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

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State laws that limit how much hospitals are paid by uninsured patients provide a unique opportunity to study how financial incentives of health care providers affect the care they deliver. We estimate the laws reduce payments from uninsured patients by 25–30 percent. Even though the uninsured represent a small portion of their business, hospitals respond by decreasing the amount of care delivered to these patients, without measurable effects on a broad set of quality metrics. The results show that hospitals can, and do, target care based on financial considerations, and suggest that altering provider financial incentives can generate more efficient care. (*JEL* G22, H75, I11, I13, I18)

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

After a hospital visit, patients typically receive a bill showing three different prices for each service: the official list price, the price negotiated by the insurer (if applicable), and the amount remaining for the patient. As recently as the late

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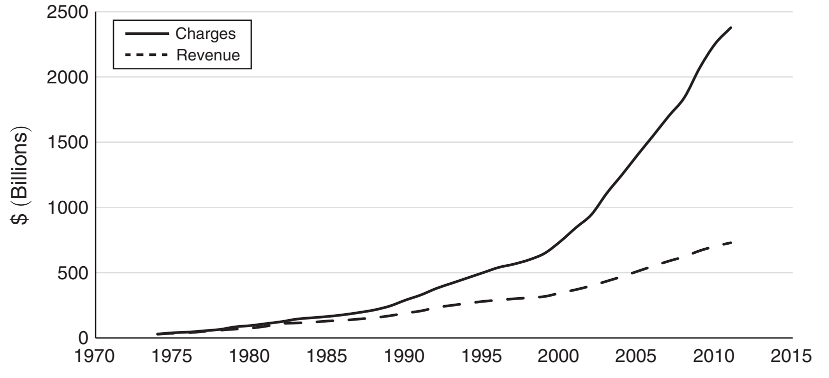

Figure 1. Charges and Revenues for US Hospitals, 1974–2012

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

1970s, hospitals typically collected the full list price for the services delivered. In the years since, list prices have increased substantially, and now bear little relationship to either hospital expenses or payments made on behalf of insured patients (Tompkins, Altman, and Eilat 2006). As depicted in Figure 1, while hospital spending has increased rapidly (9 percent annually), it has been far exceeded by growth in charges (12.4 percent annually).

While insured patients benefit from the negotiated discounts, the uninsured are typically billed 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 for the same services from a public or large private insurer. Thus, FPLs create competing incentives for care delivery by reducing both the price to the consumer and the payment to the provider. This allows us to determine whether overall changes in care are dominated by patient versus provider responses to the changing financial incentives.

We first use the Medical Expenditure Panel Survey and hospital financial data to establish that FPLs do impose binding price ceilings for uninsured patients. We estimate that the price for hospital care for the average uninsured patient falls by 25 to 30 percent. We then use data from the Nationwide Inpatient Sample, in an event study framework, to show that hospitals substantially decrease the amount of inpatient care delivered to uninsured patients in response. The introduction of a FPL leads to a 7 to 9 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

[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.

2005). The effects we observe also illustrate how provider behavior can generate the type of insurance-based care disparities that have been well documented (e.g., Levy and Meltzer 2008).

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

High and seemingly arbitrary hospital list prices have garnered significant attention in recent years, are often cited as creating considerable financial distress for uninsured patients (Anderson 2007; Dranove and Millenson 2006; Reinhardt 2006; and Tompkins, Altman, and Eilat 2006), and FPLs appear to be an increasingly popular solution.[2] 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.[3] While evidence has shown that hospitals comply with FPLs (Melnick and Fonkych 2013), ours is the first study of how fair pricing laws affect the amount and quality of health care given to uninsured patients.

In addition, FPLs provide a new and compelling opportunity to study how providers alter care in response to financial incentives, and how this ultimately affects patient outcomes. Our study complements an existing literature that mostly studies Medicare policy changes from the 1980s and 1990s. Much of the evidence comes from the 1983 introduction of the Prospective Payment System (PPS), which moved Medicare from reimbursing hospitals for their costs of providing services (plus a modest margin), to almost exclusively reimbursing hospitals a flat rate based on the diagnoses of a patient. Research suggests it led to relatively large reductions in length of stay and the volume of hospital admissions (Coulam and Gaumer 1992), more patients being treated in outpatient settings (Hodgkin and McGuire 1994, Ellis and McGuire 1993), but no substantive reductions in quality of care (Chandra, Cutler, and Song 2012). Another body of work focuses on more targeted Medicare fee changes, and yields mixed results. Recently, Clemens and Gottlieb (2014) show how area-specific price shocks from a 1997 Medicare rule change lead physicians to increase care and invest more in medical technology, while leaving health outcomes

[2] Twelve states have enacted FPLs thus far, several others are considering legislation, and courts in several more are adjudicating class action lawsuits that could ultimately impose similar restrictions.

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

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

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

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.[6]

Although the income limit varies by state, in each case the vast majority of uninsured patients are covered. Thus, for most of our analysis we will not distinguish between these six different laws. There are several substantive differences, such as whether prices are capped relative to public versus 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 online Appendix A.

I. Price Changes Imposed by Fair Pricing Laws

It is not immediately clear that FPLs impose meaningful (i.e., binding) price ceilings. It is well-known that outside of these laws, hospitals provide discounted or free "charity care" to certain uninsured patients, and struggle to collect payment from others. If instead of mandating new discounts, FPLs primarily formalize those that are already achieved through these less formal channels, we would expect them

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

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

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

Table 1—Fair Pricing Laws by State

State	Year enacted	Income limit as percent of 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

Notes: FPLs cover the facility charge rather than those of separately billing doctors. The facility charge is approximately 85 percent 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 percent of poverty for a family of four, and Illinois sets the cap at 300 percent for rural hospitals.

to have limited effect on hospital behavior.[7] In this section, we analyze several data sources that indicate FPLs do reduce payments by uninsured patients to hospitals on the order of 25 to 30 percent.[8]

A. *Medical Expenditure Panel Survey*

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

The Medical Expenditure Panel Survey (MEPS) is a nationally representative survey of health care use and spending in the United States. Critical to our work, it provides the most reliable publicly available patient-level data about payments from uninsured patients. To improve the reliability of payment data, the MEPS verifies self-reported payments with health care providers when possible.[9] Our sample includes all patients with either public or no insurance in the MEPS between 2000 and 2004[10] who went to the hospital at least once, resulting in 21,168 patient-year observations. Each individual is interviewed five times over two years, but for our

[7] 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.

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

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

[10] The data lack state identifiers so we select this period because it precedes the earliest FPL.

Table 2—Summarizing Hospital Charges and Collections by Payer Type

Insurance status	Count	Mean hospital charges	Mean percentage of list price collected
Public insurance	17,276	$13,046	38
Uninsured	3,892	$5,035	37

Note: These data are from the Medical Expenditure Panel Survey from 2000–2004.

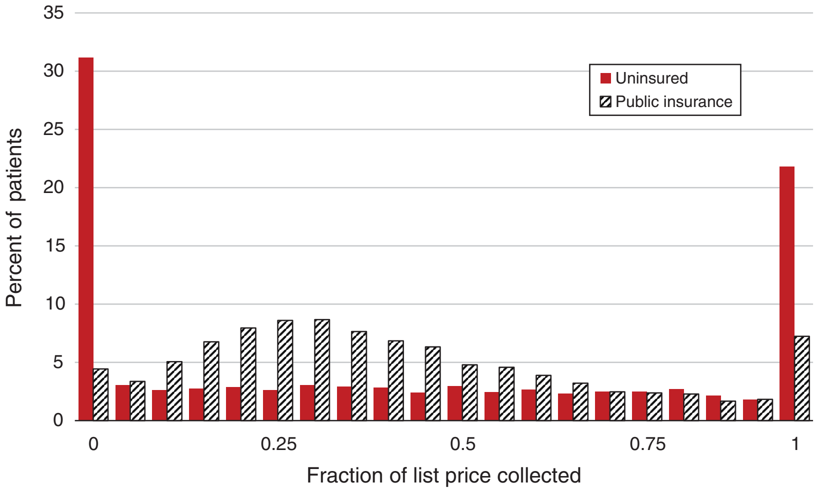

Figure 2. Distribution of Percentage of List Price Paid by Publicly Insured and Uninsured Patients—Excluding High Income Uninsured

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

analysis we ignore the panel structure of the data and pool all year-person observations. We split our sample into two groups: those who had public insurance at some point in the year (Medicare or Medicaid), and those who had no insurance at any point in the year.

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

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.[11] For this exercise, we exclude the highest income uninsured patients who are generally not covered by FPLs, but a version

[11] In online Appendix C, we show that Medicare and Medicaid patients have very similar payment distributions.

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

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

Ideally, we would use these data to compare payments from uninsured patients before and after FPLs are enacted. Unfortunately, the number of uninsured patients who have hospital expenditures in the MEPS is too small to perform this type of state-level analysis.[15] Instead, we can generate a prediction of how much FPLs would reduce payments by approximating the payment cap. Specifically, we match each uninsured patient in our data (excluding those with high enough incomes to not qualify for FPLs) with a publicly insured patient who has a similar bill size.[16] 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. Although this method may overestimate or underestimate the cap for any given uninsured patient, on average it will reflect payments made with caps that are based upon the typical publicly insured patient (as does the

[12] 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.

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

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

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

[16] 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.

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	−0.234	0.0211	0.213	0.084
	(0.00267)	(0.0148)	(0.0106)	(0.00479)
log(charges)	−0.004	−0.022	−0.043	−0.036
	(0.000726)	(0.00121)	(0.00167)	(0.00140)

Notes: 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. The sample size for each regression is 21,168.

modal FPL). In more than 500 simulations of this exercise, the projected payments from uninsured patients fall by an average of 31 percent, or $1,800 per inpatient.[17]

B. *Hospital Financial Data*

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

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

[17] This exercise abstracts from the variety of federal, state, and local programs that pay hospitals for providing uncompensated care. Although a recent estimate finds that in aggregate these programs reimburse two-thirds of uncompensated care (Coughlin et al. 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.

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

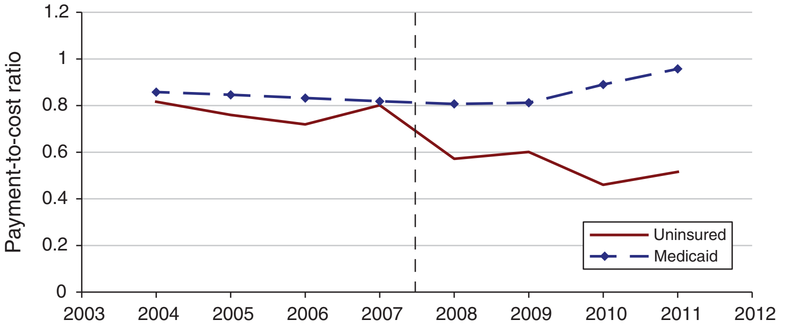

Figure 3. Payment-to-Cost Ratios by Payer in California

Notes: Payments include patient revenue from all sources. Costs include marginal costs and allocated overhead. All dollar figures are nominal.

Source: Data are from California OSHPD financial pivot files.

While California provides unusually detailed financial data, some other states do report uncompensated care (charity care and uncollectable bills). A decline in payments from the uninsured should be reflected in an increase in uncompensated care. However, other payer groups also contribute to uncompensated care, and movements can be further obscured by the rapid increases in charges that we have described previously. Still, compared to Oregon, a neighboring state that did not enact a FPL, California experienced an increase in uncompensated care consistent with Figure 3. This gives us confidence that the change in uninsured prices in California is not driven by factors that affect uninsured patients in non-FPL states, and suggests that FPLs impose meaningful changes to hospital financial incentives.

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

II. Measuring the Impact of Fair Pricing Laws on Hospital Care

A. *Inpatient Records Data*

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

Our primary data source is the Nationwide Inpatient Sample (NIS) developed by the Agency for Healthcare Research and Quality. The NIS is the largest all-payer inpatient care database in the United States. In each year, it approximates a stratified 20 percent random sample of US acute care hospitals (roughly 8 million discharges from 1,000 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 percent of hospitals are sampled each year, each hospital in our data appears an average of 2.3 times between 2003 and 2011. For the bulk of our analysis, we restrict our sample to all inpatient records for uninsured patients from 41 states (including all 6 states with fair pricing laws).[19] This gives us approximately 3.2 million observations.

B. *Empirical Framework*

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

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

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

Y_i is the outcome of interest (such as length of stay, charges, quality of care, or diagnosis); X_i is vector of patient characteristics; μ_h, γ_t, and 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: "one year prior to adoption." Although our primary specification is built upon the $FPL_{L(i)}$ dummies, at times we will also report more traditional difference-in-differences results using a single indicator variable for the presence of a FPL.

The validity of this research design relies on the assumption that outcomes in the treatment and control states would have behaved similarly in the "post-period" absent the introduction of a fair pricing law. Finding 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 throughout the results, the pre-trends we observe imply that the non-FPL states are a valid control group.

[19] Thirty-three states are present in each year of our data, with the other eight beginning to participate in the NIS after 2003. As noted earlier, we exclude Connecticut, Maryland, Maine, and Wisconsin. We also drop Massachusetts because of dramatic changes to their uninsured population after the 2006 health reform. The remaining four states do not share data with the NIS as of 2011.

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

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

To account for potential within-state correlation of outcomes, we cluster standard errors at the state level. However, as outlined in Conley and Taber (2011), this approach still requires the number of treated clusters to grow large in order to produce consistent estimates. This is relevant given that the number of treated clusters in our application is six. In the results that follow, we show that the confidence intervals produced by state-level clustering and the Conley-Taber method of inference are quite similar.

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

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

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

[22] In online Appendix E, we also investigate whether FPLs have any impact on the way hospitals set list prices.

Of course, we are ultimately more concerned with how changes in the amount of care translate into changes in health outcomes. To directly measure care quality, we employ a set of short and longer term quality metrics. For short-term metrics, we use the Inpatient Quality Indicators software package developed by AHRQ. The package calculates a battery of metrics, including in-hospital risk-adjusted mortality from selected conditions and 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 within each category into composite measures. For instance, instead of estimating changes in mortality from each individual condition or procedure, we will measure mortality from any of the conditions or procedures selected by AHRQ. To assess longer term changes in quality of care, we measure readmission rates at 30, 60, and 90 days after discharge.

Risk Adjustment.—Because FPLs may encourage strategic manipulation of diagnoses, we use the Clinical Classifications Software (CCS) categorization scheme provided by HCUP as our primary risk-adjustment method. The CCS collapses the 14,000 ICD-9-CM's diagnosis codes into 274 clinically meaningful categories. For instance, 40 ICD-9-CM codes corresponding to various types of heart attacks are aggregated into a single "Acute myocardial infarction" group. Thus, strategic diagnosing behavior that elevates minor heart attack patients to a more severe heart attack diagnosis, for example, would not change the CCS category for a patient. Controlling for CCS still provides meaningful information about the severity of the health condition, while also providing a buffer against the type of strategic diagnosing described above. Admittedly, this risk-adjustment strategy may miss more granular diagnosis information. To compensate, we also look for changes in the characteristics of the patient population that would suggest systematic changes in diagnosis patterns are driven by real changes in patient composition.

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

[23] Under the Emergency Medical Treatment and Labor Act, hospitals may only begin to inquire about ability to pay after it is clear doing so will not compromise patient care. Reports suggest that some hospitals do pull credit

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

$$(2) \quad Y_i = \alpha + \sum_{L \in K} \delta_L \, Uninsured_{L(i)} + \beta X_i + \eta_{u(i)} + \mu_{h(i)} + \gamma_{t(i)} + \chi_{q(i)} + \epsilon_i, \quad \text{where} \quad K = \{-2, 0, 1, 2\}.$$

Y_i is the outcome of interest; X_i is vector of patient characteristics which contains the same information as in the NIS; η_u, μ_h, γ_t, and χ_q are fixed effects for uninsured, hospital, year, and quarter, respectively; and $u(i)$, $h(i)$, $t(i)$, and $q(i)$ denote the insurance status, hospital, year, and quarter associated with record i.

The set of $Uninsured_{L(i)}$ dummies represent year relative to the enactment of a fair pricing law ($L = 0$ denotes the first year of enactment, which for California is 2007). For example, $Uninsured_{1(i)} = 1$ if record i is from an uninsured patient in 2008, and zero otherwise. Each of the δ_L coefficients is measured relative to the omitted category "one year prior to adoption." Equation (2) illustrates the event study specification, though we will often replace the yearly treatment dummies with a single difference-in-difference dummy for being uninsured after the FPL.

The most important difference between this specification and the one estimated with the NIS is the control group. Because these data only cover California, we cannot compare uninsured in California to uninsured in other states. Instead, we compare uninsured to the most similar insured group in the state: Medicaid patients. Identification of our treatment effects comes from comparing uninsured to Medicaid patients within the same hospitals over time. Finally, standard errors are clustered at the hospital level.

C. *Investigating Changes in Patient Composition*

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

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

reports for patients to inform collections efforts, though some advocates argue this practice may affect provision of care (see "Why Hospitals Want Your Credit Report" in the March 18, 2008 issue of the *Wall Street Journal*).

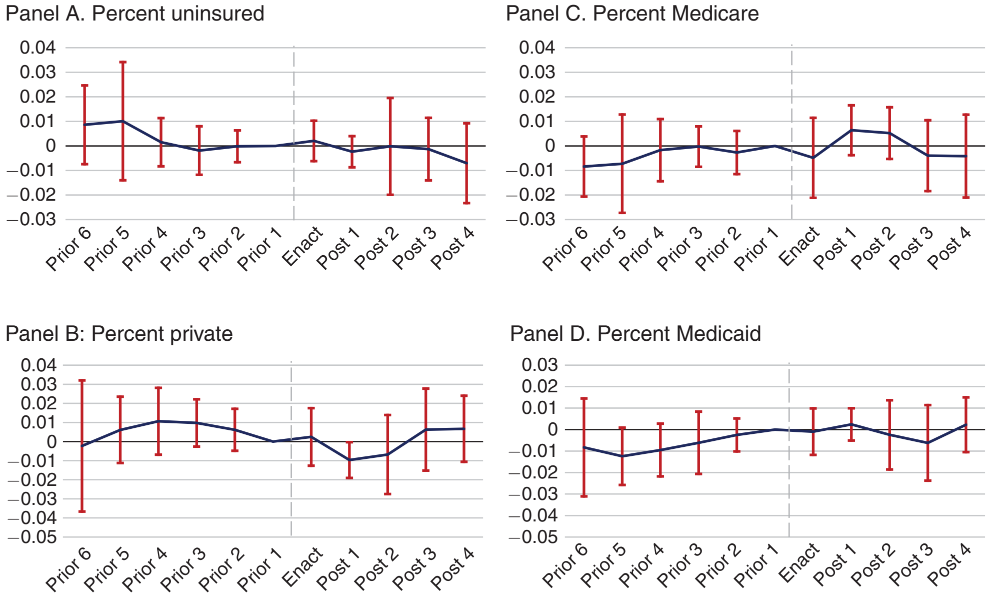

FIGURE 4. THE EFFECT OF FAIR PRICING LAWS ON THE SHARE OF INPATIENTS' STAYS ACCOUNTED FOR BY INSURANCE TYPE

Notes: We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "one year prior to enactment," so that coefficient has been set to zero. Standard errors are clustered at the state level and are illustrated by the vertical lines. Pretreatment means: Medicare: 41 percent; Medicaid: 19 percent; private: 33 percent; and uninsured: 5 percent.

are not associated with significant changes in the share of uninsured inpatient stays at hospitals. In the first two years under a FPL, we can rule out changes larger than 1 percentage point. The precision of these estimates is generally lower in later years, though coefficients remain small. In panels B, C, and D of Figure 4, we report estimates for patients with private insurance, Medicare, and Medicaid, respectively. Overall, we see little evidence that FPLs systematically change the payer mix of patients that are admitted to hospitals.

While the number of uninsured treated is stable, it is possible that the underlying composition of the uninsured is affected by FPLs. In Figure 5, we show the effect of FPLs on a number of observable characteristics of the uninsured admitted to hospitals. For context we also include estimates for the insured sample.

Panels A and B show the effect of FPLs on the average age of patients and fraction nonwhite. In both cases, the coefficients for insured and uninsured are generally similar. Moreover, in neither case do we see systematic shifts among the uninsured following enactment. The NIS does not include individual-level income, but does include a categorical variable indicating where the median income of a patient's home zip code falls in the national distribution (specifically, which quartile). Panel C shows the fraction of patients who are from a zip code with a median income in the top quartile. There is a consistent small increase in patients from higher income zip codes in treated states, though the trend appears to predate FPLs and occurs both for insured and uninsured. Particularly with the uninsured, treated

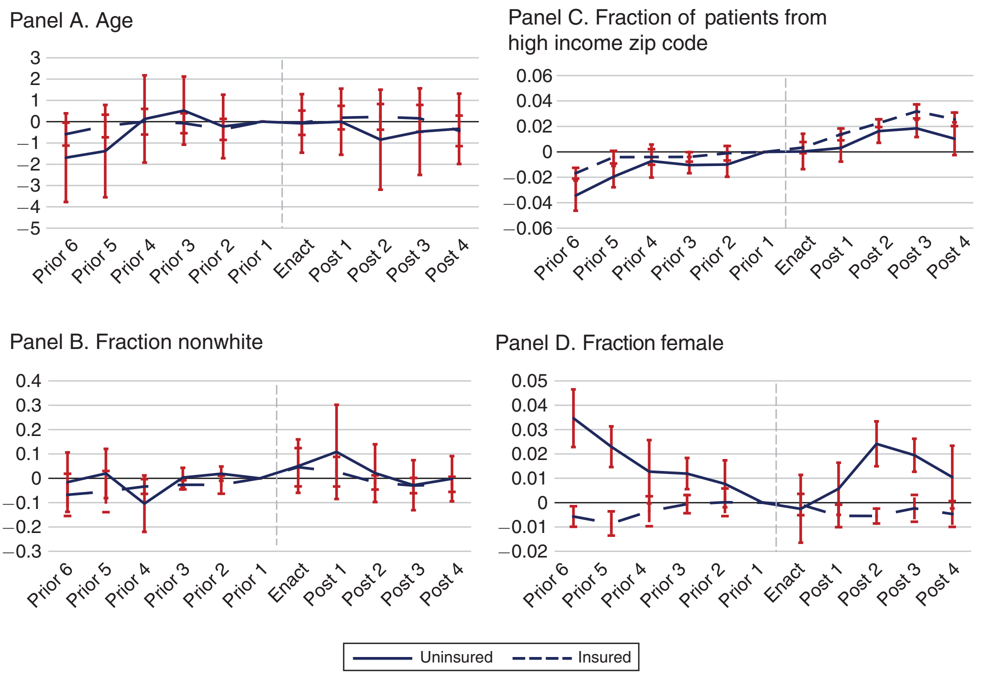

Figure 5. The Effect of Fair Pricing Laws on the Composition of Admitted Patients

Notes: We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "one year prior to enactment," so that coefficient has been set to zero. Standard errors are clustered at the state level and are illustrated by the vertical lines. Pretreatment means: age: 35.1; fraction nonwhite: 0.448; fraction from high income zip: 0.23; and fraction female: 0.48.

states were trending differently prior to enactment. Finally, the fraction of female uninsured in treated states is somewhat noisy. We observe positive coefficients in a few post-years, though the same is true of most prior years as well. Overall, we observe some changes in the characteristics of the uninsured in treatment states, though there is little indication that FPLs directly cause these shifts. We will revisit this compositional issue in the next section where we report regression results with and without controls for characteristics of the patient population.

III. Results for Quantity of Care

A. *Length of Stay*

We now test whether FPLs induce hospitals to engage in cost-reducing behavior through shortened lengths of stay for uninsured patients. The results are reported in Table 4. Model (1) reports our yearly treatment effects with no demographic or risk adjustment. In model (2), we include demographics, while model (3) we include CCS-based risk-adjusters and demographics. Standard errors are clustered at the state level.

By excluding all patient-level controls in model (1) we are measuring how FPLs affect length of stay, without attempting to control for any potential changes in the

Table 4—The Effect of FPLs on Length of Stay for Uninsured Patients

	Outcome variable: Length of stay Pretreatment mean: 4.08 days		
	No controls (1)	Demographics (2)	Demographics and risk adjustment (3)
Prior 6	−0.0992 [−0.311, 0.113]	−0.0327 [−0.196, 0.131]	0.00891 [−0.139, 0.157]
Prior 5	−0.102 [−0.336, 0.131]	−0.0488 [−0.235, 0.137]	−0.0144 [−0.138, 0.110]
Prior 4	−0.0571 [−0.281, 0.167]	−0.0493 [−0.254, 0.155]	−0.0288 [−0.190, 0.132]
Prior 3	−0.0323 [−0.166, 0.101]	−0.0464 [−0.191, 0.0982]	−0.00367 [−0.114, 0.107]
Prior 2	−0.0829 [−0.320, 0.154]	−0.0798 [−0.291, 0.132]	−0.0373 [−0.200, 0.125]
Enactment	−0.217 [−0.431, −0.002]	−0.219 [−0.397, −0.041]	−0.156 [−0.306, −0.007]
Post 1	−0.265 [−0.401, −0.130]	−0.268 [−0.375, −0.161]	−0.195 [−0.263, −0.128]
Post 2	−0.362 [−0.540, −0.185]	−0.333 [−0.470, −0.196]	−0.246 [−0.363, −0.129]
Post 3	−0.292 [−0.433, −0.150]	−0.293 [−0.417, −0.170]	−0.277 [−0.373, −0.182]
Post 4	−0.385 [−0.636, −0.134]	−0.372 [−0.591, −0.153]	−0.319 [−0.473, −0.165]
Observations	3,143,772	3,143,772	3,143,772

Notes: Estimates are based on equation (1). Standard errors are clustered at the state level, and 95 percent confidence intervals are reported in brackets. All models include hospital, year, and season fixed effects. Patient demographics included in all regressions: 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.

types of uninsured being admitted. Model (3) offers a more "apples-to-apples" comparison by measuring how hospitals treat observably similar patients before and after a FPL. Comparing results across models reveals the importance of any changes in patient attributes over time.

Across the models we do not see significant effects prior to the enactment of fair pricing laws, indicating that our treated and control states were trending similarly prior to the introduction of a FPL. In the years post-adoption we see clear and systematic evidence of reduced lengths of stay in the treated group. The magnitudes grow in the first years after enactment, which suggests that hospitals may be slow to react to FPLs, and/or hospitals learn tactics to shorten hospital stays over time.

The size of the treatment coefficients typically reduces slightly with the addition of more controls, though the estimates in model (1) fall within the confidence intervals of model (3). This is consistent with the analysis presented in the previous section—changes in composition of the uninsured are unlikely to be driving the results. Focusing on column 3, towards the end of our sample hospital stays for uninsured patients have fallen around 0.3 days, or about 7.5 percent. It is worth noting that the

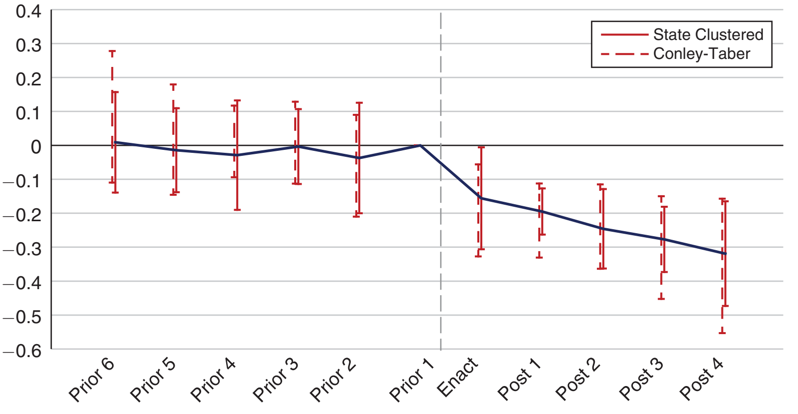

Figure 6. The Effect of Fair Pricing Laws on Length of Stay for Uninsured Patients

Notes: This figure illustrates the effect of FPLs on length of stay for uninsured patients and is based on model (3) from Table 4. Data are from the Nationwide Inpatient Sample. We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "one year prior to enactment," so that coefficient has been set to zero. The solid and dashed vertical lines indicate the 95 percent 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.

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 Prospective Payment System (PPS), which was generally considered to have a large impact on length of stay. In their literature review, Coulam and Gaumer (1992) highlight an example of a nearly 10 percent drop in length of stay in the year after the PPS. Since stays were falling in the years leading up to the PPS, though at a much lower rate, this appears to be a reasonable upper bound on the effect size. In that light, the effects we see from fair pricing laws are substantial.

In Figure 6, we illustrate the results from the specification including all demographics and CCS-based risk-adjusters. We show confidence intervals generated by state clustering and by the Conley-Taber procedure. The figure shows that the reduction in LOS is robust to the use of either method. This 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.[24] We also focus on model (3) for the remainder of our results because it is qualitatively similar to our other models.

In online Appendix F, we reestimate model (3) for each treatment state individually to investigate whether the overall effects are driven by a subset of FPL states. The reported estimates are predictably noisier, but show similar reductions in length

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

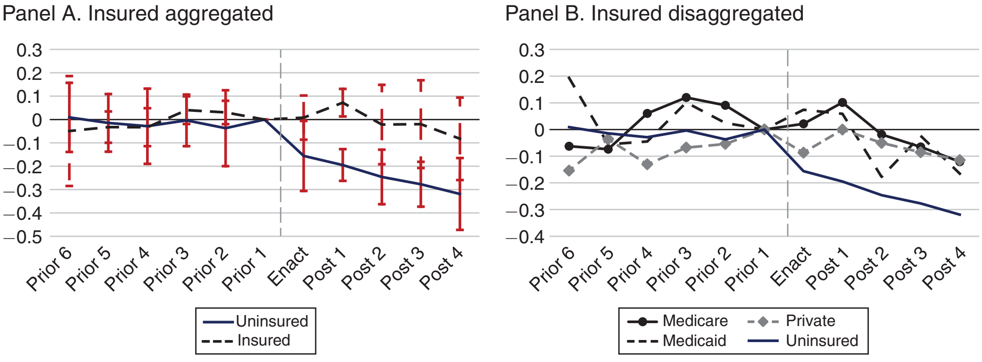

Figure 7. The Effect of Fair Pricing Laws on Length of Stay for Uninsured and Insured Patients

Notes: This figure illustrates the impact of fair pricing laws on lengths of stay for insured and uninsured patients. Data are from the NIS. Estimates are based on estimating equation (1) for each payer type. In both panels, the solid line with no markers illustrates uninsured patients. The dotted line in panel A represents all insured patients. In panel B the various insured groups are labeled. We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "one 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. Pretreatment average length of stay: uninsured: 4.08; insured (overall): 4.87; Medicare: 6.2; Medicaid: 4.69; and private: 3.73.

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

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

The fact that treatment patterns clearly diverge following a FPL provides evidence that hospitals can target treatment changes based on individuals' insurance status. This finding is in contrast to work like Glied and Graff Zivin (2002) which finds that the overall composition of insurance types affects provider behavior, but the insurance type of an individual patient has limited impact.

Table 5—Hospital Characteristics and Reactions to Fair Pricing Laws

	Length of stay	Length of stay
FPL in effect	−0.196	−0.162
	[−0.294, −0.099]	[−0.234, −0.091]
FPL in effect × for-profit	0.007	
	[−0.135, 0.150]	
FPL in effect × high pct. uninsured		−0.039
		[−0.110, 0.032]
Observations	399,444	3,143,772

Notes: Column 1 uses data from the California SID to estimate equation (2). Column 2 uses data from the NIS and estimates equation (1). Ninety-five percent confidence intervals are reported in brackets. All models include hospital, year, and season fixed effects, as well as patient demographic controls and risk-adjusters. Mean percent of uninsured patients per hospital is 4.9 percent with a standard deviation of 5.9 percent. The high percent uninsured hospitals are those for which the percentage of patients that are uninsured is above the mean for the sample.

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

Column 1 of Table 5 reveals no evidence that for-profit hospitals shorten lengths of stay for uninsured patients differently than do nonprofits. This is broadly consistent with prior work documenting limited differences between for-profit and nonprofit hospitals, such as in their provision of uncompensated care (Sloan 2000).

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

B. *Where Do Hospitals Reduce Care?*

FPLs alter the care that hospitals are willing to provide uninsured patients, but presumably, providers that value the health of their patients will target care reductions where they will be least harmful. Such a phenomenon has been illustrated in

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

Table 6—The Relationship between FPLs and Length of Stay by Patient Severity

	Length of stay	Length of stay
FPL in effect	−0.334 [−0.511, −0.138]	−0.256 [−0.357, −0.133]
FPL in effect × APR DRG weight		0.144 [0.0137, 0.270]
FPL in effect × DRG weight	0.171 [−0.0107, 0.352]	
Observations	3,132,371	3,135,532

Notes: Data are from the Nationwide Inpatient Sample and estimates are based on equation (1). Standard errors are clustered at the state level and 95 percent confidence intervals are reported in brackets. All models include hospital, year, and season fixed effects, as well as patient demographic controls and risk-adjusters. See the footnote of Table 4 for a full list of controls. Average DRG weight: 0.93; average APR-DRG weight: 0.73; standard deviation of DRG: 1.0; standard deviation of APR-DRG: 1.0.

prior literature. For example, Clemens and Gottlieb (2014) find that price shocks affect the provision of elective care considerably more than less discretionary services. In this section, we present results consistent with that ethic. Namely, hospitals focus care reductions on less severe patients and comparatively minor procedures.

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

The results are reported in Table 6. The interactions between the treatment dummy and weight capture the differential change in length of stay under FPLs by patient severity. For reference, the average DRG weight is 0.93 with a standard deviation of 1.0, while the average APR-DRG weight is 0.73 with a standard deviation of 1.0. In both cases, the point estimates suggest that FPLs induce hospitals to cut back care more for less severe patients, though with traditional DRGs the estimate falls just short of 5 percent significance. Interestingly, the estimated interaction terms in models that control for CCS (as presented here) are very similar to those from models that do not. This suggests that hospitals focus their responses to FPLs on the less severe versions of each type of patient they treat, as opposed to implementing a broad reduction in care for the less severe CCS categories.

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

Table 7—The Relationship between FPLs and Types of Procedures Delivered

	Minor		Major	
	Diagnostic	Therapeutic	Diagnostic	Therapeutic
2 years prior	0.026	0.007	0.056	−0.015
	[−0.039, 0.091]	[−0.020, 0.033]	[−0.031, 0.143]	[−0.041, 0.011]
Enact year	0.036	−0.029	0.045	−0.002
	[−0.019, 0.092]	[−0.050, −0.008]	[−0.029, 0.119]	[−0.0312, 0.027]
1 year post	0.037	−0.054	0.040	−0.022
	[−0.038, 0.112]	[−0.082, −0.026]	[−0.048, 0.128]	[−0.052, 0.008]
2 years post	0.028	−0.079	0.066	−0.027
	[−0.066, 0.121]	[−0.117, −0.042]	[−0.019, 0.151]	[−0.059, 0.006]
Observations	5,411,088	5,428,832	5,386,986	5,390,576

Notes: 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. 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. Pretreatment mean number of procedures per patient: minor diagnostic: 0.38; minor therapeutic: 0.65; major diagnostic: 0.015; major therapeutic: 0.35.

Studying procedures using the NIS is problematic due to data reporting inconsistencies,[26] but California reports this information consistently in their State Inpatient Database. Focusing on California prevents us from using uninsured patients in different states as controls, so instead we compare the uninsured in California to the most similar insured group in the state: Medicaid patients. Because the number of procedures performed is discrete, we employ a Poisson regression model.

The results in Table 7 indicate that care reductions are concentrated in minor therapeutic procedures. Further, in models shown in online Appendix H that are similar to those in Table 6 and measure differential treatment effects by severity, we find that the positive relationship between number of procedures performed and DRG weight becomes stronger after FPLs, suggesting that hospitals are more actively targeting resources to the sicker patients. Consistent with our expectations, this evidence shows that hospitals reduce care where it will likely have the least negative effects.[27]

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

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

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

Using data from all patients for 2003 and 2004 (before any FPL was enacted), we calculate the coefficient of variation for each diagnosis. Diagnosis can differ in this measure because of actual treatment flexibility, or simply because a single diagnosis code may capture a greater range of conditions than another. For this reason, we use very granular diagnosis information—each patient's primary ICD code. Using the more detailed diagnosis code gives a better measure of true variation in LOS for similar patients.

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

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

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

IV. Results for Quality of Care

A. *Short-Term Quality of Care*

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

The QIs were first developed for AHRQ by researchers at Stanford University, University of California-San Francisco, and University of California-Davis in 2002 in an effort to capture quality of care using inpatient records. Since then, they have become a standard in quality assessment, endorsed by the National Quality Forum, and frequently used in research.[28] 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

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

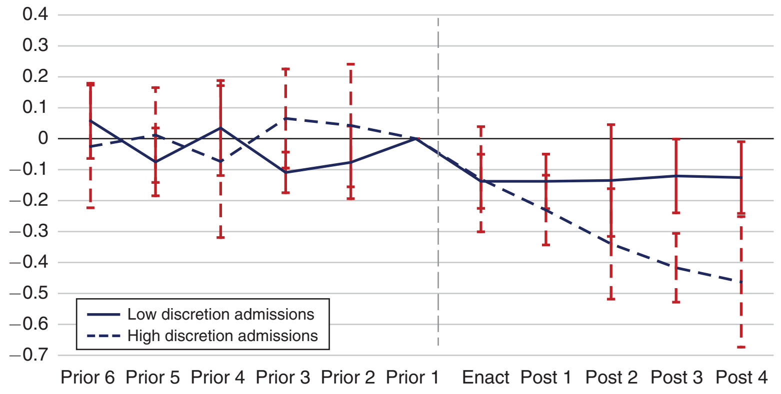

Figure 8. Comparing Changes in Length of Stay for Diagnoses with High and Low Clinical Discretion

Notes: This figure illustrates the impact of fair pricing laws on lengths of stay for diagnoses with high and low discretion for length of stay. Data are from the NIS and are based on estimating equation (1) for each group. We have plotted the coefficients on dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "one year prior to enactment," so that coefficient has been set to zero. The regressions include our full set of fixed effects, patient demographics, and risk-adjusters. See the note on Table 4 for a full list of controls. Pretreatment length of stay: high discretion: 4.6; low discretion: 3.7.

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 of procedures and conditions. We combine these into one mortality rate from any of the procedures and conditions.

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

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. Online Appendix J contains a full list, but examples include acute myocardial infarction (AMI), hip fracture, pneumonia, and hip replacement. The software identifies the appropriate patients in our data, records whether or not they died, and calculates an expected probability of death for each based upon their other diagnoses and demographic information. We include this expected probability of death as a control variable in our model. To take a broader look at mortality, we also estimate our model on the full sample of uninsured patients.

[29] For brevity, we include only graphical event study regression results. Online Appendix I contains the associated diff-in-diff results.

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

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

Incidence of Potentially Preventable In-Hospital Complications.—AHRQ has identified 13 in-hospital complications that may be preventable with better quality of care. Again, online Appendix J includes the full list, but these are issues like postoperative hemorrhage, or accidental puncture or laceration. Individually, each event is quite rare: averaging 0.16 percent 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 complication will be inflicted with at least one of them is 0.54 percent. We estimate our model with the frequency of any of these complications as the outcome variable. Much like the mortality metric, the QI software calculates an expected probability of each complication. We include this probability as a control in our model, but the results are similar with or without this variable.

Results for Short-Term Quality Metrics.—Panel A of Figure 9 shows the effect of FPLs on in-hospital mortality for selected procedures. The treatment coefficients are somewhat noisy, but do not appear to show a systematic change following FPLs. Panel B of Figure 9 shows the effect on mortality for the full uninsured population. For the full population, confidence intervals typically fall between 0.004 and −0.004 in the post-period. In-hospital mortality is less common for the overall population (1.2 percent compared to 4.1 percent for the selected conditions), so the confidence intervals on our yearly treatment effects rule out changes in mortality across all admissions of more than 4–5 percent.

The NIS only captures in-hospital mortality, so to further investigate the possibility of deaths occuring outside of the hospital we turn to mortality data published by the Centers for Disease Control and Prevention. Specifically, we study people ages 25–64, and deaths that were not due to an acute trauma (this excludes accidents, homicides, and suicides). In addition, we focus on deaths that occurred outside of hospitals that resulted from several of the most common mortality QI conditions and procedures. We study these populations both for the United States as a whole, and

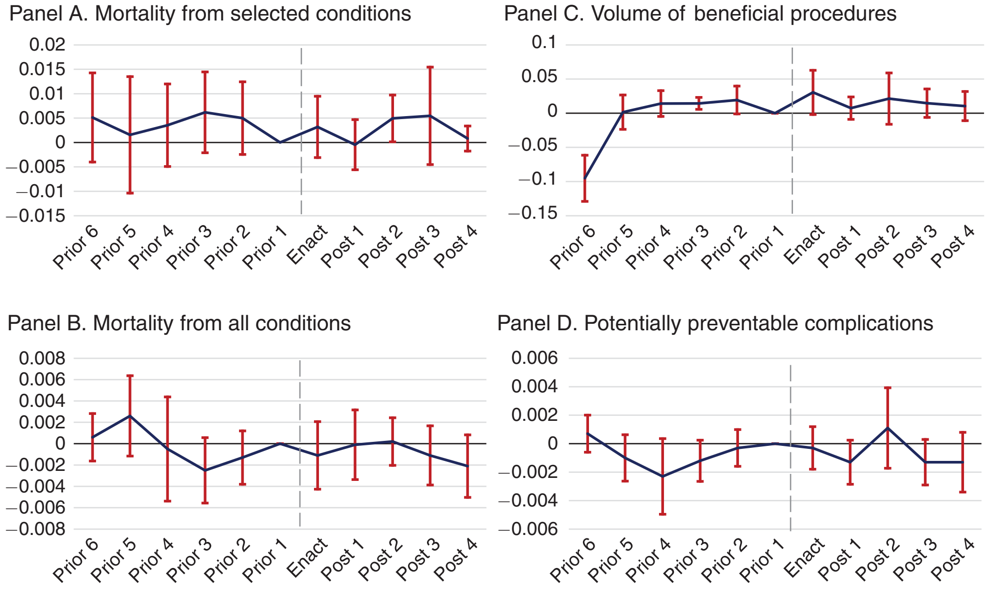

Figure 9. Measures of Quality of Inpatient Care

Notes: These graphs use data from the NIS. Estimates are based on equation (1) where the selected QI metrics as the outcome variables. The omitted dummy is "one year prior to enactment," so that coefficient has been set to zero. Standard errors are clustered at the state level. Pretreatment means: mortality for selected conditions: 4.1 percent; mortality for all conditions: 1.3 percent; beneficial procedures: 50 percent; complications: 0.54 percent.

restricted to counties with more than 25 percent uninsured.[30] Online Appendix K shows the results of this analysis. We do not see evidence that FPLs are followed by a spike in death rates outside of the hospital from these conditions.

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

B. *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

[30] 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 seventy-fifth percentile of uninsurance for 25–64 year-olds at the county level in 2012.

horizon. One way of capturing more subtle differences in care quality, such as potentially inappropriate discharges, is the 30-day, all-cause readmission rate. It is particularly compelling for our study because it could reflect complications or the need for additional care that result from the shortened stays of uninsured patients after the enactment of a FPL.

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

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

Our outcome of interest is the 30-day all-cause readmission. Specifically, a readmission is any stay that occurs within 30 days of a prior discharge for that patient. We study patients with all clinical diagnoses, and include cases where the patient is readmitted to a different hospital.[31]

We study readmissions in the California SID by comparing uninsured patients to Medicaid patients over time as outlined in equation (2). Although the patient populations may differ, those with Medicaid are likely more similar to the uninsured than are any other insured group.[32]

Figure 10 reports the results of this analysis. The small and insignificant treatment coefficients in both the pre-time and post-time periods provide evidence that the California FPL did not increase the rates of readmission for uninsured patients relative to Medicaid patients. The upper end of the confidence intervals in the post-period are between 0.002 and 0.006, meaning we can rule out increases in readmission rates of more than 3 to 6.5 percent in those years (from a base of 8.7 percentage points). The results are similar if we consider 60 or 90 day readmission rates.

In contrast to the results focusing on quantity of care, our study reveals little evidence of systematic changes to quality of care for the uninsured following a FPL. In-hospital quality measures, readmission rates, and out-of-hospital mortality are generally stable surrounding enactment of a FPL. Although precision varies across metrics, taken together, the evidence suggests limited changes in quality of care.

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

[32] We also obtained data containing readmission information from a control state (Washington). However, the patient linkage variables are reported inconsistently in successive years, making it difficult to use for this study. Still, we find similar results when we compare California uninsured to Washington uninsured, or perform a triple difference using the uninsured and Medicaid populations in both states.

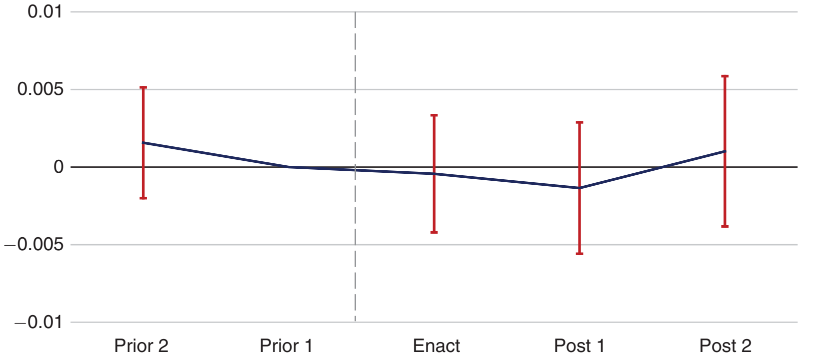

Figure 10. The Effect of Fair Pricing Laws on All-Cause 30-Day Readmission Rates for Uninsured Patients in California

Notes: Data are from the California SID and estimates are based on equation (2). We have plotted coefficients for the dummy variables indicating years relative to enactment of a fair pricing law. The omitted dummy is "one 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 percent confidence intervals. Pretreatment readmission rate: 8.7 percent.

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.

V. Conclusion

In this paper, we utilize fair pricing laws to investigate how hospitals alter care in response to financial incentives. Specifically, we establish that FPLs impose substantial payment reductions for uninsured patients (by approximately 25 to 30 percent), and then show that hospitals cut back on care to uninsured patients in response. In particular, hospitals shorten inpatient stays by 7 to 8 percent and reduce intensity of care. The reductions in length of stay are more pronounced for admissions where evidence suggests hospitals have more clinical flexibility to do so. Despite the reduction in care, we do not see evidence of deterioration in the quality of inpatient care received using a number of quality measures, and can generally rule out more than modest declines. Uninsured patients do not die in the hospital at significantly higher rates, they do not experience higher rates of medical complications, they do not receive fewer high-cost, high-tech medical procedures, and they are not readmitted with higher frequency under a FPL. Hospitals likely maintain quality while reducing quantity by focusing where care was least beneficial. For example, they concentrate care reductions on less severe patients and comparatively minor procedures.

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

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

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