Online Appendix for "Financial Incentives, Hospital Care, and Health Outcomes: Evidence from Fair Pricing Laws," by Michael Batty and Benedic Ippolito

Appendix A: Differences in FPL Provisions

Although the FPLs we study are similar, there are several generalizable differences. The first is how the laws cap prices. Capping prices at (100-115 percent) 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,[1] 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 A1 summarizes these FPL provisions by state.

Table A1—Fair pricing laws by state

State	Year enacted	Percent of poverty level covered	Percent of uninsured covered	Maximum collection amount	Free care below X percent of poverty
MN	2005	~500	86	Largest private payer	NA
NY	2007	300	76	Highest vol. payer	100
CA	2007	350	81	Highest price public payer	NA
RI	2007	300	77	Private payers	200
NJ	2009	500	87	115 percent of Medicare	200
IL	2009	~600	~95	135 percent 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 percent 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

[1] For instance, Melnick and Fonkych (2008) show that in 2000-2005, private insurers in California paid around 40 percent of charges, where public insurers paid around 20 percent.

revenue. This suggests PPS-based FPLs would produce stronger reductions in care. Similarly, mandating free care to low income patients will also give a hospital a stronger reason to reduce care.

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

To investigate, we estimate a difference-in-differences model with dummy variables for any type of FPL, PPS-based FPL, and FPL with free care. The basic FPL dummy measures the effect of a generic FPL common to all states, while the other two dummies measure the additional effects of the two law provisions. Table A2 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.[2] 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.

[2]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.

Table A2—Comparing reductions in lengths of stay by FPL provision

	Outcome Variable: Length of Stay	
Risk Adjustment:	DRG Weight	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
	[0.019,0.257]	[-0.114,0.143]
Observations	3134363	3134363

Note: Data are from the NIS. Estimates are based on Equation 1. Standard errors are clustered at the state level. CIs are reported in brackets. * p <0.10, ** p<0.05, *** p<0.01. All models include hospital, year, and season fixed effects. Patient demographics included in both models.

Appendix 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.

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

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

Table C1—Summarizing hospital charges and percentage of list price paid by payer-type

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

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

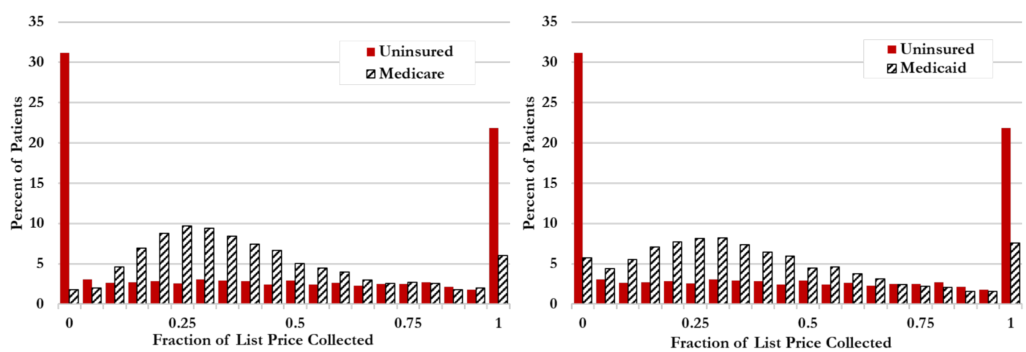

Figure C1. Distribution of percentage of list price paid for by publicly insured and uninsured patients

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

Appendix D: Fair Pricing Laws and Strategic Diagnosing

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

The relevant outcome variable for studying upcoding is the DRG weight. As described earlier, this weight represents the expected cost of treating a patient within that DRG, and is directly related to the amount Medicare will reimburse. Panel A of Figure D1 shows that unlike in other settings where hospitals have a similar incentive, FPLs do not induce upcoding for uninsured patients.[3] 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.[4] Unlike the DRG, the APR-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 D1 shows that using the finer measure, patients have been diagnosed with approximately 4 percent less severe conditions after enactment of fair pricing laws.[5] 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).[6] 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.

[3] 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.

[4] All-patient refined (APR) DRGs were developed to better suit the non-Medicare population, and are in use by Medicaid and quality reporting in some states.

[5] 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.

[6] 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.

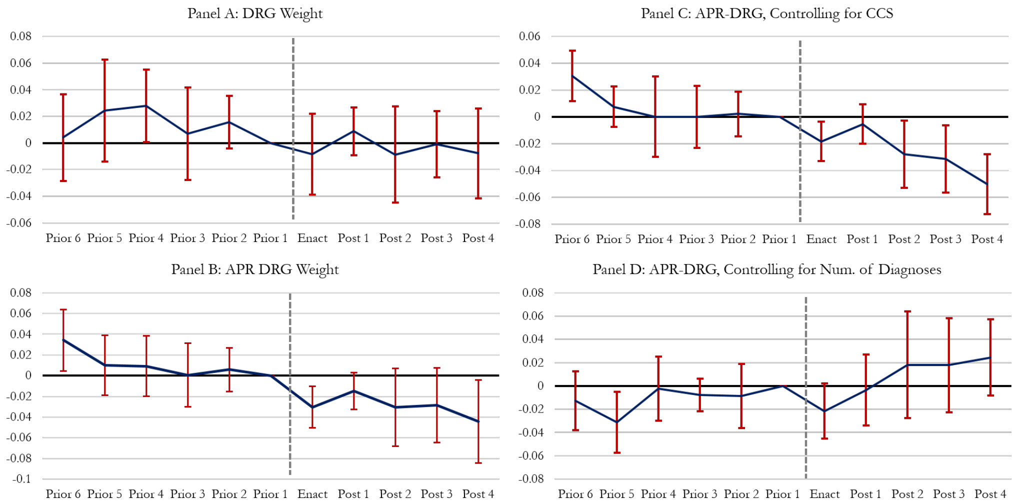

Figure D1. The Effect of FPLs on Strategic Diagnosing

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

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

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

Appendix E: Impact of FPLs on Hospital List Prices

In this section we investigate whether FPLs have any impact on hospital list prices (or "Chargemaster" prices). As outlined in the introduction to this paper, list prices have risen substantially over time, and are the basis by which uninsured patients are initially billed. This has 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 log of list price markup (or ratio of list price to costs) is the outcome variable. These price-to-cost ratios are provided by AHRQ, but are originally derived from CMS Cost Reports. Since they are reported at the hospital level, we collapse our data to the hospital-year level for this exercise. As before, standard errors are clustered at the state level. Hospital and year fixed effects are included, but seasonal fixed effects are dropped since list price data are provided annually. Average pre-treatment ratio of list price to cost is 2.9.

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

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

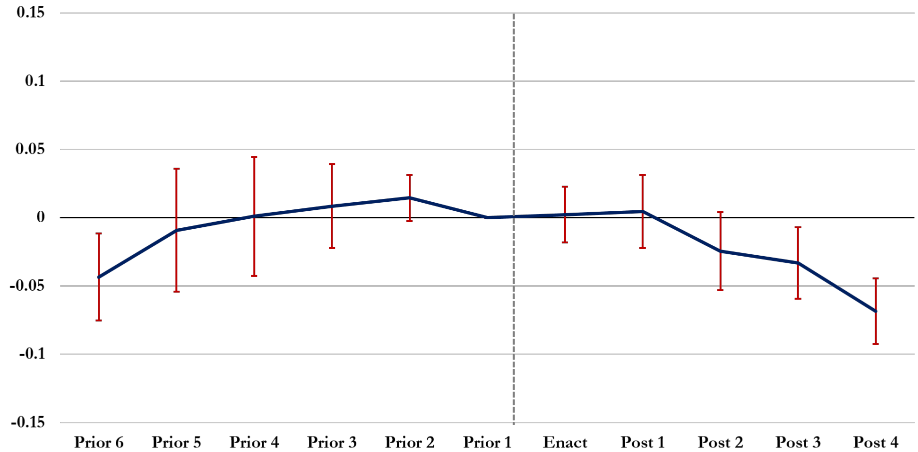

Figure E1. The Effect of FPLs on List Price Markups

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

Appendix F: Robustness Checks

F1. Placebo Test

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

$$LOS_i = \alpha + \delta \cdot \text{PlaceboFPL}_i + \beta X_i + \mu_{h(i)} + \gamma_{t(i)} + \chi_{q(i)} + \epsilon_i, \tag{F1}$$

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

In Figure F1 we report the distribution of estimated coefficients. The actual diff-in-diff estimate of -0.187 is labelled as the "true treatment estimate." The distribution of placebo estimates is centered close to zero and our true estimate falls in the lower tail. In only 1.2 percent of cases (6 instances) do we observe more negative placebo estimates, and in all such cases the placebo treatment states include actual treatment states.

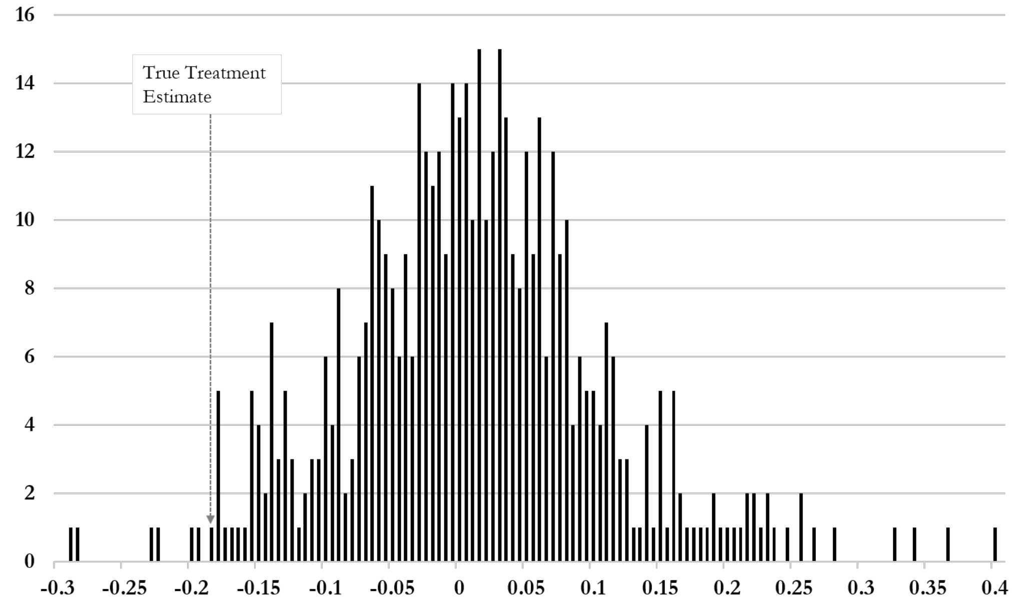

Figure F1. Effect of FPLs on Length of Stay for Uninsured Patients - Placebo Estimates

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

F2. Individual State Treatment Effects

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

Figure F2 illustrates the effect of FPLs on length of stay for each of our treated states. For clarity we only include point estimates. Note that because of differential timing of the laws, we do not observe each state for the same number of years surrounding enactment. Given the large difference in magnitude, we have graphed the Rhode Island estimates on the secondary axis. Predictably, the individual estimates are noisier, but the observed reductions in length of stay are generally similar across treated states. Notably, our largest treatment state - California - does not have an unique or unusually large response. The overall effects reported in the main text are not driven by a single state or subset of treated states. The consistency of the results across states also helps reduce the likelihood that our effects are caused by the adoption of other concurrent state policies.

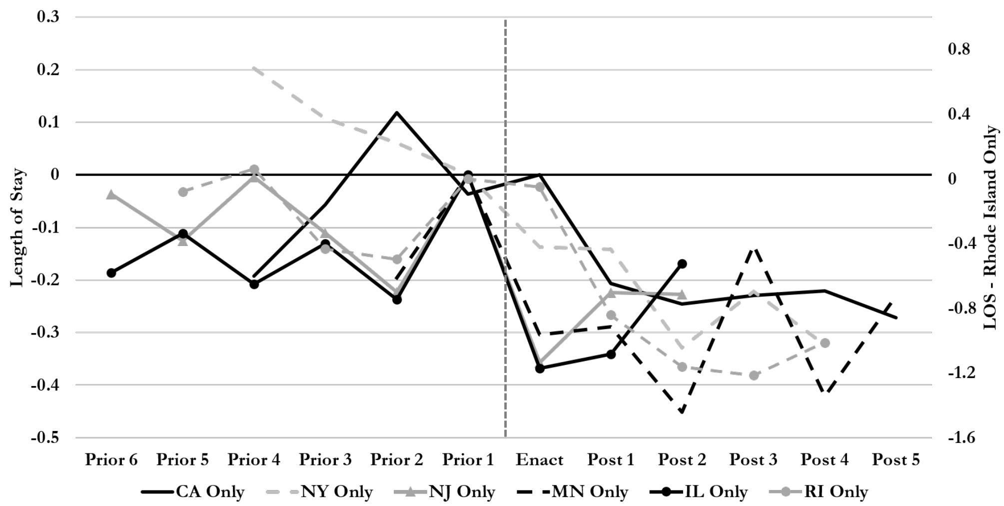

Figure F2. Changes in Length of Stay for Uninsured Patients in Individual Treatment States

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

F3. 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 F3, the results are comparable to our main specification. By the end of our analysis window, fair pricing laws are associated with a 8 percent reduction in the average length of stay.

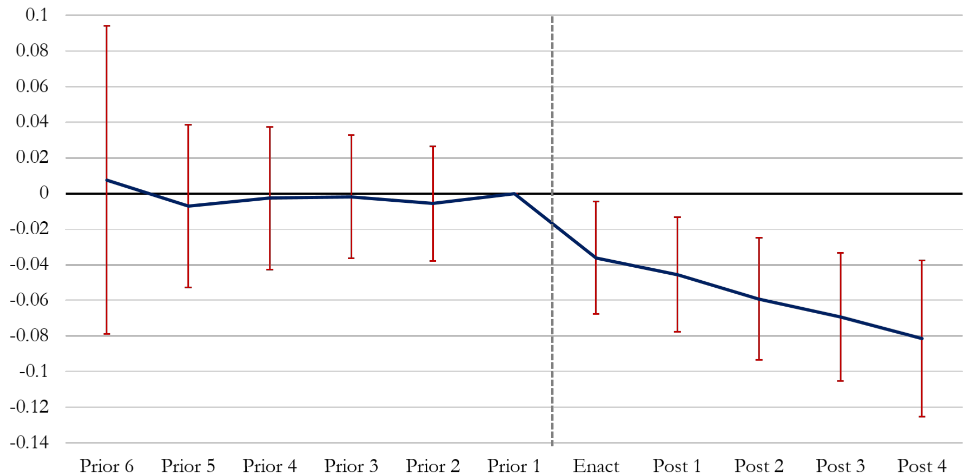

Figure F3. The Effect of Fair Pricing Laws on Length of Stay Using a Poisson Regression Model

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

F4. Alternative measures of care quantity

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

Total charges

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

Admission decisions

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

Transfers

Hospitals may attempt to reduce the burden of unprofitable patients who still require medical care by transferring them to other facilities. There are some restrictions on hospital transfer behavior. Most notably, EMTALA and various state laws prohibit transfers of medically needy patients that are driven by purely by financial considerations of the hospital. However, these guidelines allow for transfers for various health reasons. Even within legislated guidelines it is possible that hospitals are able to increase the rate at which they transfer uninsured following a FPL.

Results

The results for the alternative measures of care quantity show further evidence of cost-reducing behavior after a fair pricing law is enacted. Panel A of Figure F4 shows that reductions in (ln) total charges are consistent with those for length of stay. In total, charges fell by 7 percent after enactment of the FPL, but the decline appears to grow in magnitude over time and reaches nearly 9 percent in the later years of our sample.

Panel B show that the yearly treatment effects for potentially preventable admissions are consistently negative in the years following enactment of an FPL, though not individually significant. However, the diff-in-diff results indicate a 3 percent drop in preventable admissions (significant at the 5 percent level). This is consistent with the notion that hospitals will be more likely to treat plausibly "borderline" cases in a less costly outpatient setting after passage of an FPL. It is worth noting that these cases are relatively rare. These patients make up roughly 12 percent of admissions, meaning our point estimates would translate to a 0.36 percent reduction in overall admissions of uninsured patients. The results shown in Figure 4 indicate little evidence of a change in the overall fraction of uninsured patients admitted, but the effect measured in this section would fall within the reported confidence intervals.[7]

[7] It is worth quantifying how much this level of selection could bias our results. Under the assumption that these foregone admissions were as healthy as possible (i.e. would have been an admission of zero

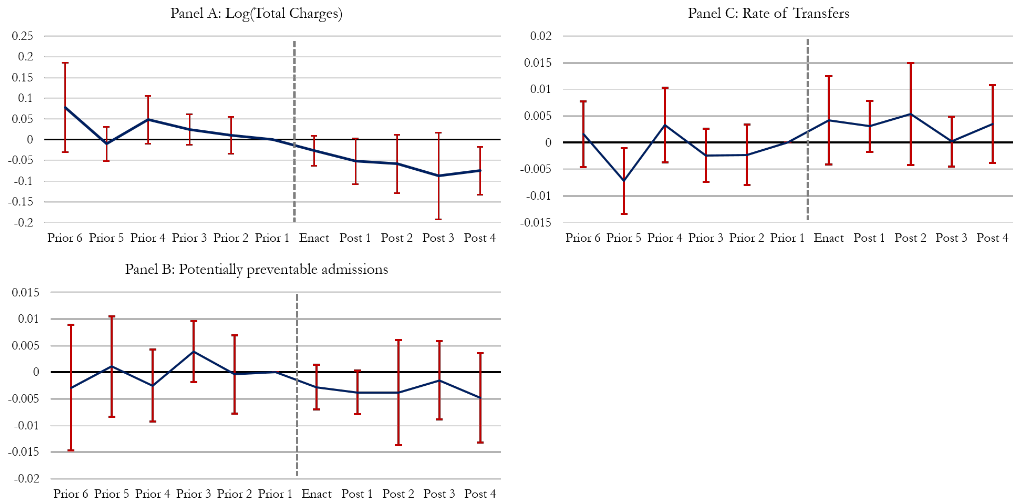

Figure F4. Alternative Margins of Hospital Response

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

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

days), length of stay following a FPL would be 0.4 percent shorter. While these patients would likely be relatively healthy, we also repeat this exercise assuming they would have been in the 95th percentile of length of stay (which corresponds to a roughly 10 day admission). This corresponds to a 0.92 percent increase in our post treatment length of stay. To put this in perspective, by two years post enactment, our treated estimates correspond to a decrease of roughly 7.5 percent in length of stay. Our treated estimates would be roughly 12 percent smaller in this scenario. It is worth noting that this type of selection would also have to occur in a way that is not captured by our risk adjustment strategy. This is not impossible, but does reduce the likelihood of large scale selection effects.

Table F1—The effect of fair pricing laws on various indicators of quantity of care delivered to uninsured patients

Outcome:	Ln(Total Charges)	Frequency of Preventable Admissions	Frequency of Transfers
Diff-in-Diff			
FPL In Effect	-0.07***	-0.004	0.005*
State clusters	[-0.106,-0.035]	[-0.008,0.0002]	[-0.004,0.013]
Observations	3085220	2677557	3118923
States	41	41	41

Note: Data are from the Nationwide Inpatient Sample for years 2003-2011. Estimates are based on Equation 1. Standard errors are clustered at the state level for yearly effects. CIs are reported in brackets. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix G: Hospital Characteristics in FPL States

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

To address this concern, we first compare the set of hospitals driving our treatment estimates to other hospitals from FPL states along a number of dimensions that could conceivably impact responsiveness to FPLs. Table G1 shows that across a number of hospital characteristics, the sample of hospitals driving our treatment estimates look similar to the rest of the hospitals from treated states. This evidence suggests that our main identifying hospitals are largely representative of hospitals from their states.

Another way to address this issue is to re-estimate our main specification using the trend weights provided by AHRQ. These weights are used to adjust for the complex sampling structure of the NIS and produce nationally representative estimates. Figure G1 illustrates the effect of FPLs on length of stay utilizing the NIS sampling weights. The estimated model includes a full set of controls and risk-adjusters (as in model (3) from Table 4). Reassuringly, the results are similar to the main results presented earlier in Figure 6.

Table G1—Comparing "Identifying" and "Non-Identifying" Hospitals in Treatment States

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

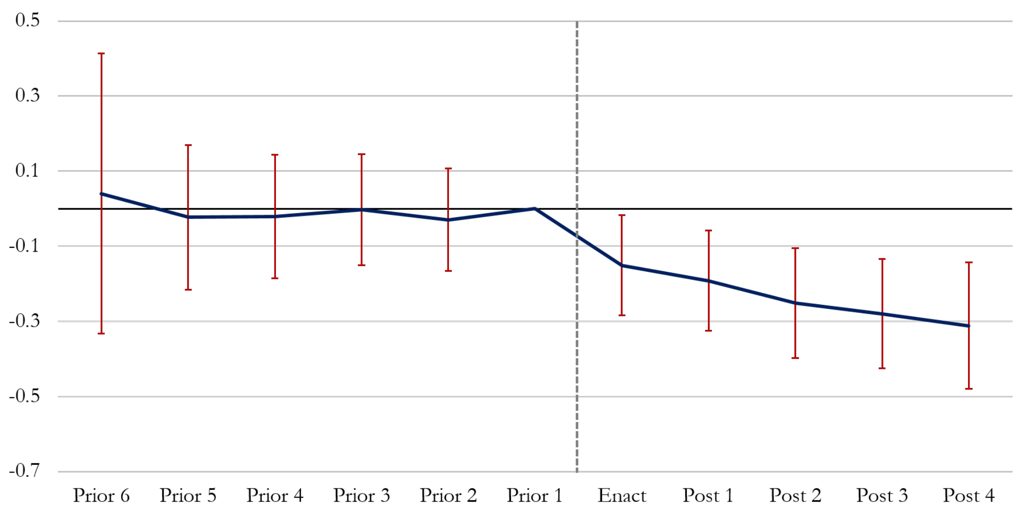

Figure G1. The Effect of Fair Price Laws on Length of Stay Using Sample Weights

Note: This figure illustrates the impact of fair pricing laws on lengths of stay for uninsured patients with the use of sample weights. Data are from the NIS. Estimates are based on Equation 1, but with the addition of sample weights We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "1 year prior to enactment," so that coefficient has been set to zero. The vertical bars indicate 95 percent 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.

Appendix H: Patient Severity and Procedure Type

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

Table H1—The Relationship Between FPLs and Types of Procedures by Patient Severity

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

Note: Data are from the California State Inpatient Database and estimates are based on Equation 2. Standard errors are clustered at the hospital level and 95 percent confidence intervals are reported in brackets. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. All models include hospital, year, and season fixed effects, as well as patient demographic controls, and risk adjusters. See the footnote of Table 4 for a full list of controls. Average DRG weight: 0.93, standard deviation of DRG: 1.0.

Appendix I: Regression Table for Quality of Care

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

Table I1—The effect of fair pricing laws on various quality metrics

Panel A: Mortality measures

Outcome Variable:	Mortality From Selected Conditions	Mortality From Selected Conditions	Mortality From Any Condition
Risk-Adjustment Strategy:	AHRQ Expected Mortality	Primary CCS	Primary CCS
FPL in effect	-0.0040**	-0.0065**	0.0002
	[-0.0077,-0.0003]	[-0.0112,-0.0019]	[-0.0012,0.0017]
Observations	276477	276477	3142717

Panel B: Non-mortality measures

Outcome Variable:	Frequency of Beneficial Procedures	Frequency of Preventable Complications	Frequency of Preventable Complications
Risk-Adjustment Strategy:	Primary CCS	AHRQ Predicted Frequency	Primary CCS
FPL in effect	0.0121	0.0001	0.0001
	[-0.0131,0.0373]	[-0.001,0.0016]	[-0.0010,0.0013]
Observations	146715	2551837	2551837

Note: Data are from the Nationwide Inpatient Sample for years 2003-2011. Estimates are based on Equation 1. Standard errors are clustered at the state level and 95 percent confidence intervals are reported in brackets. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Pre-treatment means: Mortality for selected conditions: 4.1 percent; Mortality for all conditions: 1.3 percent Beneficial procedures: 50 percent, Complications: 0.54 percent.

Appendix J: Quality Metrics

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

Mortality from selected conditions and procedures

Selected Conditions	**Selected Procedures**
Acute Myocardial Infarction	Esophageal Resection
Heart Failure	Pancreatic Resection
Acute Stroke	Abdominal Aortic Aneurysm Repair
Gastrointestinal Hemorrhage	Coronary Artery Bypass Graft
Hip Fracture	Percutaneous Coronary Intervention
Pneumonia	Craniotomy
	Hip Replacement

Use of procedures believed to reduce mortality

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

Incidence of potentially preventable in-hospital complications

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

Potentially preventable hospital admissions
((A) acute conditions, (C) chronic conditions)
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)

Appendix K: Results for CDC Death Rates

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

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

The results are illustrated in Figure K1. Panel A includes all counties, while panel B restricts attention to only those with high uninsured rates. In neither do the estimates suggest FPLs cause a systematic meaningful change in mortality rates outside of hospitals for these high-risk conditions.

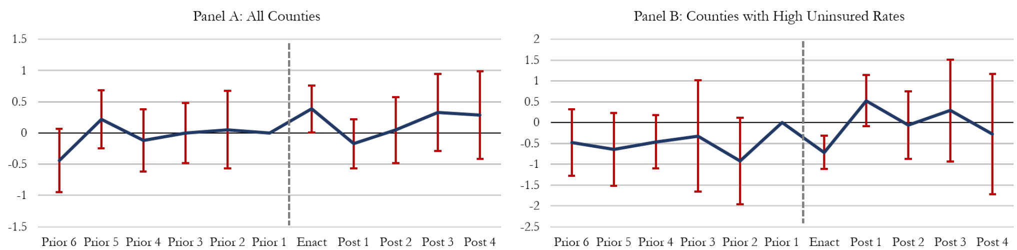

Figure K1. CDC Death Rates Surrounding FPLs

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