Finance and Economics Discussion Series
Divisions of Research & Statistics and Monetary Affairs
Federal Reserve Board, Washington, D.C.

Financial Incentives, Hospital Care, and Health Outcomes: Evidence from Fair Pricing Laws

Michael M. Batty and Benedic N. Ippolito

2015-107

Please cite this paper as:
Batty, Michael M. and Benedic N. Ippolito (2015). "Financial Incentives, Hospital Care, and Health Outcomes: Evidence from Fair Pricing Laws," Finance and Economics Discussion Series 2015-107. Washington: Board of Governors of the Federal Reserve System, http://dx.doi.org/10.17016/FEDS.2015.107.

Financial Incentives, Hospital Care, and Health Outcomes: Evidence from Fair Pricing Laws*

Michael M. Batty† and Benedic N. Ippolito‡

Working Paper
October 30, 2015

It is often assumed that financial incentives of healthcare providers affect the care they deliver, but this issue is surprisingly difficult to study. The recent enactment of state laws that limit how much hospitals can charge uninsured patients provide a unique opportunity. Using an event study framework and panel data from the Nationwide Inpatient Sample, we examine whether these regulations lead to reductions in the amount and quality of care given to uninsured patients. We find that the introduction of a fair pricing law leads to a seven to nine percent reduction in the average length of hospital stay for uninsured patients, with no corresponding change for insured patients. These care reductions are not accompanied by worsening quality of inpatient care. Overall, our results provide strong evidence that hospitals actively alter their behavior in response to financial incentives, and are consistent with the laws promoting a shift towards more efficient care delivery. The findings also add to the growing evidence that hospitals can, and do, treat patients differently based upon insurance status.

---

*The authors are grateful to Karl Scholz, Chris Taber, Jesse Gregory, John Mullahy, Bobbi Wolfe, J. Michael Collins, Justin Sydnor, Alan Sorensen, Sara Markowitz, Dave Vanness, Jason Fletcher, Tom DeLeire, and participants of the Wisconsin Public Economics Seminar 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.

† Michael Batty, Federal Reserve Board of Governors. Email: mike.batty@frb.gov

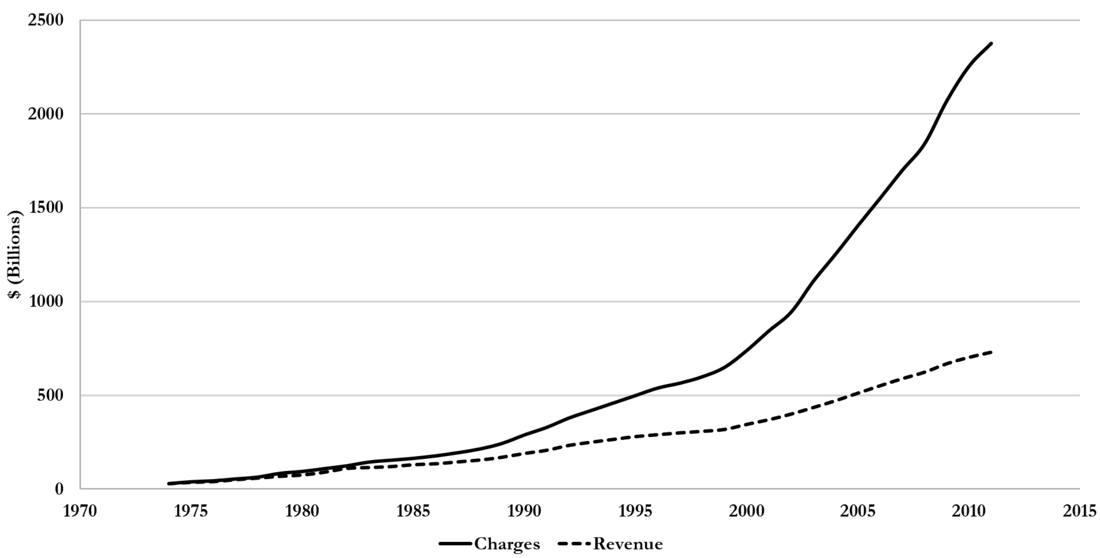
‡ Benedic Ippolito, American Enterprise Institute. Email: benedic.ippolito@aei.org

1 Introduction

It is widely believed that the way health care providers are paid affects the care they deliver. Given that most patients have limited ability to determine what care they need, or even the quality of care they have received, understanding this relationship is vital to both patients and policy makers. Unfortunately, opportunities to study this issue are relatively rare. 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). This paper takes advantage of an exogenous change in financial incentives created in the late 2000s by state fair pricing laws (FPLs) that limit payments hospitals can collect from uninsured patients.

After a hospital visit, every patient receives a bill showing three different prices for each service: the official list price, the amount paid by the insurer (if applicable), and the amount they are being asked to pay. The difference between the list price and the insurance payment represents the discount negotiated by the insurer. As recently as the late 1970s, hospitals typically collected the full list price for the services delivered. However, 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 their insurers negotiating steep discounts, the uninsured are typically billed at full list price.[1] 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 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 either patient or provider responses to the changing financial incentives.

We use data from the Nationwide Inpatient Sample in an event study framework to show that price caps imposed through fair pricing regulations substantially decrease the amount of inpatient care that hospitals deliver to uninsured patients. The introduction of a fair pricing law 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 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. Thus, FPLs appear to do more to generate efficient care, rather than lower quality care.

Ours is the first study of how fair pricing laws affect the amount and quality of health care given to uninsured patients.[2] 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.[3] 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.[4]

More broadly, FPLs provide a unique opportunity to study how providers alter care in response

[1]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.

[2]Melnick and Fonkych (2013) investigates California's FPL, but focuses upon the degree to which hospitals have defined the required financial assistance policies rather than the care they provide. They do provide evidence that California hospitals are in compliance with the state fair pricing law.

[3]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.

[4]Updated estimates are available at: http://www.cbo.gov/sites/default/files/cbofiles/attachments/03-13 CoverageEstimates.pdf. 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.

to financial incentives, and how this ultimately affects patient outcomes. Our study complements an existing literature that mostly studies a limited number of Medicare policies. Much of the current 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). While the PPS is somewhat similar to FPLs, it was a one-time change to Medicare, meaning it lacks a convincing 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 provides a natural control group to help rule out potential confounding effects.

Another body of work focuses on more targeted Medicare fee changes. Clemens and Gottlieb (2014) utilize a 1997 change to the Medicare payment formula that generated area-specific price shocks for physicians. They find that doctors do increase care and invest more in medical technology after prices increase, but health outcomes are largely unaffected. In contrast, the general finding from Rice (1983), Nguyen and Derrick (1997), Yip (1998), and Jacobson et al. (2010) is a backward-bending supply curve. Physicians increase utilization of services to offset the lost income from fee reductions. These studies say little about any associated changes in health outcomes.

FPLs offer a particularly compelling test of providers' behavioral responses for several reasons. The Medicare fee studies are based in outpatient settings, whereas we might expect care to be less flexible (particularly to reductions) with the more serious inpatient conditions we study. Also, FPLs impact a relatively small portion of patients - on average around 5 percent of a hospital's patient population. If there are fixed costs associated with restructuring or adjusting treatment plans in response to financial incentives, we would expect hospitals to be more responsive to price changes for a large group like Medicare patients. Finally, the existing literature is predominantly based upon policy changes from the 1980s and 1990s. FPLs provide an updated look at how providers respond to financial incentives in an ever changing healthcare market.

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. Further, it requires that hospitals provide free care to low to middle income uninsured patients. The law will also require that these discounts be publicized throughout the hospital (and on the bill) so uninsured patients know to apply.

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.[5]

[5]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

Table 1: Fair pricing laws by state

State	Year Enacted	Income Limit as a Percentage of Fed. Poverty Level	Percent of Uninsured Covered
Minnesota	2005	~500%	86%
New York	2007	300%	76%
California	2007	350%	81%
Rhode Island	2007	300%	77%
New Jersey	2009	500%	87%
Illinois	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 more 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 Hospital Prices Paid by the Uninsured

We have not yet established that fair pricing laws impose a meaningful (i.e., binding) price ceiling. If hospitals and the vast majority uninsured patients already negotiate final prices that are equal to or below the newly imposed price ceilings, we would expect fair pricing laws to have limited impact on hospital behavior.[6] In this section we investigate whether the caps introduced by these laws materially change the payments hospitals receive from uninsured patients.[7]

Recall that a typical FPL restricts hospitals from collecting more from an uninsured patient than they would collect from a publicly insured patient. This implies that FPLs will only bind if

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 information 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.

[6] 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.

[7] Appendix B describes the passage of California's FPL, which provides alternative evidence that hospitals believe the restrictions are meaningful.

some uninsured patients were actually paying more than the benchmark publicly insured patient (for similar services) prior to a FPL. Interestingly, previous research has shown that, on average, hospitals actually collect a similar percentage of the list price from uninsured and publicly insured patients (Hsia et al., 2008; Melnick and Fonkych, 2008). However, if the distribution of payments from uninsured patients is relatively dispersed, these laws may impose binding price ceilings for a considerable number of uninsured hospital visits. We are unaware of any existing research that documents the underlying variation in collection rates (percentages of list prices paid) from the uninsured population. In the results below, we show that the average collection rates for uninsured patients masks significant variation, with hospitals often collecting much closer to full list charges.

2.1 The Medical Expenditure Panel Survey (MEPS)

The MEPS is a nationally representative survey of health care use and spending in the United States. Critical to our work, the MEPS collects data about both the list prices and the amounts actually paid for medical care. To improve the reliability of payment data, the MEPS verifies self-reported payments with health care providers when possible.[8] Our sample includes all patients with either public or no insurance in the MEPS between 2000 and 2004 who went to the hospital at least once, resulting in 22,126 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.

Ideally, we would like to directly study how collections from uninsured patients change after a state enacts a FPL. Unfortunately, because of the limited sample size of uninsured patients who have hospital expenditures in the MEPS, we are unable to perform this type of analysis. Instead we will compare payments by insurance type before the laws were enacted . The lack of state identifiers in this data also limits how precisely we can define the period prior to the enactment of fair pricing laws, so we focus on the period from 2000 to 2004 because it immediately precedes the introduction of the earliest FPL. We split our sample into two groups: those who had no private insurance but did have public insurance at some point in the year (Medicare or Medicaid), and those who had no insurance at any point in the year. Like previous studies on this topic, our outcome of interest is the fraction of list prices collected by hospitals.

2.2 Hospital Collection Rates in the MEPS

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. We also see that patients with public insurance have considerably higher average charges. This is not surprising given that much of the population with public coverage is made up of relatively expensive patients (Medicare and disabled individuals covered by Medicaid).

[8]The results in this section do not change if we restrict the sample to only those with verified payment information. Futher, we focus on the facility rather than the "separately billing doctor" charges because only facilities charges are currently covered by the FPLs.

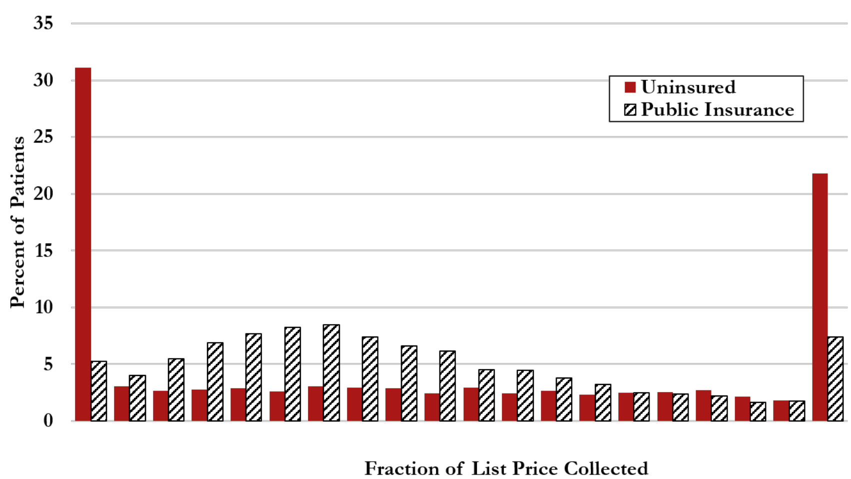
Table 2: Summarizing hospital charges and percentage of list price paid by payer-type

Insurance Status	Count	Mean Hospital Charges	Mean Percentage of List Price Collected
Public Insurance	18,187	$15,088	37%
Uninsured	3,939	$6,045	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.[9] We know these two groups pay similar amounts *on average*, but the underlying distributions of collections are quite different. Collection rates for publicly insured patients are relatively concentrated around the average rate (37 percent).[10] 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, Figure 2 excludes the highest income uninsured patients that are unlikely to be covered by FPLs, but a version of the figure including all uninsured patients is virtually identical.[11]

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).

[9] Appendix C shows that Medicare and Medicaid patients have very similar payment distributions.

[10] 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.

[11] Mahoney (2015) does find 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.

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.[12] 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.

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

	Evaluated at:			
Collection Ratio	25th Percentile	50th Percentile	75th Percentile	90th Percentile
Uninsured	-0.218***	0.033*	0.214***	0.088***
	(0.0032)	(0.0145)	(0.0113)	(0.0055)
Log(Charges)	-0.0037***	-0.0249***	-0.02473***	-0.0389***
	(0.0008)	(0.0015)	(0.0020)	(0.0017)

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 22,126.

These results suggest a substantial portion of uninsured patients are paying more than they would under FPLs, but the MEPS can also help us quantify the importance of the laws by forming a rough estimate of how much payments from uninsured patients would fall.[13] Since we cannot directly observe the price cap that would exist for a given uninsured patient, we employ two different methods to bracket the likely effect.[14] Our first approach involves shifting the distribution of collections from uninsured patients to the left, until there is no "excess mass" in the right tail (i.e., uninsured patients are paying high percentages of their bills with no greater frequency than the publicly insured). We re-allocate this excess mass by filling bins from the right until the height

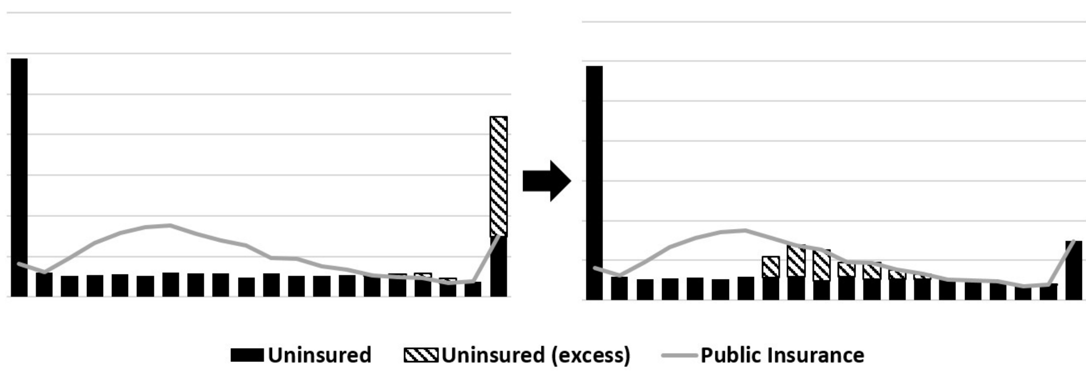
[12]We control for bill amount because sample sizes are too small to match uninsured and publicly insured patients on the basis of diagnosis.

[13]A variety of federal, state, and local programs 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 (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.

[14]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 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.

of each bin reaches the corresponding height of the distribution of collections from publicly insured patients. This process is most easily explained in Figure 3.[15] The payments that would be made under this hypothetical distribution are 11% lower than those that were actually collected without FPLs.

Figure 3: Estimating the payment reduction by shifting the distribution of percentage of list price paid for uninsured patients



Note: The histogram on the left is the same as Figure 2. The histogram on the right shifts uninsured patients until there is no weight above the publicly insured distribution.

While this is a useful starting point, it likely represents a lower bound estimate because it assumes the payment caps for uninsured patients come from the upper half of the publicly insured payment distribution, whereas the actual cap for any given high-paying uninsured patient may be farther to the left. A more balanced approach would be to estimate the change in payments if the cap for each patient were based on what a typical publicly insured patient pays. We do so by randomly matching each uninsured patient in our data to a patient with public insurance who has a similar bill size. If the uninsured patient 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.[16] 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. Over five hundred simulations of this exercise, the projected payments from uninsured patients fall by an average of 31%, or $1,800 per inpatient. It is interesting to note that although more revenue is lost from larger bills, the percentage reduction in revenue is relatively constant across the distribution of charges.

[15] Although this method prescribes how many patients within each bar should be moved to the left, it is unclear which patients should be chosen. As a results, we select them at random. Because patients can have very different bill sizes, the particular set of patients selected may impact the estimate. To remove noise, we repeat this process five hundred times and report the average results.

[16] We apply this cap only to uninsured patients with incomes below 400% of the poverty level, which based upon the income categories reported in the MEPS, is the closest approximation to the population covered by FPLs.

2.3 California Office of Statewide Health Planning and Development Data

In addition to the MEPS, the California Office of Statewide Health Planning and Development (OSHPD) produces a unique dataset that allows us to construct a useful case study about how FPLs affect payments to hospitals from uninsured patients. Starting in 1995, OSHPD collects utilization and financial data by payer category from each California hospital. For our work, the relevant data are patient visits, payments received by the hospital, and the costs of providing care (including marginal costs and allocated overhead). Although, we cannot compare outcomes for uninsured patients in California to those of uninsured patients in states that did not enact a FPL, it is informative to study how payments and costs evolve in California around its enactment. Figure 4 shows payments and costs of care per patient visit for all patients in California hospitals from 1995-2013. Payments and costs generally move together throughout this period, but at several points they diverge sharply. The first is in 2004, which corresponds to the American Hospital Association and California Hospital Association publishing voluntary guidelines for hospitals to offer discounts to uninsured patients (in response to the California legislature passing a version of the FPL that was ultimately vetoed).[17] The second, and larger divergence occurs immediately after the enactment of the FPL.[18,19] In comparison, the payments and costs for all other payer types have remained much closer to each other over time.[20]

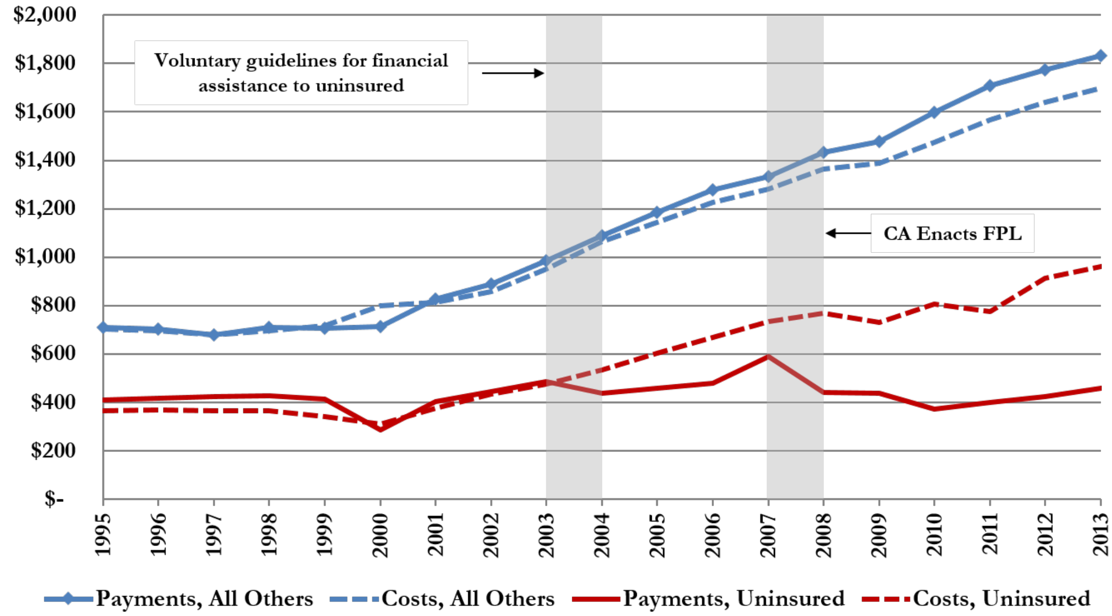
[17]Described in more detail in Appendix B.

[18]The 2008 file contains data for fiscal years that end in 2008, meaning it 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. For this reason, and because of uncertainty in how quickly hospitals implemented the FPL, we show transition periods in the figure.

[19]The third divergence in 2010 could be the result of the Great Recession limiting uninsured patients' abilities to pay.

[20]Figure 4 combines private insurance, Medicare, and Medicaid. Although relative payments from the private insured have steadily increased and those from Medicare have decreased over time, none of the individual payer classes have experienced they type of sharp movements we observe for uninsured patients.

Figure 4: Per Patient Payments and Costs of Care by Payer in California, 1995-2013



Note: Source: California OSHPD financial pivot files. Covers all hospital care in California, 1995-2013. Payments include patient revenue from all sources. Costs include marginal costs and allocated overhead. All dollar figures are nominal.

While we cannot construct a control group of uninsured patients in a non-FPL state, the change in payments from the uninsured in California provides another method to characterize the likely effect of the law. Payments from uninsured patients covered an average of 77% of costs in the four years leading up to the FPL, and then fell to 59% of costs in the two years following. This represents a 24% decrease in payments, or $2,500 per admitted patient in terms of 2013 California medical costs.[21] The case study of California produces an estimate of revenue decline that is quite similar in magnitude to our best estimate from the MEPS. Taken together, they suggest that FPLs impose a substantial burden, and give us reason to believe that hospitals may attempt to reduce their cost of treating uninsured patients.

3 Data

We study the effects of FPLs on treatment patterns using inpatient records. Each inpatient record includes detailed information on diagnoses, procedures, basic demographic information, expected 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.

[21]The OSHPD data for payments and costs combines inpatient and outpatient care, but inpatient visits are typically much more expensive. The per patient estimate of revenue lost for any type of visit is $180.

Our primary data source is the Nationwide Inpatient Sample (NIS) developed by the AHRQ. 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 window. 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).[22] This gives us approximately 3.2 million observations.

4 Empirical Strategy

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:

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

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

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, μ_h, γ_t, χ_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 δ_L coefficients is measured relative to the omitted category: "1 year prior to adoption." Observations at least three years prior, and two years post enactment are grouped together in "3 or more years prior" and "2 or more years post" categories, respectively, because identifying treatment effects further removed from the year of enactment relies on a quickly decreasing number of states. Still, the results we present are robust to specifications where years are not grouped, and in these models the magnitude of the treatment effect levels off after two years post enactment. 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

[22] Thirty-three states are present in each year of our data, with the other 8 beginning to 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.

pricing law. Finding δ_L coefficients in the "prior" years that are indistinguishable from zero would indicate the 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 in section 6, the pre-trends we observe imply that the non-FPL states are a valid control group. However, because states enacted FPLs at different times, we can also estimate the model with only FPL states, allowing them to act as their own controls. These results, shown in Appendix E, are very similar to those that include all states.

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. This suggests we should include a rich set of demographic and diagnosis control variables. However, many of the uninsured patients in our data are covered by FPLs that link the payment cap to Medicare's PPS, meaning the payment cap is determined by the diagnosis, which gives providers a reason to increase the severity. As a result, we will investigate the effects of FPLs both with and without controlling for patient diagnosis.

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 in our six treatment states before and after the introduction of fair pricing laws.[23]

Because fair pricing laws are enacted at the state level, we are concerned that unobserved factors shared by observations within the same state (including but not limited to economic conditions and other regulations) may cause correlation in their outcomes, and thus result in incorrect standard errors. We address this concern by clustering standard errors at the state level. However, as documented in Conley and Taber (2011), this approach still requires the number of treated clusters to grow large in order to produce consistent estimates. The number of treated clusters in our application is six, so we cannot rely on the assumption that the noise in any state-time random effects within our treatment states will converge to zero. To address this potential problem, we employ the method of inference developed in Conley and Taber (2011) that relies upon estimating the distribution of state-time random effects using the control states. In the results that follow, we show that the confidence intervals produced by state-level clustering and the Conley-Taber procedure are quite similar. We are not surprised by this result because the responses to the state regulations are implemented at the hospital level, and thus we do not expect state-time random effects to play a significant role.

4.1 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.[24] We choose length of stay

[23]Approximately 400, or half of the hospitals in FPL states are observed before and after enactment. Appendix F shows that hospitals that are and are not observed on both sides of FPL enactment do not differ systematically.

[24]In Appendix D we also investigate whether FPLs have any impact on the way hospitals set list prices.

(LOS) as our primary measure of quantity for several reasons. First, it is an easily measured proxy of 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 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.

Of course, we are ultimately more concerned with how changes in the amount of care translate into changes in health outcomes. While it is common to assume that less care will lead to worse outcomes, researchers have had difficulty drawing a definitive link (see Carey (2002)). To directly measure care quality, 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 procedures, 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 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.

Finally, we investigate how hospitals may attempt to circumvent fair pricing laws by altering the way they diagnose patients. Carter et al. (1990) and Dafny (2005) find that hospitals do artificially inflate the severity of diagnoses under the PPS in order to collect more revenue. Hospitals may engage in this "upcoding" behavior to increase the payment caps under FPLs that are based upon public payers. We measure severity numerically with the weight assigned to each diagnosis by CMS. This weight is designed to capture the expected resources the hospital will expend treating the patient, and is directly related to the amount Medicare will reimburse.

4.2 Risk-adjustment

Of course, the underlying severity of a patient's condition will be correlated with the amount of care given and the health outcome. We employ two methods to risk-adjust patients by controlling for their diagnoses. The first is the DRG weight, but because FPLs may encourage strategic manipulation of this metric, our second method uses the Clinical Classifications Software (CCS) categorization scheme provided by HCUP. 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, the second risk-adjustment strategy may miss more granular diagnosis information. To compensate, we also look for changes in the total patient population that would suggest systematic changes in diagnosis patterns are driven by real changes in patient composition.

4.3 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.[25] In this case we would not underestimate the true effect.

5 Results

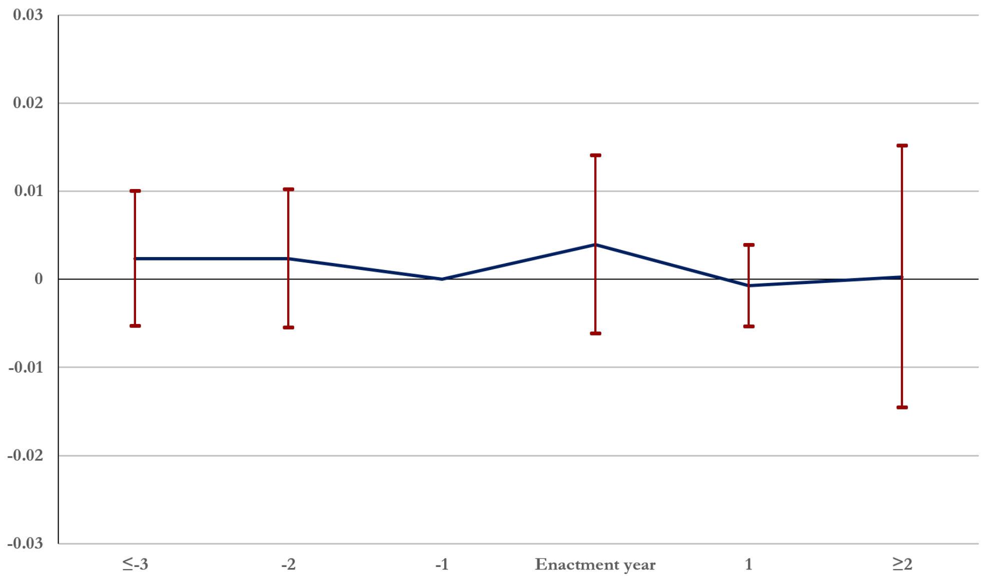
5.1 Trends in visits by uninsured patients

FPLs can be thought of as a type of catastrophic insurance, so it is conceivable that the introduction of these laws could induce more people to go without insurance, and/or more uninsured patients to seek treatment at hospitals. Any such change would be important for interpreting the results of our main analysis regarding the type and amount of care delivered. To study these margins of behavior we start with the full NIS (including patients of all payer types), and collapse our data to hospital-year observations that measure the share of inpatient records belonging to uninsured patients. We then estimate our event-study specification at the hospital-year level using the uninsured percentage as the outcome variable (and excluding individual patient characteristics).

The yearly treatment effects are plotted in Figure 5. Our results show that fair pricing laws are not associated with any significant change in the share of uninsured inpatient stays at hospitals, so we believe hospitals continue to treat essentially the same uninsured patient population after law enactment. This result is expected. Patients who are sick enough to be admitted to a hospital likely have relatively inelastic demand for hospital care on the extensive margin, and knowledge of these laws is probably acquired through the information that hospitals are required to include on hospital bills. It also suggests that there may be little demand effect of FPLs on quantity of care, and that the overall effect is likely largely determined by changes in supply.

[25] Under the EMTALA, hospitals may only begin to inquire about ability to pay after it is clear doing so will not compromise patient care. Some hospitals do pull credit reports for patients, but in order to comply with regulations they insist this information is used to inform collection efforts rather than influence treatment (see "Why Hospitals Want Your Credit Report" in the March 18, 2008 issue of the Wall Street Journal).

Figure 5: The effect of fair pricing laws on the share of inpatients stays accounted for by uninsured 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. The vertical lines show the 95% confidence intervals.

5.2 Length of stay

Next, we test whether fair pricing laws 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 basic demographic information. Model (2) includes DRG-based risk-adjusters. Model (3) includes CCS-based risk-adjusters. Our preferred specification is Model (3) because it is less susceptible to strategic diagnosing. Standard errors are clustered at the state level.

In all three models, we see no significant effects prior to the enactment of fair pricing laws, indicating that we have a valid control group. In the years post adoption we see clear and consistent evidence of reduced lengths of stay. The magnitudes grow in the 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. By two years after enactment, hospital stays for uninsured patients have fallen by seven to nine percent. It is worth noting that the smallest treatment effect within the confidence interval is approximately 4 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.

Table 4: The effect of fair pricing laws on length of stay for uninsured patients.

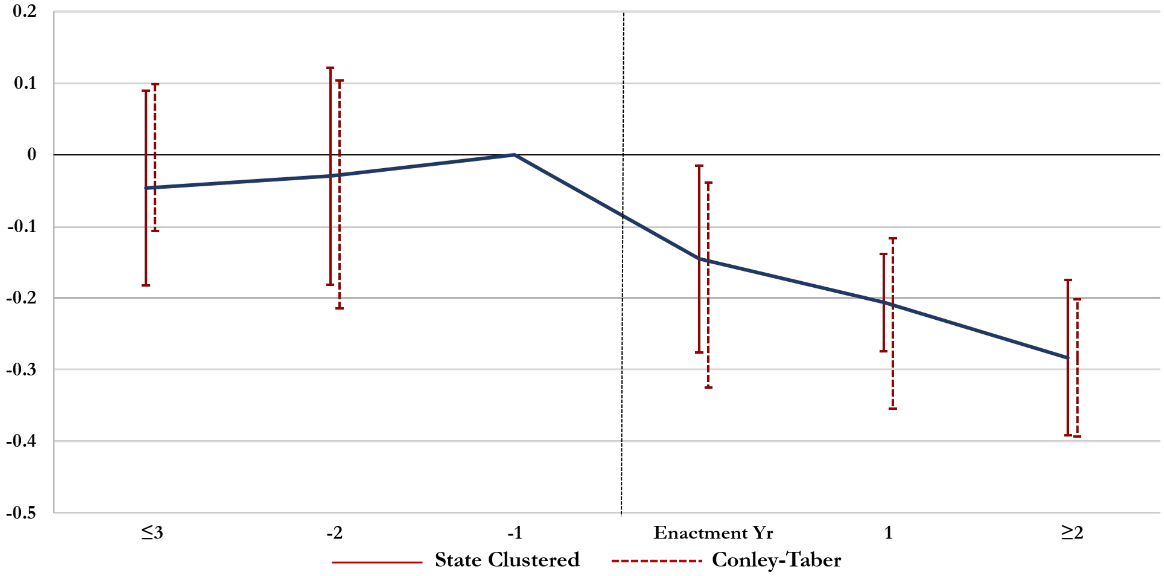
	Outcome Variable: Length of Stay Pre-treatment mean: 4.08 days		
Included Controls:	(1) Fixed Effects and Patient Demographics	(2) Fixed Effects, Patient Demographics, and DRG Risk Adjusters	(3) Fixed Effects, Patient Demographics, and CCS Risk Adjusters
3 or more years prior	-0.0851	-0.111	-0.0462
	[-0.267,0.0967]	[-0.315,0.0924]	[-0.182,0.0892]
2 years prior	-0.0677	-0.100	-0.0296
	[-0.265,0.130]	[-0.290,0.0892]	[-0.181,0.122]
Enactment year	-0.202*	-0.173**	-0.146*
	[-0.355,-0.0487]	[-0.280,-0.0673]	[-0.276,-0.0148]
1 year post	-0.285***	-0.279***	-0.206***
	[-0.404,-0.166]	[-0.407,-0.150]	[-0.274,-0.138]
2 or more years post	-0.347***	-0.294***	-0.284***
	[-0.491,-0.203]	[-0.382,-0.206]	[-0.392,-0.175]
Observations	3143772	3143772	3143772
States	41	41	41

Note: 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 are age, age^2, 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. Observations at least three years prior, and two years post enactment are grouped together in "3 or more years prior" and "2 or more years post" categories, respectively, because identifying treatment effects further removed from the year of enactment relies on a quickly decreasing number of states.

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 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.[26] We also focus on Model (3) for the remainder of our results because it is qualitatively similar to our other models.

[26] 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.

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



Note: This figure is based on model (3) from Table 4. 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. See the note on Table 4 for a full list of controls.

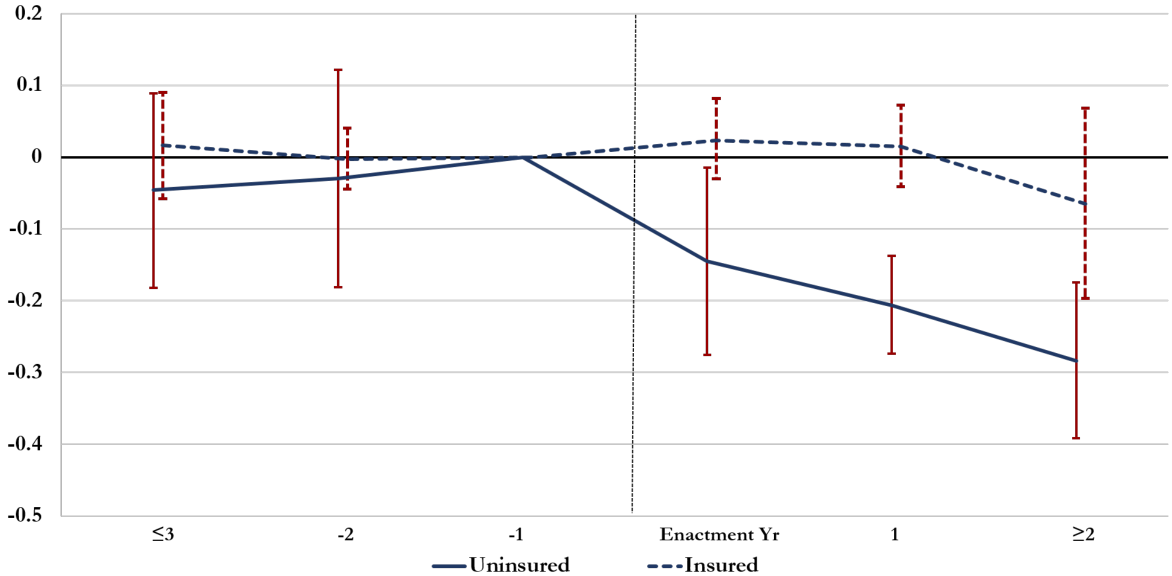
5.2.1 Results for Insured Patients

For robustness, we test whether similar reductions in length of stay occur for insured patients in states that enacted fair pricing laws. It is worth noting that there are far more insured patients than uninsured in the NIS (roughly 65 million versus 3.2 million). To attain comparable power in both studies, we randomly select a sample of 3.2 million insured patients for this analysis. As shown in Figure 7, we see no significant changes in LOS for insured patients after the implementation of a fair pricing law. This suggests the results we observe for uninsured patients are not driven by other state-level factors that affect all patients. Figure 8 breaks the overall "insured" group into its three major payer types (omitting confidence intervals for legibility) and shows that they behave similarly. Moreover, the changes in care for uninsured patients are still apparent when measured against a group of patients that frequently serves as the payment cap under FPLs (Medicare), and against the insured population that is likely most similar to the uninsured (Medicaid).

5.2.2 Hospital Ownership Status

Because they may have different incentive structures, it is natural to ask whether for-profit and non-profit hospitals respond differently to FPLs. For-profit hospitals are relatively rare in our treatment states (primarily due to state rules regarding hospital ownership), so we focus this analysis on

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

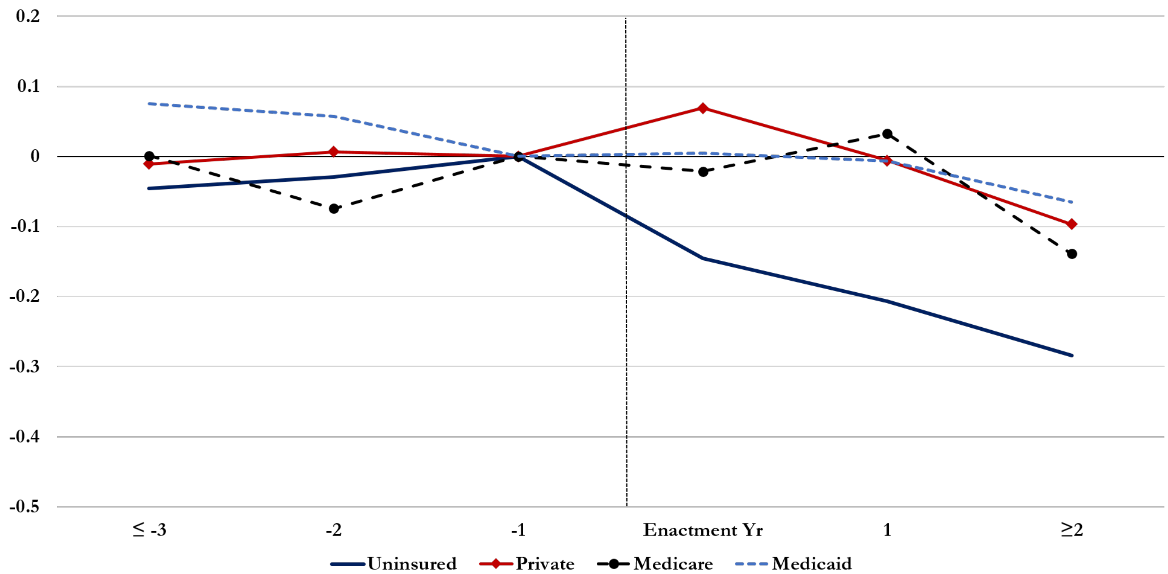


Note: This figure illustrates the impact of fair pricing laws on lengths of stay for insured and uninsured patients. The solid line is based on model (3) from Table 4, while the dotted line is based on the same specification estimated on a sample of insured patients. We have plotted the coefficients 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. 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.

California. To bolster the sample size we utilize the State Inpatient Database, which includes the universe of California hospitals. We estimate whether the treatment pattern of uninsured patients in for-profit hospitals changes relative to that of uninsured patients in non-profit hospitals. Table 5 shows the results of a difference-in-differences model with the change in LOS in for-profit hospitals measured relative to that in non-profit hospitals. It reveals no evidence that for-profit hospitals responded differently from non-profits.

Our result is consistent with a body of literature showing only limited differences between for-profit and non-profit hospitals. For example, Sloan (2000) shows that they provide similar amounts of uncompensated care, and Sloan et al. (2001) shows they provide similar quality of care. Non-profit hospitals do frequently generate an operating profit that they store in reserves for future investments, and no hospital can afford to consistently lose money, so the financial incentives may be more similar than the names imply.

Figure 8: 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. The solid line with no markers is based on model (3) from Table 4, while the other lines are based on the same specification estimated on a sample of insured patients. 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.

Table 5: Hospital ownership type and reactions to fair pricing laws.

	Length of stay
Fair Price Law in Effect	-0.196***
	[-0.294,-0.0988]
Fair Price Law in Effect at For-Profit	0.00731
	[-0.135,0.150]
Observations	399444

Note: Standard errors are clustered at the state 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.

5.2.3 High vs. Low Number of Uninsured Patients

There may be some fixed costs to organizing an institutional response to these new policies, in which case we would predict that hospitals with a higher proportion of uninsured patients are likely the ones which will respond most strongly to a FPL. To investigate, we split the population of hospitals into two groups based on whether the fraction of stays that are from uninsured patients is above or below average (4.8 percent of stays). Table 6 shows the results when we interact the FPL dummy with the indicator for treating a high proportion of uninsured patients. We do not see clear evidence that treating more uninsured patients elicits a stronger reaction to these laws, which may suggest that there are not large fixed costs associated with determining how to reduce care.

Table 6: Hospital characteristics and reactions to fair pricing laws.

	Length of stay
Fair Price Law in Effect	-0.162***
	[-0.234,-0.0907]
Fair Price Law in Effect x High Pct Uninsured	-0.0391
	[-0.110,0.0317]
Observations	3143772

Note: Standard errors are clustered at the state 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.

5.2.4 Long vs. Short Lengths of Stay

We may also expect cost-reducing behavior to differ for patients with high and low utilization. Specifically, FPLs are more likely to be binding for patients who receive less treatment, because these patients are more likely to have been able to pay large portions of their bills prior to a fair pricing law. Then, we would expect to see hospitals react more strongly for these types of patients. We study this issue by splitting the sample (by CSS diagnosis group) into high and low utilization patients. We then re-estimate our model for the highest and lowest quartiles of expected LOS.

Results are reported in the second and third columns of Table 7 (column 1 shows the overall results). While the absolute reductions in LOS are considerably larger for the highest quartile of expected LOS, they are similar in percentage terms (5 percent bottom quartile, and 6 for the highest quartile). This does not support our prediction that FPLs may impose more meaningful constraints for relatively low cost stays. One plausible explanation is that there is more clinical flexibility to shorten longer stays, whereas shorter stays more quickly bump up against the minimum required treatment. This is also consistent with similar percentage revenue reductions across the charge distribution.

Table 7: The effect of fair pricing laws on length of stay for high and low utilization patients

	Outcome Variable: Length of Stay		
Sample Composition:	Full Sample	Lowest quartile of expected LOS	Highest quartile of expected LOS
Pre-treatment mean LOS:	4.08	2.11	6.24
Fair pricing law	-0.201***	-0.100***	-0.380***
	[-0.296,-0.105]	[-0.148,-0.0528]	[-0.661,-0.099]
Observations	3143772	3143772	3143772

Note: Standard errors are clustered at the state level. CIs are reported in brackets. * p<0.05, ** p<0.01, *** p<0.001. All results are from model (3): 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.

5.3 Short-Term Quality of Care

We have established that hospitals cut back on care for uninsured people after an FPL goes into effect, but it is not clear how this will affect quality of care and subsequent health outcomes. In this section we test whether the observed reductions in length of stay are accompanied by any observable decrease in the short-term quality of care received by uninsured patients 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 best measure quality of care using inpatient records. Since then, they have become a standard in quality assessment, endorsed by the National Quality Forum, and employed in numerous articles published in leading journals.[27] The QIs we study are organized into three categories:

- 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 above, 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 will 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.[28]

[27] For a list of publications unsing the AHRQ QIs see http://www.qualityindicators.ahrq.gov/Resources/Publications.aspx

[28] For brevity, we include only graphical regression results for our preferred yearly treatment model (3). Appendix G contains the associated regression tables.

5.3.1 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 H 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.[29] To take a broader look at mortality, we also estimate our model on the full sample of uninsured patients.

5.3.2 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." Appendix H 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%.

5.3.3 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 H includes the full list, but these are issues like postoperative hemorrhage, or accidental puncture or laceration. We may expect to see an increase in these events if they occur due to neglect, such as a doctor placing lower priority on checking on an uninsured patient who is now less financially valuable, but less so if they occur because of specific errors made during a procedure.

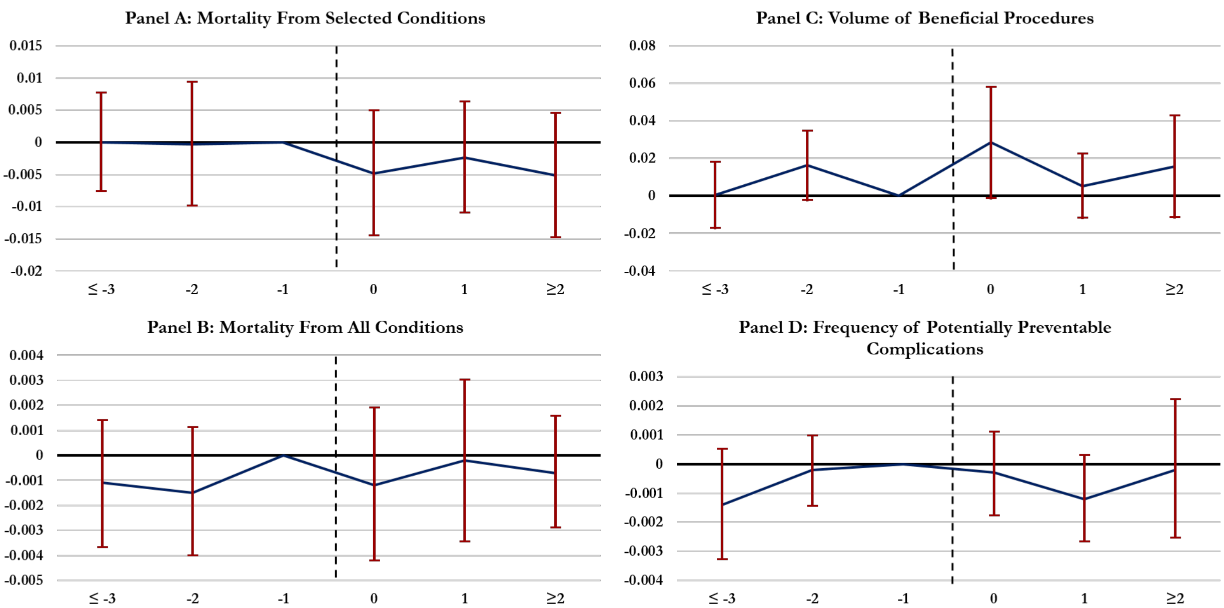
Individually, each event is quite rare: averaging 0.16% of the at-risk population (as defined by the QI software). When viewed together, the probability that an individual who is at risk for at least one of them will be inflicted with at least one of them is 0.54%. We estimate our model with the frequency of any of these complications as the outcome variable. Much like the mortality

[29] If FPLs induce strategic changes to diagnoses, it is possible this would affect the expected probability of death and bias our results. To address this concern we also estimate this model using a less granular risk-adjustment procedure based on CCS categories. The results (shown in Appendix G) are very similar.

metric, the QI software calculates an expected probability of each complication. We include this probability as a control variable in our model.

5.3.4 Results for short-term quality metrics

Figure 9: Measures of Quality of Inpatient Care



Note: These graphs show yearly treatment coefficients and associated state-clustered standard errors from our event study specification using 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. 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.

As shown in Panel A of Figure 9, FPLs are not associated with an increase of in-hospital mortality for the QI conditions and procedures. Although none of the yearly treatment effects are significant, the pattern of point estimates suggests a decline in mortality. When pooled together in a difference-in-differences model, the treatment effect is negative and significant at the 5 percent level, and corresponds to an 11 percent drop in in-hospital mortality. Panel B also fails to reveal an increase in mortality for the full uninsured population, but this time neither the event study, nor the differences-in-differences model suggest a drop in mortality.

While it may be surprising that FPLs are associated with decreases in mortality for the QI conditions, there are a few possible explanations. First, FPLs could encourage hospitals to replace a more aggressive and expensive treatment with one that is cheaper and less aggressive. While the less aggressive treatment may have lower expected benefit to the patient, it may also carry a lower mortality risk. Also, less time in the hospital may lead to fewer hospital-acquired infections. Finally, and perhaps most importantly, we are measuring *in-hospital* mortality. As shown earlier, FPLs are associated with considerable reductions in length of stay for uninsured patients. Mechanically, this reduces the amount of time for a patient to die in a hospital. Even if this is the primary driver of our results, it is still difficult to say whether this would be a positive or negative impact of FPLs. If FPLs are encouraging hospitals to forego potentially life-saving treatments for which a patient would have been willing to pay absent the price cap, this would have negative welfare implications. On the other hand, if hospitals are transferring terminally ill patients to locations that provide less costly end-of-life care, we may view this as an overall improvement in care delivery.

The NIS does not follow patients after they leave the hospital, and thus it provides little insight into which is the more likely scenario. However, the CDC publishes death rate statistics for the entire US population that are quite informative.[30] We construct a panel of age-adjusted death rates by state from 1999-2010. The data do not allow us to identify deaths of uninsured people, but we are able to focus on populations that have high concentrations on the uninsured, and where the deaths could have been affected by the quality of hospital care. 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 can focus on deaths that occurred outside of hospitals, and those that resulted from several of the most common mortality QI conditions and procedures. Finally, we study these populations both for the US as a whole, and restricted to counties with more than 25% uninsured.[31] Appendix I shows the results of this analysis, but in no case do they suggest FPLs are followed by a spike in death rates. Thus, we find no support for the argument that FPLs cause hospitals to release patients they could otherwise save.

Panel C of Figure 9 reveals no evidence that FPLs are associated with a decrease in the use of beneficial procedures. This suggests that when hospitals do cut back on care for uninsured patients, they are doing it in ways that preserve the type of care for which there is clear evidence of effectiveness. Finally, panel D of Figure 9 shows no evidence that FPLs increase the incidence of potentially preventable complications. Taken together, our data fail to reveal clear signs of deterioration of short-term care quality after enactment of a fair pricing law.

5.4 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 popular metric for 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

[30] We use the CDC WONDER online database to query the multiple cause of death mortality file.

[31] 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.

channels through which these rates depend on care quality (e.g. Ahmad et al., 2013). CMS has taken note of evidence like this and in recent years has 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.[32]

We study readmissions in the California SID by comparing uninsured patients to Medicaid patients over time in our event study specification. Although the patient populations may differ, those with Medicaid are likely more similar to the uninsured than are any other insured group. Further, Medicaid patients appear to be a valid control group in our analysis of length of stay (Figure 8), and the readmission data presented below suggests the same.[33]

Figure 10 reports the results of this analysis. The small and insignificant treatment coefficients in both the pre and post time periods provide strong 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 roughly .004, which means we can rule out increases in readmission rates of more than 4-5 percent (from a base of 8.7 percentage points). The results are similar if we consider 60 or 90 day readmission rates.

Overall, our study reveals no evidence that quality of care or health outcomes deteriorated while patients are in the hospital, nor do we observe patients more frequently developing health problems that push them back into the hospital in the following months. 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.

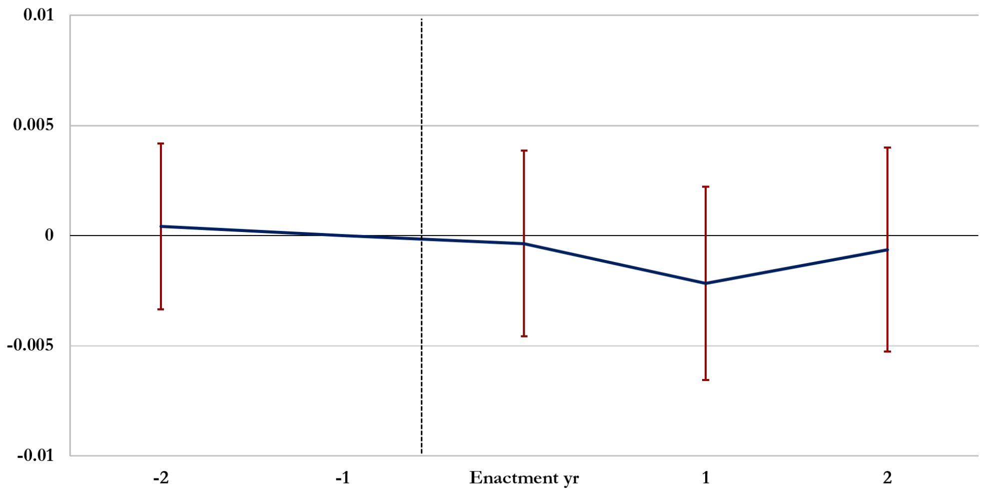
5.5 Alternative measures of care quantity

As described above, 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.

[32] All cases where a patient died during an initial stay were omitted from this analysis (since readmission is not possible).

[33] 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 sates.

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



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 hospital level. The vertical lines show the 95% confidence intervals.

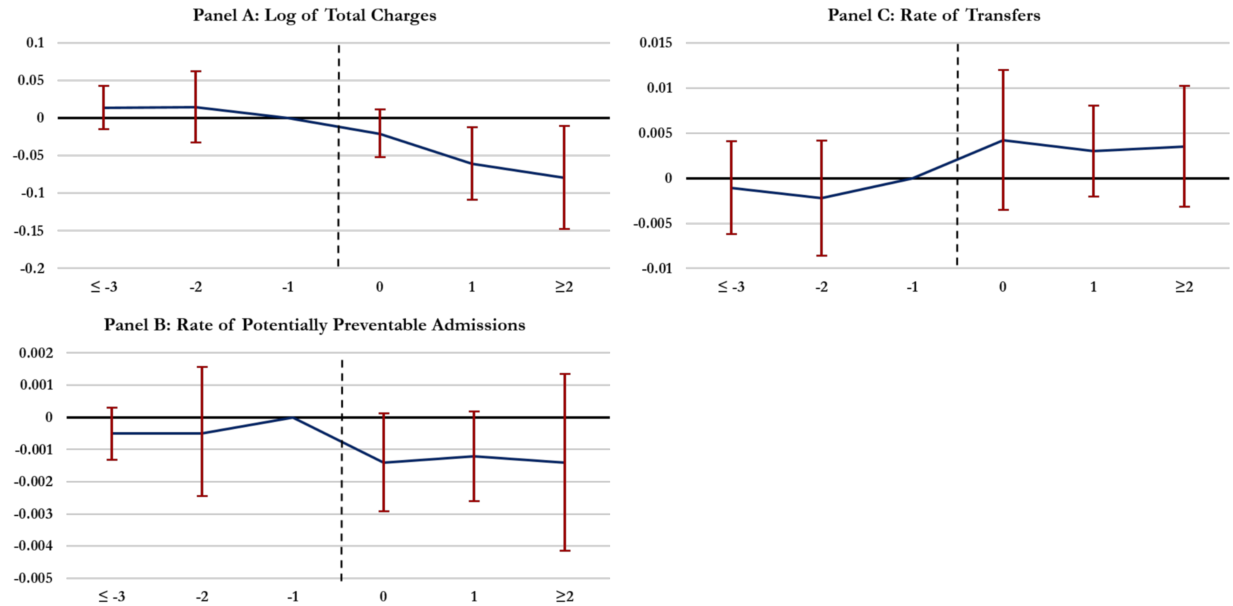
5.5.1 Total charges

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 potential limitation since we cannot separately identify hospital-specific charge trends and the effects of FPLs.

5.5.2 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 H). Several examples are COPD/asthma, and complications from diabetes. The 13 conditions account for approximately 12% of admissions in our data.

Figure 11: Alternative Margins of Hospital Response



Note: These graphs show yearly treatment coefficients and associated state-clustered standard errors from our event study specification using the alternative measures of care quantity as outcome variables. 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.

5.5.3 Transfers

Hospitals may attempt to reduce the burden of unprofitable patients who still require medical care by transferring them to other facilities. The EMTALA and various state laws prohibit transfers that are driven by financial considerations of the hospital, but the guidelines encourage transfers when they are in the patient's best interest. These reasons are often medical, such as hospitals specializing in the treatment of different conditions, but they can also be financial, like only certain hospitals accepting the patient's insurance. There will be situations when it is clear that one hospital is better suited to treat the patient because, for instance, only it has access to a particular piece of equipment. But it is also easy to imagine scenarios where the relative advantages of treatment in different locations are less clear. Thus, it is plausible that hospitals more frequently lean on the medical justifications of a transfer when the patient represents an expected loss rather than a profit. If this is the case, price ceilings would make hospitals more likely to transfer uninsured patients.

5.5.4 Results for alternative measures of quantity

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 11 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 8% by two years post.

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 always significant. However, the diff-in-diff results indicate a 3 percent drop in preventable admissions (significant at the 5% level). This suggests that when faced with a borderline case, hospitals will be more likely to treat the patient in a less costly outpatient setting after passage of an FPL. However, as shown in section 6.1, these cases appear to be rare enough that they do not materially affect the overall patient population.[34]

Finally, panel C shows evidence that hospitals transfer more of their uninsured patients after fair pricing laws are enacted.[35] 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.

5.6 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.[36] 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.[37] Unlike the DRG, the APR-

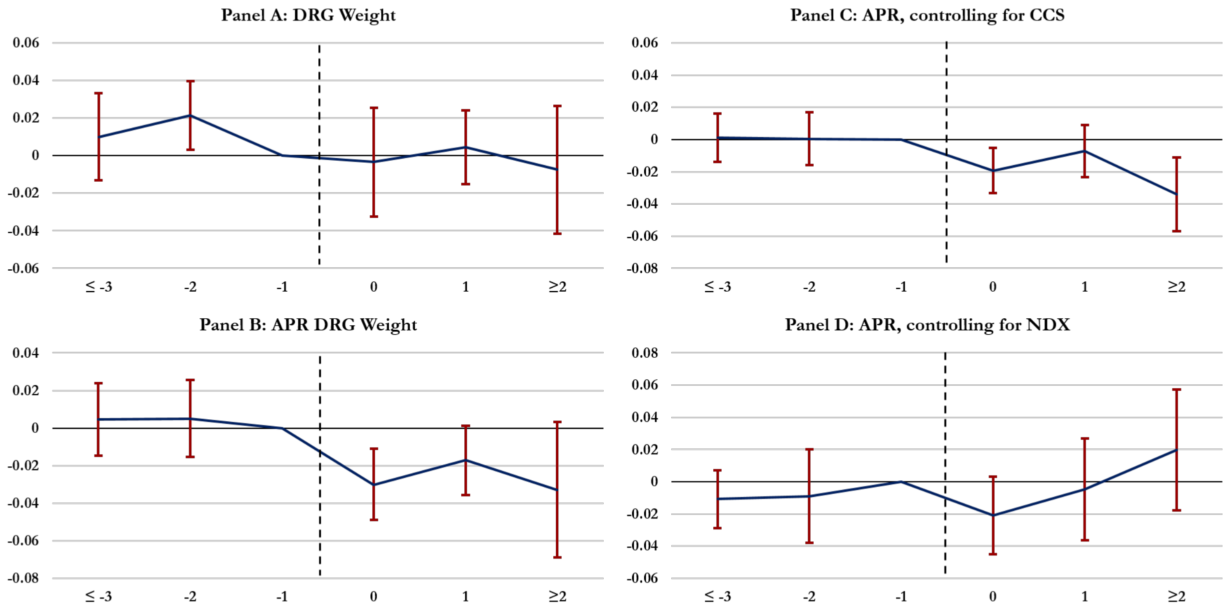
[34]To the extent that marginal cases are admitted to the hospital less frequently after FPLs, the remaining admitted population would have more severe conditions on average, and thus would bias against our findings of care reductions.

[35]These results do not distinguish between transfers to another acute care hospital and transfers to other types of care facilities, but the estimate for each type of transfer is similar to the overall results.

[36]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.

[37]All-patient refined (APR) DRGs were developed to better suit the non-Medicare population, and are in use by

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. 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.

DRG assigned is unlikely to directly affect the 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.[38] Interestingly, the reduction 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).[39] 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

Medicaid and quality reporting in some states.

[38] 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.

[39] 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.

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.

6 Conclusion

In this paper, we utilize fair pricing laws to investigate how hospitals alter care in response to financial incentives. Specifically, we test whether these laws impact the quantity and quality of care delivered to uninsured patients. We find that when governed by fair pricing laws, hospitals do cut back on care to uninsured patients. They shorten inpatient stays by seven to nine 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 clear evidence of deterioration in the quality of inpatient care received using a number of quality measures. Uninsured patients do not die in the hospital at higher rates, they do not experience higher rates of medical complications, they do not receive fewer "beneficial" medical procedures, and they are not readmitted with higher frequency than absent a FPL. Finally, even though upcoding diagnoses could increase the maximum collections under a FPL (in states with PPS-like regulations), we do not see evidence of such strategic diagnosing behavior by hospitals.

Of course, our work has limitations. First, the NIS does not report how much hospitals actually collect from uninsured patients, so we cannot directly measure the reduction in hospital bills for the uninsured, nor any relief from the debt collection process. Beyond this, it is important to note that our results do not imply that any further price reductions in this market would not harm short or long term quality of care. Determining the threshold below which further cuts would have observable adverse health outcomes is a topic for further study.

Overall, our study provides strong evidence that providers do respond to financial incentives, but suggests they do so by forgoing relatively low value care. Still, the implications for patient welfare are not immediately clear. In a typical consumer 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 clear 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.

The notion that there is not a direct link between healthcare and health outcomes at the margin may be surprising, but there is theoretical and empirical evidence which suggests that efficiency gains in the U.S. healthcare system are attainable. In seminal work, Arrow (1963) discusses the breakdown of market forces in healthcare. These issues are exacerbated for the uninsured patients we study who do not have insurance companies to help them act as better informed consumers. In practice, researches have documented wide geographic variation in health spending (both between the U.S. and other industrialized nations as well as among different regions in the U.S.) without the associated differences in health that one might suspect.[40] Evidence like this has led the Institute of Medicine to conclude that roughly 30% American health spending is wasteful (Smith et al., 2013). Our findings are consistent with the view that the financial pressures placed on providers via Fair Pricing Laws reduced some of this excess care.

[40]For a discussion see Sutherland et al. (2009).

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.

Kenneth Arrow. "Uncertainty and the Welfare Economics of Medical Care". *The American Economic Review*, pages 941–973, 1963.

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.

Kathleen Carey. "Hospital Length of Stay and Cost: A Multilevel Modeling Analysis". *Health Services and Outcomes Research Methodology*, 3(1):41–56, 2002.

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.

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.

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.

Frank A Sloan, Gabriel A Picone, Donald H Taylor, and Shin-Yi Chou. Hospital ownership and cost and quality of care: is there a dime’s worth of difference? *Journal of health economics*, 20 (1):1–21, 2001.

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.

Jason M Sutherland, Elliott S Fisher, and Jonathan S Skinner. “Getting Past Denial: The High Cost of Health Care in the United States”. *New England Journal of Medicine*, 361(13):1227–1230, 2009.

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 broadly 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,[41] 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**	**Percent of Fed. Poverty Level Covered**	**Percent of Uninsured Covered**	**Maximum Collection Amount**	**Free Care below X% of Poverty**
Minnesota	2005	~500%	86%	Largest private payer	NA
New York	2007	300%	76%	Highest volume payer	100%
California	2007	350%	81%	Highest price public payer	NA
Rhode Island	2007	300%	77%	Private payers	200%
New Jersey	2009	500%	87%	115% of Medicare	200%
Illinois	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.

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

[41] 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%.

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.[42] Based upon this limited evidence, mandating 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	
Included Controls:	Fixed Effects, Patient Demographics and DRG Weight	Fixed Effects, Patient Demographics, and CCS Category
FPL	-0.367***	-0.269***
	[-0.443,-0.292]	[-0.329,-0.209]
PPS-Based FPL	0.250***	0.138***
	[0.181,0.319]	[0.0981,0.177]
Free-Care FPL	0.138*	0.0148
	[0.0188,0.257]	[-0.114,0.143]
Observations	3134363	3134363

Note: 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.

[42] 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.

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 Schwartzeneger 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)

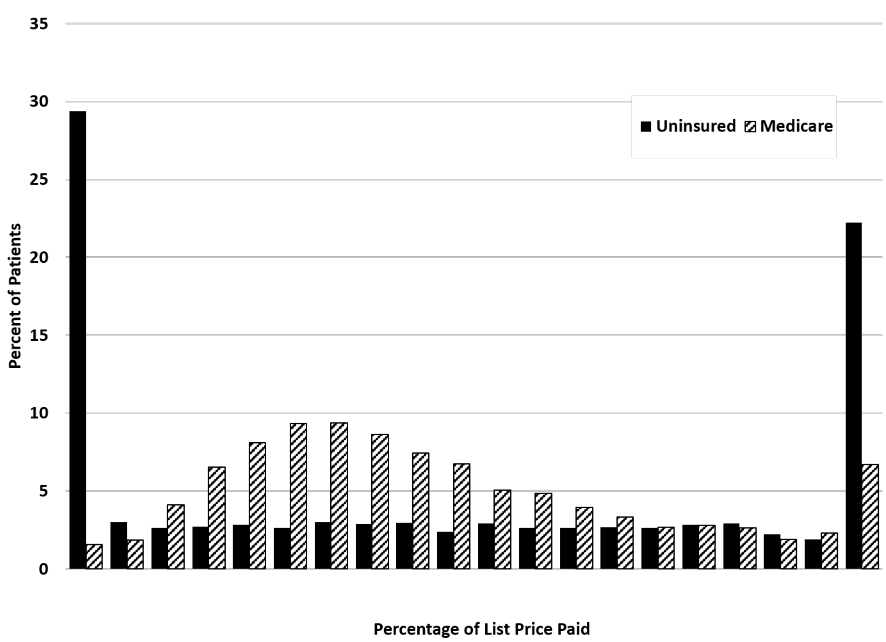
In section 2 we present the distributions of percentage of list price paid for publicly insured and uninsured patients. We do so because the price caps imposed by FPLs are based upon a mix of Medicare and Medicaid payments, rather than because we believe the publicly insured patients are comparable to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

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	18,187	$15,088	37%
Medicare	9,252	$19,881	39%
Medicaid	8,935	$10,126	34%
Uninsured	3,939	$6,045	37%

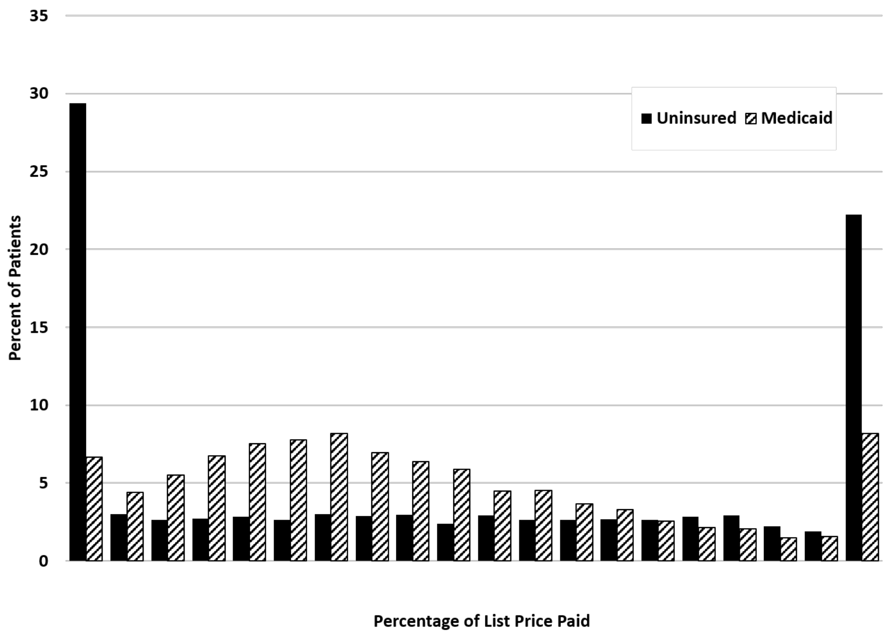
Note: The data are from the Medical Expenditure Panel Survey from 2000-2004.

Figure 13: Distribution of percentage of list price paid for Medicare and uninsured patients



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

Figure 14: Distribution of percentage of list price paid for Medicaid and uninsured patients



Note: The data are taken from the MEPS for the years 2000-2004.

D Impact of FPLs on Hospital List Prices

In this section we investigate whether FPLs had any impact on hospital list prices (or "Chargemaster" prices). As outlined in the introduction to this paper, list prices have risen substantially over time. While list prices are largely irrelevant to insured patients whose insurers negotiate large discounts off list prices, they are the basis by which uninsured patients are initially billed. This has lead 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.

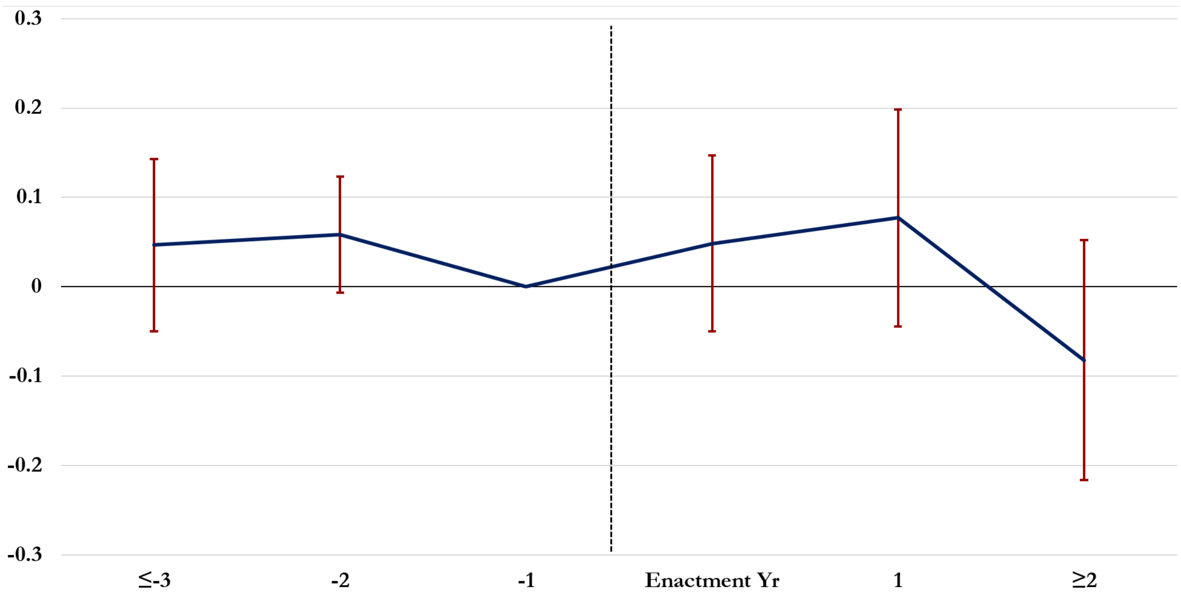
If generating revenues from uninsured patients is a motivation for increasing list prices, then it is possible that FPLs may reduce, or slow the growth of, list prices. By capping the maximum collection from uninsured patients below the list price, FPLs effectively render the list price irrelevant for 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 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 we are interested in hospital-level reactions to FPLs, 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.

The results are shown in Figure 15. Prior to the enactment of FPLs, list price markups are trending similarly to markups in control states. After the introduction of FPLs we do not see any evidence of a divergence in pricing patterns between treated and control states. FPLs do no appear to alter hospital list prices.

There are a few potential explanations for this null effect. First, it is possible that collections from uninsured patients, while a popular theory on list pricing, is not a major motivation for hospital pricing. It is also hospital list prices do target uninsured patients, but only those exempt from protection under FPLs. Recall that FPLs generally cover all but the wealthiest uninsured (those abover 400-600% of the poverty level). If high list prices are only targeted at this exempted group, then FPLs may not have any impact on list prices.

Figure 15: The Effect of FPLs on List Price Markups



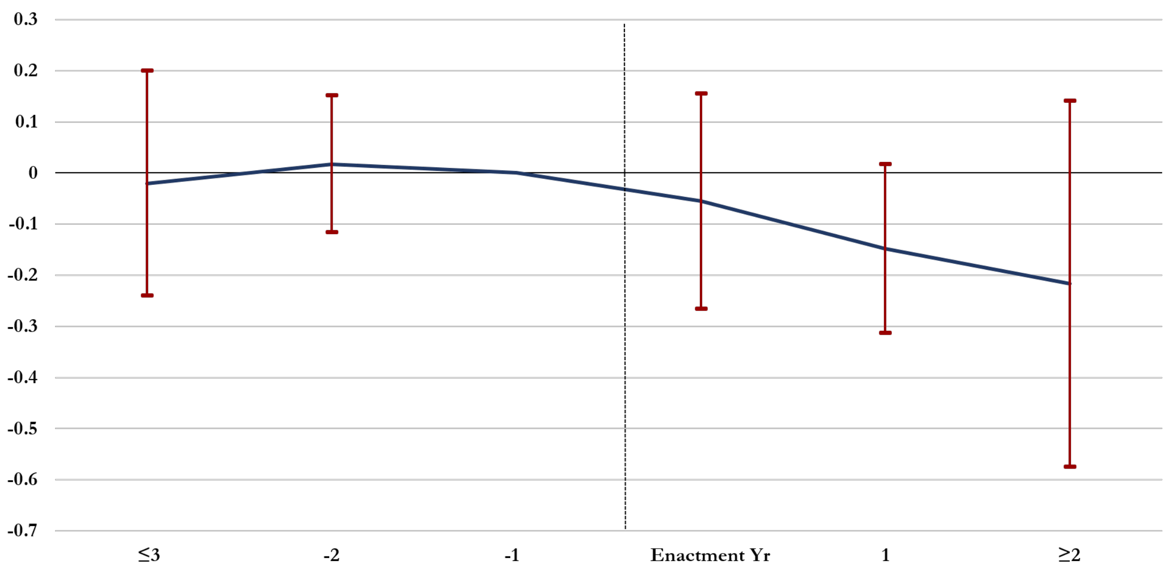
Note: This figure illustrates the impact of fair pricing laws list prices in treatment states (specifically the ratio of list prices to costs, or the markup). We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "1 year prior to enactment," so that coefficient has been set to zero. The vertical bars indicate 95% confidence intervals based on state clustering. Data are collapsed to the hospital-year level. Year and hospital fixed effects are included, but seasonal fixed effects are removed since price levels are reported yearly.

E Additional Robustness Checks

E.1 Treatment Effects on LOS Including Only FPL States

Because of the differential timing of FPLs, we can identify treatment effects with just the set of states that ever pass a FPL. In our primary analysis, however, we include all states that never pass a FPL in our control group. These states help to improve the precision of our estimates, and based on our pre-FPL results, it appears they are a reasonable control group. As a more conservative robustness check, we re-estimate the effect of FPLs on length of stay excluding all uninsured patients from states that never pass a law. Figure 16 shows that the results are quite similar, though predictably, eliminating control states reduces the precision of our estimates. If we pool all post-FPL years in a difference-in-differences model the reduction in length of stay is significantly different from zero.

Figure 16: Comparing Changes in Length of Stay for Uninsured Patients Using only Treatment States

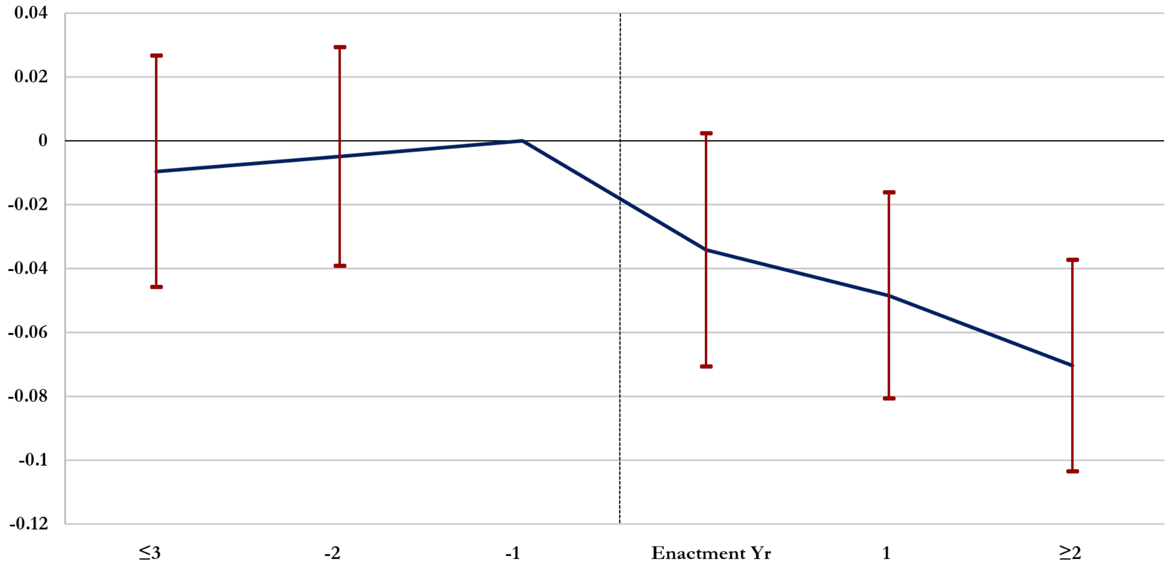


Note: This figure illustrates the impact of fair pricing laws on lengths of stay for uninsured patients from treatment states only. The solid line is based on model (3) from Table 4, but estimated only on the sample of uninsured patients from states that ever pass a Fair Pricing laws. We have plotted 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. 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.

E.2 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 17, the results are comparable to our main specification. By the end of our analysis window, fair pricing laws are associated with a 7 percent reduction in the average length of stay.

Figure 17: 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. 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. 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.

F 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 11 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.

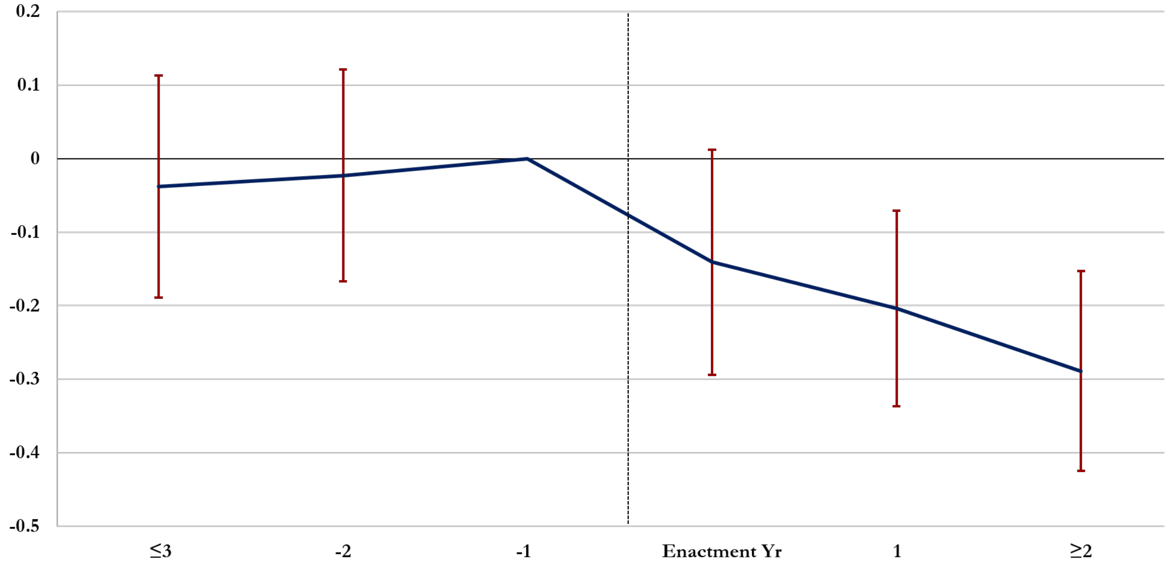
Table 11: The effect of fair pricing laws on various indicators of quantity of care delivered to uninsured patients

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

Note: Data are from the Nationwide Inpatient Sample for years 2003-2011. [a] indicates variable only available beginning in 2007.

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 very similar to the main results presented earlier in Figure 6.

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. The solid line is based on model (3) from Table 4 but with the inclusion 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.

G Regression Tables for Quality of Care and Alternative Measures of Quantity

This appendix shows the additional regression tables for the quality and alternative measures of quantity that were referenced in section 6.

Table 12: The effect of fair pricing laws on various indicators of quantity of care delivered to uninsured patients

Outcome Variable:	Ln(Total Charges)	Frequency of Preventable Admissions	Frequency of Transfers
Diff-in-Diff Specification			
FP Law In Effect	-0.0657***	-0.00366	0.00466*
state clusters	[-0.103,-0.0286]	[-0.0075,0.0002]	[-0.0037,0.0130]
Conley-Taber	[-0.1005,-0.0352]	[-0.011,0.003]	[0.0023, 0.0130]
Event Study Specification			
3 or more years prior	0.0138	-0.0005	-0.0011
	[-0.0152,0.0428]	[-0.0013,0.0003]	[-0.0063,0.0041]
2 years prior	0.0143	-0.0005	-0.0022
	[-0.0331,0.0616]	[-0.0025,0.0016]	[-0.0086,0.0042]
Enactment year	-0.0207	-0.0014	0.0042
	[-0.0525,0.0112]	[-0.0029,0.00012]	[-0.0035,0.0120]
1 year post	-0.0610*	-0.0012	0.0029
	[-0.109,-0.0127]	[-0.0026,0.0002]	[-0.0021,0.0080]
2 year post	-0.0795*	-0.0014	0.00348
	[-0.148,-0.0108]	[-0.0042,0.0013]	[-0.0032,0.0102]
Observations	3110006	2699691	3144168
States	41	41	41

Note: Data are from the Nationwide Inpatient Sample for years 2003-2011. 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.

Table 13: 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	
Risk-Adjustment Strategy:	AHRQ Expected Mortality	Demographics and Primary CCS Diagnosis	Demographics and Primary CCS Diagnosis	Demographics and Primary CCS Diagnosis	AHRQ Predicted Frequency	Demographics and Primary CCS Diagnosis
Diff-in-Diff Specification						
Fair pricing law	-0.0040*	-0.0065**	0.0002	0.0121	0.0001	0.0001
in effect	[-0.0077,-0.0003]	[-0.0112,-0.0019]	[-0.0012,0.0017]	[-0.0131,0.0373]	[-0.001,0.0016]	[-0.0010,0.0013]
Event Study Specification						
3 or more years prior	0.00001	-0.0022	-0.0011	0.0005	-0.0014	-0.0016
	[-0.0077,0.0077]	[-0.0125,0.0081]	[-0.0037,0.0014]	[-0.0170,0.0180]	[-0.0033,0.0005]	[-0.0035,0.0002]
2 years prior	-0.0003	-0.0026	-0.0015	0.0163	-0.0002	-0.0004
	[-0.0098,0.0093]	[-0.0131,0.0080]	[-0.0040,0.0011]	[-0.0022,0.0348]	[-0.0015,0.0010]	[-0.0017,0.0010]
Enactment year	-0.0048	-0.0087	-0.0012	0.0284	-0.0003	-0.0001
	[-0.0145,0.0050]	[-0.0226,0.0053]	[-0.0042,0.0019]	[-0.0014,0.0581]	[-0.0018,0.0011]	[-0.0009,0.0006]
1 year post	-0.0024	-0.0046	-0.0002	0.0053	-0.0012	-0.0004
	[-0.0110,0.0063]	[-0.0140,0.0049]	[-0.0035,0.0030]	[-0.0117,0.0223]	[-0.0027,0.0003]	[-0.0017,0.0010]
2 or more years post	-0.0051	-0.0117	-0.0007	0.0158	-0.0002	-0.0016*
	[-0.0148,0.0045]	[-0.0242,0.0008]	[-0.0029,0.0016]	[-0.0113,0.0429]	[-0.0025,0.0022]	[-0.0032,-0.0001]
Observations	276477	276477	3142717	146715	2551837	2551837
States	41	41	41	41	41	41

Note: Data are from the Nationwide Inpatient Sample for years 2003-2011. Standard errors are clustered at the state level. 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.

H Quality Metrics

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

H.1 Mortality from selected conditions and procedures

Conditions:

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

Procedures:

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

H.2 Use of procedures believed to reduce mortality

Beneficial Procedures:

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

H.3 Incidence of potentially preventable in-hospital complications

Preventable Complications:

- Death in Low-MortalityDRGs
- 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

H.4 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)

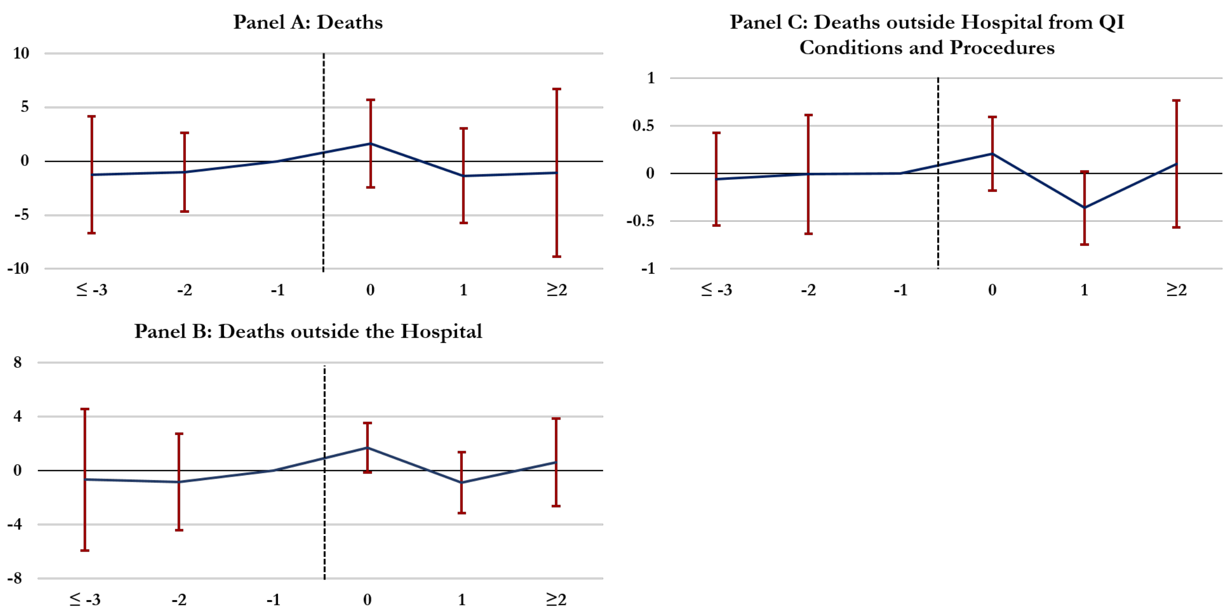
- Bacterial pneumonia (A)
- Urinary tract infection (A)
- Angina without procedure (C)

I Results for CDC Death Rates

We study three outcomes for non-injury deaths of 25-64 year-olds from 1999-2010. 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). The outcomes are overall deaths, deaths that occurred outside of the hospital, and deaths that occurred outside the hospital from one of the mortality QI procedures and conditions. We first study the entire US population, and then focus on counties with more than 25% uninsured.

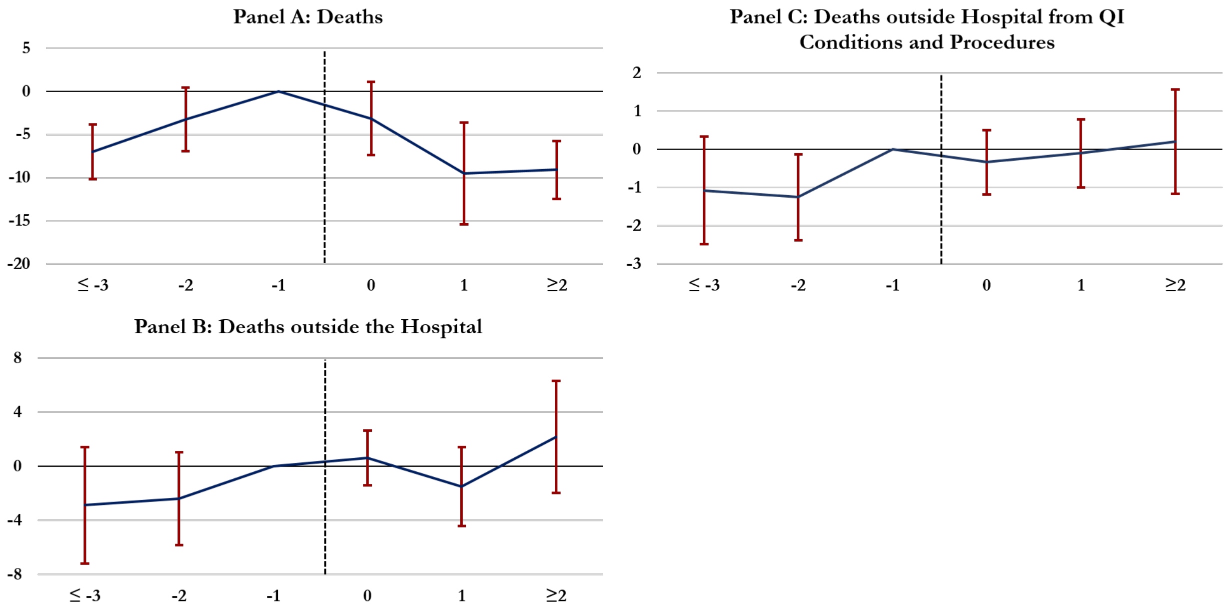
We start with our event study framework. Since our data is a state-year panel, we do not have patient-level control variables, and employ state as opposed to hospital fixed effects. We add state-specific linear time trends to account for differential drift in death rates over the time period (both treatment 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.

Figure 19: Death Rates in the United States



Note: These graphs show yearly treatment coefficients and associated state-clustered standard errors from our event study specification using the CDC death rates as the outcome variables. 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.

Figure 20: Death Rates in Counties with Many Uninsured



Note: These graphs show yearly treatment coefficients and associated state-clustered standard errors from our event study specification using the CDC death rates as the outcome variables. 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.