including true treated states). Over 500 iterations we observe reductions as large as ours in only 1.2 percent of cases (and each such case includes actual treatment states).

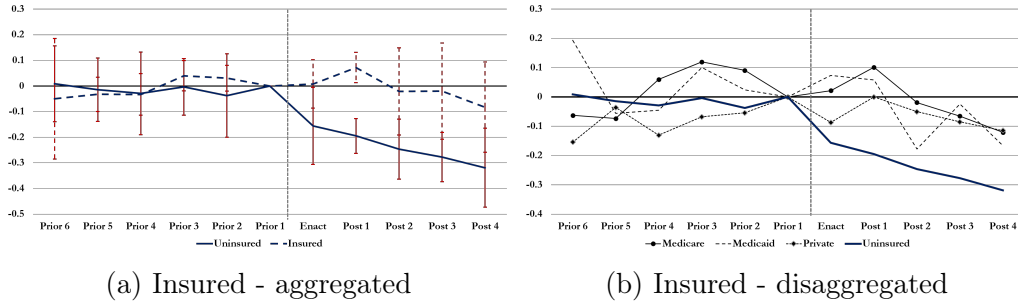
### **Results for Insured Patients**

Next, we test whether similar reductions in length of stay occur for insured patients in states that enacted fair pricing laws. As shown in Figure 7a, following the enactment of a FPL we observe a divergence in LOS trends between uninsured and insured patients. In the post period, estimated coefficients for the insured are centered around zero. The lower end of confidence intervals are generally between -0.1 and -0.2, which correspond to effect sizes of 2 to 4 percent of a baseline length of stay of 4.8. The one exception to this is four years post enactment where we observe non-trivial overlap of confidence intervals across payer types, though the insured estimate does not approach significance. It is possible this lack of a result obscures meaningful impacts among a subset of insured patients. Figure 7b breaks the overall "insured" group into its three major payer types (omitting confidence intervals for legibility). Compared to the uninsured, these groups are less stable prior to enactment, however, the evidence suggests the experience of uninsured patients is not mirrored in one of the insured subgroups.

The fact that treatment patterns clearly diverge following a FPL provides evidence that hospitals can target treatment changes based on individuals' insurance status. This finding is in contrast to work like Glied and Zivin (2002) which finds that the overall composition of insurance types affects

provider behavior, but the insurance type of an individual patient has limited impact.

Figure 7: Comparing Changes in Length of Stay for Uninsured and Insured Patients



*Note:* This figure illustrates the impact of fair pricing laws on lengths of stay for insured and uninsured patients. Data are from the NIS. Estimates are based on estimating Equation 1 for each payer type. In both panels, the solid line with no markers illustrates uninsured patients. The dotted line in Panel (a) represents all insured patients. In Panel (b) the various insured groups are labelled. We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. The regressions includes our full set of fixed effects, patient demographics, and risk-adjusters. See the note on Table 4 for a full list of controls. Pre-treatment average length of stay: Uninsured: 4.08, Insured (overall): 4.87, Medicare: 6.2, Medicaid: 4.69, Private: 3.73.

## Hospital Characteristics

In this section we investigate whether certain types of hospitals respond more to FPLs than others. Because they may have different incentive structures, it is natural to begin by looking for differences between for-profit and non-profit hospitals. For-profit hospitals are rare in our treatment states (primarily due to state rules regarding hospital ownership), so we focus this analysis on California where for-profits are more common.

Column (1) of Table 5 reveals no evidence that for-profit hospitals shorten lengths of stay for uninsured patients differently than do non-profits. This is broadly consistent with prior work documenting limited differences between

for-profit and non-profit hospitals, such as in their provision of uncompensated care (Sloan, 2000).

It is also easy to imagine that well-equipped hospitals that caters to more affluent patients would respond differently than safety-net hospitals. For example, safety-net hospitals may be under greater resource strain due to FPLs, though it's possible they placed less emphasis on extracting revenue from the uninsured prior to FPLs. We proxy these differences by splitting the sample of hospitals based upon the fraction of their patients that are uninsured. On average, roughly five percent of patients are uninsured. Column (2) of Table 5 shows no clear evidence that treating more uninsured patients elicits a stronger reaction to these laws. These results, as well as those generated by splitting hospitals along a variety of other characteristics,<sup>25</sup> suggest that broad classes of hospitals find that FPLs are material to their financial performance and respond accordingly.

Table 5: Hospital characteristics and reactions to fair pricing laws.

	Length of stay	Length of stay
FPL in Effect	-0.196***	-0.162***
	[-0.294,-0.0988]	[-0.234,-0.0907]
FPL in Effect x For-Profit	0.00731	
	[-0.135,0.150]	
FPL in Effect x High Pct Uninsured		-0.0391
		[-0.110,0.0317]
Observations	399444	3143772

*Note:* Column (1) uses data from the California SID to estimate Equation 2. Column (2) uses data from the NIS and estimates Equation 1. Confidence intervals are reported in brackets. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . All models include hospital, year, and season fixed effects, as well as patient demographic controls, and risk adjusters. Mean percent of uninsured patients per hospital is 4.9% with a standard deviation of 5.9%.

<sup>25</sup>We found little difference in hospital response to FPLs when splitting the sample along other characteristics such as income of patients and cost-to-charge ratio.

## 4.2 Where do Hospitals Reduce Care?

FPLs alter the care that hospitals are willing to provide uninsured patients, but presumably, providers that value the health of their patients will target care reductions where they will be least harmful. Such a phenomenon has been illustrated in prior literature. For example, Clemens and Gottlieb (2014) find that price shocks affect the provision of elective care considerably more than less discretionary services. In this section we present results consistent with that ethic. Namely, hospitals focus care reductions on less severe patients and comparatively minor procedures.

We first compare patients with similar general diagnoses (CCS category) but different severity levels within each diagnosis (DRG weight). For example, the CCS for heart attacks includes DRGs for "heart attack with complications" and for "heart attack without complications". Traditional DRGs were designed for the Medicare population, and thus do not include as much granularity for some conditions, such as those related to maternity. For this reason, we also report results controlling instead for All Payer Refined (APR) DRGs, which are designed for an "all payer" population, and thus include more severity levels within a CCS for a wider variety of conditions.

The results are reported in Table 13. The interactions between the treatment dummy and weight capture the differential change in length of stay under FPLs by patient severity. For reference, the average DRG weight is 0.93 with a standard deviation of 1.0, while the average APR-DRG weight is 0.73 with a standard deviation of 1.0. The estimates suggest that FPLs induce hospitals to cut back care more for less severe patients. Interestingly, the estimated interaction terms in models that control for CCS (as presented here) are very similar to those from models that do not. This suggests that hospitals focus their responses to FPLs on the less severe versions of each type of patient they treat, as opposed to implementing a broad reduction in care for the less severe CCS categories.



Table 6: The Relationship Between FPLs and Length of Stay by Patient Severity

	Length of Stay	Length of Stay
FPL in Effect	-0.334**	-0.256***
	[-0.511,-0.138]	[-0.357,-0.133]
FPL in Effect x APR DRG Weight		0.144*
		[0.0137,0.270]
FPL in Effect x DRG Weight	0.171	
	[-0.0107,0.352]	
Observations	3132371	3135532

*Note:* Data are from the Nationwide Inpatient Sample and estimates are based on Equation 1. Standard errors are clustered at the state level in column. Confidence intervals are reported in brackets. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . All models include hospital, year, and season fixed effects, as well as patient demographic controls, and risk adjusters. See the footnote of Table 4 for a full list of controls. Average DRG weight: 0.93, average APR-DRG weight: 0.73, standard deviation of DRG: 1.0, standard deviation of APR-DRG: 1.0.

In addition to shortening lengths of stay, FPLs may induce hospitals to provide fewer services during a stay. In this section we investigate whether FPLs affect the number, or types, of procedures provided to the uninsured. The NIS categorizes procedures as either diagnostic or therapeutic, and either major (in the operating room) or minor (outside the OR). This scheme provides a clear way to broadly segment procedures by invasiveness and resource use.

Studying procedures using the NIS is problematic due to data reporting inconsistencies,<sup>26</sup> but California reports this information consistently in

<sup>26</sup>States restrict how many procedures the NIS can report for a patient. This upper limit varies across states (from 6 to 30 at baseline), and changes markedly over the data window (conditional on changing the limit, the typical state increases it by nearly 20 procedures). Changing the maximum number of procedures is particularly problematic because it appears to impact how procedures well below the cap are reported in at least some states.

their State Inpatient Database. Focusing on California prevents us from using uninsured patients in different states as controls, so instead we compare the uninsured in California to the most similar insured group in the state: Medicaid patients. Because the number of procedures performed is discrete, we employ a Poisson regression model.

The results in Table 7 indicate that care reductions are concentrated in minor therapeutic procedures. Further, in models shown in Appendix H that are similar to those in Table 13 and measure differential treatment effects by severity, we find that the positive relationship between number of procedures performed and DRG Weight becomes stronger after FPLs, suggesting that hospitals are more actively targeting resources to the sicker patients. Consistent with our expectations, this evidence shows that hospitals reduce care where it will likely have the least negative effects.<sup>27</sup>

---

<sup>27</sup>Another potential underpinning for this result comes from Clemens et al. (2015) who note that the fee-for-service schedules they study often reimburse based on average cost, leaving relatively high margins for capital-intensive services. Moreover, diagnostic services like imaging tend to be more capital intensive. As such, price restrictions imposed by FPLs may disproportionately shift therapeutic services to generating net negative revenues, while maintaining positive ones for more capital-intensive diagnostic ones.

Table 7: The Relationship Between FPLs and Types of Procedures Delivered

	Minor		Major	
	Diagnostic	Therapeutic	Diagnostic	Therapeutic
2 yrs prior	0.026 [-0.039,0.091]	0.007 [-0.020,0.033]	0.056 [-0.031,0.143]	-0.015 [-0.041,0.011]
Enact yr	0.036 [-0.019,0.092]	-0.029** [-0.050,-0.008]	0.045 [-0.029,0.119]	-0.002 [-0.0312,0.027]
1 yr post	0.037 [-0.038,0.112]	-0.054*** [-0.082,-0.026]	0.040 [-0.048,0.128]	-0.022 [-0.052,0.008]
2 yrs post	0.028 [-0.066,0.121]	-0.079*** [-0.117,-0.042]	0.066 [-0.019,0.151]	-0.027 [-0.059,0.006]
Obs	5411088	5428832	5386986	5390576

*Note:* Data are from the California State Inpatient Database and estimates are based on Equation 2. Standard errors are clustered at the hospital level. Confidence intervals are reported in brackets. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . All models include hospital, year, and season fixed effects, as well as patient demographic controls, and risk adjusters. See the footnote of Table 4 for a full list of controls. Pre-treatment mean number of procedures per patient: minor diagnostic: 0.38; minor therapeutic: 0.65; major diagnostic: 0.015; major therapeutic: 0.35.

Finally, we would expect hospitals to reduce care where they have more clinical discretion or flexibility to do so. One way to proxy for this discretion is though within-diagnosis variation in length of stay. Diagnoses with high variation in length of stay likely represent those with more variation in treatment patterns, some of which generate considerably shorter stays. Those with low variation likely represent diagnoses with less latitude to alter treatment paths.

Using data from all patients for 2003 and 2004 (before any FPL was enacted), we calculate the coefficient of variation for each diagnosis. Diagnosis can differ in this measure because of actual treatment flexibility, or simply because a single diagnosis code may capture a greater range of conditions

than another. For this reason we use very granular diagnosis information - each patient's primary ICD code. Using the more detailed diagnosis code gives a better measure of true variation in LOS for similar patients.

We keep every diagnosis that has at least 100 observations over those two years. Omitting these 1,690 rare diagnoses leaves us with 7,842 diagnoses covering nearly 90 percent of our full sample of uninsured patients. Diagnoses with below median coefficients of variation of LOS are considered "low discretion admissions" and those above median, "high discretion admissions."

Below we illustrate the effect of FPLs on length of stay for high and low discretion diagnoses. Estimated treatment effects are considerably larger among the high discretion portion of admissions. Pre-treatment average length of stay is slightly different between the two groups: 4.6 days for high discretion and 3.7 for low discretion. By two years post-enactment LOS has fallen by around 0.45 days, or 9.8 percent of baseline for the high discretion group. The point estimates for the low discretion group never exceeds 0.175 days, or 4.7 percent of baseline.

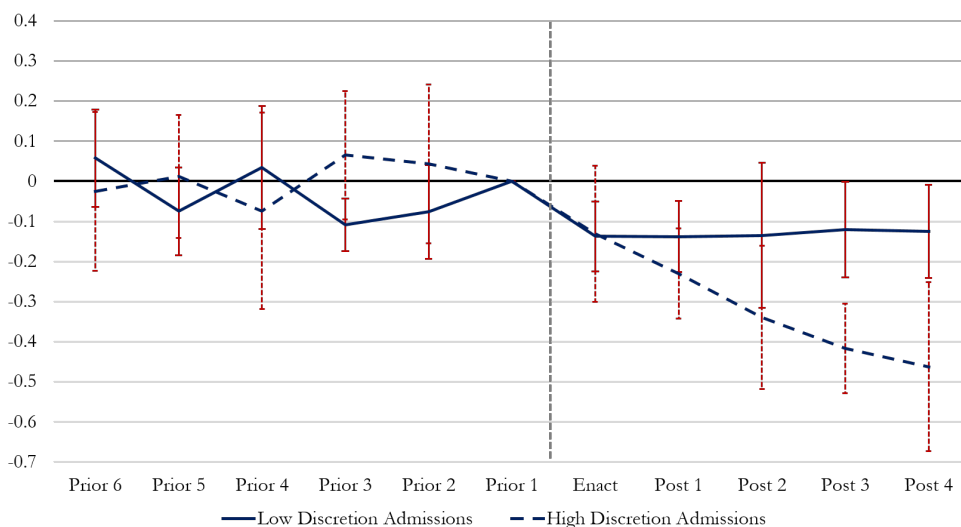
While hospitals clearly respond to the financial incentives embedded in FPLs, the evidence presented in this section suggests they do so in ways to minimize the effect on quality of care.

## 5 Results for Quality of Care

### 5.1 Short-Term Quality of Care

We have established that hospitals reduce care for uninsured patients after an FPL goes into effect, and that they do so by focusing on what we would expect to be relatively low value care. Still, these changes may or may not affect quality of care and subsequent health outcomes. In this section we show there is little evidence that reductions in care are accompanied by observable

Figure 8: Comparing Changes in Length of Stay for Diagnoses With High and Low Clinical Discretion



*Note:* This figure illustrates the impact of fair pricing laws on lengths of stay for diagnoses with high and low discretion for length of stay. Data are from the NIS and are based on estimating Equation 1 for each group. We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. The regressions includes our full set of fixed effects, patient demographics, and risk-adjusters. See the note on Table 4 for a full list of controls. Pre-treatment length of stay: High Discretion: 4.6, Low discretion: 3.7.

decreases in short-term quality of care as measured by the Inpatient Quality Indicators (QI).

The QIs were first developed for AHRQ by researchers at Stanford, UC-San Francisco, and UC-Davis in 2002 in an effort to capture quality of care using inpatient records. Since then, they have become a standard in quality assessment, endorsed by the National Quality Forum, and frequently used in research.<sup>28</sup> The QIs we study are organized into three categories:

<sup>28</sup>For a list of publications using the AHRQ QIs see

- Mortality from selected conditions and procedures
- Use of procedures believed to reduce mortality
- Incidence of potentially preventable in-hospital complications

Since we are interested in overall quality, we create one aggregate measure for each group. For example, the QI software package separately calculates mortality rates from each of a selected set procedures and conditions. We combine these into one mortality rate from any of the procedures and conditions.

Our quality analysis employs the same empirical approach presented in Equation 1, but with each of the QIs used as our dependent variable, and risk-adjustment variables calculated by the QI software (described below) as additional controls. As with most of the prior analysis, we focus on comparing uninsured patients in states with FPLs to uninsured patients in states without. We first briefly describe each metric, and then present the results together.<sup>29</sup>

### **In-hospital mortality from selected conditions and procedures**

AHRQ selected 13 conditions and procedures where evidence indicates that mortality rates vary significantly among hospitals, and that this variation is driven by the care delivered by those hospitals. Appendix J contains a full list, but examples include acute myocardial infarction, hip fracture, pneumonia, and hip replacement. The software identifies the appropriate patients in our data, records whether or not they died, and calculates an expected probability of death for each based upon their other diagnoses and demographic information. We include this expected probability of death as a control variable in our model. To take a broader look at mortality, we also

---

<http://www.qualityindicators.ahrq.gov/Resources/Publications.aspx>

<sup>29</sup>For brevity, we include only graphical event study regression results. Appendix I contains the associated diff-in-diff results.

estimate our model on the full sample of uninsured patients.

### **Use of procedures believed to reduce mortality**

AHRQ has identified six “intensive, high-technology, or highly complex procedures for which evidence suggests that institutions performing more of these procedures may have better outcomes.” For simplicity, we will refer to these as “beneficial” procedures. Appendix J includes the full list of these procedures, but an example is coronary artery bypass graft (CABG). Like before, the use of these procedures varies significantly among hospitals. In practice, we estimate our model using a dummy for admissions where these procedures are performed as the dependent variable.

Although we can estimate this model on the entire population, we prefer to do so on a subset of patients who are actually candidates for these procedures because using the entire population may obscure meaningful changes within the more relevant subgroup. AHRQ does not identify such a population, but the data show that these procedures are heavily concentrated among patients within a few CCS diagnosis categories (mostly related to AMI or other forms of heart disease). Specifically, 95% of these procedures are performed on patients within just 3% of CCS categories (5% of patients). Conditional on being in this group, the usage rate of the procedures is roughly 50%.

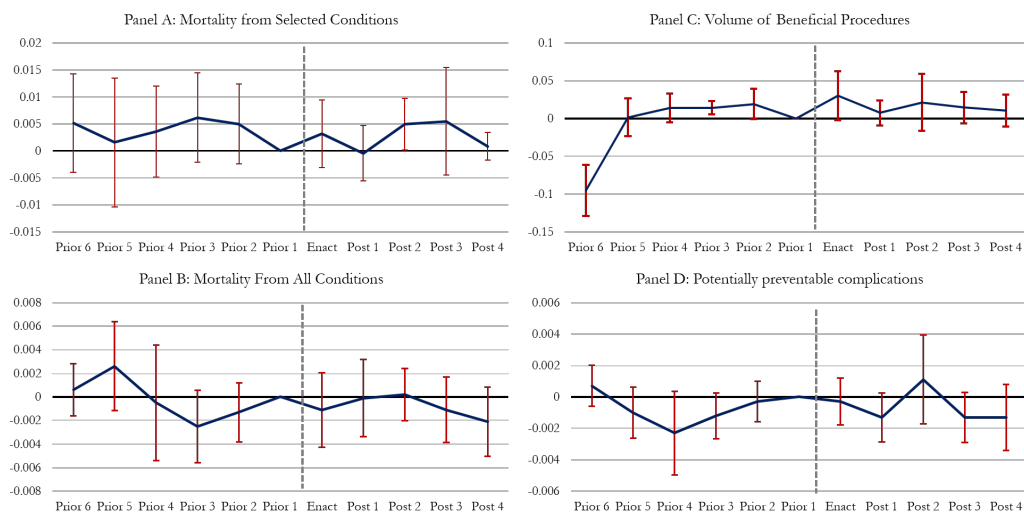
### **Incidence of potentially preventable in-hospital complications**

AHRQ has identified thirteen in-hospital complications that may be preventable with better quality of care. Again, Appendix J includes the full list, but these are issues like postoperative hemorrhage, or accidental puncture or laceration. Individually, each event is quite rare: averaging 0.16% of the at-risk population (as defined by the QI software). When viewed together,

the probability that an individual who is at risk for at least one of them will be inflicted with at least one of them is 0.54%. We estimate our model with the frequency of any of these complications as the outcome variable. Much like the mortality metric, the QI software calculates an expected probability of each complication. We include this probability as a control in our model, but the results are similar with or without this variable.

### Results for short-term quality metrics

Figure 9: Measures of Quality of Inpatient Care



*Note:* These graphs use data from the NIS. Estimates are based on Equation 1 where the selected QI metrics as the outcome variables. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. Standard errors are clustered at the state level. Pre-treatment means: Mortality for selected conditions: 4.1%; Mortality for all conditions: 1.3% Beneficial procedures: 50%, Complications: 0.54%.

Panel A of Figure 9 shows the effect of FPLs on in-hospital mortality for selected procedures. The treatment coefficients are somewhat noisy, but do not appear to show a systematic change following FPLs. Panel B shows



the effect on mortality for the full uninsured population. For the full population, confidence intervals typically falling between 0.004 and -0.004 in the post period. In-hospital mortality is less common for the overall population (1.2% compared to 4.1% for the selected conditions), so the confidence intervals on our yearly treatment effects rule out changes in mortality across all admissions of more than 4-5 percent.

The NIS only captures in-hospital mortality, so to further investigate the possibility of deaths occurring outside of the hospital we turn to mortality data published by the CDC. Specifically, we study people ages 25-64, and deaths that were not due to an acute trauma (this excludes accidents, homicides, and suicides). In addition, we focus on deaths that occurred outside of hospitals that resulted from several of the most common mortality QI conditions and procedures. We study these populations both for the US as a whole, and restricted to counties with more than 25% uninsured.<sup>30</sup> Appendix K shows the results of this analysis. We do not see evidence that FPLs are followed by a spike in death rates outside of the hospital from these conditions.

Panel C of Figure 9 shows the effect of FPLs on the use of high-tech and costly “beneficial” procedures. Absent an unusual year six years before enactment (only identified by two treated states), the trend is generally stable surrounding enactment. The lower end of the confidence interval in the difference-in-differences estimate represents a decline of only 2.5 percent. Finally, panel D of Figure 9 show the impact of FPLs on the incidence of potentially preventable complications. Coefficients are generally small, however, given the rarity with which these complications occur this metric is also less precisely estimated, and the diff-in-diff results can only rule out increases of more than roughly 15 percent. While some estimates have limited precision, taken together, our data fail to reveal clear signs of deterioration

---

<sup>30</sup>The Census Bureau publishes estimates of insurance rates at the county level at <https://www.census.gov/did/www/sahie/>. Twenty-five percent represents approximately the 75th percentile of uninsurance for 25-64 year-olds at the county level in 2012.

of short-term care quality after enactment of a fair pricing law.

## 5.2 Longer-Term Quality

While the short-term metrics suggest little change in care quality following an FPL, it is also possible that changes may only become apparent over a longer time horizon. One way of capturing more subtle differences in care quality, such as potentially inappropriate discharges, is the 30-day, all-cause readmission rate. It is particularly compelling for our study because it could reflect complications or the need for additional care that result from the shortened stays of uninsured patients after the enactment of a FPL.

While some patients will experience health events that require readmission regardless of the care quality during the original stay, hospitals providing higher quality care should have more success in keeping their patients out of the hospital. To this point, research has documented wide variation in readmission rates across hospitals (e.g. Jencks et al., 2009), and has established channels through which these rates depend on care quality (e.g. Ahmad et al., 2013). In light of this CMS has recently deployed financial incentives encouraging hospitals to lower readmission rates.

Our main data source, the NIS, does not track patients over time. Fortunately the State Inpatient Database (SID) for our largest treatment state, California, does allow us to determine whether different hospital stays represent the same patient. The California SID covers the universe of inpatient stays in California each year. Other than the additional patient linkage variables, the variables contained in NIS and California SID are largely identical.

Our outcome of interest is the 30-day all-cause readmission. Specifically, a readmission is any stay that occurs within 30 days of a prior discharge for that patient. We study patients with all clinical diagnoses, and include cases

where the patient is readmitted to a different hospital.<sup>31</sup>

We study readmissions in the California SID by comparing uninsured patients to Medicaid patients over time as outlined in Equation 2. Although the patient populations may differ, those with Medicaid are likely more similar to the uninsured than are any other insured group.<sup>32</sup>

Figure 10 reports the results of this analysis. The small and insignificant treatment coefficients in both the pre and post time periods provide evidence that the California FPL did not increase the rates of readmission for uninsured patients relative to Medicaid patients. The upper end of the confidence intervals in the post period are between .002 and .006, meaning we can rule out increases in readmission rates of more than 3 to 6.5 percent in those years (from a base of 8.7 percentage points). The results are similar if we consider 60 or 90 day readmission rates.

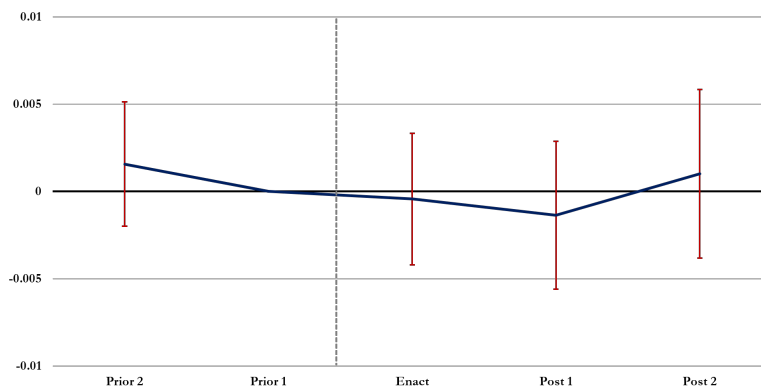
In contrast to the results focusing on quantity of care, our study reveals little evidence of systematic changes to quality of care for the uninsured following a FPL. In-hospital quality measures are generally stable surrounding enactment, and longer term outcomes, as measured by readmission rates and out of hospital mortality, follow similar paths. Although precision varies across metrics, taken together, the evidence suggests limited changes in quality of care. While we cannot rule out more subtle differences in quality, this suggests that care forgone as a result of FPLs was contributing relatively little to patient health.

---

<sup>31</sup>All cases where a patient died during an initial stay were omitted from this analysis (since readmission is not possible).

<sup>32</sup>We also obtained data containing readmission information from a control state (WA). However, the patient linkage variables are reported inconsistency in successive years, making it difficult to use for this study. Still, we find similar results when we comparing CA uninsured to WA uninsured, or performing a triple difference using the uninsured and Medicaid populations in both states.

Figure 10: The effect of fair pricing laws on all-cause 30-day readmission rates for uninsured patients in California



*Note:* Data are from the California SID and estimates are based on Equation 2. We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. Standard errors are clustered at the hospital level. The vertical lines show the 95% confidence intervals. Pre-treatment readmission rate: 8.7%

## 6 Conclusion

In this paper, we utilize fair pricing laws to investigate how hospitals alter care in response to financial incentives. Specifically, we establish that FPLs impose substantial payment reductions for uninsured patients (by approximately 25 to 30 percent), and then show that hospitals specifically cut back on care to uninsured patients in response. They shorten inpatient stays by seven to eight percent, reduce intensity of care, treat certain marginal patients in outpatient rather than inpatient settings, and more frequently transfer patients to other care facilities. Despite the reduction in care, we do not see evidence of deterioration in the quality of inpatient care received using a number of quality measures, and can generally rule out more than modest

declines. Uninsured patients do not die in the hospital at significantly higher rates, they do not experience higher rates of medical complications, they do not receive fewer high-cost, high-tech medical procedures, and they are not readmitted with higher frequency under a FPL. Hospitals likely maintain quality while reducing quantity by focusing where care was least beneficial. For example, they concentrate care reductions on less severe patients and comparatively minor procedures.

The implications for patient welfare are not immediately clear. In a typical market, any price ceiling that prevents a transaction from occurring would be welfare reducing. However, because patients ultimately aim to purchase health rather than healthcare, and it can be difficult to determine how effectively the latter produces the former, the lessons from the typical consumer market may not apply. Given that the price restrictions introduced by FPLs are not associated with evidence of worsening quality, and they likely significantly reduce financial strain, our results are broadly consistent with the idea that these laws improve consumer welfare and push the market closer towards an efficient outcome.

Failing to observe a trade-off between the amount of care and health outcomes may be surprising, but theoretical work has long established that efficiency gains in healthcare may be possible (e.g. Arrow, 1963). To this point, our results align with the aforementioned empirical literature on the Medicare PPS and fee-changes, which generally finds that providers alter care in response to financial incentives in ways that have limited impact on patient outcomes. An important goal of research is to disentangle where, and to what extent, these kinds of efficiency gains are possible moving forward.

## References

- Faraz S. Ahmad, Joshua P. Metlay, Frances K. Barg, Rebecca R. Henderson, and Rachel M. Werner. Identifying hospital organizational strategies to reduce readmissions. *American Journal of Medical Quality*, 28(4):278–285, 2013.
- Gerard Anderson. “From Soak the Rich to Soak the Poor: Recent Trends in Hospital Pricing”. *Health Affairs*, 26(3):780–9, 2007.
- David Card, Carlos Dobkin, and Nicole Maestas. “The Impact of Nearly Universal Insurance Coverage on Health Care Utilization: Evidence from Medicare”. *American Economic Review*, 98(5):2242–58, 2008.
- David Card, Carlos Dobkin, and Nicole Maestas. “Does Medicare Save Lives?”. *The Quarterly Journal of Economics*, 124(2):597–636, 2009.
- Grace M. Carter, Joseph P. Newhouse, and Daniel A. Relles. “How Much Change in The Case Mix Index is DRG Creep?”. *Journal of Health Economics*, 9(4):411–428, 1990.
- Amitabh Chandra, David Cutler, and Zirui Song. “Chapter Six - Who Ordered That? The Economics of Treatment Choices in Medical Care”. In Thomas G. McGuire Mark V. Pauly and Pedro P. Barros, editors, *Handbook of Health Economics*, volume 2 of *Handbook of Health Economics*, pages 397 – 432. Elsevier, 2011.
- Jeffrey Clemens and Joshua D Gottlieb. “Do Physicians’ Financial Incentives Affect Medical Treatment and Patient Health?”. *The American Economic Review*, 104(4):1320–1349, 2014.
- Jeffrey Clemens, Joshua D Gottlieb, and Timea Laura Molnar. The anatomy of physician payments: Contracting subject to complexity. Technical report, National Bureau of Economic Research, 2015.
- Timothy G Conley and Christopher R Taber. “Inference With Difference in Differences With a Small Number of Policy Changes”. *The Review of Economics and Statistics*, 93(1):113–125, 2011.
- Teresa A Coughlin. “Uncompensated Care for the Uninsured in 2013: A Detailed Examination”. 2014. The Urban Institute.
- Robert Coulam and Gary Gaumer. “Medicare’s Prospective Payment System: A Critical Appraisal”. *Health Care Financing Review*, 12, 1991.

- Leemore S Dafny. “How Do Hospitals Respond to Price Changes?”. *The American Economic Review*, 95(5):1525–1547, 2005.
- Joseph J Doyle. Health insurance, treatment and outcomes: using auto accidents as health shocks. *Review of Economics and Statistics*, 87(2): 256–270, 2005.
- David Dranove and Michael Millenson. “Medical Bankruptcy: Myth Versus Fact”. *Health Affairs*, 25(2):w74–w83, 2006.
- Randall P. Ellis and Thomas G. McGuire. “Supply-Side and Demand-Side Cost Sharing in Health Care”. *Journal of Economic Perspectives*, 7(4): 135–151, 1993.
- Amy Finkelstein, Sarah Taubman, Bill Wright, Mira Bernstein, Jonathan Gruber, Joseph P Newhouse, Heidi Allen, Katherine Baicker, et al. “The Oregon Health Insurance Experiment: Evidence from the First Year”. *The Quarterly Journal of Economics*, 127(3):1057–1106, 2012.
- Sherry Glied and Joshua Graff Zivin. How do doctors behave when some (but not all) of their patients are in managed care? *Journal of Health Economics*, 21(2):337–353, 2002.
- Dominic Hodgkin and Thomas G. McGuire. “Payment Levels and Hospital Response to Prospective Payment”. *Journal of Health Economics*, 13(1):1 – 29, 1994.
- Renee Y. Hsia, Donna MacIsaac, and Laurence C. Baker. “Decreasing Reimbursements for Outpatient Emergency Department Visits Across Payer Groups From 1996 to 2004”. *Annals of Emergency Medicine*, 51(3):265–274, 2008.
- Louis Jacobson, Robert LaLonde, and Daniel Sullivan. “Earnings Losses of Displaced Workers”. *The American Economic Review*, 83(4):685–709, 1993.
- Mireille Jacobson, Craig C Earle, Mary Price, and Joseph P Newhouse. “How Medicare’s Payment Cuts for Cancer Chemotherapy Drugs Changed Patterns of Treatment”. *Health Affairs*, 29(7):1391–1377, 2010.
- Stephen F. Jencks, Mark V. Williams, and Eric A. Coleman. Rehospitalizations among patients in the medicare fee-for-service program. *New England Journal of Medicine*, 360(14):1418–1428, 2009. doi: 10.1056/NEJMsa0803563. PMID: 19339721.

- Helen Levy and David Meltzer. “The Impact of Health Insurance on Health”. *Annual Review of Public Health*, 29:399–409, 2008.
- Neale Mahoney. Bankruptcy as implicit health insurance. *The American Economic Review*, 105(2):710–746, 2015.
- Willard G Manning, Joseph P Newhouse, Naihua Duan, Emmett B Keeler, and Arleen Leibowitz. “Health Insurance and the Demand for Medical Care: Evidence From a Randomized Experiment”. *The American Economic Review*, pages 251–277, 1987.
- Glenn Melnick and Katya Fonkych. “Hospital Pricing and the Uninsured: Do the Uninsured Pay Higher Prices?”. *Health Affairs*, 27:116–22, 2008.
- Glenn Melnick and Katya Fonkych. “Fair Pricing Law Prompts Most California Hospitals To Adopt Policies To Protect Uninsured Patients From High Charges”. *Health Affairs*, 32(6):1101–1108, 2013.
- Nguyen X. Nguyen and Frederick W. Derrick. “Physician Behavioral Response to a Medicare Price Reduction”. *Health Services Research*, 32(3): 283, 1997.
- U. Reinhardt. “The pricing of U.S. hospital services: chaos behind a veil of secrecy”. *Health Affairs*, 25(1):57–69, 2006.
- Thomas H. Rice. “The Impact of Changing Medicare Reimbursement Rates on Physician-Induced Demand”. *Medical care*, 21(8):803–815, 1983.
- Elaine Silverman and Jonathan Skinner. Medicare upcoding and hospital ownership. *Journal of health economics*, 23(2):369–389, 2004.
- Frank A Sloan. Not-for-profit ownership and hospital behavior. *Handbook of health economics*, 1:1141–1174, 2000.
- Mark Smith, Robert Saunders, Leigh Stuckhardt, J Michael McGinnis, et al. “*Best Care at Lower Cost: The Path to Continuously Learning Health Care in America*”. National Academies Press, 2013.
- Christopher Tompkins, Stuart H. Altman, and Efrat Eilat. “The Precarious Pricing System For Hospital Services”. *Health Affairs*, 25(1):45–56, 2006.
- Winnie C Yip. “Physician Response to Medicare Fee Reductions: Changes in the Volume of Coronary Artery Bypass Graft (CABG) Surgeries in the Medicare and Private Sectors”. *Journal of Health Economics*, 17(6):675–699, 1998.



## A Differences in FPL Provisions

Although the FPLs we study are similar, there are several generalizable differences. The first is how the laws cap prices. Capping prices at (100-115%) the amount paid by public insurers, as opposed to private insurers (or cost), is significant not only because reimbursement from public payers is typically lower,<sup>33</sup> but also because it is explicitly based upon a patient’s diagnosis rather than the medical services actually delivered. In contrast, most private insurers use a variety of payment mechanisms, including a non-trivial amount of fee-for-service reimbursement. Second, in addition to the limit on charges for medium income uninsured patients, several FPLs mandate free care for low income patients. Table 8 summarizes these FPL provisions by state.

Table 8: Fair pricing laws by state

State	Year Enacted	% of Fed. Level Poverty Covered	% of Unin. Covered	Maximum Collection Amount	Free Care below X% of Poverty
MN	2005	~500%	86%	Largest private payer	NA
NY	2007	300%	76%	Highest vol. payer	100%
CA	2007	350%	81%	Highest price public payer	NA
RI	2007	300%	77%	Private payers	200%
NJ	2009	500%	87%	115% of Medicare	200%
IL	2009	~600%	~95%	135% of cost	200%

*Note:* New Jersey’s free care provision was actually part of a law passed in the early 1990s so our study does not capture its effect. New York also provides discounted care on a sliding scale between 100% and 250% of the poverty line.

<sup>33</sup>For instance, Melnick and Fonkych (2008) show that in 2000-2005, private insurers in California paid around 40% of charges, where public insurers paid around 20%.

There is reason to believe that these provisions may alter how hospitals respond to FPLs. Tying a FPL to the PPS used by public payers means the payment cap is determined by the diagnosis, and additional treatment will not generate marginal revenue. This suggests PPS-based FPLs would produce stronger reductions in care. Similarly, mandating free care to low income patients will also give a hospital a stronger reason to reduce care.

Our data allows some, albeit limited opportunity to study these differences. Minnesota's FPL contains neither provision, while California and New Jersey are based upon the PPS, and New York, Illinois, and Rhode Island include a significant amount of free care to the poorest uninsured patients. Thus, Minnesota can be used as a reference against which to measure the effects of the two provisions. Unfortunately, all the variation in the laws occurs across rather than within states, so this analysis may be confounded by other unobservable state-level factors. In addition, the fact that states either have PPS-based FPLs or provide free care means we have limited independent variation upon which to identify the different effects (recall, New Jersey's free care provision is from a pre-existing law).

To investigate, we estimate a difference-in-differences model with dummy variables for any type of FPL, PPS-based FPL, and FPL with free care. The basic FPL dummy measures the effect of a generic FPL common to all states, while the other two dummies measure the additional effects of the two law provisions. Table 9 reports the results of this model.

As expected, we observe reductions in care with all types of FPLs. However, the additional provisions do not produce stronger responses. Because the effects of these provisions are identified relative to only one fairly small state, Minnesota, we believe this analysis reveals more about their relative rather than absolute effects.<sup>34</sup> Based upon this limited evidence, mandating

---

<sup>34</sup>Minnesota's FPL is also unique because it is the result of a voluntary agreement that came about after a lengthy negotiation and threat of law suit by the state Attorney General.

free care appears to produce a stronger incentive to reduce hospital stays than does linking payment to the PPS. Although both provisions essentially reduce the marginal revenue of treatment to zero, the free care may produce a stronger effects because it is clear the patient represents a loss to the hospital, whereas the patient may still be profitable in aggregate under a PPS-based FPL.

Table 9: Comparing reductions in lengths of stay by FPL provision

	Outcome Variable: Length of Stay	
Risk Adjustment:	DRG Weight	CCS Category
FPL	-0.367*** [-0.443,-0.292]	-0.269*** [-0.329,-0.209]
PPS-Based FPL	0.250*** [0.181,0.319]	0.138*** [0.0981,0.177]
Free-Care FPL	0.138* [0.0188,0.257]	0.0148 [-0.114,0.143]
Observations	3134363	3134363

*Note:* Data are from the NIS. Estimates are based on Equation 1. Standard errors are clustered at the state level. CIs are reported in brackets. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . All models include hospital, year, and season fixed effects. Patient demographics included in both models.

## B Legislative Path to Fair Pricing Laws

Another way to assess whether FPLs impose real constraints is to study how hospitals have received them. We suspect they would be hesitant to invest political and financial capital fighting a law that is both popular among the public, and would have minimal impact on their operations. A brief look into the legislative process in California suggests that hospitals were concerned with its potential impact (similar stories apply to the passage of fair pricing regulations in New York and Illinois). In the early 2000s, a series of newspaper articles brought attention to examples of uninsured patients who were charged much more for hospital care than were other payers. Motivated by this perceived inequity, California's legislature passed a fair pricing law in 2003 which was very similar to what was ultimately enacted several years later. In response to mounting public and legislative pressure, both the American Hospital Association and California Hospital Association published guidelines for their member hospitals about financial assistance policies for uninsured patients. These guidelines advocated for the development and publication of financial assistance policies, but include few specifics on what these policies should include. They also contained no enforcement or accountability mechanisms. In early 2004, Governor Schwarzeneger vetoed the fair pricing bill, arguing that the voluntary guidelines should be given a chance to work. By late 2006, health advocates and legislators effectively argued that the voluntary guidelines were not appropriately addressing the issue, and they enacted what is California's current fair pricing law. Though ultimately unsuccessful, these attempts to avoid legislation suggest that hospitals believe these laws do introduce meaningful constraints.

## C Percentage of List Price Paid by Medicare and Medicaid Patients (MEPS)

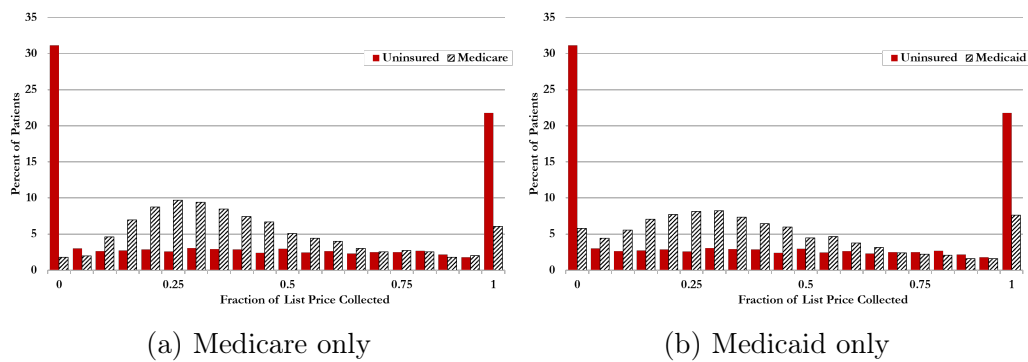
In Section 2 we present the distributions of percentage of list price paid for publicly insured and uninsured patients. We do so because the price caps imposed by FPLs are based upon a mix of Medicare and Medicaid payments, rather than because we believe the publicly insured patients are comparable to uninsured patients. In this section we show that the broad payment patterns hold whether we focus only on the Medicare or Medicaid distributions.

Table 10: Summarizing hospital charges and percentage of list price paid by payer-type

Insurance	Count	Mean Hospital Charges	Mean Percentage of List Price Paid
Public Insurance	17,276	\$13,046	38%
Medicare	9,595	\$17,027	39%
Medicaid	7,460	\$7,859	34%
Uninsured	3,892	\$5,035	37%

*Note:* The data are from the Medical Expenditure Panel Survey from 2000-2004.

Figure 11: Distribution of percentage of list price paid for Medicare, Medicaid, and uninsured patients



*Note:* Panels (a) and (b) compare collection rates from the uninsured to patients with Medicare and Medicaid, respectively. Data are taken from the Medical Expenditure Panel Survey from 2000-2004.

## D Fair Pricing Laws and Strategic Diagnosing

We have shown that hospitals restrict the quantity of care under fair pricing laws, but it may also be possible to circumvent price controls. Recall that most of the states we study enacted FPLs based on public payers who use prospective payment systems - where payments are almost entirely determined by a patient's diagnosis, rather than amount of care received. In these states the maximum collection after the imposition of a FPL is a direct function of a patient's diagnosis. So hospitals could artificially inflate the diagnosis to increase the maximum amount they can collect (this behavior is often termed "DRG creep").

The relevant outcome variable for studying upcoding is the DRG weight. As described earlier, this weight represents the expected cost of treating a patient within that DRG, and is directly related to the amount Medicare will reimburse. Panel A of Figure 12 shows that unlike in other settings where hospitals have a similar incentive, FPLs do not induce upcoding for uninsured patients.<sup>35</sup> One possible explanation for the null results is that upcoding under FPLs only increases the maximum amount a hospital can collect, while upcoding Medicare patients increases the payment with certainty.

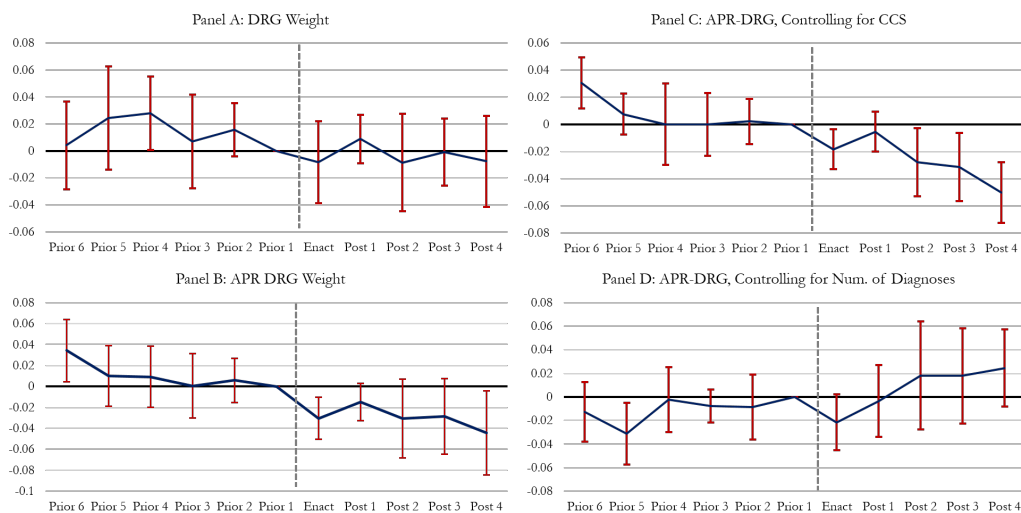
Although DRG weight often determines the FPL payment cap, all-patient refined (APR-DRG) weight is a more granular measure of severity. For our purposes, the primary distinction is that each class of diagnosis is separated into four rather than three severity levels. The two measures are determined by the same set of information (ICD codes), but given the extra granularity, it is possible to alter the APR-DRG while leaving the DRG unchanged.<sup>36</sup> Unlike the DRG, the APR-DRG assigned is unlikely to directly affect the

---

<sup>35</sup>We also see no evidence of strategic diagnosing if we use the approach used in Silverman and Skinner (2004), where upcoding is detected by an increase in the percentage of pneumonia patients assigned the most lucrative pneumonia diagnosis.

<sup>36</sup>All-patient refined (APR) DRGs were developed to better suit the non-Medicare population, and are in use by Medicaid and quality reporting in some states.

Figure 12: Strategic Diagnosing



*Note:* These graphs show yearly treatment coefficients and associated state-clustered standard errors from our event study specification using the DRG weight and APR DRG weight as outcome variables. Data are from the NIS. Estimates are based on Equation 1. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. Each regression includes hospital, year, and season fixed effects. All models also include the patient demographics and risk-adjusters. See the footnote of Table 4 for a full list of controls. Panels C and D add clinical classification category and number of diagnoses respectively. Average DRG weight: 0.93; average APR-DRG weight: 0.73.

payment received by hospitals in our sample. Instead, we study the APR-DRG because we consider it to be a more complete numerical representation of the diagnosis. Surprisingly, Panel B of Figure 12 shows that using the finer measure, patients have been diagnosed with approximately 4% less severe conditions after enactment of fair pricing laws.<sup>37</sup> Interestingly, the reduction

<sup>37</sup>Several of the yearly estimates are just outside of conventional significance level, but the difference-in-differences estimate is significant. Also, if we control for patient severity in our quantity and quality of care regressions using APR-DRG rather than CCS category or DRG we still find significant effects, but the magnitudes are slightly reduced.



in severity persists if we control for the CCS diagnosis category (Panel C), but not if we control for number of individual diagnoses recorded (Panel D).<sup>38</sup> This is consistent with our suspicion that strategic diagnosing occurs by altering the severity within a disease category (such as by omitting a complicating factor), rather than moving from one category to another.

To some extent, the reduction in diagnosis may be a natural result of shorter lengths of stay. With patients spending less time in the hospital, doctors have less time to observe and record the type of ancillary conditions that are being omitted. Alternatively, a strategic explanation for the reduction in APR-DRG weight is that hospitals feel a need to match the diagnosis to the treatment delivered. With the financial value of uninsured patients falling under fair pricing laws, and hospitals scaling back the amount of care they deliver, doctors may shade their initial diagnosis to justify the planned reduction in care. A doctor's own sense of medical ethics is one channel by which he or she could discount a potentially complicating aspect of the patient's condition, but doctors and hospitals are also subject to external reviews of the care they provide. The review that likely carries the most weight is medical malpractice, where an expert offers an opinion about whether the care delivered meets the defined practice guidelines for the patient's condition.

The potential reasons to lower the severity of the diagnosis does create some tension with the incentive to upcode because the APR-DRG and DRG are related. It is interesting to note that while making this trade-off, providers appear able target diagnosis shading (as measured by the more granular APR weight) in a way that does not lower the DRG weight, and thus avoids an adverse financial outcome for the hospital.

---

<sup>38</sup>Our data records up to sixty diagnoses made by the doctor for each patient (average is 5.5). We do not show the result here, but there is a significant reduction in the number of diagnoses after FPLs.

## E Impact of FPLs on Hospital List Prices

In this section we investigate whether FPLs have any impact on hospital list prices (or “Chargemaster” prices). As outlined in the introduction to this paper, list prices have risen substantially over time, and are the basis by which uninsured patients are initially billed. This has led some to suggest that one of the explanations for high, and increasing, list prices is that hospitals are attempting to extract higher revenues from uninsured patients.

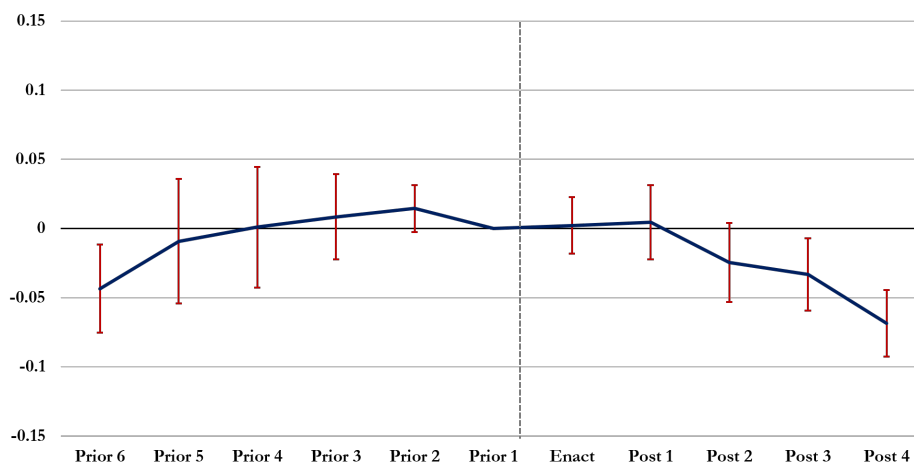
If generating revenues from uninsured patients is a motivation for increasing list prices, then it is possible that FPLs may reduce, or slow the growth of, list prices. By capping the maximum collection from uninsured patients below the list price, FPLs effectively render the list price irrelevant for uninsured patients. If this is the case, hospitals would have a diminished incentive to increase prices as aggressively.

To investigate this we run our event study specification where the log of list price markup (or ratio of list price to costs) is the outcome variable. These price-to-cost ratios are provided by AHRQ, but are originally derived from CMS Cost Reports. Since they are reported at the hospital level, we collapse our data to the hospital-year level for this exercise. As before, standard errors are clustered at the state level. Hospital and year fixed effects are included, but seasonal fixed effects are dropped since list price data are provided annually. Average pre-treatment ratio of list price to cost is 2.9.

The results are shown in Figure 13. In the years leading up to enactment of an FPL, list price markups are largely trending similarly to markups in control states. After the introduction of FPLs we do not see an immediate divergence in pricing patterns between treated and control states. In the longer run there is a slight reduction in list prices of about five percent. FPLs do not appear to have large effects on list pricing behavior, though they may slow their growth slightly in the longer term.

This muted effect may suggest that collections from uninsured patients, while a popular theory on list pricing, is not a major motivation for hospital pricing. It is also possible that since the relatively high-income uninsured are not covered by FPLs, hospitals still have reason to increase prices to generate revenue from this subgroup.

Figure 13: The Effect of FPLs on List Price Markups



*Note:* This figure illustrates the impact of fair pricing laws list prices in treatment states (specifically the ratio of list prices to costs, or the markup). Data are from the NIS and collapsed to the hospital-year level. Estimates are then based on Equation 1 (without the inclusion of any patient characteristics). We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. The vertical bars indicate 95% confidence intervals based on state clustering. Year and hospital fixed effects are included, but seasonal fixed effects are removed since price levels are reported yearly. Pre-treatment mean list price to cost ratio: 2.9.

## F Robustness Checks

### F.1 Placebo Test

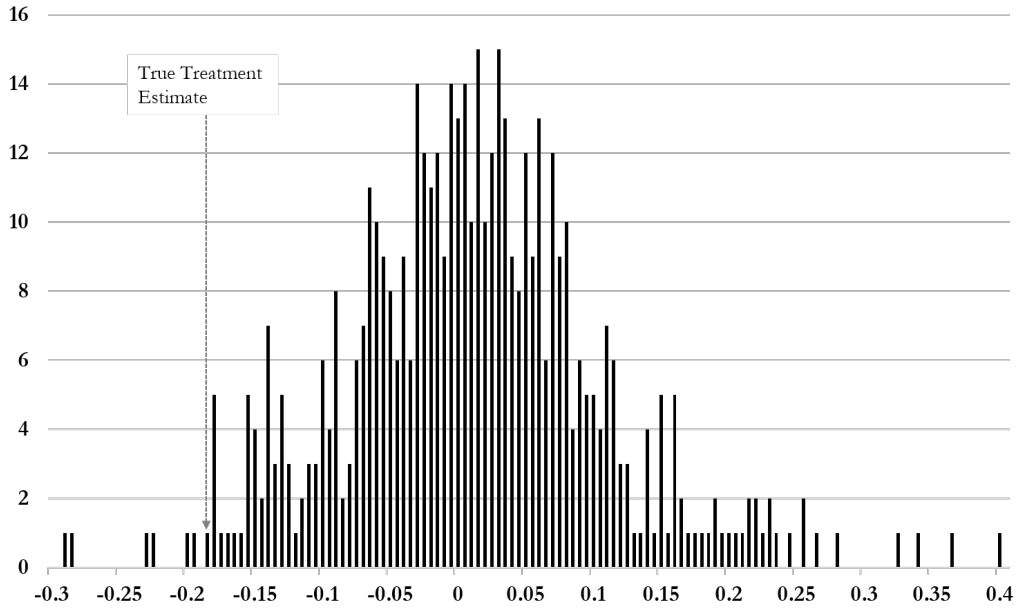
To test the robustness of our main result we run a placebo test where we systematically miss-assign treatment status. For this process we randomly select 6 states from the full set, including those actually treated, and assign them as one of our treated states. We then estimate:

$$LOS_i = \alpha + \delta \cdot \text{PlaceboFPL}_i + \beta X_i + \mu_{h(i)} + \gamma_{t(i)} + \chi_{q(i)} + \epsilon_i, \quad (3)$$

where PlaceboFPL is a binary variable equal to one for individuals in a placebo treatment state after enactment. The model includes patient demographics and diagnosis information in  $X_i$ , and hospital, year, and quarter fixed effects. Standard errors are clustered at the state level. We estimate this model for 500 combinations of treated states.

In Figure 14 we report the distribution of estimated coefficients. The actual diff-in-diff estimate of -0.187 is labelled as the "true treatment estimate." The distribution of placebo estimates is centered close to zero and our true estimate falls in the lower tail. In only 1.2 percent of cases (6 instances) do we observe more negative placebo estimates, and in all such cases the placebo treatment states include actual treatment states.

Figure 14: Effect of FPLs on Length of Stay for Uninsured Patients - Placebo Estimates



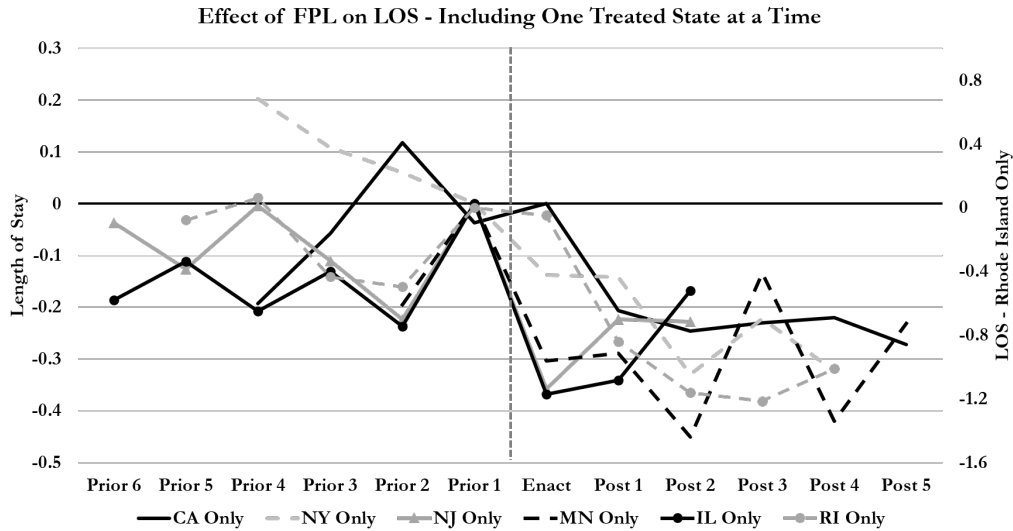
*Note:* This figure illustrates the distribution of placebo estimates for the impact of fair pricing laws on lengths of stay for uninsured patients. Data are from the NIS. Estimates are based on Equation 1. The difference-in-difference estimate from each of the 500 estimates is plotted in this histogram. The regressions includes our full set of fixed effects, patient demographics, and risk-adjusters. See the note on Table 4 for a full list of controls.

## F.2 Individual State Treatment Effects

In our main results we measure the average effect of FPLs across our six treated states. It is possible that this average response obscures considerable variation in reactions across states. For example, a large effect in a state like California may mask conflicting results in some of the others. In this section we disaggregate the general result by re-estimating our main event-study specification for each treatment state individually (comparing each to all non-treated states).

Figure 15 illustrates the effect of FPLs on length of stay for each of our treated states. For clarity we only include point estimates. Note that because of differential timing of the laws, we do not observe each state for the same number of years surrounding enactment. Given the large difference in magnitude, we have graphed the Rhode Island estimates on the secondary axis. Predictably, the individual estimates are noisier, but the observed reductions in length of stay are generally similar across treated states. Notably, our largest treatment state - California - does not have an unique or unusually large response. The overall effects reported in the main text are not driven by a single state or subset of treated states. The consistency of the results across states also helps reduce the likelihood that our effects are caused by the adoption of other concurrent state policies.

Figure 15: Changes in Length of Stay for Uninsured Patients in Individual Treatment States



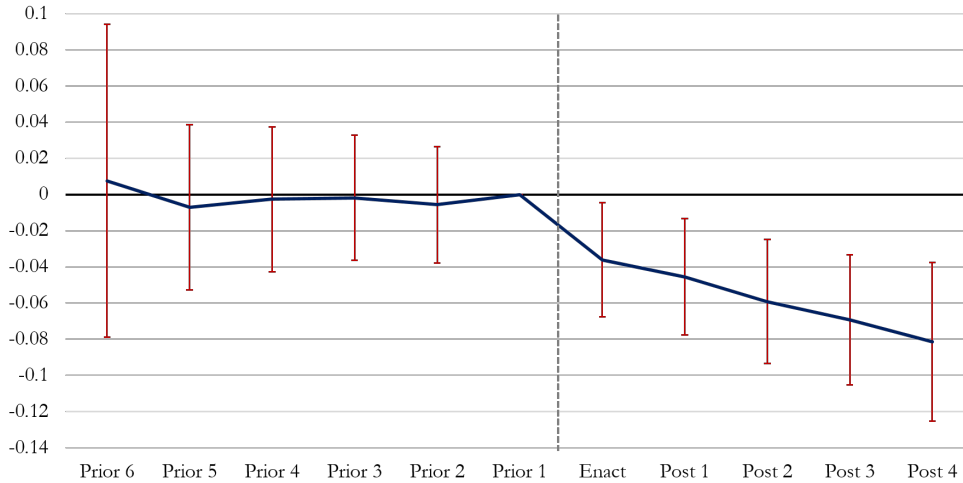
*Note:* This figure illustrates the impact of fair pricing laws on lengths of stay for uninsured patients from each treatment state separately. Data are from the NIS and estimates are based on Equation 1 with only one treated state included each time. We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. For clarity, we have omitted confidence intervals from the figure. The regressions includes our full set of fixed effects, patient demographics, and risk-adjusters. See the note on Table 4 for a full list of controls.

### F.3 Using a Count Regression Model

Given that our primary outcome variable, length of stay, is reported as integers in our data, one might consider using a count regression model as an alternative method of analysis. In this section we report results using a Poisson regression. The estimated model includes our full set of controls and risk-adjusters. As shown in Figure 16, the results are comparable to our main specification. By the end of our analysis window, fair pricing laws are

associated with a 8 percent reduction in the average length of stay.

Figure 16: The Effect of Fair Pricing Laws on Length of Stay Using a Poisson Regression Model



*Note:* This figure illustrates the impact of fair pricing laws on lengths of stay for uninsured patients using a Poisson regression model. Data are from the NIS and estimates a Poisson regression model based on Equation 1. See the note on Table 4 for a full list of controls. We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. The vertical bars indicate 95% confidence intervals based on clustering at the hospital level. Pre-treatment mean length of stay: 4.08.

## F.4 Alternative measures of care quantity

As described in the text, length of stay is our preferred measure of the quantity of care hospitals deliver to uninsured patients. Here we study several alternative measures of care quantity: hospital charges, admission decisions, and transferring patients. We include these both as robustness checks for our length of stay results, and also to investigate other margins upon which hospitals may ration care to uninsured patients. We first briefly describe each



measure, and then present the results of our event study models together.

### **Total charges**

FPLs limit the portion of the bill that hospitals can collect, but not what is actually listed on the bill. Thus, the charges reported in our data reflect the care given rather than the direct limits imposed by the laws. Total charges may provide a better measure of the intensity of care of a hospital stay as long as they bear some, albeit an inflated, relationship with costs. While arguably a more comprehensive measure of resource use, the variation in rates of charge increases among hospitals introduces a limitation since we cannot separately identify hospital-specific charge trends and the effects of FPLs.

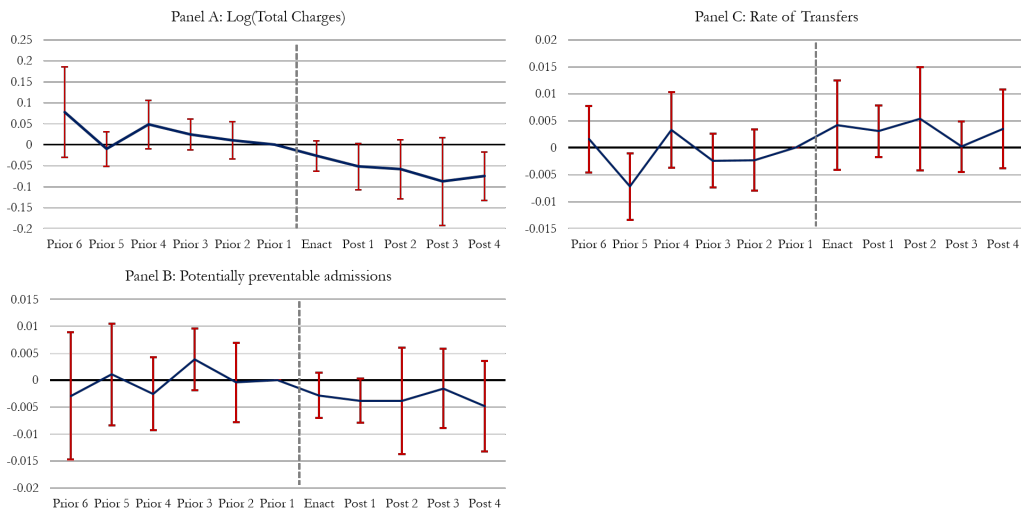
### **Admission decisions**

The QI software also calculates the rate of admissions that could potentially have been avoided. These are generally marginal admissions from conditions that could alternatively be treated in outpatient settings, or prevented with more comprehensive primary care. We study these admission rates to determine if fair pricing laws are associated with hospitals pushing more of these patients to outpatient care, which is typically lower cost. There are 13 such conditions identified by AHRQ (listed in Appendix J). Several examples are COPD/asthma, and complications from diabetes. The 13 conditions account for approximately 12% of admissions in our data.

### **Transfers**

Hospitals may attempt to reduce the burden of unprofitable patients who still require medical care by transferring them to other facilities. There are some restrictions on hospital transfer behavior. Most notably, EMTALA

Figure 17: Alternative Margins of Hospital Response



*Note:* These graphs show the effect of FPLs on alternative measures of care quantity. Data are from the NIS. Estimates are based on Equation 1. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. Each regression includes hospital, year, and season fixed effects. All models also include the patient demographics and risk-adjusters. See the footnote of Table 4 for a full list of controls. Pre-treatment mean: Log(charges): 9.6; Potentially preventable admissions: 12%; Transfers: 8%.

and various state laws prohibit transfers of medically needy patients that are driven by purely by financial considerations of the hospital. However, these guidelines allow for transfers for various health reasons. Even within legislated guidelines it is possible that hospitals are able to increase the rate at which they transfer uninsured following a FPL.

## Results

The results for the alternative measures of care quantity show further evidence of cost-reducing behavior after a fair pricing law is enacted. Panel A of Figure 17 shows that reductions in (ln) total charges are consistent with

those for length of stay. In total, charges fell by 6.5% after enactment of the FPL, but the decline appears to grow in magnitude over time and reach 7-9 percent in the later years of our sample.

Panel B show that the yearly treatment effects for potentially preventable admissions are consistently negative in the years following enactment of an FPL, though not individually significant. However, the diff-in-diff results indicate a 3 percent drop in preventable admissions (significant at the 5% level). This is consistent with the notion that hospitals will be more likely to treat plausibly "borderline" cases in a less costly outpatient setting after passage of an FPL. It is worth noting that these cases are relatively rare. These patients make up roughly 12 percent of admissions, meaning our point estimates would translate to a 0.36 percent reduction in overall admissions of uninsured patients. The results shown in Figure 4 indicate little evidence of a change in the overall fraction of uninsured patients admitted, but the effect measured in this section would fall within the reported confidence intervals.<sup>39</sup>

Finally, panel C shows evidence that hospitals transfer more of their uninsured patients after fair pricing laws are enacted. Again, the yearly treatment dummies fall short of significance, but the diff-in-diff estimate is significant at the 5% level. On average, 8% of patients are transferred, so these estimates represent approximately a 6% increase.

---

<sup>39</sup>It is worth quantifying how much this level of selection could bias our results. Under the assumption that these foregone admissions were as healthy as possible (i.e. would have been an admission of zero days), length of stay following a FPL would be 0.4 percent shorter. While these patients would likely be relatively healthy, we also repeat this exercise assuming they would have been in the 95th percentile of length of stay (which corresponds to a roughly 10 day admission). This corresponds to a 0.92 percent increase in our post treatment length of stay. To put this in perspective, by two years post enactment, our treated estimates correspond to a decrease of roughly 7.5% days. Our treated estimates would be roughly 12 percent smaller in this scenario. It is worth noting that this type of selection would also have to occur in a way that is not captured by our risk adjustment strategy. This is not impossible, but does reduce the likelihood of large scale selection effects.

Table 11: The effect of fair pricing laws on various indicators of quantity of care delivered to uninsured patients

Outcome:	Ln(Total Charges)	Frequency of Preventable Admissions	Frequency of Transfers
<i>Diff-in-Diff</i>			
FPL In Effect	-0.07***	-0.004	0.005*
State clusters	[-0.106,-0.035]	[-0.008,0.0002]	[-0.004,0.013]
Conley-Taber	[-0.106,-0.049]	[-0.0085,0.0027]	[0.002, 0.013]
Observations	3085220	2677557	3118923
States	41	41	41

*Note:* Data are from the Nationwide Inpatient Sample for years 2003-2011. Estimates are based on Equation 1. Standard errors are clustered at the state level for yearly effects, and both state clustering and Conley-Taber are shown for DD results. CIs are reported in brackets. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Each regression includes hospital, year, and season fixed effects. All models also include the patient demographics and risk-adjusters. See the footnote of Table 4 for a full list of controls.

## G Hospital Characteristics in FPL States

The treatment effects we estimate are driven by the 432 hospitals in FPL states that we observe both before and after enactment. This section investigates whether there is any evidence that our results are driven by biased hospital sampling. The primary concern is that if certain hospitals respond more or less strongly to FPLs, and those hospitals are disproportionately identifying our treatment effect, then our estimates may be biased.

To address this concern, we first compare the set of hospitals driving our treatment estimates to other hospitals from FPL states along a number of dimensions that could conceivably impact responsiveness to FPLs. Table 12 shows that across a number of hospital characteristics, the sample of hospitals driving our treatment estimates look similar to the rest of the hospitals from treated states. This evidence suggests that our main identifying hospitals are largely representative of hospitals from their states.

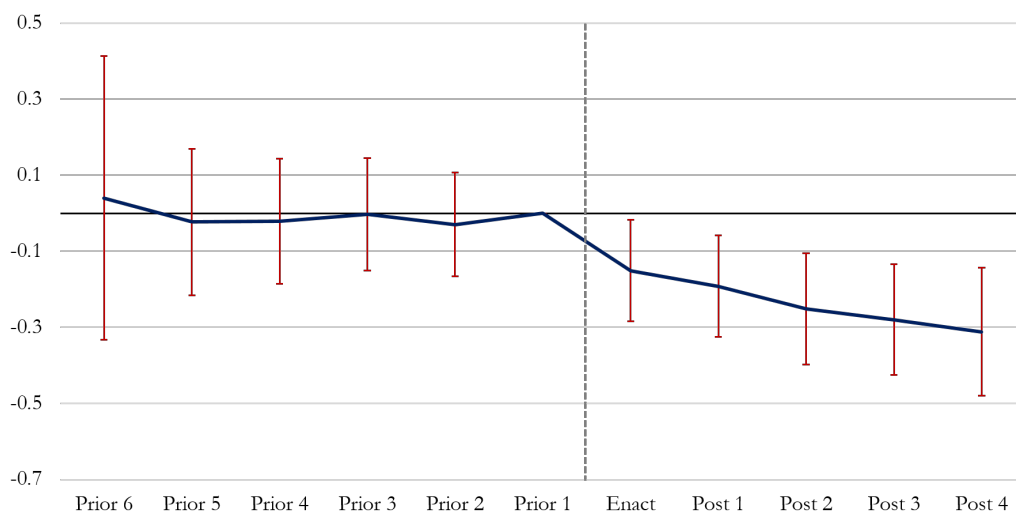
Another way to address this issue is to re-estimate our main specification using the trend weights provided by AHRQ. These weights are used to adjust for the complex sampling structure of the NIS and produce nationally representative estimates. Figure 18 illustrates the effect of FPLs on length of stay utilizing the NIS sampling weights. The estimated model includes a full set of controls and risk-adjusters (as in model (3) from Table 4). Reassuringly, the results are similar to the main results presented earlier in Figure 6.

Table 12: Comparing “Identifying” and “Non-Identifying” Hospitals in Treatment States

	“Identifying” Hospitals	“Non-identifying” hospitals from treated states
<i>Ownership Characteristics</i>		
For-profit	12.2%	11.5%
Non-profit	71.9%	70.7%
Government, non-federal	15.7%	17.7%
Member of multi-hospital system <sup>a</sup>	59.1%	57.4%
<i>Size</i>		
Total discharges per year	10,544	9,974
<i>Location</i>		
Urban	78.1%	75.5%
<i>Teaching Status</i>		
Teaching Hospital	25.2%	26.4%
<i>Patient Characteristics</i>		
Percent Uninsured	4.58%	4.54%
Number of Hospitals	432	461

*Note:* Data are from the Nationwide Inpatient Sample for years 2003-2011. <sup>a</sup> indicates variable only available beginning in 2007.

Figure 18: The Effect of Fair Price Laws on Length of Stay Using Sample Weights



*Note:* This figure illustrates the impact of fair pricing laws on lengths of stay for uninsured patients with the use of sample weights. Data are from the NIS. Estimates are based on Equation 1, but with the addition of sample weights. We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. The vertical bars indicate 95% confidence intervals based on hospital clustering. The regressions includes our full set of fixed effects, patient demographics, and risk-adjusters. See the note on Table 4 for a full list of controls.

## H Patient Severity and Procedure Type

This appendix shows how the effects that FPLs have on different types of procedures vary with patient severity. The estimates are produced by comparing uninsured patients to those covered by Medicaid in the California SID. In each case, the positive relationship between number of procedures performed and DRG Weight becomes stronger after the FPL, suggesting that hospitals are more actively targeting resources to the sicker patients.

Table 13: The Relationship Between FPLs and Types of Procedures by Patient Severity

	Minor		Major	
	Diagnostic	Therapeutic	Diagnostic	Therapeutic
FPL	-0.0037	-0.0758***	-0.006	-0.05***
	[-0.1,0.09]	[-0.1,-0.051]	[-0.07,0.054]	[-0.076,-0.024]
FPL x DRG	0.0165***	0.0108***	0.0153**	0.022***
	[0.004,0.029]	[0.005,0.02]	[0.002,0.029]	[0.015,0.029]
Observations	5,410,552	5,428,295	5,386,451	5,390,041

*Note:* Data are from the California State Inpatient Database and estimates are based on Equation 2. Standard errors are clustered at the hospital level. Confidence intervals are reported in brackets. \* p<0.05, \*\* p<0.01, \*\*\* p<0.001. All models include hospital, year, and season fixed effects, as well as patient demographic controls, and risk adjusters. See the footnote of Table 4 for a full list of controls. Average DRG weight: 0.93, standard deviation of DRG: 1.0.



## I Regression Table for Quality of Care

This appendix shows the additional regression tables for the quality and alternative measures of quantity.

Table 14: The effect of fair pricing laws on various quality metrics

Outcome Variable:	Mortality From Selected Conditions		Mortality From Any Condition	Frequency of Beneficial Procedures		Frequency of Preventable Complications	
	AHRQ Expected Mortality	Primary CCS		Primary CCS	AHRQ Primary CCS	Predicted Frequency	Primary CCS
<i>Diff-in-Diff</i>							
FPL in effect	-0.0040*	-0.0065**	0.0002	0.0121	0.0001	0.0001	0.0001
	[-0.0077,-0.0003]	[-0.0112,-0.0019]	[-0.0012,0.0017]	[-0.0131,0.0373]	[-0.001,0.0016]	[-0.001,0.0013]	[-0.001,0.0013]
Observations	276477	276477	3142717	146715	2551837	2551837	2551837
States	41	41	41	41	41	41	41

*Note:* Data are from the Nationwide Inpatient Sample for years 2003-2011. Estimates are based on Equation 1. Standard errors are clustered at the state level. CIs are reported in brackets. \* p<0.05, \*\* p<0.01, \*\*\* p<0.001. Pre-treatment means: Mortality for selected conditions: 4.1%; Mortality for all conditions: 1.3% Beneficial procedures: 50%, Complications: 0.54%.

## J Quality Metrics

Below we list the specific quality metrics employed in each of the four categories.

### Mortality from selected conditions and procedures

---

**Selected Conditions**

Acute Myocardial Infarction  
Heart Failure  
Acute Stroke  
Gastrointestinal Hemorrhage  
Hip Fracture  
Pneumonia

---

**Selected Procedures**

Esophageal Resection  
Pancreatic Resection  
Abdominal Aortic Aneurysm Repair  
Coronary Artery Bypass Graft  
Percutaneous Coronary Intervention  
Craniotomy  
Hip Replacement

### Use of procedures believed to reduce mortality

---

**Procedures**

Esophageal Resection  
Pancreatic Resection  
Abdominal Aortic Aneurysm Repair  
Coronary Artery Bypass Graft  
Percutaneous Coronary Intervention  
Carotid Endarterectomy

## Incidence of potentially preventable in-hospital complications

### Procedures

---

Death in Low-Mortality DRGs  
Pressure Ulcer Rate  
Death among Surgical Inpatients  
Iatrogenic Pneumothorax Rate  
Central Venous Catheter-Related Blood Stream Infection  
Postoperative Hip Fracture Rate  
Postoperative Hemorrhage or Hematoma Rate  
Postoperative Physiologic and Metabolic Derangement Rate  
Postoperative Respiratory Failure Rate  
Postoperative Pulmonary Embolism or Deep Vein Thrombosis Rate  
Postoperative Sepsis Rate  
Postoperative Wound Dehiscence Rate  
Accidental Puncture or Laceration Rate

## Potentially preventable hospital admissions

### Potentially Preventable Conditions ((A) acute, (C) chronic):

---

Diabetes short-term complications (C)  
Diabetes long-term complications (C)  
Uncontrolled diabetes (C)  
Lower extremity amputation from diabetes (C)  
Perforated appendix (A)  
COPD/Asthma in older adults (C)  
Asthma in younger adults (C)  
Hypertension (C)  
Heart failure (C)  
Dehydration (A)  
Bacterial pneumonia (A)  
Urinary tract infection (A)  
Angina without procedure (C)

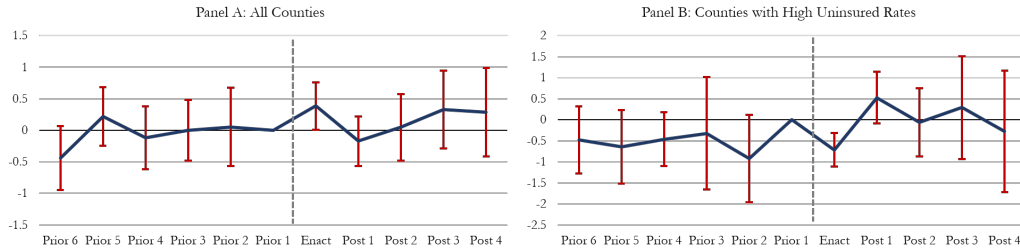
## K Results for CDC Death Rates

In this section we use mortality data from the CDC to investigate mortality outside of hospitals for high-risk conditions. Specifically, we focus on non-injury deaths of 25-64 year-olds from 1999-2010 that occurred outside the hospital. Moreover, we are able to restrict our attention to deaths from one of the mortality QI procedures and conditions mentioned in Section J. Each is measured as an age-adjusted death rate per 100,000 people (the age-adjustment is calculated by the CDC to account for the aging population over time). We repeat this analysis for the entire US population as well as for counties with more than 25% uninsured.

Since our data is a state-year panel, we do not have patient-level control variables, and employ state as opposed to hospital fixed effects. We add state-specific linear time trends to account for differential drift in death rates over the time period (both treatment and control states experiences roughly linear declines in age-adjusted death rates, but the trend in treatment states is steeper). Thus, the year effects measure deviations from these trends that are common to all states, and the yearly FPL dummy variables measure deviations that are specific to treatment states.

The results are illustrated in Figure 19. Panel A includes all counties, while panel B restricts attention to only those with high uninsured rates. In neither do the estimates suggest FPLs cause a systematic meaningful change in mortality rates outside of hospitals for these high-risk conditions.

Figure 19: CDC Death Rates Surrounding FPLs



*Note:* These graphs illustrate the impact of FPLs on CDC mortality rates for deaths from selected conditions and procedures that occur outside of a hospital. Estimates are based on evaluating Equation 1 at the county level. The omitted dummy is “1 year prior to enactment,” so that coefficient has been set to zero. Each regression includes state and year fixed effects and state-specific linear time trends.