

# Transparency and Negotiated Prices: The Value of Information in Hospital-Supplier Bargaining

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## Abstract

We empirically examine the role of information in business-to-business bargaining between hospitals and suppliers of medical technologies. Using a new data set including all purchase orders issued by over sixteen percent of US hospitals 2009-14, and differences-in-differences identification strategies based on both timing of hospitals' joining a benchmarking database and on new products entering the market, we find that access to information on purchasing by peer hospitals leads to reductions in prices. These reductions are concentrated among hospitals previously paying high prices relative to other hospitals and for products purchased in relatively large volumes, and they appear to result from resolving asymmetric information problems between hospitals and their suppliers. We estimate that the achieved savings due to information provision amount to twenty six percent of the savings we would observe if all hospitals paying above average prices for a given product at a point in time were to instead pay the average price. These results have implications for understanding the potential of introducing more information into relatively opaque business-to-business markets, including the emerging role of intermediaries offering benchmarking data and policymakers' calls for transparency in medical device pricing.

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# 1 Introduction

Business-to-business markets make up a large part of the economy, but they often lack transparency in the sense that suppliers negotiate different contracts with different buyers, and a buyer typically has limited information regarding other buyers' contracts. This lack of transparency has been a source of policymaker scrutiny in medical devices.<sup>1</sup> And as technology has made data easier to collect, distribute, and analyze, many business-to-business markets have seen the entry of information intermediaries who facilitate buyers' ability to benchmark the prices they negotiate.<sup>2</sup> Prior research in consumer goods markets (Sorenson 2000; Jin and Leslie 2003; Zettelmeyer et al 2006; Scott Morton et al 2011; Bronnenberg et al 2014) has largely confirmed the economic intuition that information facilitates search and decision making for buyers with imperfect information regarding product quality or costs. However, the implications of this type of increased transparency are not obvious – theoretically or empirically – when both buyers and suppliers have market power and prices are negotiated. In these business-to-business markets, price variation across buyers for the same product need not be due to information frictions (Crawford and Yurukoglu 2012; Grennan 2013, 2014; Gowrisankaran et al 2015; Ho and Lee 2015; Lewis and Pflum 2015), there is often no search across sellers in that a product is only available directly from its manufacturer, and negotiators on both sides are professionals employed by firms and thus with different expertise and incentives than the typical consumer. In this paper, we use a new data set on all purchase orders issued by more than sixteen percent of US hospitals between 2009-14 to estimate the impact of access to benchmarking information on the prices hospitals negotiate with their suppliers.

We have two primary goals: (1) to estimate the treatment effect of transparency in negotiated prices, where transparency takes the form of benchmarking information on hospital supply prices; and (2) to inform theory development on the role of this type of information in negotiated price markets. Hospital supplies and devices are a particularly important case for this analysis as they are estimated to account for 24 percent of the dramatic growth in inpatient hospital costs between 2001 and 2006 (Maeda, et al. 2012), and policymakers have argued that improvements in hospital-supplier contracting may hold great potential for reducing health care system cost growth.<sup>3</sup> Indeed, across a broad set of product categories, there is substantial variation in prices across hospitals – for the top fifty hospital supplies by expenditure in our data, the average standard deviation of prices across hospitals for the same exact product and month is ten percent of the mean price. Recent legislation has proposed that the variation in prices across hospitals is at least in part due to a lack of transparency in these input markets,

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<sup>1</sup>For example, Senator Angus King of Maine recently added an amendment to a tax bill that would increase price transparency for medical devices, stating that “To the extent that prices of implantable medical devices . . . are not disclosed, the ability of hospitals to bring price information to bear in negotiations and decisions is clearly limited.” (“King Calls,” 2014)

<sup>2</sup>In addition to the hospital purchasing context we study, with product categories ranging from cottons swabs to pacemakers, we are aware of business-to-business “price transparency” benchmarking services emerging in areas as diverse as home appliances and television advertising.

<sup>3</sup>For example, the recent Acute Care Episode demonstration, a bundled payment pilot orchestrated by the Centers for Medicare and Medicaid Services, found that lower costs at demonstration sites were achieved largely due to improved contracting with suppliers. See Calsyn and Emanuel (2014) for a discussion.

and further that increasing transparency would lower average prices. The policy attention given to these prices reflects both a concern for the financial viability of hospitals and also a concern that rising supply costs over time filter downstream into higher costs for the health system and consumers.

For many of the most important product categories in medical technology, individual hospitals typically negotiate directly with the product’s manufacturer. Hence, any impact of information on the prices other buyers are paying for a product must enter through this negotiation (in contrast with the more well-studied case of price-taking consumers shopping among multiple retailers offering different prices for the same item). Based on the policy and economics literature on this setting (see, e.g., Pauly and Burns, 2008), as well as on conversations with market participants, the most promising candidate mechanisms through which benchmarking information might have an impact in this context are: (1) a model in which hospitals face uncertainty about suppliers’ costs or bargaining parameters, so that price transparency reduces the degree of uncertainty and the equilibrium dispersion in negotiated prices; and (2) an agency model in which price transparency allows hospital managers to better observe purchasing agents’ effort and, in turn, provide improved incentives to purchasing agents to reduce prices. In order to investigate the mechanisms that underly any price effects attributable to benchmarking, we relate the negotiation procedure in this setting to Rubinstein’s (1985) model of bargaining with incomplete information and Holmstrom’s (1982) model of moral hazard in teams. We empirically test the predictions of each model and provide evidence on the underlying mechanisms.

Our analysis is based on a unique data set from a large hospital supply benchmarking service, covering all purchase orders issued by sixteen percent of US hospitals between 2009 and 2014. In order to control for a host of differences across product categories, we focus our analysis on price negotiations for coronary stents and thus limit our sample to 508 facilities with cardiac catheterization services. Stents are a desirable category because they are important (one of the largest categories, comprising two percent of hospital supply spend in our sample and about \$2 billion annually in the US overall) and typically have simple linear contracts (so the price observed on the purchase order is the price paid). Stents are also physician preference items where doctor usage decisions are relatively insensitive to price, making negotiating lower prices the main mechanism via which a hospital can obtain savings. Given the observed price dispersion for stents alone, potential savings are substantial – if all hospitals paid the minimum price paid by any hospital (for a given stent in a given month), they would save 18 percent on average. The database is generated by monthly submissions from the member hospitals on prices and quantities of each item purchased, at the manufacturer stock-keeping-unit (SKU) level. Importantly, new member hospitals joining the database are asked to submit 12 months of retrospective data, so for any hospital joining during our sample period (about one third of the hospitals in the data) we observe data in pre- and post-information states.

Because different hospitals join at different times, we can construct differences-in-differences estimators based on the prices negotiated by hospitals with and without access to the bench-

marking information, controlling for time-invariant differences at the hospital-product level and product-specific time trends. The assumption underlying this approach is that timing of a hospital joining the benchmarking service is uncorrelated with latent hospital- or hospital-product specific price trends in stents. This strategy would fail and result in an upward bias of information effects if hospitals join when they are experiencing increases in stent prices, or a downward bias if hospitals join when they are enacting other cost-cutting measures (for stents) beyond benchmarking. The exogeneity of join timing is supported by the qualitative fact that stents are just one of many inputs a hospital purchases (and also one frequently purchased via the catheter lab business unit as opposed to central purchasing) and by quantitative evidence from event studies that show no statistically significant divergence of pre-trends. We provide further support for this assumption by developing a set of tests focusing on *new* products entering the market during our sample period.

New product introductions provide useful variation for identification along several dimensions. First, new product introduction timing provides even more plausibly exogenous timing, removing any sources of bias at the timing of join that are transient and not persistent over time. Second, and perhaps more importantly, because no information on others' prices is available when a new product first enters the market, comparing prices between hospitals pre- and post-join immediately upon a product's introduction (before *either* hospital type has access to information) offers a difference between these hospitals that sweeps out any persistent sources of bias of join timing. Third and finally, new product introductions offer a strategy to separate our two theoretical mechanisms of interest: as we argue in our theoretical discussion in Section 3, the asymmetric information mechanism wherein hospitals use benchmarking information to learn about suppliers relies upon concurrent availability of data on others' prices, but the agency mechanism wherein hospitals use benchmarking information to create better contracts for their purchasing negotiators relies only on the fact that such information will be available in the future. Thus new product entry events allow us to separate the information treatment effects into (1) an agency / contracting effect (plus any persistent bias associated with initial timing of join) and (2) an asymmetric information / "learning about supplier type" effect.

The estimated average treatment effect across product-hospital-months for coronary stents suggests that simply having access to the information in the database results in small price reductions. This average estimate, however, conceals substantial heterogeneity. Hospital-products whose prices are above the 80<sup>th</sup> percentile experience price declines of -\$30 per stent upon accessing database information (to give this context, the average size sample hospital uses 700 stents annually and the average stent price is just over \$1,300). The price declines are larger for product-hospital combinations with larger purchase volumes at stake – for hospital-products above the 75<sup>th</sup> percentile in monthly purchase volume prior to joining the database, price effects increase to -\$70 at the 80<sup>th</sup> price percentile, compared to only -\$20 for hospital-products with lower purchase volumes.

The heterogeneity in results can be rationalized by a model of bargaining under asymmetric information or a model in which there is an agency problem in incentivizing effort toward

negotiation, depending on parameters. We thus go further and estimate specifications that rely on entering products to identify separate parameters for price effects attributable to asymmetric information and agency. The results indicate that asymmetric information consistently explains a substantial portion of the effect (while agency effects are noisier and not statistically different from zero in all specifications).

In interpreting the above-described results, it's important to note that prices are "sticky" in this and other business-to-business markets. In order for benchmarking to have an effect, the buyer must engage the supplier to negotiate a new contract, as the term of the existing contract may not expire for a year or more. Thus, our estimates of the treatment effect of benchmarking on prices will be an underestimate of the treatment effect of benchmarking on prices negotiated in a given contract. Motivated by this fact, we also estimated treatment effects of benchmarking on both the likelihood of renegotiation and also on prices observed in months in which we observe renegotiation taking place. These analyses demonstrate that price effects are generated by increasing the likelihood of renegotiation *and* by generating larger price decreases conditional on renegotiation, suggesting that the benefits of transparency in the form of benchmarking are dampened somewhat by stickiness of contracts and other costs of putting information to use in business-to-business settings.

Ultimately, the estimated effects of benchmarking are modest relative to baseline stent prices, but they are large relative to the variation in baseline stent prices. Among hospitals achieving low prices before accessing benchmarking data, there is little opportunity for savings and indeed no significant savings are achieved. However, among hospitals learning that there are large opportunities for savings, 12-51% of potential savings are achieved after benchmarking data are accessed. Across all hospitals, savings on drug-eluting stents are estimated to be 26% of potential savings.

## 1.1 Related Literature, Public Policy, and Roadmap

This paper relates to literatures on bargaining and on the role of informed buyers on market outcomes. For the latter, much of the prior literature has measured how information affects search and outcomes such as price (Sorenson 2000) and quality (Jin and Leslie 2003; Bronnenberg et al. 2014) in markets where buyers are price-taking consumers, generally finding that effects of information are on average null or beneficial to buyers. To our understanding, only Zettelmeyer et al (2006) and Scott-Morton et al (2011) examine the effects of information on consumers when prices are negotiated, showing that information from website research affects the prices consumers negotiate for car purchases. Their studies are quite comprehensive in that they contain data on a variety of consumer characteristics, search, bargaining preferences, and information. Our paper extends this literature to business-to-business bargaining, eliminating the mechanism of search across retailers for the same product, and focusing on the mechanisms via which information affects the price the buying firm is able to negotiate with the same supplying firm.

An emerging empirical bargaining literature (Crawford and Yurukoglu 2012; Grennan 2013,

2014; Gowrisankaran, Nevo, and Town 2014; Ho and Lee 2015; Lewis and Pflum 2015), has thus far modeled business-to-business negotiations of perfect information with exogenously given bargaining parameters.<sup>4</sup> Our analyses of the effects of information generally and the mechanisms of asymmetric information and negotiator agency in particular provide tests of both of these assumptions in our context. Our finding that information matters suggests that information may be one source of heterogeneity in the bargaining parameters being estimated in those studies, and at the least suggests the information of buyers and sellers – and potential changes to that information – should be thought about carefully when performing empirical estimation and policy analysis.

Finally, our estimates provide a first step towards thinking about the transparency policies that have been proposed for medical technology markets. In our study, the transparency provided by the benchmarking service leads to a decrease in the top part of the price distribution for the most used products, but does not fully eliminate the price variation across buyers that has concerned policy-makers. This result comes with caveats as a full analysis of transparency on a nationwide scale would take into account supply side responses to transparency, which can negate or overturn welfare-positive demand-side effects via greater obfuscation (Ellison and Ellison 2009), facilitating collusion (Albek et al. 1997), or forcing coordination not to price discriminate via secret discounts (Grennan 2013). Our research design and the variation in the data will not allow us to estimate the potential effects of the first two responses, to the extent they exist. However, to the extent that suppliers know when buyers join our benchmarking database (and anecdotal evidence suggests that they do), then our estimates will incorporate the net effects of both buyers becoming informed and also the potential reluctance of suppliers to cut any individual buyer a deal when that information will become part of other buyers' future information set.

The paper proceeds by first examining the data and providing background on hospital purchasing in Section 2. Section 3 discusses potential theoretical mechanisms and predictions for how benchmarking data might affect negotiated prices, based on existing theory and claims of industry participants. Section 4 clarifies how we use differences-in-differences research designs that leverage plausibly exogenous variation in the data to measure the treatment effect of information. Section 5 presents our results on the average treatment effects and also heterogeneous treatment effects at different points in the price and quantity distributions designed to better understand the mechanisms behind the theoretical predictions. Section 6 concludes.

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<sup>4</sup>Larsen (2015) is distinct in estimating a bargaining game of two-sided incomplete information about valuations in the used car wholesale market. Our theoretical motivation differs in that we model uncertainty over bargaining parameters instead of valuations. Because our class of models exclude negotiation breakdown, they have received less attention in theoretical and empirical literatures that focused on understanding issues such as labor strikes. However, we see removing the emphasis on breakdown as a desirable feature in ours and other business-to-business contexts where negotiators know the other side well and surplus split is the focus.

## 2 Data and Background on Hospital Purchasing

Health care in the hospital setting has high fixed capital costs in facilities and equipment, but it also has high variable costs in the form of trained labor and disposable/implantable medical devices. As mentioned in the introduction, these devices have been targeted recently as a potential driver of increasing health care costs, with the lack of transparency in the market as a suspected potential mechanism behind higher prices. In this Section, we provide some background on how such devices are used and purchased, and we describe the unique data set and research setting that allow us to obtain the first empirical estimates on how transparency would impact these markets.

Hospitals are typically reimbursed a fixed amount by private or public insurers based on the services they provide, and the inputs in our purchase order data are required to perform these services. Thus, these input prices reflect costs that, at least in the short run, come directly from the hospital’s bottom line. For this reason, hospitals are keen to find ways to reduce input costs, and the availability of benchmarking services offers one hope of doing so.

We focus our inquiry on a single product category, coronary stents, an example of the high-tech, high-dollar “physician preference items” which are at the center of the policy discussions regarding health care costs and transparency of device pricing. For this type of device, usage is driven by physicians choosing which product to use to treat a given patient, while prices are determined in negotiation between a hospital administrator and a representative of the product’s manufacturer. There is typically no “search” in the conventional sense, as a given product can only be purchased directly from its manufacturer. The manufacturer holds inventory on-site at the hospital, and the purchase is made when the physician pulls the product off the shelf and implants it into the patient. Contracts (at least for stents) typically specify a linear price to be paid for whatever number of product is used within the contract duration, often a year.

In our conversations with industry participants, hospital purchasing practices for products like stents vary widely across organizations. Some hospitals have large materials management or purchasing departments with agents who specialize in negotiations, but these departments vary in practices regarding the scope of agent responsibility and incentive contracting. Sometimes a large business unit such as the catheter lab (in the case of stents) will coordinate its own purchasing separately from the rest of the hospital. Finally, with respect to the type of benchmarking information we study, some hospitals may have access to differing amounts of information on the prices other hospitals pay via their group purchasing organization, hospital system, or informal networks of peers.

### 2.1 Hospital Purchase Order Data

The primary data set used in this study comes from a unique database of all supply purchases made by about sixteen percent of US hospitals during the period 2009-2014. This includes a wide range of products, encompassing commodities such as cotton swabs and gloves as well as physician preference items such as stents and orthopedic implants. There are 1.9m distinct

products in almost 3,000 product categories in the data, which are reported monthly. For each transaction, we observe price, quantity (with relevant units), transaction date, product (manufacturer SKU and Universal Medical Devices Nomenclature System (UMDNS) code),<sup>5</sup> and supplier. We observe unique (but anonymous) identifiers for each hospital and the data include several coarse hospital characteristics: census region, facility type, and number of beds.

Table 1 displays some summary statistics regarding the transactions data. See Appendix A for details on sample construction. We observe transactions for 2,111 members, 1,013 of which are hospitals or health systems, and 508 of which are sample facilities that purchase stents. On average, we observe 31 months of transactions for all members, 41 for sample members. We observe purchases in more product categories for sample hospitals than for all members on average (1,143 vs. 462). The average facility in our sample spends \$3.4 million per month on all supplies, \$80 thousand of which is dedicated to coronary stents. As expected, hospitals and health systems generate the majority of the spending on stents – 60% of hospitals and health systems purchased stents during 2009-2014, vs. 30% across all members.

**Table 1:** Summary Statistics from Purchase Order Database

	All Members [N=2,111]		Hospitals/Health Systems [N=1,013]		Sample Members [N=508]	
	Mean	SD	Mean	SD	Mean	SD
Months of Data	31.2	21.2	36.8	21.9	41.4	21.4
Product Categories	462.1	502.7	854.7	442.0	1,143.1	313.4
Total Spend/Month (\$m)	1.1	2.7	2.2	3.6	3.4	3.2
Purchases Stents?	0.30	0.46	0.59	0.49	1.00	0.00
Total Spend/Month on Stents (\$k)	22.0	55.4	44.9	72.9	80.4	73.8

The sample hospitals in the purchase order data voluntarily joined a subscription service that allows them to benchmark purchasing by comparing their own prices and quantities to those of other hospitals in the database and thus may not be a random sample of US hospitals. In particular, subscription is costly, so we expect hospitals with greater concerns about supply costs to be overrepresented in the database. The left panel of Figure 1 compares the distribution of sample hospitals across US census regions to that of American Hospital Association (AHA) member hospitals with cardiac catheterization labs.<sup>6</sup> The Figure also compares our sample to another outside dataset based on Millennium Research Group’s (MRG) survey of catheter labs (the source that major device manufacturers subscribe to for detailed market research). The MRG survey is explicitly intended to provide an accurate picture of market shares and prices by US region. The Figure shows that, relative to both comparison samples, the west region is overrepresented in the benchmarking database sample, while the south is underrepresented. We also note that the average sample hospital is larger than the average US

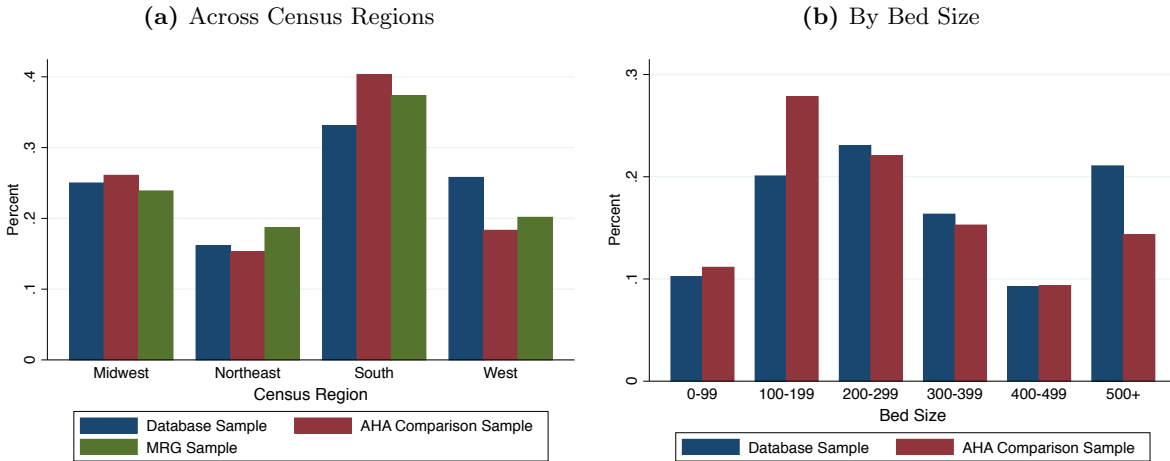
<sup>5</sup>UMDNS is a standard international coding system for medical devices developed by the ECRI Institute.

<sup>6</sup>Data from the AHA Annual Hospital Survey for 2012. Hospitals with catheterization labs defined as those listed as having in-hospital adult or pediatric interventional or diagnostic catheterization services.



hospital with cardiac catheterization capabilities – the right panel of Figure 1 shows that the sample contains disproportionately fewer hospitals in the  $< 200$  beds range and disproportionately more hospitals in the  $\geq 500$  beds range, relative to AHA hospitals that would purchase stents. We do not have access to bed size for the MRG sample, but we do find that, prior to joining the benchmarking database, the member facilities in our estimation sample purchased in significantly higher volumes (68 vs. 33 stents per month) and obtained slightly lower prices (\$1,766 vs. \$1,806 per drug-eluting stent), than the hospitals in the MRG sample.

**Figure 1: Distribution of Benchmarking Database vs. Comparison Hospitals**



The representation of larger facilities with better negotiation outcomes *ex ante* in our sample may be due to small hospitals’ limited ability to afford access to the database, though we would expect a countervailing effect to come from large hospitals’ ability to purchase custom benchmarking services from consulting firms. All of our parameter estimates are internally valid in that they come only from the sample of hospitals who join the benchmarking database, exploiting the existence of pre/post data and staggered join dates. In our later discussion of the policy implications of our paper, we return to the issue of representativeness and the external validity of our results, using the MRG sample (which seems roughly representative) to extrapolate our estimates to the population of US hospitals.

## 2.2 Coronary Stents

As noted above, we focus on coronary stents in our empirical analysis. Coronary stents are small metal tubes placed into narrowed coronary arteries to widen them and allow blood flow to the heart. The original technology, the bare metal stent (BMS), was approved in the early 1990s; in the early 2000s, the drug-eluting stent (DES) was introduced as an improvement over the older technology with lower risk of restenosis, a condition that may arise when scar tissue builds up around the stent and restricts blood flow yet again.

Stents are an important product category, both in terms of overall sales and also as a

percentage of hospital supply costs. In the US, hospitals spend more than two billion dollars annually on stents used in over 700,000 procedures<sup>7</sup>; in our transactions data, stents comprised two percent of overall supply costs among all members. Table 2 summarizes the stent transactions data for the sample on which we perform our estimation. The average sample hospital submitted stent transactions in 41 months. In a given month, sample hospitals spent \$80,000 on 59 stents. The Table shows each statistic separately by hospital bed count; larger hospitals generally submitted more months' data and, as logic would indicate, purchased more stents per month for a greater total monthly expenditure. Hospitals with  $\geq 500$  beds spent more than double the amount that the smallest hospitals did on stents per month. The vast majority of transactions in our data are for drug-eluting (as opposed to bare metal) stents; in the remainder of our description of the data and in our results, we focus on drug-eluting stents. See Appendix D.3 for bare metal stent data and results.

**Table 2:** Summary Statistics – Stent Hospitals Only

Bed Size	Members	Months of Data	Monthly Exp. (\$ k)	Monthly Quantity	% DES
0-99	52	31.4 (19.4)	59.0 (56.6)	45.0 (44.4)	82.0 (10.8)
100-199	102	40.0 (20.3)	45.5 (43.3)	33.5 (31.5)	81.6 (12.1)
200-299	117	43.4 (22.0)	55.6 (45.9)	40.7 (33.5)	77.4 (14.3)
300-399	83	41.0 (20.7)	73.5 (46.9)	53.6 (33.1)	79.7 (11.6)
400-499	47	41.4 (21.3)	128.9 (92.1)	93.5 (65.5)	79.6 (12.2)
500+	107	45.9 (22.0)	135.2 (94.2)	97.7 (65.6)	81.1 (9.5)

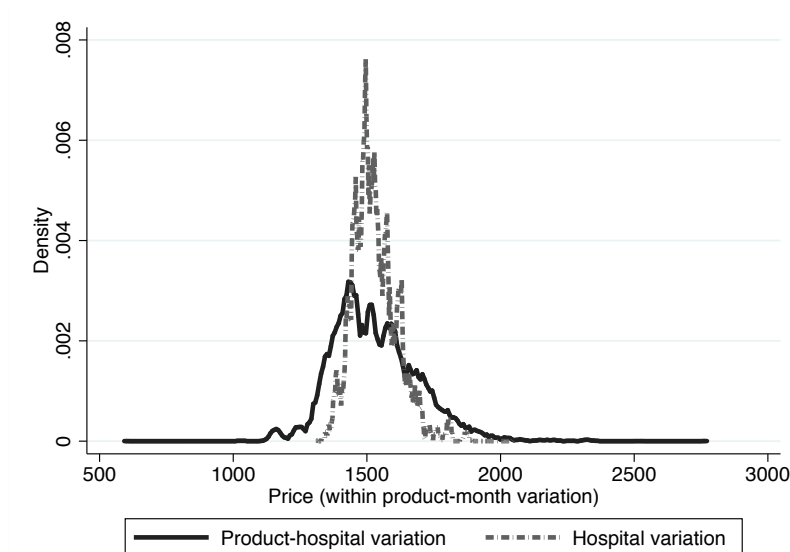
Prices for stents have fallen substantially over time as products have proliferated. During 2009-2014, we observe data for twenty branded products sold by four manufacturers – Abbott, Cordis, Medtronic, and Boston Scientific (with Cordis exiting the market in 2011), and average prices decreased by about 30 percent. These dynamics mean that controlling for time trends will be important. However, after controlling for trends, price differences across hospitals remain substantial.

In Figure 2, we show the distribution of prices across hospitals and products for drug-eluting stents, controlling for time trends. The Figure displays the distributions of hospital-product and hospital fixed effects, obtained from a regression of prices on dummies for hospital-product combinations (or, respectively, hospitals) and product-month fixed effects. The Figure and summary statistics below illustrate the wide cross-hospital variation in prices for the same product at the same point in time, with a standard deviation of \$170 and mean of \$1,530, for a

<sup>7</sup>700,000 estimate from Waldman, et al. (2013), referencing stent procedures in Medicare enrollee population. Two billion dollar figure based on authors' calculations using Boston Scientific's reported US revenue in 2012 (BSX 10-K 2012) and Boston Scientific's 2012 market share in purchase order data.

coefficient of variation of 0.11. Product-hospital effects explain much of this variation with an  $R^2 = 0.88$  for the residual price variation (after product-month detrending). Hospital effects in turn explain almost half of the product-hospital variation, with an  $R^2 = 0.37$ .

**Figure 2: Distribution of Prices Across Hospitals**



	mean	sd	cv = $\frac{\mu}{\sigma}$	20 <sup>th</sup> <sub>%ile</sub>	50 <sup>th</sup> <sub>%ile</sub>	80 <sup>th</sup> <sub>%ile</sub>	N (unique obs.)
$p$ (within product-month)	1530	170	0.11	1392	1509	1661	32,223
$\bar{p}_{jh}$ (within product-month)	1530	160	0.10	1398	1511	1662	2,227
$\bar{p}_h$ (within product-month)	1530	84	0.06	1462	1518	1594	507

$N = 32,223$  product-hospital-month observations with product-month trends removed.

$N_{jH} = 2,227$  product-hospital effects explain  $R^2(p, p_{jh}) = 0.88$  of this variation.  $N_H = 507$

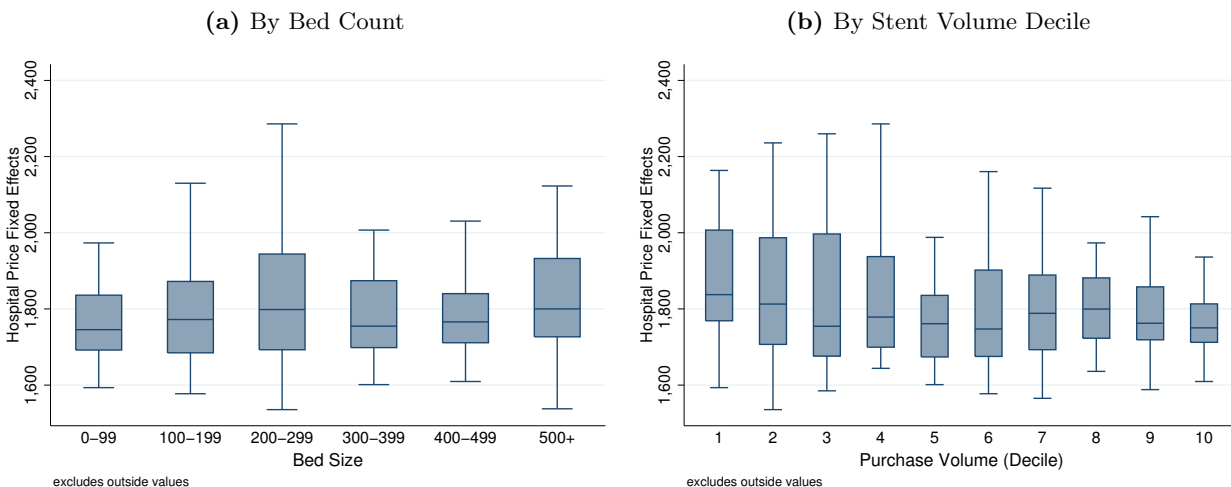
hospital effects explain  $R^2(p_{jh}, p_h) = 0.37$  of the product-hospital variation.

Interestingly, this observed price dispersion is not well-explained by hospital characteristics that might seem a priori to be important for negotiation. For example, in spite of the fact that the largest hospitals spend twice the dollar amount on stent purchases as the smallest hospitals do, we observe no clear relationship between hospital size and stent prices. See the left panel of Figure 3, in which we display a box plot of drug-eluting stent prices for each category of bed count.<sup>8</sup> Mean prices are, if anything, increasing in bed count, though the differences are not statistically significant. Part of this (lack of) relationship is likely due to the heterogeneity in purchasing behavior across hospitals with similar bed counts – e.g., small cardiac specialty hospitals may purchase stents in greater quantities than similarly-sized acute care hospitals. We cannot directly observe measures of hospital specialization; however, we do observe purchase volume. In the right panel of Figure 3, we also show box plots of stent prices for each decile of monthly stent purchasing volume. Here, we do see a relationship between “size” and price – the hospitals with the smallest purchasing volumes have price distributions which are spread slightly upward relative to that of the hospitals with the largest volumes, so that low-volume

<sup>8</sup> “Prices” are hospital fixed effects obtained from a regression of price on hospital and product-month fixed effects.

hospitals’ prices have larger means and variances than high-volume hospitals. For drug-eluting stents, 10<sup>th</sup> decile hospitals’ prices are 7% lower than those obtained by 1<sup>st</sup> decile hospitals. These differences are economically and statistically significant; however, the price distributions for the high-volume and low-volume hospitals overlap substantially, so that there is a great deal of unexplained hospital price heterogeneity conditional on purchasing volume.

**Figure 3:** Distribution of Prices Across Hospitals



<i>Drug-Eluting Stent Prices by Size Category (Regression Results)</i>																
$\beta^{\mathbb{1}\{h \in \text{bed size } x\}} =$						$\beta^{\mathbb{1}\{h \in \text{decile } x\}} =$										
0 – 99	100 – 199	200 – 299	300 – 399	400 – 499	500+	1	2	3	4	5	6	7	8	9	10	
1,785	1,801	1,852	1,778	1,805	1,829	1,916	1,864	1,830	1,832	1,766	1,784	1,801	1,807	1,787	1,774	
(38)	(27)	(30)	(25)	(32)	(22)	(54)	(40)	(44)	(36)	(26)	(31)	(29)	(25)	(25)	(32)	

Estimated mean hospital fixed effects within bed size categories and decile of monthly purchase volume. Hospital fixed effects obtained from regression of price on hospital and product-month fixed effects, pre-join data only. Mean estimates from regression of fixed effects on indicators for size. Standard errors from nonparametric bootstrap of entire procedure, resampling at hospital level.

One potential explanation for this residual heterogeneity may be that stents are “physician preference items”: products whose demand is determined in large part by preferences of brand-loyal physicians and which are particularly prominent targets for cost savings by hospital administrators. Policymakers have long argued that the primacy of physician preference in determining demand for such products has limited hospitals’ ability to constrain costs using negotiating tools such as standardization. Consistent with this, we observe no evidence of standardization affecting prices or usage in our purchasing data. See Appendix B for detail.

In a different data set and time frame, Grennan (2013, 2014) found evidence that heterogeneity in stent prices across hospitals could be explained in part by heterogeneity in physician brand loyalty, but this left a large residual heterogeneity in hospital-product *bargaining ability* (Nash weights in a structural model of full-information bargaining). Motivated by policymaker interest in (lack of) transparency in device prices and the existence of the benchmarking intermediary whose data we study, we entertain the possibility that part of this heterogeneity

in bargaining abilities may be due to heterogeneity in information among hospitals, and that transparency in the form of information on other hospitals prices might affect this.

### **2.3 Benchmarking Information Treatment and Information Access Data**

The information treatment considered in this study is one in which hospitals observe the distribution of other hospitals' prices and quantities and, in so doing, receive information about their relative performance in purchasing. In our empirical setting, sample hospitals were able to access information of this type in several ways: The basic interface members access upon logging in presents graphical analytics for "potential savings" opportunities at the supplier level. Savings potential is determined by the total dollars that might have been saved in the previous year based on the hospital's volume of purchase and the mean/min prices paid by other hospitals at the manufacturer-SKU level. By clicking through to each manufacturer, the hospital could observe these potential savings broken down by SKU. Further, an interested hospital could filter this comparison to look at only similar hospitals to itself in terms of geography and bed size, and could even click through to access the other hospitals' (de-identified) purchase order data points that were used to construct the analytics. By repeating this final step for each SKU purchased, member hospitals could in principle construct the full purchase order database used in this study, though this process would require a great deal of patience due to the large number of SKUs each hospital purchases and to the daily download restrictions imposed by the web site that hosts the benchmarking service.

In order to analyze the effects of price transparency on negotiations, we obtained clickstream data on the precise timing (to the minute) of all members' website logins. Combined with the purchase order database, which includes the date on which each purchase order was loaded into the database in addition to the month in which each transaction occurred, we are able to reconstruct the analytics a given member would have been presented with upon logging into the database, as well as the more granular data it would have been able to click through to access at each point in time. The next Section examines the theoretical mechanisms via which this type of information might affect the prices hospitals negotiate for medical devices.

## **3 Theory: Negotiated Prices and Benchmarking Information**

There are two primary mechanisms that market participants and economic theory suggest for how benchmarking information could be useful to hospital buyers: (1) in reducing asymmetric information about how low a price the supplier is willing to concede to; and (2) in helping to better solve the agency problem between the hospital and its procurement negotiators by providing a tool for the hospital to monitor negotiator performance relative to the market aggregate. Below we outline relatively simple theoretical models that capture each of these effects, and use the models to generate testable predictions we can then take to the empirical analysis.

Our models are built from the baseline of the Rubinstein (1982) model of alternating offers

bargaining. This model is useful because it allows for extension in clear and tractable ways to our mechanisms of asymmetric information about supplier parameters and negotiator agency. It is also useful because it forms the underpinning for a large subsequent literature in theoretical bargaining (Rubinstein 1985; Binmore, Rubinstein, and Wolinsky 1983; Horn and Wolinsky 1988; Collard-Wexler, Gowrisankaran, and Lee 2014) as well as a recent industrial organization literature in empirical bargaining studies (Crawford and Yurukoglu 2012; Grennan 2013, 2014; Gowrisankaran, Nevo, and Town 2014; Ho and Lee 2014; Lewis and Pflum 2015). The predictions of the model extend well to empirical settings because the “discount factors” that parameterize bargaining strength in the Rubinstein model can be thought of more generally as proxies for a host of factors that might affect a real-world negotiation such as impatience, opportunity costs of time, laziness, or fear of negotiation breakdown.

Before we consider incomplete information, it is helpful to briefly outline the logic of the Rubinstein (1982) complete information game as a starting point. The model has a single buyer negotiating with a single supplier over a per-unit surplus  $V = wtp - c$  equal to the buyer’s willingness-to-pay for a unit of the supplier’s product, minus the supplier’s marginal cost of manufacturing and distributing a unit of the product.<sup>9</sup> Beginning with the buyer, each player in turn makes a proposal for the division of the surplus. After one player has made an offer, the other must decide to accept or reject it and make a counteroffer in the next round. Players discount continued rounds of bargaining. The buyer has discount factor  $\delta^B$  and the supplier has a discount factor  $\delta^S$ , both in  $(0, 1)$ .

The unique subgame perfect equilibrium of this game is for it to end in the first round with the buyer making an offer that the seller accepts. The intuition for this equilibrium is that the buyer offers just enough so that the seller is indifferent between accepting the offer and rejecting, incurring a period’s discounting, and making a counteroffer (which would in turn be just enough for the buyer to be indifferent between accepting and continuing). The resulting price in this equilibrium is:

$$p^{CI} := c + \delta^S \frac{1 - \delta^B}{1 - \delta^B \delta^S} V. \quad (1)$$

In the institutional setting of bargaining over coronary stents, the typical negotiation occurs between agents/employees of the hospital and device manufacturer, negotiating on behalf of their employers. On the hospital side, the negotiator will typically be either the catheter lab business unit manager responsible solely for catheter lab operations or a purchasing / materials management professional in the hospital operations department, who may be responsible for a variety of product categories across the hospital. On the device manufacturer side, the negotiator will typically be a regional sales manager. Thus for both negotiators, their respective discount factors ( $\delta^B, \delta^S$ ) should be thought of as coming from some combination of negotiator skill and the incentives they face as agents of their respective employers. The potential for uncertainty regarding the skill and/or incentives faced by manufacturer negotiators will be the

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<sup>9</sup>As noted later in our predictions (and discussed and analyzed in detail in Grennan (2013,2014)),  $V_{jht}$  should be thought of as the incremental value created by stent  $j$  for the set of patients for which the doctors at hospital  $h$  choose to use  $j$  over alternative stents or non-stent treatments, given physician preferences over all stents available at time  $t$ .

primary focus of our theorizing as to the potential mechanisms via which transparency in the form of price benchmarking information might impact prices; that uncertainty may be faced by hospital negotiators directly, or instead by the managers responsible for the incentives embodied in the hospital negotiators' employment contracts. We focus on the case where uncertainty is embodied only in the discount factors and not the value over which negotiations occur. This seems to be the primary source of potential uncertainty in coronary stent negotiations, where doctor preferences are typically quite well known by those involved in the negotiation and marginal costs are small relative to the surplus created. It is also consistent with anecdotal evidence of little if any equilibrium breakdown in negotiations, which is a hallmark of models of incomplete information about values.<sup>10</sup> In the Sections that follow, we build off of this baseline model to derive predictions on how benchmarking information might affect prices in cases of asymmetric information (where hospital negotiators are uncertain about the manufacturer negotiator's skill or incentives as embodied in  $\delta^S$ ) and negotiator agency (where hospital negotiator  $\delta^B$  has an effort component which hospital managers cannot observe or infer).

### 3.1 Asymmetric Information about Supplier Bargaining Parameters

We follow Rubinstein (1985) to model uncertainty of hospital buyers about the bargaining parameter of a given supplier. The model departs from the complete information model outlined above in that the supplier is either of weak type with discount factor  $\delta_w^S$  or strong type with discount factor  $\delta_s^S$  ( $1 > \delta_s^S > \delta_w^S > 0$ ). The supplier knows his own type, but the buyer has only a subjective prior  $\omega_w$  of the probability that the supplier is the weak type.

The equilibrium split of this surplus depends on both the type of the supplier and the prior of the buyer as follows: Rubinstein (1985) shows that there exists a cutoff prior  $\omega^*$  such that if the buyer is sufficiently pessimistic about the seller being the weak type  $\omega_w < \omega^*$ , then the buyer simply offers what she would offer the strong type in a complete information game of Rubinstein (1982):

$$p_s^{CI} := c + \delta_s^S \frac{1 - \delta^B}{1 - \delta^B \delta_s^S} V, \quad (2)$$

and both seller types accept this offer. However, if the buyer is more optimistic about the probability that the seller is the weak type  $\omega_w > \omega^*$ , then the buyer offers:

$$p_w^{AI} := c + \delta_w^S \frac{1 - \delta^{B^2}(1 - \omega_w) - \delta^B \omega_w}{1 - \delta^{B^2}(1 - \omega_w) - \delta^B \delta_w^S \omega_w} V, \quad (3)$$

which the weak seller type accepts. The strong seller type will reject this offer, and counteroffer with a price that would make a weak seller no better off than  $p_w^{AI}$ , but that the strong seller

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<sup>10</sup>We thank Brad Larsen for this observation. Note that without data on breakdown or beliefs, we cannot test this assumption directly. Because the surplus and bargaining parameters enter the price multiplicatively, similar types of uncertainty regarding either would yield similar predictions for equilibrium prices conditional on agreement.

strictly prefers:

$$p_s^{AI} := c + \frac{1 - \delta^{B^2}(1 - \omega_w) - \delta^B \omega_w}{1 - \delta^{B^2}(1 - \omega_w) - \delta^B \delta_w^S \omega_w} V, \quad (4)$$

which the buyer accepts.

This equilibrium has direct implications for what we would expect to happen to prices in a move from this type of asymmetric information to complete information. First, note that  $p_s^{CI} > p_s^{AI} > p_w^{AI} > p_w^{CI}$  (where  $p_w^{CI}$  is the equilibrium price for the weak supplier type with complete information). Thus the weak type seller is strictly better off with asymmetric information. The strong type seller is weakly worse off (strictly whenever the buyer's prior is sufficiently optimistic). A sufficiently pessimistic buyer is also weakly worse off without information. For more optimistic buyers, whether information would make them better off ex-ante depends on parameter values.

In our context we are interested in when a buyer might be interested in benchmarking information that would reveal the seller's type, and what would happen to price in such a case. For simplicity, we will assume that this information would fully reveal a seller's type, though the qualitative results should extend to a signal extraction problem where the information moves the buyer's prior in the direction of the truth. The intuition for how this unfolds in practice is a scenario where a manufacturer sales representative says "This is the best price I can offer. Corporate won't let me go any lower." Benchmarking information allows the hospital negotiator to perform the due diligence of checking the prices at other hospitals in order to attempt to verify or refute this statement.

**Prediction 1 (Direct Information Effect on High Prices)** If information is costless, pessimistic buyers will always become informed. This information will cause a proportion of the highest prices  $p_s^{CI}$  to fall to  $p_w^{CI}$  for those cases where the supplier was in fact the weak type. Thus exposure to benchmarking information should lead to some of the highest prices falling.

**Prediction 2 (Direct Information Effect on High Prices with High Quantity)** If information is costly to obtain (in the sense that searching and analyzing the data takes time that could be used on other productive activity), a pessimistic buyer will become informed whenever the expected benefit  $\omega_w(p_s^{CI} - p_w^{CI})q$  exceeds the cost of information. This information will cause a proportion of the highest prices  $p_s^{CI}$  to fall to  $p_w^{CI}$  for those cases where the supplier was in fact the weak type. Thus exposure to benchmarking information should lead to some of the highest prices falling, among those products with the highest quantity used.

**Prediction 3 (Indirect Information/Competition Effect on All Prices)** With imperfect substitute products, under reasonable assumptions on how the negotiation for one product affects the disagreement payoff of other product negotiations, a fall in price of substitute product  $j$  will decrease the surplus up for negotiation for other products  $-j$ ,



leading to a decrease in the prices of other products  $-j$ , all else equal.<sup>11</sup> Thus exposure to benchmarking information that leads to a fall in a high price for  $j$  should also lead to a fall in any price for other products  $-j$ , and the size of this fall will be increasing to the extent the products are good substitutes for  $j$ .

### 3.2 Negotiator Agency

Another mechanism via which benchmarking information could be valuable to buyers would be through providing aggregate information to help the buying firm solve a moral hazard problem with its purchasing agent who negotiates with the supplier. Modifying Holmstrom (1982) to our context, let price  $p_h$  at hospital  $h$  be as in the full information Rubinstein bargaining game. However, instead of the hospital negotiator's bargaining parameter being exogenous, the price will be a function of the hospital agent's *choice* of discount factor  $\delta_h^B$  and the discount factor of the supplier, which takes value  $\delta_w^S \epsilon_h$  with probability  $\omega_w$  and  $\delta_s^S \epsilon_h$  with probability  $1 - \omega_w$ . As before, the discount factor of the strong supplier type  $1 > \delta_s^S > \delta_w^S > 0$  is greater than that of the weak type.  $\epsilon_h$  is a random term distributed uniform on  $[0, 1]$ . It is important to note that the realization of  $\epsilon_h$  is independent across hospital buyers, but whether the seller is weak or strong is common to all buyers. The realizations of both of these random variables are observable to the negotiating agents, but not to the principals who manage them at their hospitals.

A moral hazard problem arises in this setting because bargaining effort is costly and provides the agent disutility  $v(\delta_h^B)$ . The agent is compensated by some contract based on the price  $m(p_h)$ . The agent is risk averse in money, so the optimal solution to the agency problem involves risk sharing between the principal and the agent. Holmstrom (1982) shows how if agents face some common parameter which is uncertain from the principals' perspectives, then relative performance evaluation compared to some aggregate sufficient statistic can be used to write a better contract with each agent. In our context, the bargaining parameter of the supplier plays the role of an uncertainty (from each principal's view) faced by each purchasing agent. And thus price benchmarking data provides exactly the sort of information that would be useful to a hospital in designing better incentive contracts for its purchasing agents. The intuition in our real-world setting is one where with the benchmarking data, hospital administrators can make their negotiators' performance reviews contingent on the prices they negotiate relative to other hospitals for the same product. This motivates the following Predictions:

**Prediction 4 (Monitoring Effect on Prices)** If buyer negotiators are imperfect agents of the buying firm, then benchmarking information (observing the distribution of price realizations across hospitals  $\{p_h\}_{h=1}^H$ ) allows the principal to estimate whether the seller is the weak or strong type, and thus reduce the risk to which the agent is exposed and

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<sup>11</sup>This will be the case in any model where disagreement payoffs are a function of the prices agreed to with other manufacturers, which has been the case in the empirical bargaining literature thus far and much of the negotiation with externalities theory. It would not be the case in a model such as the Core, where disagreements are based on the primitive of willingness-to-pay and costs.

write a contract which induces more bargaining effort and a lower price than in the case where only  $p_h$  is observed.<sup>12</sup>

**Prediction 5 (Monitoring Effect on Prices with High Quantity)** If buyer negotiators are imperfect agents of the buying firm, but it is costly for hospital managers to search and analyze the data in a way that allows them to write better contracts, then managers will use benchmarking information (observing the distribution of price realizations across hospitals  $\{p_h\}_{h=1}^H$ ) to write a contract which induces more bargaining effort by the agent and a lower price than in the case where only  $p_h$  is observed if  $(p_h(m) - p_h(m(\{p_h\}_{h=1}^H)))q_h$  exceeds the cost of information use.

### 3.3 New Product Entry and the Timing of Benchmarking Information Effects

An interesting feature that differs between the asymmetric information about supplier bargaining type mechanism and the negotiator agency mechanism is the timing during which benchmarking information is valuable to the buyer. In the asymmetric information case, benchmarking is only useful to the extent that data on other buyers' prices for the same product are *currently* available in the database at the time of negotiation. By contrast, even if there is no current data on others' prices for a given product, the agency mechanism allows for managers to incentivize agents today based on performance assessments taking place in the *future* using benchmarking data yet to be collected.

This difference between the timing of information required for the two mechanisms is especially relevant when new products enter the market. By the nature of how the benchmarking database is constructed, there will be no data available on a product for the first month or two it is on the market, and little data for the first 1-2 quarters. Thus those who engage in their first negotiation for a product early after its release do so without *current* benchmarking information, even if they have access to the database. This motivates our next theoretical predictions:

**Prediction 6 (New Product Entry Separates Asymmetric Information and Agency)**

For newly introduced products, when they are first released to the market, differences between prices negotiated in the first, uninformed round of negotiation and the second, informed round of negotiation must be due to informing negotiators about the seller's bargaining parameter, rather than altering moral hazard. That is, hospital managers can write effort-contingent contracts with purchasing agents in the first round as well as the second round, but cannot learn about the seller's bargaining parameter until the second round.

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<sup>12</sup>The model as written has a strong prediction that this effect will be independent of price. However, in general the prediction of how the price distribution would move with information depends on where in the model the current heterogeneity is coming from. For example, if the heterogeneity were due to different levels of risk aversion among negotiators, then benchmarking information would tend to decrease the highest prices more than the lowest.

Finally, new product introductions are also of interest because, at the time of new product entry, benchmarking data contain new information only on the specific entering product. Because product entry happens over time and is uncorrelated with the timing of hospitals joining the database, the average member will have already gained any informational advantage about previously available products prior to the new product’s entry. This motivates yet another prediction:

**Prediction 7 (New Product Entry Isolates Own-product Effects)** For newly introduced products, any difference in prices between informed and uninformed buyers must be due to own-product effects, as no new information about previously existing products has been introduced.

### 3.4 Dynamic Considerations: “Sticky” Contracts, Persistence of Learning, and Supply Responses

In the interest of clearly illustrating the fundamental ideas behind the two theoretical mechanisms of interest, we have abstracted from the reality of hospital purchasing, where contracts are negotiated for a set period of time but sometimes renegotiated before that time, where the same negotiators on the buyer and supplier side may interact repeatedly over time, and where suppliers might change their behavior in response to buyers using benchmarking information. Here we consider how these effects would likely show up (or not show up) in our empirical analysis.

While a hospital joining the benchmarking database has immediate access to the same data we do on the prices other hospitals are paying for any product, translating that access into differences at the negotiating table still involves a series of steps. In the Propositions above, it was noted that information may be costly to use in the sense that someone at the hospital must anticipate sufficient potential gains for a product to search and analyze the data. Another important friction to consider is that the hospital must engage the supplier to negotiate a new contract (the term of the existing contract may not expire for up to a year or more). To the extent that renegotiation is not frictionless, it will take time and effort to get to the negotiating table and come to a new deal: prices will be “sticky”. This will tend to bias the effect of information toward zero.

The same supplier salesperson may be in charge of negotiating contracts for a bare-metal and a drug-eluting stent. She may also negotiate for the next generation drug-eluting stent when it is released. To the extent that learning about types in the models above captures something that is specific and unchanging over time about that salesperson and the incentives she faces, there will be less asymmetric information and scope for learning, biasing the effect of benchmarking information toward zero.

While demand side effects of information are generally null or beneficial to buyers, to the extent that suppliers know when buyers join the benchmarking database (or transparency is imposed via public policy), then supply side responses can negate or overturn these effects through greater obfuscation (Ellison and Ellison 2004), facilitating collusion (Albek et al. 1997),

or forcing suppliers not to price discriminate via secret discounts (Duggan and Scott Morton 2006; Grennan 2013). Because suppliers will typically know when a hospital is using the benchmarking service, our treatment effects will capture this last effect of reluctance to give discounts when they are no longer secret (at least in part, though the information will only be available to the benchmarking members, not the entire market), but not other supplier obfuscation efforts that might take effect if all buyers had access to benchmarking information, and not collusion that might be facilitated by a public information mechanism. Thus our estimates will be a useful, yet not complete, piece of information in considering large-scale transparency policies. The next Section outlines how the variation in our data identifies the treatment effects of the information intervention we observe.

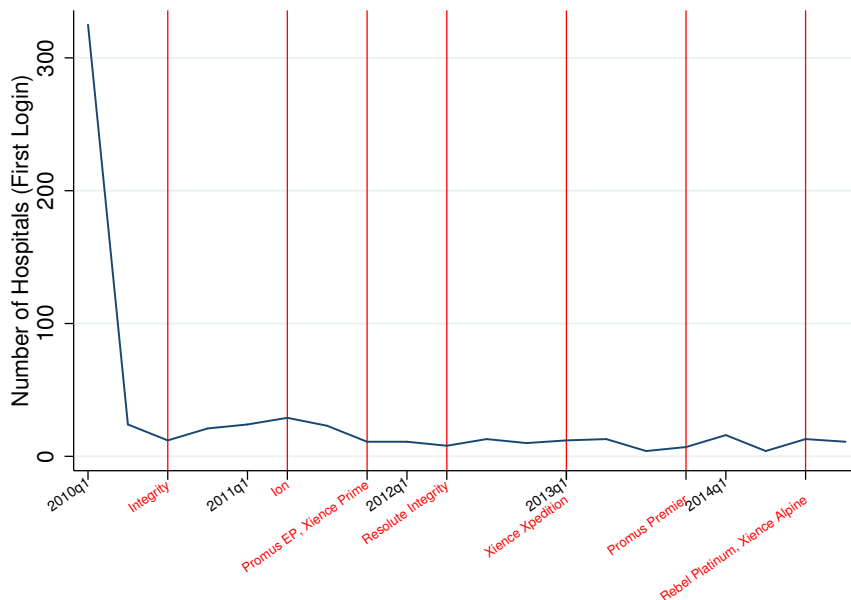
## 4 Research Design: Identification of Information Treatment Effects

The ideal experiment to empirically examine the effect of transparency on prices would be one in which some hospitals were randomly assigned to receive benchmarking data, while others were not. As noted above, the context that allows us to have access to this rare data on business-to-business purchase orders is that the sample hospitals voluntarily joined a subscription database. Our discussions of identification in this Section and of treatment effects in Section 5 focus on the issue of internal validity – consistently estimating causal information effects for the hospitals in our sample. In the final Section, we return to the issue of potential selection into our sample and the external validity of our estimated effects for policies that advocate the rollout of transparency in the form of benchmarking information for all US hospitals. The key features of the data that allow us to estimate causal treatment effects of price transparency for the hospitals in our sample are: (1) that new members submit one year of retrospective data when they first join the benchmarking database, and continue to submit monthly data thereafter; and (2) that new members join over time in a staggered (and seemingly random) fashion.

Thus, for hospitals that joined during the 2009-14 period, we observe data before and after they were first able to access the benchmarking information available in the database. Figure 4 shows the time series of hospitals joining the database between 2010 and 2014. One technical quirk of the data is that the database vendor rolled out a new version of its database web interface in early 2010 and re-invited all current members to “join” at that point. Thus, for members “joining” in early 2010, we cannot cleanly identify their pre-period and we exclude those members’ “pre-join” data from our analyses. After March 2010, 14 hospitals join the database in each quarter, on average.

The availability of both pre- and post-join data for hospitals joining the database at different points in time allows us to use a differences-in-differences strategy to estimate the treatment effect of having access to benchmarking information. The logic behind this identification strategy is illustrated in Figure 5. In our sample, there are no pure “control” hospitals – all hospitals by definition access the benchmarking data at some point. However, different hospitals join the

**Figure 4:** Count of Hospitals Joining in Each Quarter

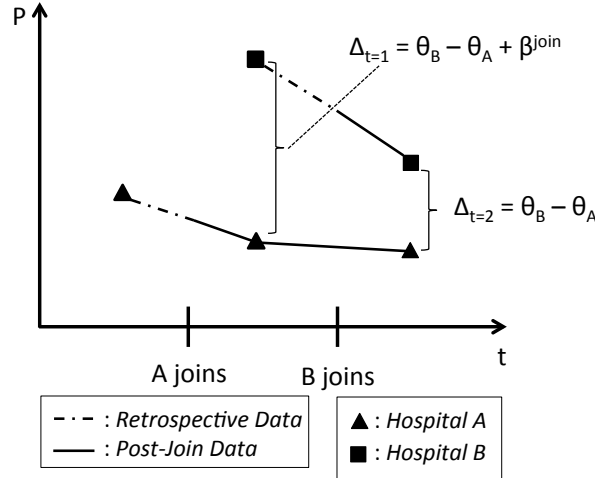


database at different points in time. Suppose there are two hospitals, hospital  $A$  and hospital  $B$ , where  $A$  joined the database one period before hospital  $B$ . Under the standard differences-in-differences assumption of parallel trends, we can isolate the treatment effect of joining the database on prices by comparing the price trends between the two hospitals for their overlapping time periods. Overlapping periods where both are in the same information state identify any fixed difference between the hospitals unrelated to information access (in practice, we analyze transparency effects across many products, so we capture these using product-hospital fixed effects). In the Figure, these time-invariant differences are identified by the term  $\Delta_{t=2}$ .<sup>13</sup> Overlapping periods where hospitals are in different information states identify the difference between the two hospitals *plus the difference of access to benchmarking information* – in Figure 5, this difference is  $\Delta_{t=1}$ , taken at the point where  $A$  has joined and  $B$  has not yet. The difference between these two differences identifies the treatment effect of access to information,  $\beta^{join} = \Delta_{t=1} - \Delta_{t=2}$ . In our empirical setting, for any given product-month, we observe many hospitals in pre- and post-information states, allowing us to estimate not only time-invariant differences across hospital-products but also product-specific time trends, which we capture using time fixed effects and a product-specific linear trend. The time trends are important because prices decreased steadily over time during our period of interest – if we omitted controls for time trends, we would estimate larger effects of access to information based solely on the negative price trend coinciding with hospitals’ pre- and post-information periods.

The primary concern with this identification strategy is that timing of a hospital joining

<sup>13</sup>In Figure 5, the difference in fixed effects is identified where both hospitals are post-join, but in many cases hospitals that join in different months will have overlapping pre-join data as well.

**Figure 5:** Graphical Illustration of Identification Based on Timing of Join



the database may be correlated with other contemporaneous factors that may impact price trends at that hospital. For example, a hospital may be inspired to join the database due to particular concerns about price trends, which would bias our results upward by underestimating the counterfactual prices joining hospitals would face if they did not join. On the other hand, a joining hospital might concurrently be undertaking other initiatives intended to constrain prices, such as hiring new personnel or contracting other outside consulting services, which would bias our results downward by conflating the effects of these other initiatives with the effect of access to the benchmarking information. In Section 5, we bring to bear multiple pieces of qualitative (that stents are only one of many products a hospital purchases) and quantitative (event studies of trends around join timing; comparison to price trends in a different data set of hospital stent purchases; information variation from the introduction of new products) evidence regarding this issue. Our ultimate conclusion is that there is little evidence for timing of join being endogenous with respect to stent price trends for much of our sample, and that even the strongest such bias not ruled out by our tests would leave our main qualitative results unchanged.

#### 4.1 Using New Product Entry to Identify Mechanisms

As noted above, we also rely on an additional source of identification: new product entry. New products (at the time of their entry, when they are in fact “new”) provide another opportunity to identify the above information effect, and further allow us to identify a treatment effect of joining the database but not having concurrent data on other hospitals’ purchases. This is because of the timing of information availability. When a new product is first introduced, no information on other hospitals’ purchases of that product will be available in the database

for several months.<sup>14</sup> In any given month in the year following new product entry, there are on average 56 more members we observe having transactions for new products than there are members whose transactions data are actually loaded into the database. This lag will be in part due to data submission and loading, and in part due to our observing data for members that have not yet joined the benchmarking database – in the year following product entry, nine percent of members observed in the average month are pre-join. This lag implies that, *during the first months after new product introduction*, we have overlapping periods where one hospital is post-join (treated, but without concurrently available data) and the other is pre-join (untreated). See Appendix A for detail.

The time period for our study contains many meaningful product introductions. In Figure 4, we note the timing of entry of nine new products between 2010 and 2014 (of the twenty products sold during this time period overall). This allows us to identify a treatment effect of access to benchmarking information *via a mechanism that does not require concurrent access to data on other hospitals’ purchases*; in Section 3, we outline one such mechanism, in which *joining* the benchmarking database allows hospitals to resolve a negotiator agency problem *even before* benchmarking data are available. We term this the agency (“Ag”) effect for the sake of exposition. Once information for the new product becomes available in the database, the same logic as for non-entering products applies: overlapping periods where one hospital is post-join (treated) and the other is pre-join (untreated) identify an overall treatment effect of access to benchmarking information, which is the combination of the agency effect and an information (“info”) effect that requires other hospitals’ data.<sup>15</sup>

Figure 6 illustrates this identification strategy graphically. Again, we have hospital A joining the database before hospital B; in this example, hospital A joins well before the product enters the market and hospital B joins after the product enters. Once the product enters, each hospital negotiates prices; hospital B is untreated, while hospital A is treated (“Ag”) in the sense that it has joined but has no concurrent data on other hospitals (for example, hospital A may have resolved the agency problem). In the next period, after price data are submitted, loaded, and released to database members, hospital B remains untreated, but hospital A receives another treatment (“Info”) in the form of information on other hospitals’ prices. In the final period, hospital B has joined the database and received the full treatment effect of access to benchmarking data (“Ag” + “Info”); hospital A retains both treatments in the final period as well. We thus now have three differences that identify three different objects: in the final period, we identify the fixed hospital differences ( $\Delta_{t=3}$ ); in the penultimate period, we identify the fixed differences plus the “agency” and “information” effects ( $\Delta_{t=2}$ ); and in the first period, we identify the fixed differences plus the “agency” effect only ( $\Delta_{t=1}$ ). These three differences

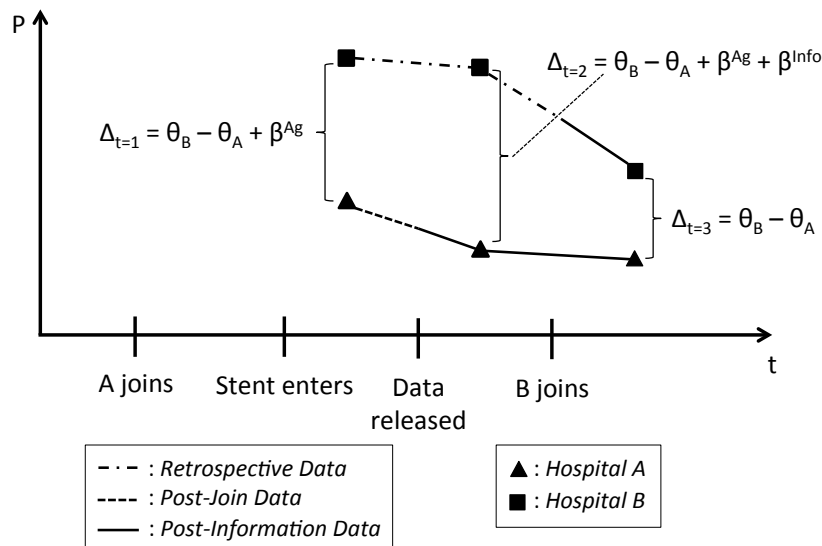
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<sup>14</sup>Benchmarking data will not become available for the new products until members submit their purchase order data and they are loaded into the database, a process that will take several months, depending on the timing of purchasing, data sharing lag after purchase, and data entry lag after sharing. There may be an additional lag before joining hospitals become informed if they do not frequently log in to the database.

<sup>15</sup>For new products, we also note the minor difference that the identification of fixed differences between hospitals (product-hospital fixed effects) is driven entirely by overlapping periods where both hospitals are post-join (treated), as there can be no overlapping pre-treatment periods before the product is introduced.

allow us to separately identify the agency ( $\beta^{Ag} = \Delta_{t=1} - \Delta_{t=3}$ ) and information ( $\beta^{Info} = \Delta_{t=2} - \Delta_{t=3} - \beta^{Ag}$ ) effects. For each entering product, in the months close to the timing of join, we observe many hospitals in each treatment/control state, allowing us to estimate product-specific time trends as well.

**Figure 6:** Graphical Illustration of Identification Based on Timing of Join and New Product Entry



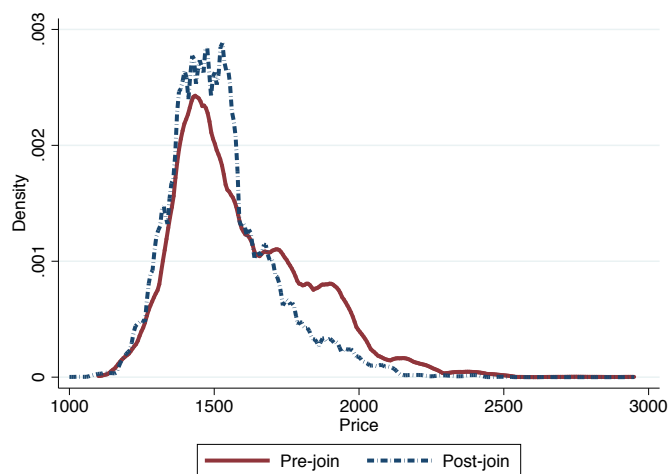
Though it is not the primary way in which we prefer to think about the usefulness of entering products, entering products also provide two types of robustness checks regarding any potential bias due to timing of join. First, for hospitals that have joined some time ago, new product introductions offer another point in time – a point in time not “near” when they decided to join – at which to compare them to untreated hospitals. The assumption that timing of join is not endogenous with respect to new product entry is supported by Figure 4, in which we do not see spikes of joining around product entry times. Second, any persistent bias associated with something different besides information at hospitals who have joined or not will be included in the difference between pre- and post-join hospitals in the first few months after new product introduction (labeled  $\beta^{Ag}$  in the previous discussion). Thus even in the most extreme case, our estimate of any “asymmetric information” effect where hospitals use information concurrently available in the database to negotiate better prices (labeled  $\beta^{Info}$  above) would be free of such bias. The next Section implements variants of these research designs to estimate the magnitude of and mechanisms underlying the information intervention we observe.



## 5 Estimation and Results: How Information Affects Negotiated Prices

In order to preview our approach and results in a simple graphical manner, we begin by simply splitting the sample into pre- and post-join observations and plotting the prices. Figure 7 displays the histograms of prices paid for drug-eluting stents across the entire sample, split between these two groups. The raw data clearly suggests the primary impact of access to the benchmarking information: hospitals with information are much less likely to pay the highest prices.

**Figure 7:** Histograms of Price Distributions: Pre- and Post-Information



In this Section, we estimate regressions to more carefully measure and understand this effect of information, accounting for time-invariant differences across hospitals (or hospital-product combinations), imbalance in the sample before and after information shocks, and downward trends in prices over time. We begin with an event study of the differences between treated and untreated groups around the time of information access. The event study allows us to be as transparent as possible in establishing the effects we find and in discussing any potential biases around information timing. We then conduct a series of analyses aimed directly at testing the theoretical predictions of Section 3: examining effects conditional on pre-information price and quantity distributions, and using new product entry explicitly to disentangle asymmetric information and agency mechanisms (again noting that any remaining worries about bias due to endogenous join timing will be captured in our measure of the agency mechanism). We also examine the underlying drivers of the price effects by separately considering the effects of information on the likelihood of renegotiation and on price changes conditional on renegotiation. Finally, we use our estimates to extrapolate to the overall effect of access to benchmarking information on the hospitals in our sample and to consider the potential effect of the types of transparency being called for by policymakers. In all cases, we focus our analysis on drug-

eluting stents only, as they make up the majority of the usage and spending in the market. Bare metal stents are available in Appendix D.3.

All of the regressions we present are extensions of a baseline specification implementing the difference-in-differences around the timing of join. Letting  $P_{jht}$  denote the price observed for product  $j$ , hospital  $h$ , and month  $t$ , our preferred specification controls for hospital-product fixed effects  $[\theta_{jh}]$ , month fixed effects  $[\theta_t]$ , and separate linear time trends for each product  $[\gamma_j * (t - t_{min_j})]$  (where  $t_{min_j}$  is the first period in which we observe data for product  $j$ : either the beginning of our sample or the month of entry of product  $j$  into the market):<sup>16</sup>

$$P_{jht} = \beta^{Info} * \mathbb{1}_{\{post_{hjt}\}} + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j}) + \varepsilon_{jht}.$$

Here,  $\mathbb{1}_{\{post_{hjt}\}}$  is an indicator equal to one after a hospital first accesses information in the benchmarking database (and data for the given product are available in the database) and zero prior, making the coefficient  $\beta^{Info}$  an estimator for the treatment effect. All of the regressions and results below extend this specification to allow for varying types of heterogeneity in this treatment effect.

### 5.0.1 Event studies around timing of information

In our first analysis, we estimate a flexible version of the treatment effect – rather than regressing price on a dummy variable for having access to information, we use an event study specification that includes indicators for each month relative to the hospital’s “info” date:

$$P_{jht} = \sum_{mo=-12}^{+12} \beta^{Info,mo} * \mathbb{1}_{\{mo=t-t_{info_{hj}}\}} + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j}) + \varepsilon_{jht}$$

The value in the event study versus the baseline regression is that it allows us to examine differences in trends between our treatment and control hospitals that provide evidence regarding the presence of potential biases around timing of information as well as regarding any lags in the treatment effect due to sticky prices. For now, the analysis includes all products – entering products as well as products that were present in the market at the beginning of the sample. The timing of information for entering products is here defined as the first date at which the member logs into the database when there are meaningful data on other hospitals’ purchases loaded into the database.<sup>17</sup> Accordingly, we consider this to be a pooled “information” effect. To the extent that an agency mechanism were a meaningful determinant of prices after join, these estimates would understate the magnitude of the effects of benchmarking data

<sup>16</sup>We also present multiple specifications with alternative sets of fixed effects and time trends in addition to our preferred specification.

<sup>17</sup>In the current results, this is the first login after six months post-entry – on average, nine hospitals’ data would be available two months after entry, vs. one hundred hospitals’ data six months after entry. Results are similar for non-entering products only and for different definitions regarding how much data needs to be available to provide meaningful information.

on prices.<sup>18</sup> We show results estimated only from “timing of join” variation in Appendix D.1 and find our discussion unaffected; hence we defer further consideration of “information” vs. “agency” effects until we arrive at the results that separately identify mechanisms.

Figure 8 shows results for these estimated differences between treated and untreated prices. The plot shows evidence of a slight decline in prices prior to accessing information, though the pre-trends in price in the six months leading up to the timing of information are essentially zero.<sup>19</sup> After the hospital accesses the benchmarking information, there is a steady downward trend in the price coefficients. The downward trend in the post-period may be due to price stickiness in that it may take newly-informed hospitals some time to arrive at the bargaining table.

As noted above, the date of “information” may be when a hospital joins the database or when it, as an existing member, receives information on a new product. Our discussion of potential bias has primarily focused on the potential existence of contemporaneous factors affecting prices around the timing of join. Figure 20 in Appendix D.1 shows the event study regression results estimated using only timing of join for identification; we see a nearly identical pattern, albeit with larger standard errors due to the smaller sample, indicating no evidence of a pre-trend in the months just before hospitals join the database.

In general, estimates for each relative month effect are insignificant and there is not strong evidence of a trend break. The pooled “post” effect from a differences-in-differences specification is close to zero (-\$3) and statistically insignificant. Moreover, estimated patterns are similar across the different specifications of controls, though standard errors are larger in the richest specification shown (Version 3).<sup>20</sup>

When interpreting these results, it is important to note that this is the price effect of simply having *access* to information in the database. It may understate the effect of access to information on stents if, for example, the hospital joins the database because of an interest in benchmarking its orthopedic implant prices and never considers the stent information. It could also underestimate the effect of information on price *negotiation* if there is a delay in price changes due to sticky contracts (which both institutional knowledge and the post-period trend noted above suggest is the case).<sup>21</sup> Finally, the average treatment effect of information on price is pooled across all hospital-products in the database, some of which have substantial opportunities for savings and some of which do not.

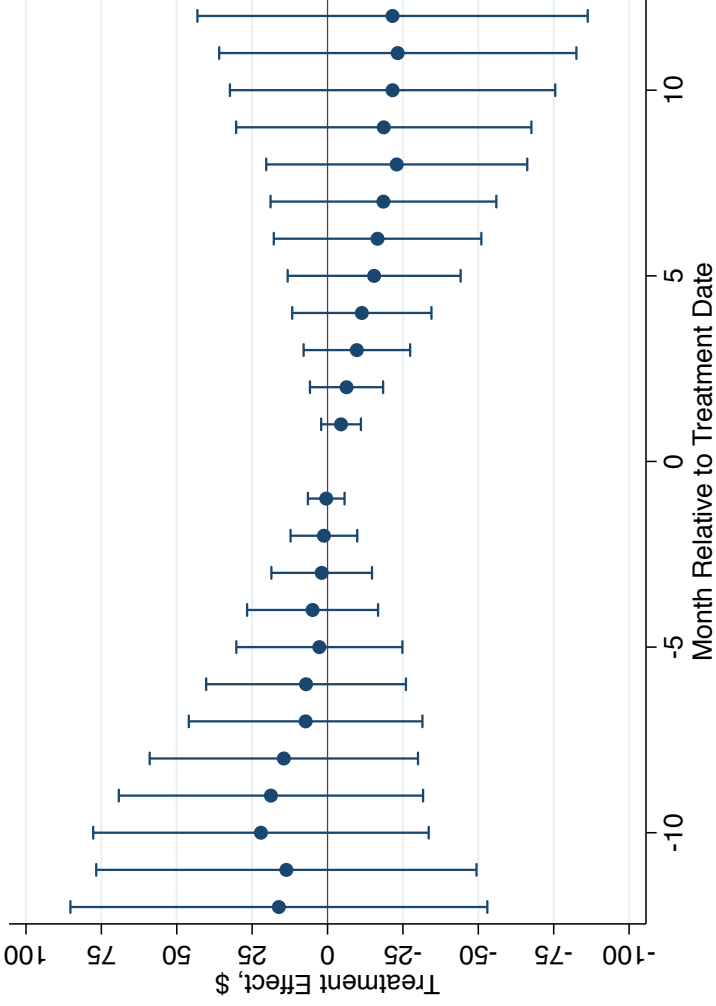
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<sup>18</sup>I.e., in Figure 6, we would be identifying a treatment effect of  $\beta^{Info}$  by comparing  $\Delta_1$  and  $\Delta_2$ , effectively omitting  $\beta^{Ag}$  from consideration for the time being.

<sup>19</sup>It should be noted that there are fewer “pre-info” observations available 6-12 months prior to accessing information because of the presence of entering products and because some hospitals do not submit retrospective data until a few months after joining the database. Accordingly, the earlier relative month effects are less precisely estimated.

<sup>20</sup>It was not possible to estimate the monthly event studies with hospital-product and product-month fixed effects. However, the quarterly event study with hospital-product and product-month fixed effects is essentially identical to the quarterly event study with hospital-product and product-specific linear trends.

<sup>21</sup>We investigate this issue in some of the following results, but we argue that the treatment effect we estimate – the combined effect of information on a particular price negotiation and the probability that price negotiation occurs – is the more important treatment effect of interest for policy as it estimates an overall value of access to benchmarking information for decreasing the total spend of hospitals on medical inputs over time.



Version	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	1	2	3	4	5	6	7	8	9	10	11	12
1	17 (14)	15 (14)	20* (11)	20* (11)	17* (9)	3 (7)	-1 (6)	-5 (5)	3 (5)	0 (4)	-1 (3)	3 (3)	3 (3)	-8† (3)	-10† (4)	-14† (4)	-16† (4)	-20† (5)	-21† (5)	-22† (6)	-24† (6)	-22† (7)	-20† (7)	-21† (8)
2	22 (14)	19 (14)	19* (11)	22** (10)	16* (9)	7 (8)	5 (8)	1 (7)	9 (6)	8 (6)	4 (5)	6 (5)	-13† (4)	-17† (4)	-18† (5)	-21† (6)	-27† (7)	-27† (7)	-25† (7)	-24† (8)	-24† (8)	-21** (9)	-23† (9)	-18* (9)
3	16 (35)	14 (32)	22 (28)	19 (26)	14 (23)	7 (20)	7 (17)	3 (14)	5 (11)	2 (9)	1 (6)	0 (3)	-4 (3)	-6 (6)	-10 (9)	-11 (12)	-15 (15)	-17 (18)	-19 (19)	-23 (22)	-19 (25)	-22 (28)	-23 (30)	-22 (33)

$N = 23,016$  member-product-months. Includes 507 members, twelve months pre- and post-join only. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Version 3) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*) at the 5% level; (\*\*) at the 10% level.

**Figure 8:** Event Studies of Treatment Effect of Access to Benchmarking Information

## 5.0.2 Treatment heterogeneity across the price distribution

Prediction 1 of Section 3 predicted that, in a model with asymmetric information regarding the supplier’s bargaining parameter, benchmarking would lead to price decreases in the upper part of the price distribution (for agency, whether the effect would be at the top or throughout the price distribution depended on the specifics of the model). Accordingly, for each member’s first login to the database, we compare the member’s price for each product purchased in the year prior to login to the full distribution of prices for the same product across all hospitals during the same period. We then flag each product-hospital pair based on its pre-join price relative to percentiles of the price distribution. In regression form, we interact the indicator for a hospital having access to information in the database,  $\mathbb{1}_{\{post_{hjt}\}}$ , with dummy variables for each pre-join price quintile,  $\mathbb{1}_{\{quintile_{jh,pre}\}}$ , allowing for heterogeneous treatment effects depending on whether the hospital was paying a high or low price (relative to other hospitals) for the product at the time of “information”:

$$P_{jht} = \beta_{quintile}^{Info} * \mathbb{1}_{\{post_{hjt}\}} * \mathbb{1}_{\{quintile_{jh,pre}\}} + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j}) + \varepsilon_{jht}$$

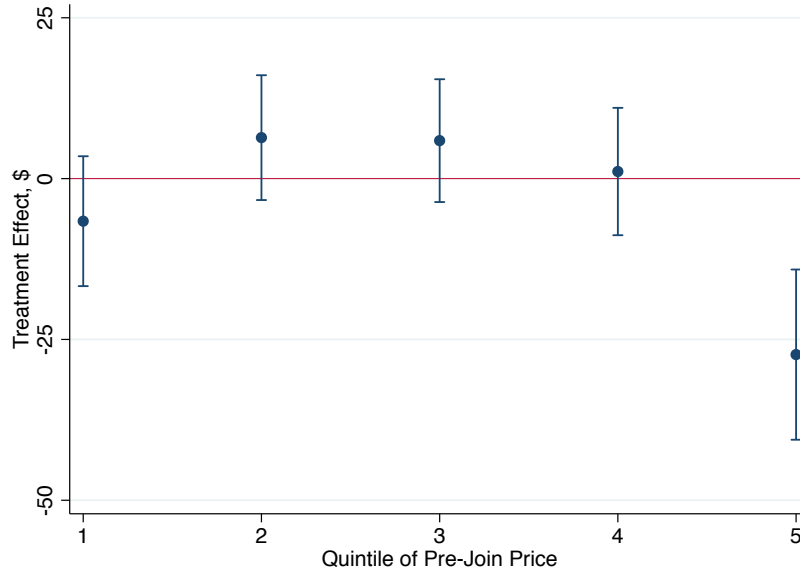
$$\mathbb{1}_{\{quintile_{jh,pre}\}} = \mathbb{1}_{P_{jh,pre} \in quintile(\{P_{jh',pre}\}_{h'=1}^H)}$$

where the coefficient  $\beta_{quintile}^{Info}$  is the treatment effect of accessing information in the benchmarking service, for each quintile of the pre-information price distribution. Figure 9 shows the results.

The treatment effects exhibit substantial heterogeneity depending on the pre-information price the hospital was paying for a product relative to others. The treatment effects are statistically zero in all but the top quintile of the pre-information price distribution, where the effect is -\$27 in our preferred specification. This evidence is consistent with Prediction 1, which predicted that, absent benchmarking, pessimistic hospitals would pay suppliers high prices regardless of those hospitals’ true bargaining parameter, so that benchmarking would lead those hospitals to negotiate lower prices after joining. It is also worth noting that we do not see evidence that the lower part of the distribution shifts upward significantly, which would be suggestive evidence of mean reversion (we define the interaction term based on previous periods’ prices).

The estimated treatment effects do not vary substantially across specification *except* in the top quintile of prices. There, the estimates are significantly smaller when we control for hospital-by-product (rather than hospital and product) fixed effects. This may be evidence of hospitals’ shifting volume to less expensive products after accessing the database; we consider the preferred specification to be a conservative estimate of the effects of information on negotiated price within hospital-product.

We also performed the event study analysis separately for each quintile of the price distribution. The results for the top quintile of the pre-information price distribution are shown in Figure 10. The Figure repeats the patterns from the ATE: Again, the pre-trends in the six months pre-information are essentially zero, while there is a steady decline in prices af-



Version	Pre-info price quintiles ( $\beta_{quintile}^{Info} =$ )				
	1	2	3	4	5
1	4 (6)	17 <sup>†</sup> (6)	9 (6)	-2 (6)	-55 <sup>†</sup> (9)
2	-1 (7)	11 (7)	4 (7)	-7 (8)	-63 <sup>†</sup> (10)
3	-7 (5)	6 (5)	6 (5)	1 (5)	-27 <sup>†</sup> (7)
4	-10 (6)	1 (6)	2 (6)	-3 (6)	-34 <sup>†</sup> (8)

$N = 32,453$  member-product-months. Includes 508 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 9:** Treatment Effect Estimates Throughout the Price Distribution

ter information access – a year after join, the treatment effect is -\$96 relative to the “info” date. The effects in the 6-12 months prior to information access are negative, though not significant; it appears that, if anything, pre-trends prior to joining the database would lead the differences-in-differences estimates to be an understatement of the effects of information on price. Focusing on the timing of join only, Figure 21 in Appendix D.1 shows a similar lack of trend leading up to the join date and a steep and steady decline in prices after join – the point estimate 12 months after join is larger (-\$154) but not statistically significantly so (recall that the estimation sample is much smaller in the “join-only” regressions).

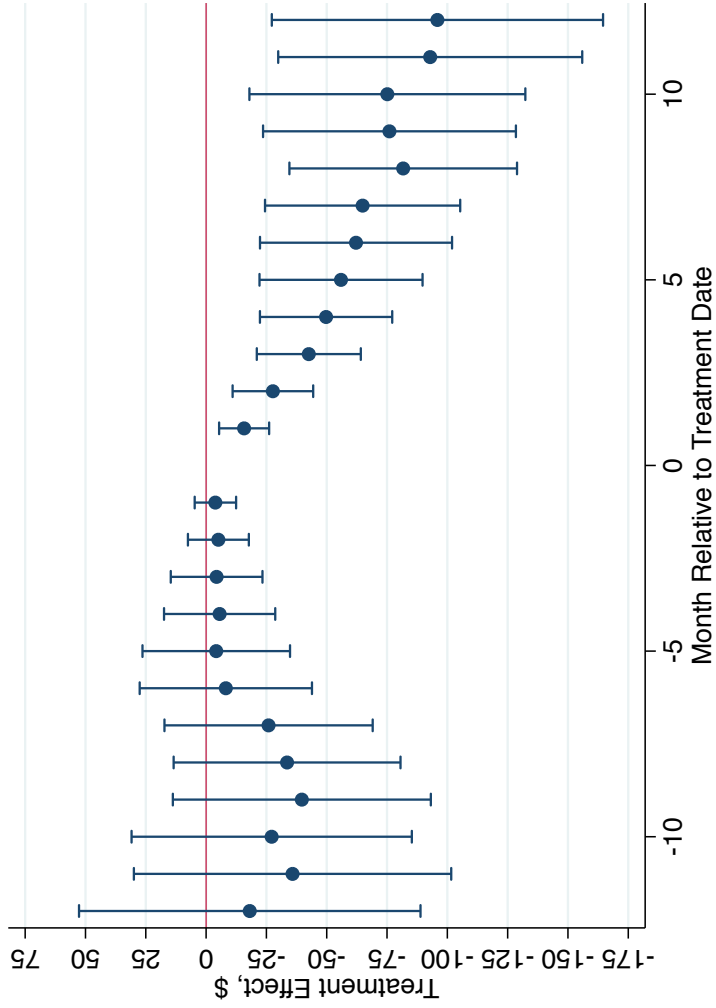
We consider these results as strong suggestive evidence that the estimated treatment effects are due to accessing the benchmarking data rather than to any potential sources of bias. The evidence of steeper negative price trends after join in the top quintile of the price distribution than there are in average prices suggests that, if there are indeed factors that cause prices to decrease after join that are unrelated to information access, they must disproportionately impact hospital-products whose prices are relatively high in the pre-period, a fact which would be unknown to parties whose behavior impacts prices without them accessing the database. In subsequent results, we will proceed under the assumption that any bias due to join timing is small (though again we note for the most skeptical interpretation that any remaining bias due to timing of join will be absorbed with our measure of the agency effect in our mechanism test, so that we are able to obtain a “clean” asymmetric information effect). Also, for the sake of statistical power and for expositional simplicity, we return to estimating pre-/post-treatment effects, rather than breaking them down by month relative to information access.

## 5.1 Mechanisms: Where and Why Does Information Matter Most?

The above results established that transparency in the form of access to benchmarking information leads to lower prices for product-hospital cases where the hospital is in the upper quintile of the price distribution (across hospitals) for that product. In this Section, we test the further predictions from Section 3 to better understand the mechanisms behind these price reductions. We first allow for treatment effects to vary with purchase volume so that we may investigate whether product-hospitals with high expenditures at stake experience larger average price changes, in keeping with a model with effort cost of search and renegotiation (Predictions 2 and 5). Next, we use the fact that for new products no benchmarking information is available in the database until several months after product entry to separate the asymmetric information mechanism from the agency mechanism (Prediction 6). Finally, we decompose the estimated price effects into price effects conditional on renegotiation and price effects due to greater likelihood of renegotiation.

### 5.1.1 Costs of putting information to use: treatment effects vary with quantity

To the extent that search and renegotiation are costly, Predictions 2 and 5 predict that benchmarking data will be sought and used most effectively for hospitals and products purchased in high quantities. To investigate these predictions, we interact our “post” variable (again



Version	Month relative to join date ( $\beta_5^{Join,mo} =$ )																							
1	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	1	2	3	4	5	6	7	8	9	10	11	12
	-24	-33	-20	-24	-14	-21*	-14	-27†	-27†	-22†	-14**	-8	-23†	-40†	-52†	-61†	-64†	-74†	-77†	-88†	-88†	-78†	-104†	-104†
	(20)	(25)	(22)	(19)	(16)	(17)	(12)	(8)	(8)	(7)	(7)	(6)	(7)	(8)	(9)	(9)	(9)	(11)	(10)	(13)	(13)	(11)	(13)	(13)
2	-17	-15	-10	-11	-6	-8	-14	-29†	-22**	-15*	-9	-6	-26†	-43†	-54†	-65†	-70†	-80†	-80†	-89†	-83†	-75†	-104†	-102†
	(19)	(24)	(22)	(18)	(15)	(17)	(13)	(10)	(9)	(8)	(7)	(7)	(7)	(9)	(9)	(10)	(11)	(13)	(11)	(13)	(14)	(12)	(13)	(13)
3	-18	-36	-27	-40	-34	-26	-8	-4	-6	-4	-5	-4	-16†	-28†	-43†	-50†	-56†	-62†	-65†	-82†	-76†	-75**	-93†	-96†
	(36)	(34)	(30)	(27)	(24)	(22)	(18)	(16)	(12)	(10)	(6)	(4)	(5)	(9)	(11)	(14)	(17)	(20)	(21)	(24)	(27)	(29)	(32)	(35)

$N = 23,016$  member-product-months. Includes 507 members, twelve months pre- and post-join only. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Version 3) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

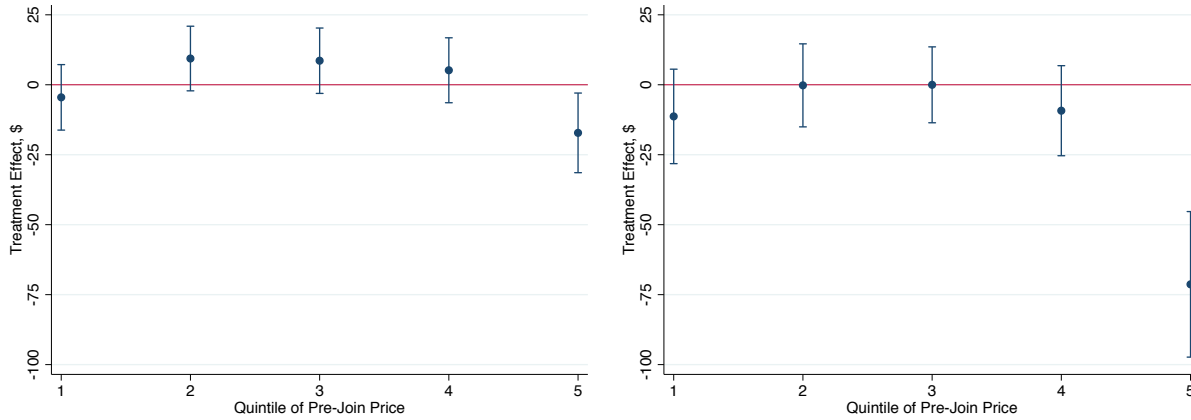
**Figure 10:** Event Studies of Treatment Effect of Access to Benchmarking Information, Top Quintile of Price Only



separately for each pre-information price quintile), with a dummy equaling one for hospital-product combinations with high purchase volumes in the period prior to information access. To implement this, we generate a dummy variable based on the quantity used of each product at each hospital:  $\mathbb{1}_{\{high_{jh,pre}^q\}}$  is equal to one for hospital-products with monthly purchase volume above the 75<sup>th</sup> percentile in the months prior to join. The specification we estimate is:

$$\begin{aligned}
 P_{jht} &= \beta_{quintile,low^q}^{Info} * \mathbb{1}_{\{post_{hjt}\}} * \mathbb{1}_{\{quintile_{jh,pre}\}} \\
 &\quad + \beta_{quintile,high^q}^{Info} * \mathbb{1}_{\{post_{hjt}\}} * \mathbb{1}_{\{quintile_{jh,pre}\}} * \mathbb{1}_{\{high_{jh}^q\}} \\
 &\quad + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j}) + \varepsilon_{jht} \\
 \mathbb{1}_{\{high_{jh}^q\}} &= \mathbb{1}_{Q_{jh,pre} \geq prctile75\{Q_{jh',pre}\}_{h'=1}^H}
 \end{aligned}$$

where  $\beta_{quintile,low^q}^{Info}$  now estimates the treatment effect, by price quintile, for lower volume products; and  $\beta_{quintile,low^q}^{Info} + \beta_{quintile,high^q}^{Info}$  now estimates the treatment effect, by price quintile, for higher volume products. The results are shown in Figure 11.



(a) Low Quantity

(b) High Quantity

Version	Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info}$ =)					Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} + \beta_{quintile,high^q}^{Info}$ =)				
	1	2	3	4	5	1	2	3	4	5
1	5 (7)	19 <sup>†</sup> (6)	16 <sup>**</sup> (7)	1 (7)	-51 <sup>†</sup> (9)	3 (10)	13 (9)	-4 (9)	-10 (10)	-73 <sup>†</sup> (15)
2	-1 (8)	12 (8)	11 (8)	-4 (8)	-60 <sup>†</sup> (11)	0 (11)	7 (10)	-12 (10)	-14 (11)	-79 <sup>†</sup> (16)
3	-4 (6)	9 (6)	9 (6)	5 (6)	-17 <sup>**</sup> (7)	-11 (9)	0 (8)	0 (7)	-9 (8)	-71 <sup>†</sup> (13)
4	-9 (7)	4 (7)	6 (7)	1 (7)	-23 <sup>†</sup> (8)	-12 (9)	-4 (8)	-5 (8)	-12 (9)	-78 <sup>†</sup> (13)

$N = 32, 453$  member-product-months. Includes 508 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 11:** Treatment Effect Estimates Across the Price and Quantity Distributions

The estimates show that the price treatment effect is largest for high-volume hospital-products in the upper part of the price distribution. At -\$71, the treatment effect for high-quantity hospital-products is double the effect for low-quantity hospital-products in the preferred specification. These results are consistent with a positive effort cost of search and renegotiation leading to decreases in high prices for high-volume purchase combinations in particular. It is worth noting that high-price, high-volume products are those that would be flagged by the benchmarking database interface as targets for renegotiation according to the “potential savings” analytic.

In sum, the heterogeneity results indicate that the treatment effects of information are largest exactly where we most expect to see them – among hospital-products in the upper part of the price distribution pre-join, among products with the largest budgetary impact on hospitals ex ante, and in hospital-products with the largest potential savings. As shown in Appendix D.2, we see a similar pattern when we modify the regression sample to focus on only the twelve months pre- and post-information, when we identify treatment effects based only on the information shock of database “join”, and when we limit the sample to hospitals only. We also see a similar pattern for bare metal stents, though the top quintile treatment effects are smaller as would be expected given the lower volumes at stake. See Appendix D.3 for detail.

### 5.1.2 Differentiating between agency and asymmetric information mechanisms

The  $\beta^{Info}$  estimates thus far have provided a treatment effect of access to the benchmarking information, subsuming several potential theoretical mechanisms, in particular the agency and asymmetric information mechanisms that market participants put forth, as outlined in our Section 3. In this Section, we will separate these two theories. The key insight that we rely upon is that the different theories require different *timing* of access to information – using the benchmarking data to resolve asymmetric information about the seller’s bargaining type requires concurrent access to the data, while using the benchmarking data to better resolve agency problems within the hospital by designing negotiator contracts with higher powered incentives and less risk only requires the knowledge that the data will eventually be available for the negotiator’s performance review. New product introductions offer variation in the timing of access to information, allowing us to separate these theoretical mechanisms. The fact that no information is available in the database on prices hospitals negotiate for a new product during the first several months after its introduction means that, during this time, differences between prices negotiated for that product by hospitals post- and pre-join must be attributable to the agency mechanism, not asymmetric information.

In practice we implement this separation of the two mechanisms by including two separate indicator variables regarding join and information. The first term is simply an indicator for all hospital-months after the hospital joins the benchmarking database. We also add an interaction term with our join variable that is equal to one for product-hospital-months more than six months after the introduction of that product. Almost all hospitals negotiate their first contract with a new product by the first or second month after its introduction, but the resulting

purchase order data will not begin to show up in the benchmarking database until month three or four. By month six, there are enough observations in the database for a hospital to develop a useful estimate of its place in the price distribution for that product. The specification we estimate is:

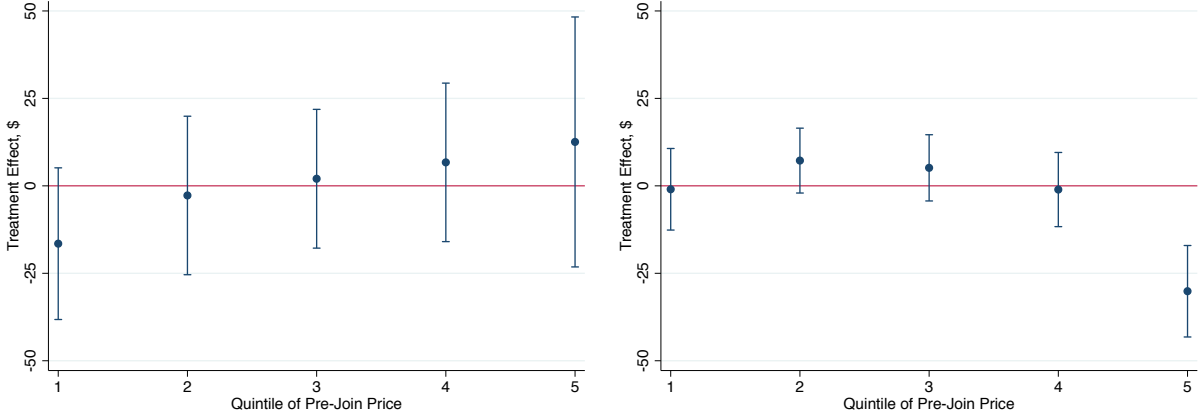
$$\begin{aligned}
P_{jht} = & \beta_{quintile}^{Agency} * \mathbb{1}_{\{post_{ht}^{join}\}} * \mathbb{1}_{\{quintile_{jh,pre}\}} \\
& + \beta_{quintile}^{Info} * \mathbb{1}_{\{post_{hjt}^{join}\}} * \mathbb{1}_{\{quintile_{jh,pre}\}} * \mathbb{1}_{\{(t-t_{min,j})>6\}} \\
& + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min,j}) + \varepsilon_{jht}
\end{aligned}$$

where  $\mathbb{1}_{\{(t-t_{min,j})>6\}}$  is a dummy equal to one greater than six months after a product’s entry date and zero during the first six months when zero to little concurrent benchmarking information is available. Given that a hospital-product’s position in the price and quantity distribution cannot be determined before transaction data are observed, the “agency” effect is estimated based on some of the same observations that are used to calculate the price and quantity distributions – before benchmarking information is observed for a given product but after hospitals have joined the database. The interactions between the post-join, or “agency,” effect and the indicators for position in the price distribution should be interpreted as capturing the effect of having *joined* the database on the price level within each price quintile – that is, the regression determines whether joining the database, absent information, shifts the upper, or lower, part of the price distribution. The results are shown in Figure 12.

While the separate results are estimated more imprecisely, the point estimates consistently suggest that the asymmetric information effect explains a substantial portion of information on prices. For the product-hospital fixed effects model shown in the Figure, the effect of information on price is approximately -\$30 in the top 20% of the price distribution, which is nearly identical to our main results. The estimated effect of agency on price is extremely noisy, but not statistically significantly different from zero after controlling for unobserved differences across hospital-product combinations. However, in specifications versions 1 and 2 with hospital fixed effects only,  $\beta^{Ag}$  and  $\beta^{Info}$  each explain approximately half of the total effect. It is possible that they difference between these two studies points to challenges with attenuation bias in the product-hospital fixed effect models, which leave very little identifying variation, especially for  $\beta^{Ag}$ .

While our interpretation of the event study evidence is that bias due to endogenous timing of join is unlikely to be large, it is important to note that in the most pessimistic case that the timing of join correlates with other hospital activities unrelated to benchmarking information that decrease prices, this bias will be captured in  $\beta^{Agency}$  but not  $\beta^{Info}$ . This is because in our research design,  $\beta^{Agency}$  is identified by any differences between pre- and post-join hospitals that are *not* due to contemporaneous access to information. Consistent with the discussion of the event studies by quintile, these estimates would suggest that, if anything, the total effect of information on price estimated in the first quintiles specification is biased toward zero.

Our most robust finding is that for the presence of asymmetric information in these negoti-



(a) Agency

(b) Asymmetric Info

Version	Pre-info price quintiles ( $\beta_{quintile}^{Agency} =$ )					Pre-info price quintiles ( $\beta_{quintile}^{Info} =$ )				
	1	2	3	4	5	1	2	3	4	5
1	8 (12)	14 (11)	-9 (10)	-21* (12)	-60 <sup>†</sup> (21)	3 (6)	12** (5)	15 <sup>†</sup> (5)	8 (7)	-33 <sup>†</sup> (7)
2	19 (13)	31** (12)	1 (11)	-11 (14)	-53** (22)	-8 (9)	-2 (8)	4 (8)	-2 (10)	-46 <sup>†</sup> (10)
3	-17 (11)	-3 (12)	2 (10)	7 (12)	13 (18)	-1 (6)	7 (5)	5 (5)	-1 (5)	-30 <sup>†</sup> (7)
4	-3 (12)	16 (12)	16 (11)	18 (12)	25 (19)	-15* (8)	-11 (7)	-11 (7)	-15** (7)	-47 <sup>†</sup> (9)

$N = 32,453$  member-product-months. Includes 508 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (<sup>†</sup>) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 12:** Treatment Effect Estimates Across the Price Distribution, Separating Agency and Asymmetric Information Mechanisms

ations. Our finding of a statistically and economically significant (and free of joint timing bias)  $\beta^{Info}$ —concentrated among those paying the highest prices before obtaining information—is consistent with the theory of asymmetric information bargaining based on Rubinstein (1985).

For empirical work, this suggests that asymmetric information may be among the effects driving the heterogeneity found in bargaining parameter estimates in studies using full information Nash Equilibrium of Nash Bargaining models, such as Crawford and Yurukoglu (2012) and Grennan (2013, 2014). In the most innocuous case, it suggests these information and incentive issues should be kept in mind when thinking about the factors driving bargaining outcomes. A corollary to this is that when considering counterfactuals with negotiated prices, it may be important to consider how information might change in the counterfactual regime, and in what way any information changes might induce changes in the relevant bargaining parameters used in estimating the negotiated outcomes.

### 5.1.3 Price changes with “sticky” contracts

All of the price coefficient estimates reported above can be described as capturing the combined effect of information on the probability that price negotiation occurs *and* on prices arrived at during each price negotiation. We consider this to be the treatment effect of interest for policy, as it estimates the overall value of access to benchmarking information for decreasing the total spend of hospitals on medical inputs over time, taking into account the stickiness of prices in real-world hospital-supplier contracting. That is, the above estimates measure the treatment effect of information on prices paid, whereas another object of interest would be the treatment effect of information on prices negotiated (which requires that renegotiation take place). In this Section, we separately consider the effects of information on price *conditional on renegotiation* and on the *likelihood of renegotiation*.

In order to estimate these two effects, we sort transactions for each hospital-product by month and group observations with the same price together within month. We then flag each hospital-product-month as including a renegotiation event if we observe that prices change *and* that the price change “sticks” for two cumulative months after the renegotiation event (or until the final observed purchase for that member-product). This is a relatively conservative method for flagging renegotiations; the results are qualitatively similar (though larger in magnitude) using a less conservative method that flags all months in which average prices change. We then estimate the usual price quintiles specification on the subset of months in which renegotiation takes place:

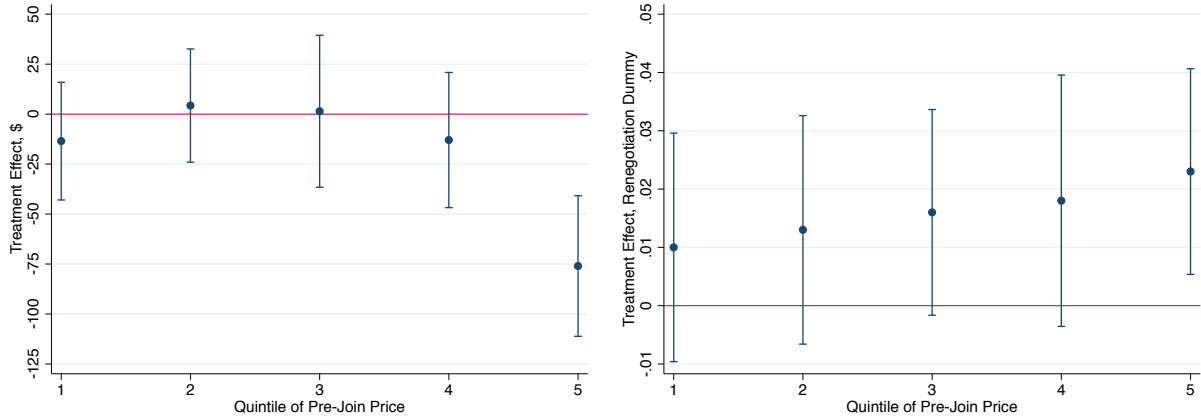
$$P_{jht} = \beta_{quintile}^{Info} * \mathbb{1}_{\{post_{h,jt}\}} * \mathbb{1}_{\{quintile_{j,h,pre}\}} + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j}) + \varepsilon_{jht}$$

as well as a specification where the dependent variable is a dummy for renegotiation taking

place:

$$\mathbb{1}_{\{reneh_{jt}\}} = \beta_{quintile}^{Info} * \mathbb{1}_{\{post_{ht}\}} * \mathbb{1}_{\{quintile_{jh,pre}\}} + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j}) + \varepsilon_{jht}.$$

In the main estimation sample, renegotiations take place in 9% of observations (member-product-months with any transactions). Transactions do not occur in every month for every hospital-product, so this corresponds to a little under one renegotiation per year for the average hospital-product (for which we observe a time horizon of at least one year). In the pre-information sample, prices decrease on average by \$25 at each renegotiation. Hence, we would expect small price changes to occur if information led to larger price decreases at each renegotiation *or* if information increased the likelihood of renegotiation. The results are shown in Figure 13.



(a) Conditional on Renegotiation						(b) $\mathbb{1}_{\{post_{ht}\}}$				
Version	Pre-join price quintiles ( $\beta_{quintile}^{Join} =$ )					Pre-join price quintiles ( $\beta_{quintile}^{Join} =$ )				
	1	2	3	4	5	1	2	3	4	5
1	-7 (12)	13 (12)	11 (14)	-22 (14)	-91 <sup>†</sup> (19)	.008 (.008)	.002 (.009)	.018** (.008)	.018* (.01)	.034 <sup>†</sup> (.008)
2	-11 (16)	5 (15)	1 (17)	-25 (17)	-111 <sup>†</sup> (23)	.017* (.009)	.01 (.01)	.023** (.009)	.025** (.011)	.04 <sup>†</sup> (.009)
3	-14 (15)	4 (14)	1 (19)	-13 (17)	-76 <sup>†</sup> (18)	.01 (.01)	.013 (.01)	.016* (.009)	.018 (.011)	.023** (.009)
4	-8 (19)	4 (19)	-3 (22)	-11 (20)	-80 <sup>†</sup> (22)	.022** (.011)	.022* (.012)	.024** (.01)	.029** (.012)	.032 <sup>†</sup> (.011)

$N = 6,510$  member-product-months in regressions conditional on renegotiation.  $N = 32,453$  member-product-months in renegotiation dummies regression. Includes 508 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 13:** Treatment Effects Conditional on Renegotiation and on Occurrence of Renegotiation

The results in the left panel of Figure 13 show that the effect of information on price conditional on renegotiation is substantially larger than the effect of information on price paid.

In the top quintile of the price distribution, we see that the price decrease at renegotiation is about \$75 larger when hospitals have access to benchmarking information and learn that their previous prices were relatively high. The effects are estimated imprecisely, but the 95% confidence intervals indicate that price decreases at renegotiation are two to three times larger with information than without. The right panel of the Figure shows that information increases the likelihood of renegotiation throughout the price distribution – in the top quintile of the price distribution, information increases the probability of renegotiation by 2.3 percentage points, which is nearly one-third the baseline probability of renegotiation.

Taken together, these results show that information affects price paid through two channels, by making renegotiation more likely to occur and by leading to larger price decreases when renegotiation takes place.

## 5.2 Achieved Savings from Information

Thus far we have considered the magnitude of the information effects in dollar terms at the product-hospital-transaction level, allowing for heterogeneity in treatment effects. In this Section, we use those treatment effect estimates – properly weighted according to the observed volume and price distributions – to calculate the savings achieved due to access to benchmarking information. We compare this percent “achieved savings” to the “potential savings” numbers that are based on the pre-information heterogeneity in prices across hospitals.<sup>22</sup> We then display the distribution of savings across hospitals in dollars per year. These numbers are informative for examining the value of the benchmarking service whose data we study, and also as a step towards projecting the potential aggregate savings in the case that a transparency policy such as the ones proposed by policymakers were to achieve the same treatment effect as the benchmarking service we study. We take care to interpret these projections with caution due to the potentially selected nature of our sample and the potential supply side responses to a nationwide policy that may not be captured in our treatment effect.

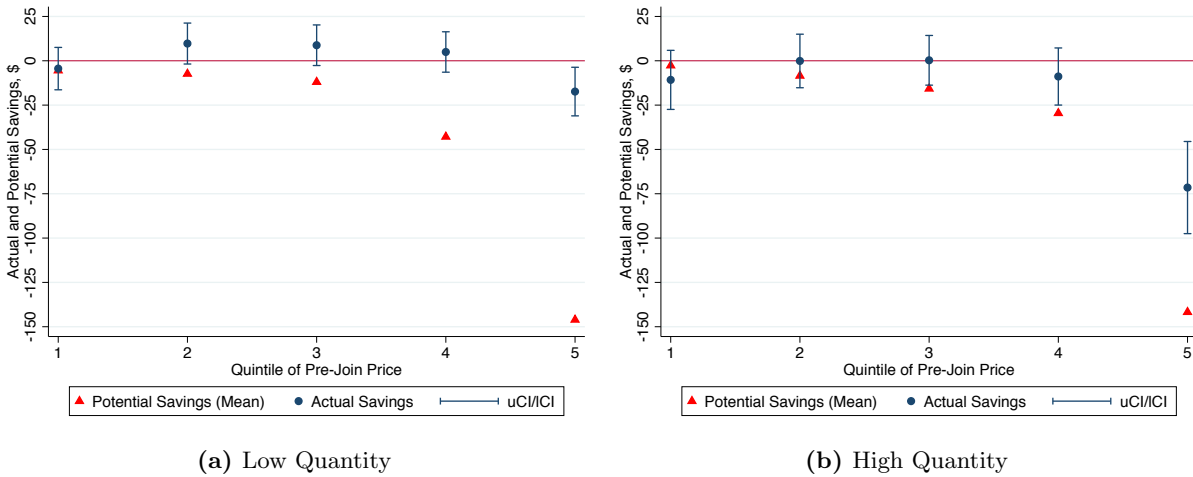
Recall that, in Section 2, we constructed “potential savings” based on the heterogeneity in the prices paid by different hospitals for the same product over the same period of time. We constructed this metric using the subset of our data containing each hospital’s year of pre-information data, and extracting a product-hospital specific fixed effect, controlling for product-time fixed effects ( $\hat{p}_{jh}$  from the regression  $p_{jht} = \hat{p}_{jh} + \hat{p}_{jt} + \hat{\varepsilon}_{jht}$ ). Potential savings is defined as the difference between this and the mean of the distribution across hospitals for each product:  $PS_{jh} := \max\{0, \hat{p}_{jh} - \bar{p}_j\}$ .

Similarly, we can use our estimated treatment effects across the price and quantity distributions to construct “achieved savings” due to access to the benchmarking information service, in dollars per hospital-product:  $AS_{jh}^{Info} := \hat{\beta}^{Info}(quintile_{jh}, high_{jh}^q)$ .

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<sup>22</sup>Depending on the specific model of the world, achieved savings need not be bounded above by potential savings, and further the potential savings may be due largely to hospital- or product-hospital-specific factors that have nothing to do with information. However, among many reasonable models of the world, potential savings is exactly an upper bound for what might be achieved by information, and given the policy interest based upon the observed variation in prices across hospitals, this seems like a natural and useful benchmark.

Figure 14 displays potential and achieved savings per stent across product-hospitals based on their position in the price/quantity distributions. Each calculation is based only on the data used to estimate the regression specifications (e.g., excluding pre-join data for facilities joining the database before Q1 2010). High-price (and particularly high-price, high-quantity) hospital-products achieved substantial savings – in the top quintile of the price distribution, hospitals achieved 12-51% of potential savings (defined as savings that would accrue if all prices  $\hat{p}_{jh}$  were altered to  $\tilde{p}_{jh} = \min\{\hat{p}_{jh}, \text{mean}_h\{\hat{p}_{jh}\}\}$ ). Savings are not substantial for lower points in the price distribution, but it should be noted that potential savings are mechanically not substantial for hospital-products already achieving lower prices.



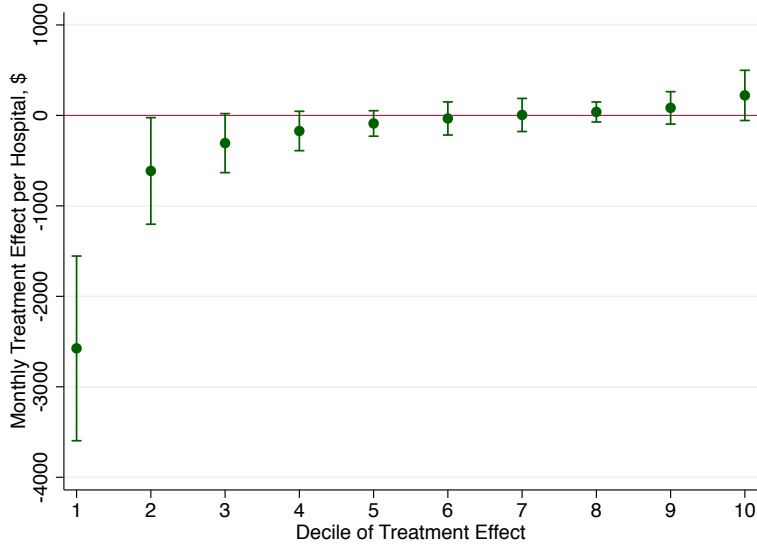
Version	Pre-join price quintiles ( $PS/AS_{quintile,low^q} =$ )					Pre-join price quintiles ( $PS/AS_{quintile,high^q} =$ )				
	1	2	3	4	5	1	2	3	4	5
Potential savings	-6	-7	-12	-43	-146	-3	-8	-16	-30	-142
Achieved savings	-4	10*	9	5	-17**	-11	0	0	-9	-72 <sup>†</sup>
	(6)	(6)	(6)	(6)	(7)	(8)	(8)	(7)	(8)	(13)

$N = 32,453$  member-product-months. Includes 508 members. Both actual and potential savings based on regressions using “Version 3” controls. Potential savings calculated using pre-information data only. Bootstrapped standard errors based on 1,000 draws from full variance-covariance matrix of parameter estimates shown in parentheses. Original standard errors clustered at hospital-product level. Superscript (<sup>†</sup>) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 14:** Achieved Savings vs. Potential Savings per Stent

Figure 15 displays the distributions of savings achieved by the hospitals in our sample, in terms of total savings per hospital per month. The average hospital achieves \$346 in savings on stents per month, but this average effect conceals substantial heterogeneity. 10% of sample hospitals save \$2,500 per month on stents alone, and savings are statistically significant below the 20<sup>th</sup> percentile. At the top of the distribution, the estimates indicate that 10% of hospitals lose \$221 after joining the database, but none of the positive effects are statistically significant at conventional levels. Across all hospitals, savings on drug-eluting stents are estimated to be 26% of potential savings.





Percentile of Treatment Effect ( $TE_{decile}/month/hospital=$ )									
10	20	30	40	50	60	70	80	90	100
-2575	-614	-306	-172	-89	-34	5	38	83	221
(521)	(300)	(166)	(111)	(72)	(93)	(93)	(56)	(91)	(142)

Bootstrapped standard errors based on 1,000 draws from full variance-covariance matrix of parameter estimates shown in parentheses. Original standard errors clustered at hospital-product level. Achieved savings weighted up to hospital level using purchase quantities of each product per month within each hospital's reporting horizon.

**Figure 15: Achieved Savings Across Hospitals (\$/month/hospital)**

### 5.2.1 Policy Implications

The “achieved savings” figures above are limited in their applicability for policy analysis by two factors. First, they are based on a selected sample of hospitals that voluntarily joined a purchasing database. Joining hospitals may differ from other hospitals in the US in both observable and unobservable ways; thus, our results may not generalize to US hospitals overall. Second, our results may be “partial equilibrium” results in that they may not capture the effects of a policy of full transparency applied to all US hospitals.

To the first issue, we can extend our results to a “more” representative sample of US hospitals using the previously mention MRG survey of catheter labs (the source that major device manufacturers subscribe to for detailed market research). To perform this analysis, we flag each outside sample hospital based on the position in the price and quantity distributions it would have held for its most recent observation month. We then apply the treatment effects we estimated allowing for price and quantity heterogeneity to all outside sample hospital observations. As discussed above, the regression sample hospitals are larger and have ex ante lower prices; we find higher savings for high price and high quantity hospitals, so there are two countervailing effects that cause this counterfactual savings calculation to differ from the above estimates. On balance, we estimate that an average outside sample hospital would have achieved \$421 in savings on stents per month (vs. \$346 in our regression sample) if it accessed benchmarking data. This would suggest that our estimates are conservative with respect to the US population of facilities overall. However, we cannot control for unobservable ways in

which our sample differs from the average US hospital with a catheterization lab (e.g., joiners may have better management practices and be better able to utilize databases). We hope to explore this issue in ongoing work with better data on database hospitals.

To the second issue, our analysis is one of the effect of introducing benchmarking information to a hospitals in a way that we expect captures supply responses in a limited fashion. To the extent manufacturers know when a hospital has access to this information (which we understand they do), then our estimates incorporate effects on hospitals who get “bad news” when joining the database or reluctance of manufacturers to lower prices to database members. We also do not find any evidence of increasing “obfuscation” in the form of proliferation of SKUs as more hospitals adopt the benchmarking database over time. However, our data does not allow us to be certain the extent to which the incentives for and activities regarding supply side price cuts, collusion, or obfuscation might change with a rollout of a broader transparency policy.

## 6 Conclusion

This paper conducts one of the first studies of the impact of information in negotiated price markets. It brings new insights into the potential economic impacts of the rise in benchmarking data services marketed towards buyers in business-to-business markets, as well as calls for greater transparency in these markets by policymakers. Our empirical study is done in the context of hospital supply purchasing, an area where there has been keen interest in information as a way to decrease hospital supply costs. We use new data on all purchase orders issued by over sixteen percent of US hospitals from 2009-14 and a differences-in-differences research design to compare the prices negotiated by hospitals with and without benchmarking information on what other hospitals pay. The estimated average treatment effect of this type of information across all product-hospital-months for coronary stents ranges is small, but masks dramatic heterogeneity. We estimate that the conditional average treatment effects are large for hospitals paying especially high prices for a given product, and even larger when these products are also used in large volume. There is also evidence that stickiness in price renegotiation mutes the value hospitals can reap from benchmarking information.

While our results suggest that on net policies or intermediaries that increase transparency may indeed lower the prices hospitals pay for medical supplies, our hope is that this study opens more doors than it closes. Coronary stents are just one product category (albeit an important one), and the results are likely to be different for different medical products, let alone for different industries. While our data contains purchase orders for nearly 3,000 categories and 2 million product SKUs, analysis of other product categories using this price and quantity data alone may be complicated by the impact of unobserved nonlinearities or bundling in contracts. We believe this reinforces the need for more data collection and theory development.

In the large existing theory on bargaining and incomplete information, we were surprised that no model quite captured the main phenomena of interest here. We see modeling frictions in the use of information and the potential for information to affect within-firm agency frictions in negotiation as two especially interesting areas suggested by our analysis for future theory

development.

# Appendices

## A Data Appendix

The raw transactions data contain XXX observations for XXX members across XXX product categories and XXX SKUs. In this project, we focus on coronary stents – for this subset of products, we observe XXX observations for XXX members across XXX brands.

In order to construct each member’s information set upon joining the database and later upon new products’ entry, we linked the transactions data with several additional datasets relating each individual login ID with associated members and login activity. The first of these is a “clickstream” dataset containing timestamped observations of unique IDs’ login activity, to the minute.<sup>23</sup> The second is a membership “hierarchy” file linking individual database members with parent accounts for those cases where members are part of a set of health care organizations purchasing membership jointly. The third file associates each login ID with the given individual’s direct-linked member organization and broader access level – i.e., the individual with ID X may work with member 1 but have access to the data for all members 1, 2, and 3 under the same parent organization. For individuals with higher-level access, data for all associated members is automatically reported to them when they log in to the database. Accordingly, when we observe a click for a particular login ID, we associate that click with all linked member organizations for which that login has access.

We use the above-described datasets to determine each date of login for each member-product. The goal of this exercise is to determine when benchmarking data on a given product would enter each member’s information set. For non-entering products, this is the date of the first observed login for each member. For entering products, this is the date of the first observed login for each member after six months post-entry. This is to account for the lag between transactions occurring for new products and transactions being submitted by member facilities, loaded into the database, and viewed by members logging in. The left panel of Figure 16 displays the steady increase over time in the count of members for which transactions for the average entering product are observed, and demonstrates the lag with which members’ transactions become available in the database for benchmarking purposes.<sup>24</sup> In any given month in the year following new product entry, there are on average 56 more members we observe having transactions for new products than there are members whose transactions data are actually loaded into the database. Part of this difference is due to the fact that we observe transactions data for members that have not yet joined the benchmarking database – in the year following product entry, nine percent of members observed in the average month are pre-join. To see this more concretely, the right panel of Figure 16 displays the trend in the number of hospitals purchasing the average new product, overall and for pre-join hospitals in

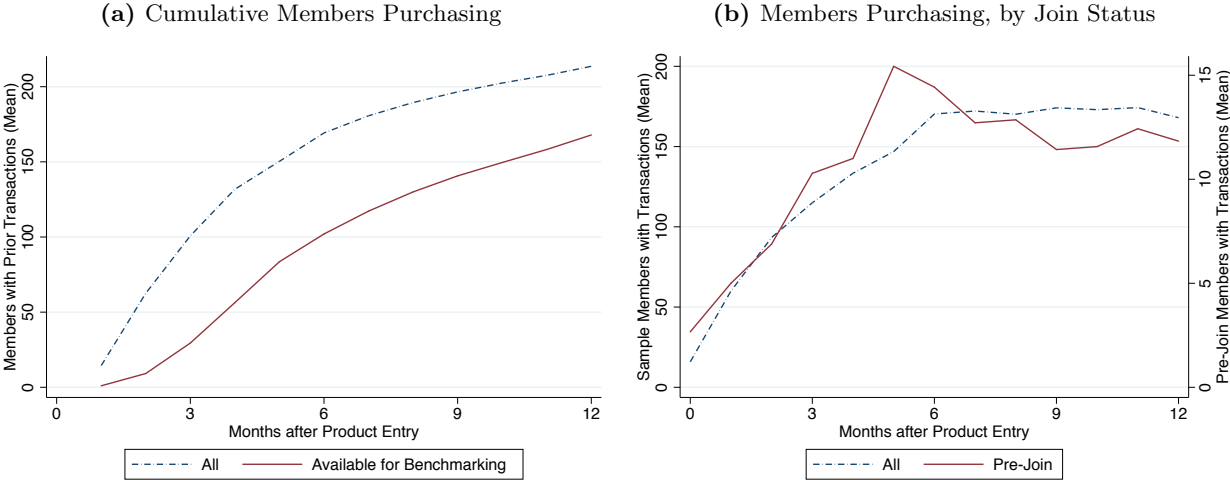
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<sup>23</sup>Each login ID corresponds to a unique individual’s account within a member. For example, a given database member may have login accounts for a number of different purchasing managers, administrators, and department clinicians.

<sup>24</sup>There may be an additional lag before joining hospitals become informed if they do not frequently log in to the database.

particular. For each new product, we observe 10-15 hospitals in the pre-join state within a short window after product entry, and the time period for our study contains many meaningful product introductions. This is precisely what allows us to separately identify effects of *joining* the database per se, versus actually having access to information *on a particular product*, on prices. In Figure 4 in the main text, we note the timing of entry of nine new products between 2010 and 2014 (of the twenty products sold during this time period overall).

**Figure 16:** Transactions Observed After New Product Entry

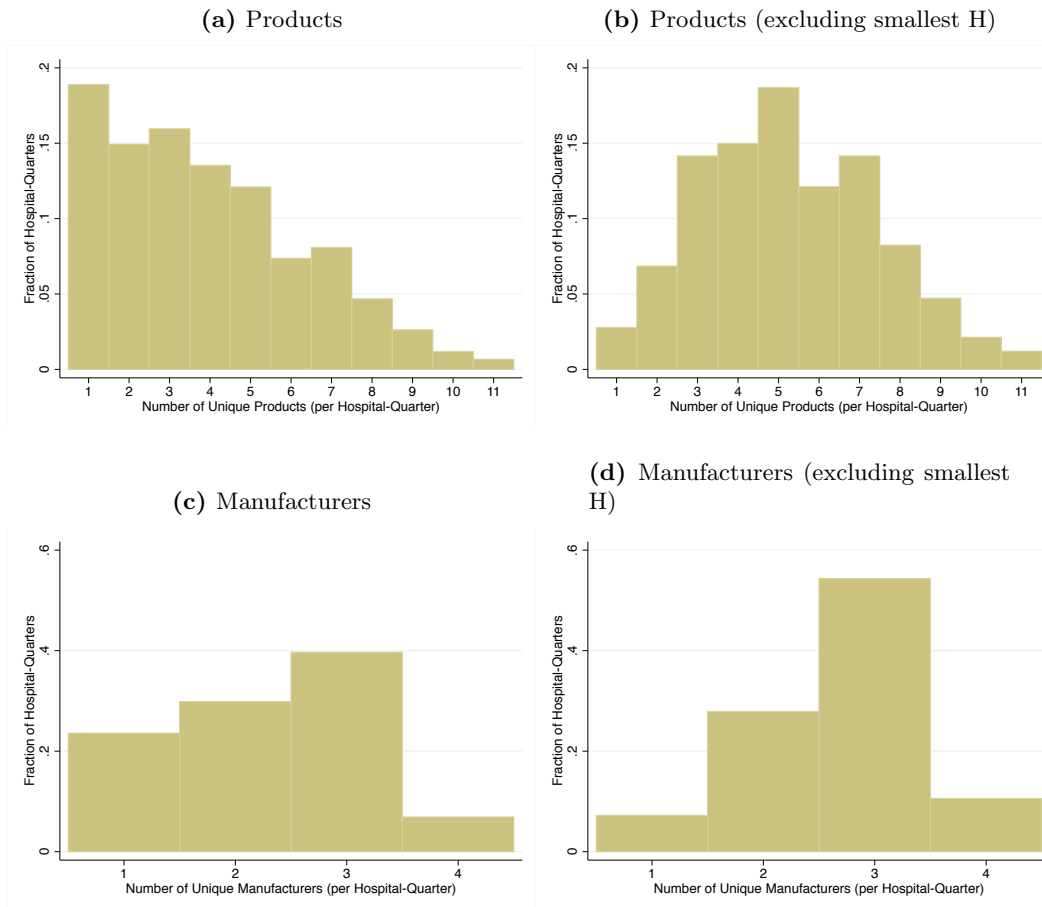


We use the linked login and transactions data to calculate each member-product’s position in the pre-information price and quantity distributions. All calculations are specific to the first *informed* login for the given member-product, as defined above. Following the approach used in the database to aggregate data across all members’ transactions and present summary statistics to members logging in at a point in time, we calculate percentiles of the price distribution using all members’ *most recent* transactions for the same product, in the past year, that would have been loaded into the database prior to the login. We calculate percentiles of the quantity distribution using all members’ total quantity purchased per month for the same product in the past year. Across all specifications, we consistently include only those observations that can be used to estimate the richest specification with interactions based on point in the price and quantity distributions – this requires that we observe pre-information data for the given member-product. The final analysis sample of drug-eluting stents only includes XXX transactions for XXX members and XXX brands in XXX months between MMYYYY and MMYYYY. XXX of the included brands entered the market during this time horizon. We collapse the transaction-level data to perform all analyses at the member-product-month level (with mean price as the dependent variable). We do this in order to avoid overweighting member-products that tend to have multiple transactions per month. The analytic sample contains XXX observations.

## B Checks for Standardization and Share-based Contracts

It is important for our analysis that the prices we observe are comparable across observations in the sense that there are not important contract dimensions that we do not observe (e.g. bundling, exclusive, or market share based contracting). Our conversations with industry participants indicate that stents tend to have simple linear price contracts, so that we can be confident our transactions data captures real prices. Where possible, we confirm these assumptions in the data.

**Figure 17:** Histograms – Number of Unique ProductsManufacturers per Hospital-Quarter



Regressions of price on indicators for exclusivity:		
Specification	$\beta^{Excl}$	(s.e.)
$p_{jht} = \beta^{Excl} \mathbf{1}_{\{ \mathcal{J}_{ht} =1\}} + \theta_{jt} + \epsilon_{jht}$	12.2	(11.5)
$p_{jht} = \beta^{Excl} \mathbf{1}_{\{ \mathcal{J}_{ht} =1\}} + \theta_{jt} + \theta_h + \epsilon_{jht}$	-9.4	(10.7)
$p_{jht} = \beta^{Excl} \mathbf{1}_{\{ \mathcal{J}_{ht} =1\}} + \theta_{jt} + \theta_{jh} + \epsilon_{jht}$	-2.9	(8.0)
$p_{jht} = \beta^{Excl} \mathbf{1}_{\{ \mathcal{M}_{ht} =1\}} + \theta_{jt} + \epsilon_{jht}$	18.3	(13.5)
$p_{jht} = \beta^{Excl} \mathbf{1}_{\{ \mathcal{M}_{ht} =1\}} + \theta_{jt} + \theta_h + \epsilon_{jht}$	-7.5	(9.9)
$p_{jht} = \beta^{Excl} \mathbf{1}_{\{ \mathcal{M}_{ht} =1\}} + \theta_{jt} + \theta_{jh} + \epsilon_{jht}$	-4.2	(6.8)

$N = 32,223$ . Standard errors clustered at hospital level,  $N_h = 507$  in first two specifications and product-hospital level  $N_{jh} = 2227$  in product hospital fixed effects specification.

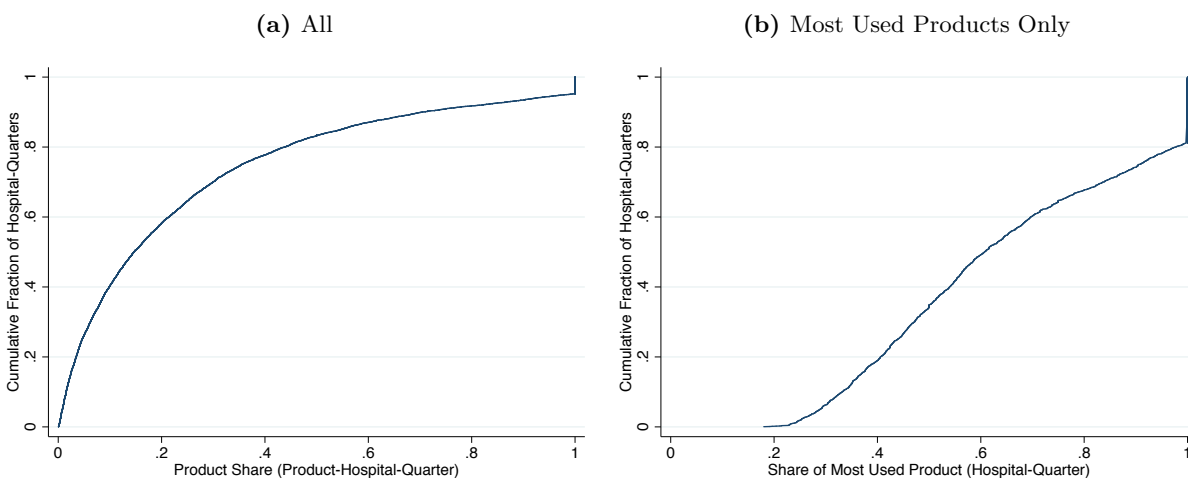
In Figure 17, we show histograms of total unique manufacturers and stent products (brands)

purchased over each quarter by each hospital in the sample. The vast majority of hospitals purchase multiple brands from multiple manufacturers, rather than purchasing a single most-preferred product for the whole facility. Panels (b) and (d) show these histograms for only hospitals above the 25 percentile in total stent volume, and show even less “exclusivity” with less than three percent of hospital-quarters seeing a single product used and seven percent seeing a single manufacturer used. These patterns are consistent with the anecdotes that exclusivity does not play a systematic role in stent contracting, and that the majority of the already small amount of observed “exclusivity” occurs at smaller hospitals where a small number of physicians and patients drive the sole sourcing in usage via physician preference and/or small usage samples at the quarter level.

As a further check, we look at the pricing consequences of the observed sole sourcing in the usage data. For the minority of hospitals that do happen to use only a single product or manufacturer in a given quarter, we create an indicators for  $\mathbf{1}_{\{|\mathcal{J}_{ht}|=1\}}$  and  $\mathbf{1}_{\{|\mathcal{M}_{ht}|=1\}}$  and regress price on each indicator and product-month fixed effects  $\theta_{jt}$ . The resulting small, positive point estimates are not statistically different from zero, again suggesting that the small amount of sole sourcing observed is due to other factors besides contracting concerns.

We also check for any evidence of near-exclusivity in the form of market share based contracts (which we are told are commonly used for many medical products, but not stents). Figure 18 plots the cumulative density of observations by product market share at the hospital-quarter level. We do not observe the bunching that we would expect if contracts commonly specified market share thresholds in either the full sample (panel (a)) or restricting to the most used product at each hospital (panel (b)).

**Figure 18:** Cumulative distributions by market share.



## C Usage Pattern Changes and Price Sensitivity of Demand

In general the price benchmarking information treatment could influence quantities as well as prices. The status of stents and other expensive medical technologies as “physician preference items” where physician demand is based on strong preferences and relatively insensitive to price suggests this would be unlikely here, but we perform a set of analyses here to provide a check of this hypothesis and also provide proof of concept for how this analysis might be incorporated in the case of products where demand is more sensitive to price.

There are two primary ways in which quantities might adjust to benchmarking price information and subsequent renegotiations: (1) In a context where contracts specify quantities or market shares in addition to price, renegotiations to obtain better prices might also involve large quantity or share commitments—this effect was tested and ruled out in our analysis in Appendix B. (2) In a context where quantity is responsive to price, negotiation of better prices would lead to increased usage in the products with the largest relative price decreases. We analyze this second case here.

We run the regression specifications allowing for heterogeneous treatment effects of information depending on pre-join prices and quantities, but here with quantity  $q_{jht}$  as the dependent variable (results are qualitatively similar and so unreported for markets shares and log transformations):

$$Q_{jht} = \beta_{quintile}^{Info} * \mathbb{1}_{\{post_{hjt}\}} * \mathbb{1}_{\{quintile_{jh,pre}\}} + \theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j}) + \varepsilon_{jht}$$

where  $\beta_{quintile}^{Info}$  estimates the treatment effect, by price quintile. The results are shown in Figure 19.

Version	Pre-info price quintiles ( $\beta_{quintile}^{Info} =$ )				
	1	2	3	4	5
$\theta_h + \theta_j + \theta_t + \gamma_j * (t - t_{min_j})$	0.2 (1.5)	1.4 (1.9)	1.3 (1.7)	0.7 (1.7)	2.4 (1.1)
$\theta_h + \theta_{jt}$	-0.1 (1.6)	2.0 (2.0)	1.6 (1.9)	1.0 (1.8)	2.9 (1.3)
$\theta_{jh} + \theta_t + \gamma_j * (t - t_{min_j})$	3.2 (1.3)	2.7 (1.7)	3.8 (1.3)	2.9 (1.3)	3.0 (1.1)
$\theta_{jh} + \theta_{jt}$	4.0 (1.5)	3.9 (2.0)	5.2 (1.5)	4.2 (1.5)	5.0 (1.3)

$N = 32,453$  member-product-months. Includes 508 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 19:** Treatment Effect on Quantity Estimates Across the Price Distribution

If quantity were responsive to price (with downward sloping demand), then we would expect quantity/share increases in exactly the areas we see relative price decreases. Because

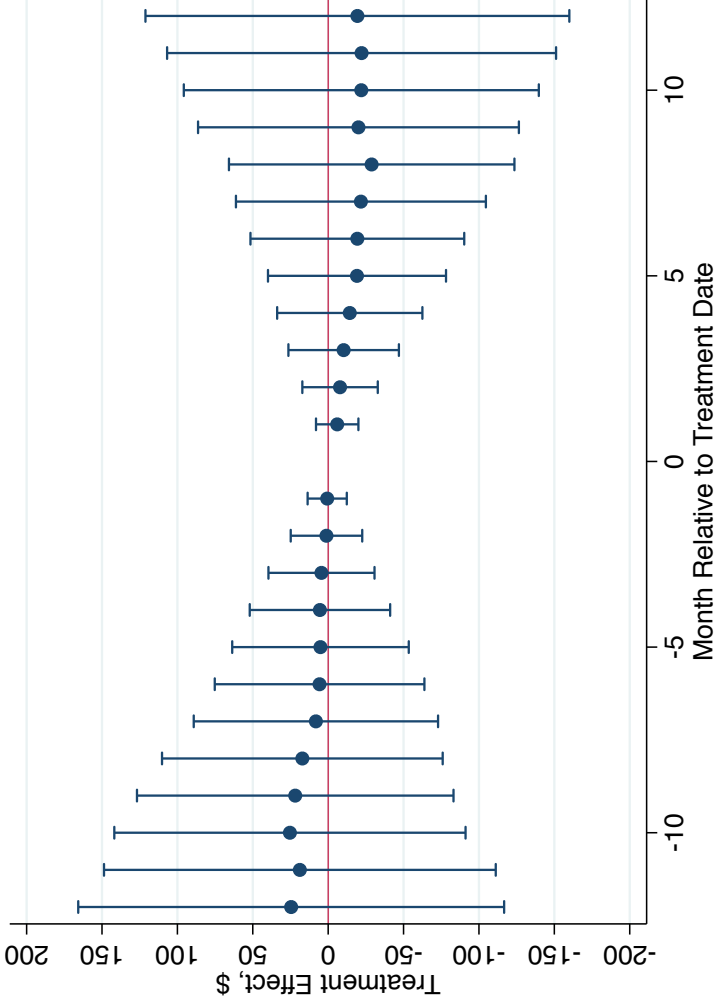


information leads to decreases in prices for products in the high price, high quantity part of the pre-information distribution, we should expect potential quantity increases for those products and decreases for other products (whose prices haven't changed, but have become higher relative to the products with price decreases). This is not the case in Table 19, where no specification shows significantly different effects (economically or statistically) across the pre-join price distribution.

## D Additional Specifications

### D.1 Isolating Identification Based on Timing of Join

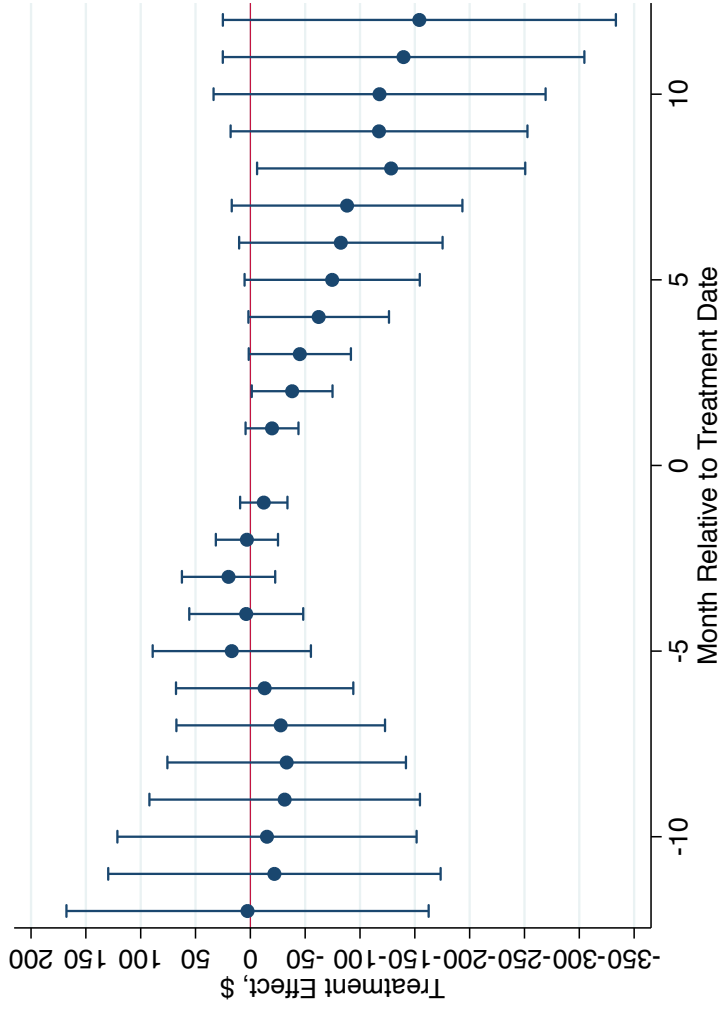
We also estimated our regression specifications focusing only on timing of join. To implement this, we limited the regression sample to non-entering products and to hospital-products where the hospital joined the database at least six months after product entry. The event study results for the average treatment effect across all hospitals and products are shown below in Figure 20. The event study results for the top quintile of the price distribution are shown in Figure 21.



Version	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	1	2	3	4	5	6	7	8	9	10	11	12
1	44 (78)	31 (72)	29 (64)	27 (58)	23 (51)	10 (45)	2 (38)	1 (32)	7 (26)	1 (20)	1 (14)	2 (8)	2 (8)	-12 (15)	-14 (21)	-19 (27)	-24 (33)	-31 (40)	-29 (47)	-33 (54)	-35 (60)	-26 (67)	-30 (73)	-28 (79)
2	-30 (160)	-38 (147)	-36 (132)	-27 (119)	-26 (105)	-31 (92)	-33 (79)	-32 (66)	-17 (53)	-16 (40)	-14 (27)	-5 (14)	-7 (15)	-2 (28)	-1 (41)	0 (55)	-2 (68)	6 (81)	12 (94)	15 (108)	15 (121)	26 (134)	30 (147)	50 (160)
3	25 (72)	19 (66)	25 (59)	22 (54)	17 (47)	8 (41)	6 (35)	5 (30)	5 (24)	4 (18)	1 (12)	1 (7)	-6 (13)	-8 (19)	-10 (25)	-14 (30)	-19 (36)	-19 (42)	-22 (48)	-29 (54)	-20 (60)	-22 (66)	-22 (72)	-19 (72)

$N = 9,786$  member-product-months. Includes 327 members, twelve months pre- and post-join only. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Version 3) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 20:** Event Study of Treatment Effect of Joining Benchmarking Database



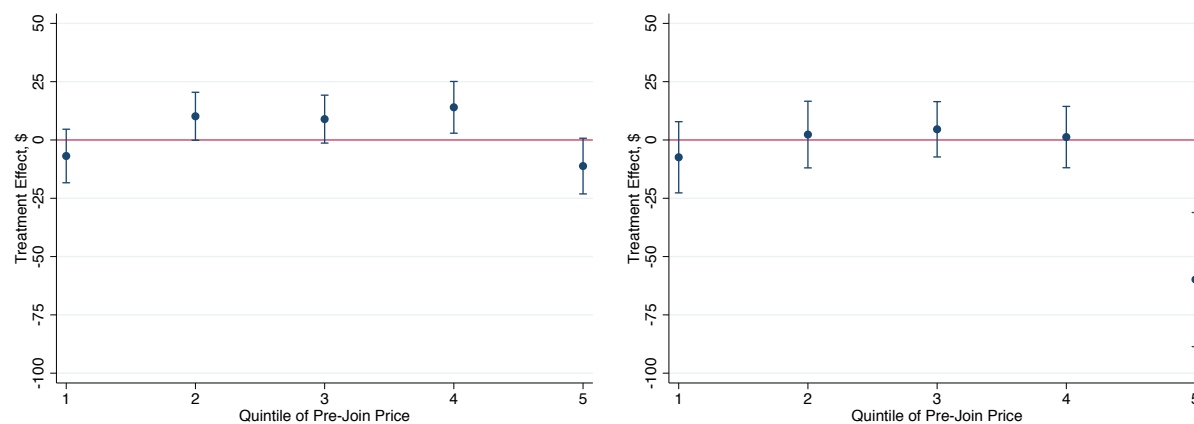
Version	Month relative to join date ( $\beta_5^{join, mo} =$ )																							
	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	1	2	3	4	5	6	7	8	9	10	11	12
1	3	-13	-3	-19	-13	-10	-6	17	17	21	7	-1	-43**	-61 <sup>†</sup>	-65**	-88**	-102**	-116**	-123**	-154**	-141*	-153*	-172*	-176*
	(93)	(86)	(78)	(71)	(63)	(55)	(46)	(40)	(30)	(24)	(19)	(17)	(23)	(28)	(38)	(54)	(47)	(54)	(60)	(71)	(77)	(86)	(92)	(100)
2	-94	-94	-79	-81	-72	-65	-52	-33	-11	-1	-10	-13	-35*	-42	-36	-51	-58	-68	-66	-86	-66	-64	-80	-76
	(167)	(154)	(140)	(126)	(112)	(98)	(83)	(69)	(55)	(43)	(30)	(21)	(32)	(45)	(60)	(74)	(88)	(101)	(116)	(130)	(144)	(157)	(171)	(171)
3	3	-22	-15	-31	-33	-28	-13	17	4	20	3	-12	-20	-38**	-45*	-62*	-75*	-83*	-88	-128**	-117*	-118	-140*	-154*
	(84)	(77)	(70)	(63)	(56)	(49)	(41)	(37)	(27)	(22)	(14)	(11)	(12)	(19)	(24)	(33)	(41)	(47)	(54)	(62)	(69)	(77)	(84)	(91)

$N = 23,016$  member-product-months. Includes 507 members, twelve months pre- and post-join only. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Version 3) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 21:** Event Study of Treatment Effect of Joining Benchmarking Database, Top Quintile of Price Only

## D.2 Alternative Samples

The following Figures show the results of our richest regression specification, allowing different treatment effects for different parts of the price and quantity distributions, for specifications that (1) focus only on the twelve months before and after information (Figure 22); (2) focus on identification based only on timing of database join (Figure 23); and (3) limit the sample to those facilities registered with the database as “hospitals” (Figure 24).



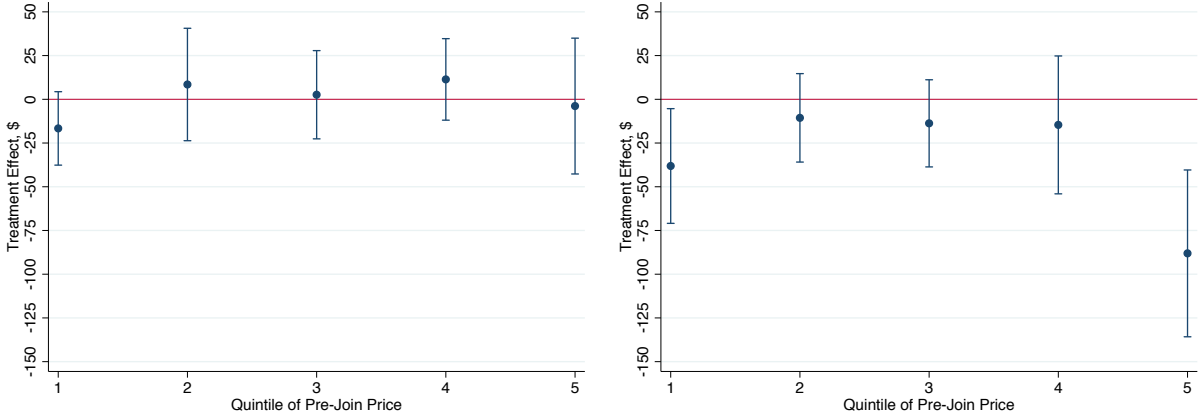
(a) Low Quantity

(b) High Quantity

Version	Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} =$ )					Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} + \beta_{quintile,high^q}^{Info} =$ )				
	1	2	3	4	5	1	2	3	4	5
1	-7 (7)	15** (6)	14** (6)	8 (7)	-30 <sup>†</sup> (9)	-11 (9)	7 (8)	7 (7)	1 (8)	-54 <sup>†</sup> (16)
2	-13 (8)	9 (8)	6 (7)	3 (8)	-38 <sup>†</sup> (9)	-16 (10)	0 (8)	-2 (8)	-3 (9)	-61 <sup>†</sup> (16)
3	-7 (6)	10* (5)	9* (5)	14** (6)	-11* (6)	-7 (8)	2 (7)	5 (6)	1 (7)	-60 <sup>†</sup> (15)
4	-11* (7)	7 (6)	5 (6)	11* (6)	-16** (7)	-10 (8)	-1 (8)	1 (6)	-2 (7)	-66 <sup>†</sup> (15)

$N = 23,016$  member-product-months. Includes 507 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (<sup>†</sup>) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 22:** Treatment Effect Estimates Across the Price and Quantity Distributions – Twelve Months Pre/Post



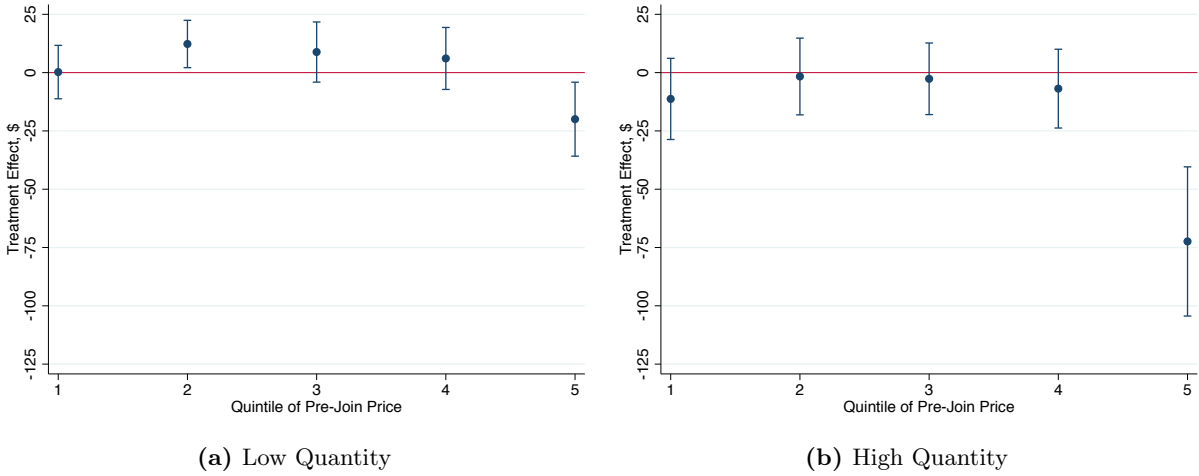
(a) Low Quantity

(b) High Quantity

Version	Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} =$ )					Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} + \beta_{quintile,high^q}^{Info} =$ )				
	1	2	3	4	5	1	2	3	4	5
1	-15 (14)	-8 (15)	13 (15)	-3 (13)	-91 <sup>†</sup> (23)	-12 (18)	-7 (15)	-46 <sup>†</sup> (18)	-23 (21)	-131 <sup>†</sup> (30)
2	-16 (13)	-6 (17)	16 (15)	-3 (14)	-98 <sup>†</sup> (22)	-6 (19)	-7 (15)	-56 <sup>†</sup> (18)	-18 (22)	-128 <sup>†</sup> (31)
3	-17 (11)	8 (16)	3 (13)	11 (12)	-4 (20)	-38 <sup>**</sup> (17)	-11 (13)	-14 (13)	-15 (20)	-88 <sup>†</sup> (24)
4	-21 <sup>**</sup> (11)	10 (17)	7 (14)	7 (13)	-4 (19)	-30 <sup>*</sup> (16)	-12 (13)	-20 <sup>*</sup> (12)	-6 (21)	-84 <sup>†</sup> (21)

$N = 14,701$  member-product-months. Includes 331 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 23:** Treatment Effect Estimates Across the Price and Quantity Distributions – ‘Join’ Variation Only



Version	Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info}$ =)					Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} + \beta_{quintile,high^q}^{Info}$ =)				
	1	2	3	4	5	1	2	3	4	5
1	8 (7)	21 <sup>†</sup> (6)	15 <sup>**</sup> (7)	-2 (8)	-53 <sup>†</sup> (11)	-1 (10)	8 (10)	-10 (10)	-7 (12)	-80 <sup>†</sup> (18)
2	2 (8)	13 (8)	11 (8)	-5 (9)	-64 <sup>†</sup> (12)	-5 (11)	2 (10)	-18 (11)	-13 (12)	-84 <sup>†</sup> (18)
3	0 (6)	12 <sup>**</sup> (5)	9 (7)	6 (7)	-20 <sup>**</sup> (8)	-11 (9)	-2 (8)	-3 (8)	-7 (9)	-72 <sup>†</sup> (16)
4	-3 (7)	7 (6)	7 (8)	2 (7)	-26 <sup>†</sup> (9)	-12 (9)	-5 (9)	-7 (9)	-10 (9)	-77 <sup>†</sup> (16)

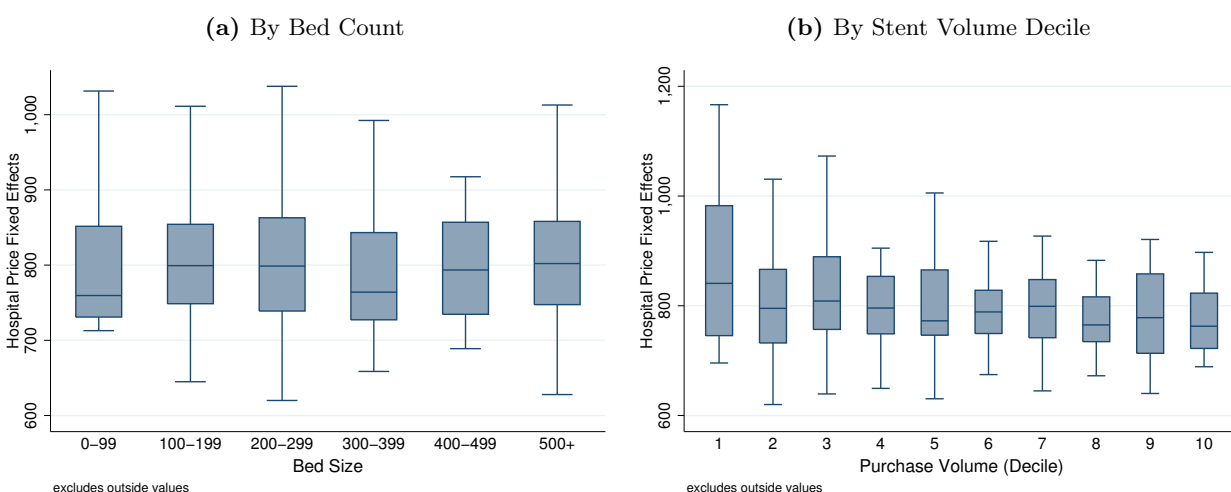
$N = 27,698$  member-product-months. Includes 436 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 24:** Treatment Effect Estimates Across the Price and Quantity Distributions – Hospitals Only

### D.3 Bare Metal Stents

In the following, we display select summary statistics and regression results for bare metal stent transactions. As with the drug-eluting stents that are the focus of this paper, there is substantial dispersion in price outcomes after conditioning on hospital size or volume. The left panel of Figure 25 shows a box plot of bare metal stent prices for each category of bed count. The right panel of Figure 25 shows box plots of stent prices for each decile of monthly stent purchasing volume. In the latter, we see a slight relationship between “size” and price – 10<sup>th</sup> decile hospitals’ prices are 11% lower than those obtained by 1<sup>st</sup> decile hospitals. However, there is a great deal of unexplained hospital price heterogeneity conditional on purchasing volume.

**Figure 25:** Distribution of Prices Across Hospitals



<i>Bare Metal Stent Prices by Size Category (Regression Results)</i>																
$\beta^{\mathbb{1}\{h \in \text{bed size } x\}} =$						$\beta^{\mathbb{1}\{h \in \text{decile } x\}} =$										
0 – 99	100 – 199	200 – 299	300 – 399	400 – 499	500+	1	2	3	4	5	6	7	8	9	10	
835	814	813	784	793	805	868	813	846	800	802	790	802	794	786	776	
(36)	(18)	(14)	(17)	(17)	(12)	(29)	(23)	(35)	(19)	(19)	(17)	(17)	(16)	(16)	(15)	

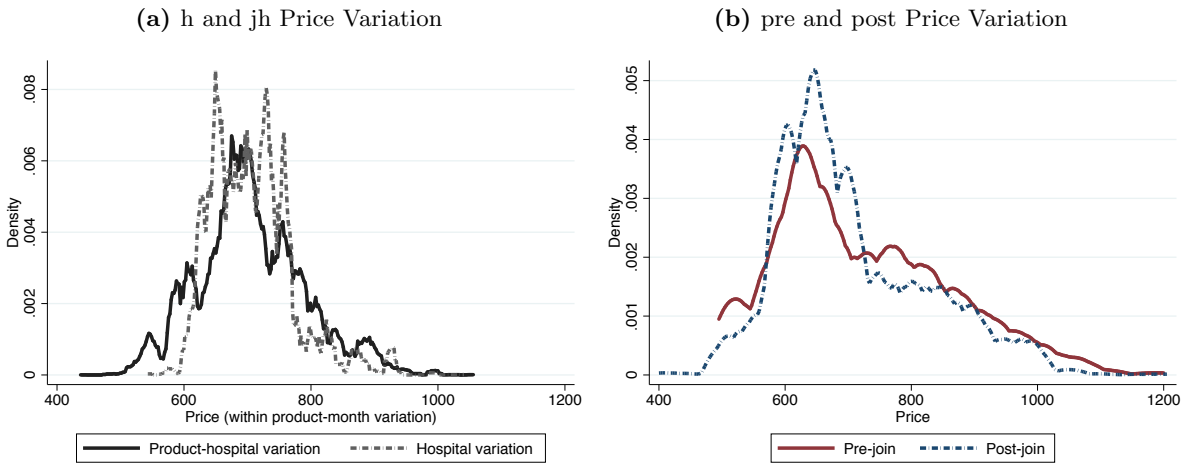
Estimated mean hospital fixed effects within bed size categories and decile of monthly purchase volume. Hospital fixed effects obtained from regression of price on hospital and product-month fixed effects, pre-join data only. Mean estimates from regression of fixed effects on indicators for size. Standard errors from nonparametric bootstrap of entire procedure, resampling at hospital level.

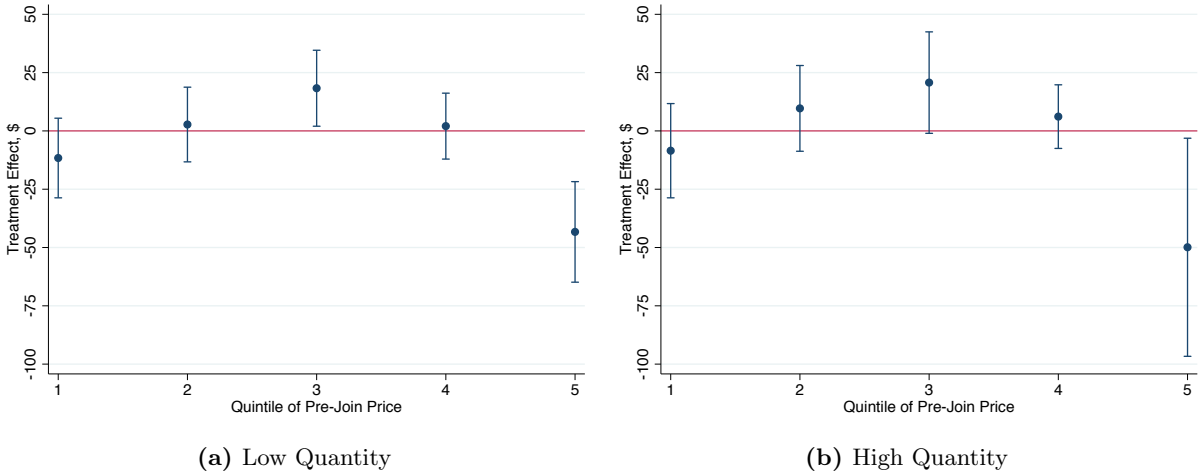
Figure 26 shows reduced form evidence of our overall results: after accessing benchmarking information, hospitals are somewhat less likely to pay the highest prices. Figure 27 shows regression results demonstrating the same phenomenon: after accessing benchmarking data, hospitals previously paying the highest prices experience price decreases, and these decreases are larger among hospitals with greater quantities at stake. In both the reduced form and regression evidence, the results are less pronounced than they were for drug-eluting stents. Given that bare metal stents are substantially less popular than drug-eluting stents, this may be yet another illustration that information effects are mediated by the total volume of purchase



at stake. Furthermore, bare metal stents are an older technology and are declining in popularity over our period of study, with sample market share of 22% in 2009 and only 14% in 2014. This implies that the *future* volume at stake for bare metal stents is relatively small, in addition to the *current* volume at stake being relatively small.

**Figure 26:** Histograms of Price Distributions: Hospital and Product-Hospital Variation and Pre- and Post-Information





Version	Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} =$ )					Pre-info price quintiles ( $\beta_{quintile,low^q}^{Info} + \beta_{quintile,high^q}^{Info} =$ )				
	1	2	3	4	5	1	2	3	4	5
1	11 (8)	10 (8)	13 (8)	-3 (9)	-59 <sup>†</sup> (13)	15 (12)	19* (10)	13 (12)	-14 (10)	-81 <sup>†</sup> (17)
2	12 (8)	15* (8)	13 (9)	-3 (10)	-57 <sup>†</sup> (13)	14 (12)	23** (10)	15 (12)	-14 (10)	-83 <sup>†</sup> (16)
3	-12 (9)	3 (8)	18** (8)	2 (7)	-43 <sup>†</sup> (11)	-8 (10)	10 (9)	21* (11)	6 (7)	-50** (24)
4	-10 (9)	8 (8)	19** (9)	3 (8)	-41 <sup>†</sup> (11)	-7 (11)	13 (10)	21* (11)	6 (7)	-50** (23)

$N = 19,106$  member-product-months. Includes 410 members. Version 1 includes hospital, product, and month fixed effects, plus linear product-specific time trends. Version 2 includes hospital and product-month fixed effects. Version 3 includes hospital-product and month fixed effects, plus linear product-specific time trends. Version 4 includes hospital-product and product-month fixed effects. Figure shows Version 3 results. Standard errors clustered at hospital (Versions 1 and 2) or hospital-product (Versions 3 and 4) level shown in parentheses. Superscript (†) indicates significant difference from zero at the 1% level; (\*\*) at the 5% level; (\*) at the 10% level.

**Figure 27:** Treatment Effect Estimates Across the Price and Quantity Distributions – Bare Metal Stents

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