Hospital Choices, Hospital Prices and Financial Incentives to Physicians

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Abstract

We investigate whether patients whose physicians have a financial incentive to control costs receive care at lower-priced hospitals than similar patients. Using California hospital discharge data from 2003, we first estimate multinomial logit demand models that include price in the choice equation. We obtain price coefficients which differ in sign with the severity of the patient's condition on entering the hospital. We then develop an estimator based on inequalities that addresses endogeneity concerns by matching patients based on insurer and severity and then averaging. The estimates indicate that the impact of price varies with the capitation rate of the patient's insurance plan; higher capitation rate plans are more averse to high priced hospitals. In constrast severity-adjusted outcome measures do not differ significantly across insurers.

PRELIMINARY AND INCOMPLETE

1 Introduction

The health reforms signed into law in March 2010 include provisions to expand health insurance coverage, subsidize premiums and increase consumer choice. The costs of these provisions are partially offset by increased taxes and fees on various entities (including new Medicare taxes on high-income brackets and fees on medical devices and pharmaceuticals). In the long term, however, many policy-makers believe that cost controls rely on health insurance programs such as Medicare and Medicaid moving away from traditional fee-for-service payment systems, which reward providers that generate high service volume, towards systems that encourage them to use resources efficiently while still providing high-quality services. The reforms begin this shift by introducing provisions to make providers who are organized as accountable care organizations (ACOs) eligible, from 2012 onwards, to share in any cost savings they achieve for the Medicare and Medicaid programs. In addition the reforms introduce pilot arrangements under which physicians providing Medicaid services will receive bundled payments that pull together fees for the components of a particular episode of care. For example under these arrangements the obstetrician's and the hospital's payments for a labor and birth episode will be combined into a single fee that is shared by the providers.[1] The goal of

[1] Medicare already bundles payments to hospitals through the D.R.G. (Diagnosis Related Group) payment system. Bundled payments to all payers for larger episodes of care have been piloted in the Medicare program, for example

both initiatives is to identify and implement a provider payment system that reduces the growth in medical care costs without compromising the quality of care.

The health policy literature has noted that similar cost control incentives are currently utilized by some health maintenance organizations (HMOs) in California and elsewhere.[2] However, relatively little is known about the effects of such incentive schemes. While numerous previous papers document low costs in HMOs compared to other insurance types, there is little evidence on the mechanisms used to reduce costs and the dimensions on which physicians respond to cost-control incentives. In this paper we use hospital discharge data from California to investigate whether physicians are more likely to refer patients to lower-priced hospitals when insurers give them a financial incentive to do so. This particular mechanism is important for two reasons. First if the mechanism is effective it has a direct effect on hospital costs, and hospital costs make up over 30% of national health care spending (Kaiser Family Foundation, 2007 data). Second, if hospital admissions are affected by their prices hospitals should take this effect into consideration when they negotiate prices with HMO's, and this should affect the outcome from those negotiations. Those outcomes are important determinants of the returns to both the hospital's investments and to engaging in market changing activities such as mergers. In particular an analysis of hospital mergers which does not take into account the effect of price increases in lowering admissions is likely to over-estimate the price increases that will result from a merger, and a demand analysis that does not take into account price effects will likely over-estimate a hospital's incentives to invest in new high-cost technologies and under-estimate its incentives to invest in cost-reducing technologies.

The process by which a patient chooses a hospital involves multiple players. Decisions are made by referring physicians in consultation with their patients. HMOs and other insurers attempt to influence physicians' choices through direct financial incentives and also less directly by making physicians' promotion on the pay scale contingent (formally or informally) on their management of costs. Direct financial incentives utilized in California include capitation contracts under which referring physicians bear financial risk for services they provide and global capitation contracts where they also bear risk for hospital services their patients receive from other providers. In 2003 73% of payments made to primary physicians by the six largest carriers in the data were capitation payments; the proportions varied substantially across carriers from 97% for Pacificare to 38% for Blue Cross.[3] However if higher quality hospitals negotiate higher prices, both insurers and physicians face a trade-off between incentives to reduce costs and the impact of any negative health outcome on their reputation. Of course the likely reputational impact of a particular hospital choice may vary with both the severity of the patient's condition and the quality of the hospital's services for that condition.

This paper investigates the response of patients' hospital referrals to physician cost control incen-

in the Medicare Participating Heart Bypass Center demonstration (see Cromwell et al 1998) and in the Acute Care Episode demonstration, currently being run, which expands the model to other types of discharges.

[2] See, for example, Hammelman et al (2009) for further information.

[3] Our dataset does not distinguish between capitation and global capitation contracts. However, for reasons discussed below, physicians involved in either type of capitation contract have an incentive to reduce the costs of their patients' hospital inpatient stays.

tives. We use hospital discharge data for privately insured managed care enrollees from California in 2003 and focus on a single diagnosis: the labor/birth episode for pregnant women. Unfortunately our dataset does not identify the physician referring each patient to hospital; we therefore cannot directly observe physician behavior. We ask whether patients in high-capitation insurers are referred to lower-priced hospitals, all else equal, than other patients, and interpret our results as suggestive evidence regarding the effect of interest.

The analysis builds on the previous literature on hospital demand. Previous papers consider the factors affecting patients' hospital choices in some detail but almost exclusively make the simplifying assumption that the patient makes her choice of hospital without any input from the insurer or the provider. In particular, the price paid by the insurer to the hospital is not included in the utility equation. We estimate models of hospital demand that allow this price to influence hospital choices, starting with discrete choice logit models that mirror the previous literature.

There are at least three problems with this procedure. Two are price measurement issues. First, price is defined as a hospital "list price" for the relevant services multiplied by a discount that differs across insurer-hospital pairs but that we observe only at the hospital level. Our baseline price variable is calculated using the observed hospital-level discount; later in the paper we use additional data to estimate the variation in discounts across insurers and incorporate this into the price measure. Second the expected price that generates hospital choices is inherently unobservable. We predict it using average realized prices for patients admitted with similar diagnoses and severities but our prediction will inevitably be imperfect.[4] The third problem relates to price endogeneity: the price of a particular procedure may be correlated with an unobserved hospital quality for that procedure. It is important to include extensive controls for hospital quality to address this issue. We would ideally include interactions between hospital fixed effects and detailed measures of patient diagnosis or severity, since the physician's preference for quality of the hospital may depend on the severity of the patient's illness and/or quality may vary across diagnoses within a hospital. Fully flexible controls are not feasible in the standard logit model so we expect the price coefficient to be biased upwards[5].

When we estimate a multinomial logit on data that pools all labor and birth discharges we obtain a positive and significant coefficient on price. However, when we narrow the sample to the least sick women the price coefficient becomes negative, consistent with the hypothesis that price is correlated with unobserved quality of hospital and quality is more important for more severe medical cases. We then allow the price coefficient to vary by insurance carrier and find that the carriers with the highest proportion of payments to physicians made through capitation contracts have negative significant price coefficients while other carriers with a higher proportion of fee-for-service contracts have insignificant coefficients on price.

[4] Throughout we will ignore measurement error in distance. We know this exists but it is small since both patient and hospital zip codes are observed in the data.

[5] A conditional logit model like that developed in Chamberlain (1980) would address this issue given its assumptions. We plan to estimate this model as a robustness test soon. However, the conditional logit does not address the price measurement problems already described.

Next we develop a methodology based on moment inequalities to address the joint problems of measurement errors in prices and price endogeneity inherent in the logit model. We assume that the hospital choice equation is additively separable in price, a non-parametric function of the severity of the patient's illness and the quality of the hospital's services for that severity, and distance to the hospital (or, more generally, a measure of the convenience of the hospital for the particular patient). The behavioral assumption is that the hospital chosen for each patient generates greater expected utility than any of the other hospitals in her choice set. This generates an inequality for each patient and each of her alternative hospitals (each hospital that is offered by her insurer, is within reasonable travel distance of her home, and that she did not choose). We identify pairs of patients who have the same severity and are enrollees in the same insurer but who chose different hospitals. By defining the alternative of each patient as the chosen hospital of the other and summing the two patients' inequalities, we difference out the severity-hospital interaction terms from the utility equation. By summing the resulting inequalities over patients and hospitals we mitigate the effects of errors in price measurement. The result is a relatively straightforward estimator of bounds on the (normalized) price coefficient.

The estimates indicate statistically significant negative price coefficients for the four insurers with the highest proportion of capitated payments. The price coefficients for the two insurers with the lowest capitation rates are less negative: the 95% confidence interval for one crosses zero while the other has a confidence interval strictly above those for most high-capitation insurers. The story changes very little when we allow the negotiated discounts to differ between insurers as a function of market and insurer characteristics. Indeed we show that the overall story seems robust to the particular specification used in the estimation. We then compare severity-adjusted outcome measures across insurers. We cannot reject the null hypothesis of no cross-insurer difference in outcomes in a test of size less than 0.001. Our general conclusion is that our results are consistent with physicians in California responding to hospital prices when they face financial incentives to do so; however this does not seem to have a significant effect on patient outcomes[6]. We are in the process of analyzing three more detailed implications of our findings. The first is a quantification of just how large a saving in hospital costs could be obtained from a change which made all the insurers in our data set have capitation rates similar to those of the highest capitation insurers. The second is an analysis of the estimated bounds on the hospital-quality/severity interactions that emanate from our procedure. Finally we will investigate what the hospital choice function tells us about the tradeoff between measures of hospital quality and price, and the extent to which this tradeoff differs across insurers.

The remainder of the paper is structured as follows. In Section 2 we briefly discuss the relevant previous literature. Section 3 describes important features of the market, particularly those relevant

[6]In fact we uncover the preferences of a composite agent, comprising the patient and her physician. However, since patients have no reason to respond to the price paid by the insurer on their behalf, we intepret the price coefficient as information on the physician responses to price differences between hospitals. Regardless of the veracity of this assertion it is the composite agent whose preferences that we do recover that determine the effect of changes in hospital or plan characteristics on hospital choices.

to California and Section 4 describes the data. Section 5 sets out the full model we wish to analyze, Sections 6 and 7 summarize the restrictions required for the logit and inequalities methods and set out their results, Section 8 considers patient outcomes and Section 9 concludes.

2 Previous Literature

The first relevant set of previous papers considers HMO gatekeeping and cost controls. Glied (2000) summarizes this literature. Her summary suggests that HMOs have lower inpatient admissions and costs than other insurers. However the study results are often difficult to interpret because, for example, physicians and patients who prefer a low treatment intensity may select into HMOs.[7] There are a few more recent studies that consider similar questions. For example, Cutler et al (2000) compare the treatment of heart disease in HMOs and traditional insurance plans and find that HMOs have 30% to 40% lower expenditures. Virtually all the difference comes from lower unit prices rather than differences in actual treatments. However they consider only heart attack patients, for whom price reductions are likely to be due to lower negotiated prices within a hospital rather than to referring patients to cheaper hospitals, so they do not investigate the latter mechanism. Escarce et al (2001) study an HMO in Michigan offering both an HMO and POS product and find that the HMO, which requires referrals for specialty care, has lower physician and drug expenditures than the POS plan which does not. Gaynor, Rebitzer and Taylor (2004) look in more detail at how HMOs achieve cost savings. They analyze physician responses to group-based financial incentive contracts within a single HMO. They find that spending on medical utilization increases with the size of the physician group receiving group-based incentives. That is, spending is negatively correlated with the intensity of incentives to limit these expenditures. The correlation is greater for outpatient expenditures than for inpatient expenditures[8]. There are also some papers evaluating recent initiatives that implement cost-control incentives like those planned for Accountable Care Organizations. For example the Alternative Quality Contract (AQC) was adopted by Blue Cross Blue Shield of Massachusetts in 2009. It introduced shared physician savings arrangements like those planned for ACOs together with quality bonuses. Song, Zafran et al (2011) find that, in the first year, this initiative was associated with reduced growth in spending and improved quality of care. Most of the savings came from referring patients to lower-cost hospitals, making the focus of our paper particularly relevant for ACOs.

The second relevant literature considers whether individual physicians respond to financial incentives by altering behavior on a patient-by-patient basis. For example, Melichar (2009), Franzini et al (2010) and Meyers et al (2006) all find evidence that individual physicians are willing to provide differential care in response to patients' insurance status.[9] None of these papers consider

[7] Gosden et al (1999) and Armour et al (2001) review the literature on the effects of financial incentives on physician behavior and come to similar conclusions. Chandra, Cutler and Song (2012) and McClellan (2011) provide more recent reviews of the previous literature on this topic.

[8] Other recent papers considering the responsiveness of health care providers to financial incentives include Ketcham, Leger and Lucarelli (2012), Limbrock (2011) and Bajari, Hong, Park and Town (2012).

[9] The applications are Medicare versus private insurance for Franzini et al (2010), capitation versus non-capitation

the mechanism of interest to us: physicians controlling costs by altering their hospital referral choices.[10]

The third, much larger literature estimates discrete choice models of hospital demand: see Gaynor and Vogt (2000) for a survey.[11] Almost all of these papers exclude the price paid by the insurer to the hospital from the utility equation. One exception is Gaynor and Vogt (2003) which uses assumptions to define a price index for each hospital that is included in the utility equation. However, that paper assumes away interactions between patient characteristics and the attributes of a particular hospital in determining procedures and therefore prices. It also does not consider the impact of physician incentives on the price coefficient.

Our inequalities analysis is similar in spirit to previous papers that match treatment to control groups based on observable data and assume that unobserved information does not affect response to treatment. The propensity score literature, and difference-in-differences analyses more generally, fall into this category.[12] Our moment inequalities methodology adds to a large literature on partially identified models; it is based on the method developed in Pakes, Porter, Ho and Ishii (2011) and Pakes (2010).[13]

Finally, our analysis of the variation in hospital discounts across different types of insurers and hospitals is related to a large previous literature analyzing the impact of buyer and seller concentration in health care markets. Dranove and Satterthwaite (2000) and Gaynor and Vogt (2000) provide good reviews of this literature. Several papers quantify the impact of insurer concentration on negotiated prices, often considering Blue Cross/Blue Shield in particular.[14] Almost all find a positive relationship between insurer market share and hospital discounts. A second set of papers considers the impact of hospital market share on prices, usually by regressing hospital price on a measure of hospital concentration.[15] Most of these studies find that increased hospital concentration is correlated with increased prices (reduced discounts). In addition, a few papers model the

coverage for Melichar (2009) and privately insured versus publicly insured and uninsured patients in Meyers et al (2006).

[10] Duggan (2000) considers hospital referrals for Medicaid patients. He finds that private hospitals in California responded to the state's Disproportionate Share Program of 1990, which increased hospital financial incentives to treat Medicaid patients, by cream-skimming the most profitable Medicaid patients from publicly-owned hospitals. The reallocation was especially pronounced for pregnant women. Duggan (2003) finds that this reallocation was concentrated in markets served by relatively many private for-profit hospitals, and that private not-for-profit hospitals responded more aggressively to the new incentives in these markets than elsewhere.

[11] Examples include Luft et al (1990), Burns and Wholey (1992), Town and Vistnes (2001), Capps, Dranove and Satterthwaite (2003), Tay (2003) and Ho (2006), all of which either omit price entirely or include only the list price (and estimate a positive or unrealistically small negative price coefficient).

[12] See Rosenbaum and Ruben (1983) for propensity score estimators and, for example, Card and Krueger (1994) for difference-in-difference estimators.

[13] The literature dates back to Frisch (1934) and Marschak and Andrews (1944). More recent papers on estimation and inference include Manski (1990), Manski and Tamer (2002), Czernozhukov, Hong and Tamer (2007) and Andrews and Soares (2009). Empirical applications include Haile and Tamer (2003) and Ciliberto and Tamer (2009).

[14] Examples include Feldman and Greenberg (1981), Adamache and Sloan (1983), Foreman et al (1996), Staten et al (1987 and 1988) and Melnick et al (1992).

[15] See for example Melnick et al (1992), Dranove et al (1993), Connor et al (1998), Simpson and Shin (1998), Keeler et al (1999) and Lynk and Neumann (1999). Burgess et al (2005) use panel data to consider a similar question. They investigate the response of hospital prices to changes in hospital system membership over time in California and find a positive system effect on hospital pricing.

bargaining process between hospitals and insurers more directly; these consistently find a positive relationship between prices and variables related to hospital bargaining power (such as hospital concentration and consumer willingness-to-pay measures) and a negative relationship between prices and insurer concentration.[16]

We conduct a preliminary analysis of the response of physician referrals to capitation payments in Ho and Pakes (2011). In that paper we regress a severity-adjusted price measure on the proportion of the insurer's payments to primary physicians that are capitated and market fixed effects and estimate a negative and statistically significant coefficient on the capitation variable.[17] This is consistent with the hypothesis that insurer capitation payments influence physician referrals. However, simple regressions like these cannot provide more than suggestive evidence since they do not account for the trade-offs made between price and other hospital characteristics in the hospital choice equation.

3 Background on the Market

The analysis in this paper focuses on enrollees of health maintenance organizations (HMOs). As of December 2002, 21.4 million consumers in California (63% of the population) were enrolled in an insured HMO plan.[18] The seven largest HMOs had 87% of the California HMO market at the end of 2002. Our analysis focuses on six of these seven: we exclude Kaiser (the largest HMO with 30.5% of the market in 2002) because the prices paid by this vertically integrated insurer to its hospitals are not observed in our data.[19]

Each HMO contracts with a network of providers (physicians and hospitals); enrollees are required to seek care only within that network. Each pregnant woman chooses an obstetrician from within the network and is referred to one of the small number of network hospitals with which the obstetrician is affiliated. The patient's choice of obstetrician is informed by the list of affiliated hospitals, which is public information. While HMOs could, in theory, influence hospital referrals for their enrollees by defining narrow hospital networks, in practice this is not usually the case.[20] Similarly, HMOs do not generally use hospital payment mechanisms that provide incentives either to control costs or improve quality. Most hospitals in California are paid by the insurance carrier

[16] Examples include Brooks et al (1997), Town and Vistnes (2001), Capps et al (2003) and Ho (2009).

[17] We adjust for severity by constructing the following price ratio measure: $p_i^{ratio} = \frac{p_i}{\bar{p}_{s_i}}$ where p_i is the hospital price for patient i and $\bar{p}_{s_i}$ is the average of that variable for same-severity patients across all hospitals in the sample.

[18] The 2003 California medical care market is described in detail in Baumgarten (2004). Several previous papers describe the contractual arrangements between health plans and physicians in California, including Rosenthal et al (2001 and 2002) and Grumbach et al (1998a. and b.).

[19] The remaining large insurers, which are included in our analysis, are Blue Cross, Blue Shield, Health Net, Pacificare, Aetna and CIGNA. Blue Cross of California is independent of other Blue plans, including Blue Shield of California, except that it is a member of the Blue Cross Blue Shield Association. It is part of the Anthem/Wellpoint organization. PacifiCare was purchased by United HealthCare in 2006.

[20] Ho (2006) finds that on average 83% of hospitals were included in each HMO's network in a sample of 43 large markets (including seven in California) in 2003. Capps, Dranove and Satterthwaite (2003) report similar evidence. Our analysis conditions on the provider network of each insurer in our data.

on a per service or per diem basis.[21]

Payment arrangements for physicians, in contrast, are often structured to generate cost-control incentives. Most HMOs contract on a non-exclusive basis with large physician groups,[22] making capitated (fixed) monthly payments to the group for every enrollee who uses it as his or her primary care clinic. The alternative is a fee-for-service payment arrangement. The extent of financial risk passed to the medical group varies across capitated contracts. In around 20% of cases the monthly ("global capitation") payment covers all services needed by the physician group's patients including inpatient hospital stays. Physician groups have a clear incentive to refer their patients to lower-cost hospitals. The remaining 80% of capitation contracts involve payments that cover only the cost of services provided by physicians within the group, perhaps with the addition of ancillary services like outpatient medical tests. The HMO makes separate payments to hospitals for providing secondary care. Physician groups again have incentives to control hospital costs because "shared risk arrangements" almost always apply, under which a spending or utilization target is set and cost savings or overruns relative to the target are shared between the physician group and the HMO.[23] Fee-for-service contracts do not generally involve shared hospital risk arrangements. Our dataset does not distinguish between global and non-global capitation arrangements. We assume that physician groups facing capitation contracts of any kind have an incentive to be affiliated with and refer patients to lower-cost hospitals, while that incentive does not exist if the physician group receives fee-for-service payments.

If capitation arrangements are to influence hospital referral choices, however, cost-control incentives must be passed from the physician group to the individual physician.[24] The connection is clear when the physician is a partner in a medical group since his or her own income is directly linked to the group's profitability but less clear for other physicians. Rosenthal et al (2002) consider this issue, tracking the flow of financial incentives from physician organizations to physicians in California. They find that the majority of physician groups receiving capitation payments pass financial risk on to individual physicians, in the form of either capitation-based compensation, cost-of-care bonuses or profit sharing.[25]

[21] Capitation payment arrangements under which the hospital bore financial risk for the services provided, which at one point were common in California, had almost died out by 2003 (apparently due largely to the increase in hospital economic power generated by hospital system formation).

[22] There are two types of physician groups: medical groups and Independent Practice Associations (IPAs). On average they each cover 50,000 lives and contain between 200-300 physicians per group. Approximately two-thirds of patients covered by non-Kaiser physician organizations are in IPAs and one-third are in medical groups (see Rosenthal et al (2001) for data). Physicians in medical groups are either employees or partners of the group. IPAs are administrative organizations that contract with independent physicians or clinics and sign network contracts with health plans on behalf of their physicians. They exist primarily to negotiate and manage capitation contracts for their member physicians. Physicians and physician practices that are members of IPAs often also provide services to the same HMOs on a fee-for-service basis outside of the IPA, although these arrangements represent a minority of patients.

[23] Rosenthal et al (2001) note that 85-90% of non-global capitation revenues were generated from contracts with shared hospital risk. Robinson and Casalino (2001) report similar findings.

[24] Individual physicians also need to be informed about relative hospital prices. Evidence from interviews with California insurers and physicians indicates that this information is often provided by the physician group practice manager.

[25] Grumbach et al (1998a) survey California IPAs and have similar findings. They also note that IPAs that are

Our dataset does not identify the physician or physician group referring each patient to hospital. We do observe the name of each patient's HMO and the percent of each insurer's primary services and other medical professional services that are capitated. In the analysis below we compare the importance of price in determining the hospital choice for patients enrolled in high-capitation insurers to its importance for those in low-capitation insurers. We interpret a positive correlation between HMO capitation rates and price sensitivity as suggestive evidence that physicians respond to financial incentives in this setting. As noted above, the previous literature on physician treatment choices indicates that they are willing to treat patients differently depending on their insurance status. Melichar (2009) is the most relevant for our study: the author uses physician survey data and physician fixed-effect regressions to demonstrate that physicians spend less time with their capitated patients than with their non-capitated patients.[26] However, it is also possible that the correlation is caused by selection of physicians who are affiliated with low-priced hospitals into capitation contracts.

There are at least two possible causal mechanisms. First, consistent with Melichar (2009), each physician may differentially refer her capitated patients to cheaper hospitals and her non-capitated patients to others. Second physicians with a majority of revenues from capitated contracts may choose to be affiliated with a relatively low-priced hospital while those with a minority of capitated contracts do not. Unfortunately our limited data prevents us from distinguishing between these mechanisms.

In addition our dataset does not precisely identify HMO enrollees for every insurer. Instead it groups together all Knox Keene enrollees for a particular insurer, defined as enrollees in plans that are overseen by the California Department of Managed Health Care (DMHC) and subject to the Knox Keene Act. All California HMOs are Knox Keene plans. In addition, Blue Cross and Blue Shield PPO products were Knox Keene plans in 2003, the year of our data.[27] 63% of Blue Shield's Knox Keene enrollees, and 72% of those for Blue Cross, were in the PPO rather than HMO product. Unfortunately we cannot distinguish between PPO and HMO enrollees for these two insurers at the individual discharge level. This introduces a measurement issue, particularly since Blue Shield and Blue Cross have the lowest capitation payment rates in our data. PPOs use the same mechanism for hospital referrals as HMOs except that patients have more discretion: by paying a relatively high out-of-pocket price they can choose to visit an out-of-network hospital or physician. Pricing policies can also be different. While an HMO enrollee probably pays the same small copay whatever hospital she chooses, approximately 15% of PPO enrollees pay a coinsurance rate (a fixed percentage of the total price) that is lower if they choose an in-network hospital than if they go outside.[28] We drop hospitals to which very few patients are admitted for these

paid on a fee-for-service basis make fee-for-service payments to their member physicians.

[26] The evidence in Cutler et al (2000), that heart attack outcomes for HMO enrollees do not differ from those of enrollees in other insurance types, suggests that there are limits to this willingness to differentiate care based on financial incentives.

[27] The PPO products of the other insurers we consider were not Knox Keene plans, so we identify their HMO enrollees precisely.

[28] The Kaiser Family Foundation Employer Health Benefits Survey 2003 shows that the difference in pricing

two insurers, expecting thereby to remove out-of-network hospitals from the data. Any remaining difference in pricing strategies for PPO plans biases our estimates towards finding no difference in price coefficients between high- and low-capitation insurers, since patients presumably have a higher sensitivity to price than do physicians and our model conflates the price coefficients of patients and physicians for Blue Cross and Blue Shield. Finally, we note below that our inequalities method controls for any additional patient discretion over the hospital choice in Blue Shield and Blue Cross compared to other insurers.

We note that the incentives generated by the California medical care system are similar to those introduced by the 2010 health care reforms in several ways. Capitation payments are similar in some respects to the payment bundling to be piloted in the Medicaid program. Both are intended to reduce the incentives, generated by fee-for-service payment systems, to provide more services than necessary and both reward physicians for referring patients to lower-priced hospitals. The difference is that bundled payments address these incentives within an episode of care while capitation payments address them both within and across episodes. The Accountable Care Organizations (ACOs) set up by the reforms for the Medicare program are particularly similar to the California system. ACOs are likely to constitute large groups of providers that negotiate shared risk arrangements (or "shared savings" contracts) with payors[29]. Current fee-for-service payments will continue but the ACO will also be eligible to share in any cost savings made relative to a pre-agreed benchmark (specific to the ACO) if the savings exceed a minimum level of approximately 2 percent. There are obvious similarities to the California institutional arrangements just described[30]. We therefore expect our analysis to be informative regarding the impact of the reforms on hospital inpatient costs; that is our evidence reinforces the belief that if care-givers are given incentives to minimize costs they direct patients to lower-priced hospitals[31].

4 The Dataset

We use four datasets. The first is hospital discharge data covering all patient discharges from hospitals in California in the year 2003 from the state's Office of Statewide Planning and Development (OSHPD). This provides information on each patient's zip code, demographic characteristics, health insurer, the hospital chosen and patient diagnosis details: both the "principal" diagnosis recorded as the major cause of admission and a list of up to 24 other diagnoses for each patient.[32] We link this

strategies was not large in that year. 14% of covered workers in a PPO plan paid a coinsurance rate, 26% paid a dollar copay and 59% paid neither. In contrast 5% of HMO enrollees paid a coinsurance rate and 49% paid a copay.

[29] Hospitals are likely to be members of ACOs. However large hospitals will likely be required to form non-exclusive relationships, i.e. to be affiliated with several ACOs in the market, in order to avoid antitrust investigation. Patients in an ACO will be free to seek care from any other provider that accepts their insurance. See Berenson and Burton (2011) for further institutional details on ACOs.

[30] The ACO payment scheme provides incentives for the physician group to reduce spending. However no rules have been set regarding how the incentives will be passed down to individual physicians.

[31] This is less true for ACOs in the Medicare market since Medicare prices are essentially fixed across hospitals. However ACOs are already being established for privately insured patients. Prices paid by private insurers vary substantially across hospitals.

[32] We have a Private Use version of the data in which patient zip code, age, race and gender are not masked.

to OSHPD hospital financial data and to hospital characteristics data from the American Hospital Association for 2003. Finally we have access to the State of California Department of Managed Health Care Annual Financial Reporting Forms for 2003. These include balance sheets, income statements and some information on enrollment, utilization and types of payment to providers for all Knox Keene plans in California. We consider only admissions records for women in labor and only private Knox Keene enrollees. Our analysis covers only the six largest insurers other than Kaiser Permanente: these make up over 96% of the non-Kaiser observations in the data. We infer the hospital network of each insurer using the discharge data by assuming that a hospital is in the network if at least 3 patients are admitted from the particular insurer.[33] Consistent with Kessler and McClellan (2000), we assume that patients consider traveling up to 35 miles to visit a general hospital and up to 100 miles to visit a teaching hospital.

We do not observe the price charged to the insurer by the hospital. Instead our data includes the list price for every discharge. As noted in Melnick (2004), list prices are essentially equivalent to the "rack rate" that hotels list for their rooms. They are a standard set of prices listed by hospitals in each year for all their services. All patients are quoted the same list price for the same service. However, only uninsured patients and some patients using an out-of-network provider are actually asked to pay the list price, and even they are frequently offered a discount by the hospital. Each insurance company has a contract with each provider in its network that defines a discount from the list price for its enrollees. We observe the average negotiated discount at the hospital level, calculated as the total contractual adjustments from private managed care payors divided by the total charges (the sum of list prices for all inpatient and outpatient episodes) for the relevant hospital-year.[34]

The relevant price for the hospital choice is the price that the decision-maker expects to pay for a given entering diagnosis or severity level. We make the weak rationality assumption that expected prices are on average correct. We construct a baseline price variable as the average realized list price for a given severity in a particular hospital multiplied by 1 minus the average hospital discount. Estimation in the inequalities methodology will rely on averages over agents so the expectational error should average out.[35]

We demonstrate below that there is meaningful variation in this price measure both across patients of different sickness levels and across hospitals. However, it is clearly subject to measurement problems. There is a trade-off between aggregation error, if our groups of similar patients for the expected list price calculation are defined too broadly, and measurement error if they are too narrow

[33] We check the implied network definitions against hand-collected data (described in detail in Ho (2006)) from seven California markets in 2003. The definition is conservative: that is, the networks implied by our methodology contain fewer hospitals than the networks in the hand-collected data and if an implied network contains a particular hospital it is also included in the hand-collected data in the vast majority of cases.

[34] Both variables are recorded in the hospital's financial statements. Contractual adjustments are defined as "the difference between billings at full-established rates and amounts received or receivable from third-party payors under formal contract agreements".

[35] We take averages over patients who enter the hospital with a given severity level. Our definitions of severities differ across our model specifications and are detailed below. Gaynor and Vogt (2003) use a similar methodology, defining price as the observed list price multiplied by 1 minus the average discount.

implying small sample problems. We return to this issue below. There may also be specification error since we observe the discount at the hospital rather than the hospital-insurer level.[36] We address this in Section 7 and Appendix 1 by using additional data on the share of each hospital's total inpatient revenues coming from each insurer to estimate an equation describing the discount as a function of hospital, insurer and market characteristics. We repeat our inequalities analysis using two price measures derived from this procedure. However we begin by ignoring specification error and using the baseline price variable since this is less dependent on modeling assumptions.

Different insurers may use different payment mechanisms to reimburse different hospitals in their networks. The major possibilities are fee-for-service payments and per-diem payments under which the hospital receives a fixed number of dollars per day of inpatient stay. We have some information at the hospital and insurer level on the payment mechanisms used but this information is not provided at the discharge level.[37] The weighted average percent of payments that are made on a per-diem basis (where the weight is the number of enrollees in the plan) is fairly low at 21%. Two of the six carriers in our data, Aetna and Health Net, report no per-diem payments in 2003. Still, there is clearly some variation in the data in terms of payment mechanisms which will generate measurement error in the price variable.

Table 1 sets out summary data on the six insurers included in the analysis; data for Kaiser is also included for comparison. These data give a broader picture of the insurers we consider than can be provided by our specific dataset. Since the effect of capitation payments on the price coefficient will be identified from variation across these six insurers, our goal here is to summarize the differences between them on other relevant dimensions. The first three columns provide enrollment data, showing that of the insurers we consider, Blue Cross, Blue Shield and Health Net have the largest commercial plan enrollment while Aetna and Cigna have the smallest. Every insurer in our dataset has over 70% of its enrollment in commercial plans. Column 4 lists the number of labor discharges included in our analysis for each plan; the breakdown is approximately proportionate to the commercial enrollment numbers. Column 5 lists the percent of each HMO's primary services that are capitated.[38] There is considerable dispersion across insurers, from Pacificare with 97%

[36] Specification error is also generated because the observed value is an average for both inpatient and outpatient services and for all managed care payors (including Point of Service plans) rather than just for Knox Keene inpatient events. If variation in discounts across insurers and plan types is known to physicians this specification error will generate selection bias in our estimates. This issue is addressed in the analysis in Section 7.3 and Appendix 1.

[37] Case-based or D.R.G. payments are also possible: our data do not distinguish between them and fee-for-service payments but we expect case-based payments to be less common since they are predominately used by Medicare rather than private payors. Capitation payments to hospitals are possible but uncommon: 72% of hospitals report zero capitation payments in our data. Our logit analysis includes all hospitals, including those that receive capitation payments. In a robustness test we redefine price to be price*(1-percent of revenues received on a capitated basis). The results are available from the authors on request; they are very similar to those from the baseline logit analysis. The inequalities analysis excludes a few hospitals reporting that more than 5% of their revenues are paid on a capitation basis; excluding all hospitals with non-zero capitation payments has very little effect on our results.

[38] Capitation payments for primary professional services are defined in the HMO Annual Financial Statements as "capitation costs incurred by the reporting entity to primary care physicians, dentists and other professionals for the delivery of medical services". They include capitation payments to obstetricians. The statements also record capitation payments to other medical professional services, including support personnel such as nurses, ambulance drivers and technicians.

capitated payments to Blue Cross with 38%. The rest of the table demonstrates that insurers with a high percent of capitated payments are not obviously different from other insurers on dimensions such as premiums per member per month, inpatient utilization and prescription drug costs. Blue Shield and Blue Cross, which have the lowest proportion of capitated payments, were historically different from other insurers. They were 501(c)(4) tax exempt as social welfare plans, acting as administrators of Medicare and providing coverage to state and federal government employees. By 2003, however, Blue Cross and Blue Shield companies were franchisees, independent of the association and each other. They were no longer tax exempt and could be for-profit corporations. In California Blue Cross was an investor-owned for-profit organization with a lower medical loss ratio (defined as medical and hospital expenses divided by premium revenues for the whole insurer) and similar inpatient utilization to other insurers in the market. Blue Shield was still somewhat different from the other insurers we consider. It was a not-for-profit company with relatively high inpatient utilization figures although its premiums and medical loss ratio were quite low. There may be less reason to believe that Blue Shield's administrators and physicians were receptive to financial incentives than those of other insurers. We return to this issue below.

Table 2 provides summary statistics on the discharges in the dataset. There are 88,157 patients and 195 hospitals.[39] There are 38 hospitals in the average patient's choice set. 27% of discharges are from teaching hospitals. The average price paid (approximated as list price*(1-average discount)) is $4,317 for labor admissions.[40] The average length of stay is 2.5 days. The importance of the distance between the patient's home and her hospital is clear from the raw data. The average distance between a patient and a hospital in her choice set is 24.6 miles; the average distance to the chosen hospital is 6.7 miles. Distance will be an important variable in the utility equation estimated below.

The table also records means for three potential measures of outcomes: death while in hospital, transfer to an acute care setting (at this hospital or a different hospital) and transfer to a special nursing facility (again at either this or a different hospital). These are useful inputs to an initial investigation of the patterns in the data although we will not use them in our full model. The average probability of each event is low for labor admissions: 0.01% for death, 0.3% for acute care transfer and 1.5% for transfer to a special nursing facility.

[39] This is the sample used for the logit analysis. The inequalities analysis has the advantage that we do not need to account for the patient's full choice set; pairwise comparisons between hospitals are sufficient for consistent estimation. We therefore exclude some hospitals with missing average discount data, whose values we fill in using regression analysis for the logits, and the small number of hospitals reporting that more than 5% capitated revenues. We are left with 70,799 patients and 157 hospitals.

[40] If discount information is missing we fill it in for the logit analysis using regression analysis. (These observations are excluded from the inequalities analysis.) For 7.5% of the hospitals in the sample we do not observe the discount for the calendar year but do observe discount data for both relevant fiscal years (from the annual financial statements; fiscal years vary across hospitals). We fill in the missing calendar year information using the predictions from a regression of calendar year discounts on fiscal year discounts and hospital characteristics (fixed effects for hospital systems, service type, control type, Hospital Referral Region, teaching hospitals and particular services provided and lagged numbers of doctors and beds, all as reported in the American Hospital Association data for 2003). The R^2 of the regression is 0.61. A further 12.1% of hospitals have missing discount data for the relevant fiscal years and the calendar year; in this case we use the predictions of a regression of calendar year discounts on hospital characteristics which has a R^2 of 0.49.

Table 3 demonstrates that the variation in price and in outcomes across patient ages and comorbidities is intuitive. Here we use a slightly different dataset that includes infant outcome variables as well as those of the mother and that follows both mother and baby over time, enabling us to calculate the probability of readmission within a 12 month period[41]. We aggregate the probabilities of death, acute care transfer and special nursing facility transfer into a single probability of discharge to a location other than home. The table indicates that women giving birth who are aged over 40 have a significantly higher average price and significantly higher probabilities of readmission within 12 months and of discharge "other than home" than younger women. Their infants have significantly higher prices and probabilities of discharge other than home; however infant readmission probabilities are not significantly different across these two groups.

We use the Charlson score (Charlson et al, 1987) as a measure of patient severity: this assigns integer-valued weights (from 0 to 6) to comorbidities other than principal diagnosis where higher weights indicate higher severity. The weights are summed to generate a single integer-valued index. For example, patients with comorbidities indicating that they have diabetes or mild liver disease would receive a Charlson score of 1; those with renal disease or any malignancy would have a Charlson score of 2; those with a metastatic solid tumor or AIDS would have a Charlson score of 6. A patient with both diabetes and renal disease would have a score of 3. The index was developed by physicians and is widely used to measure severity based on diagnoses listed in patient records. Table 3 indicates that women with higher Charlson scores in our data, and their infants, had higher prices and higher probabilities of adverse outcomes than women with lower Charlson scores. All of these differences are significant at p=0.05. Our analysis will allow the Charlson score, interacted with other severity measures such as age and principal diagnosis, to affect preferences directly.

5 The Model

The choice of hospital is made by the doctor in consultation with the patient. The patient's preferences are affected by the distance from her home to the hospital, her assessment of the severity of her condition, and by the hospital's (observed and unobserved) characteristics. The physician's choice is influenced by the patient's preferences, their assessment of the severity of the patient's condition and the quality of the hospital services for that severity, and the price charged by the hospital to the insurer. We assume that the utility function whose maximum determines the hospital (h) that patient i of insurer π is allocated to, takes the additively separable form

$$W_{i,\pi,h} = \theta_{p,\pi}(\delta_{\pi,h} lp(c_i, h)) + g_\pi(q_h(s), s_i) + \theta_{d1,\pi} d(l_i, l_h) + \theta_{d2,\pi} d(l_i, l_h)^2 + \varepsilon_{i,\pi,h} \quad (1)$$

where

- $lp(c_i, h)$ is the expected list price for a patient with characteristics c_i at hospital h and $\delta_{\pi,h}$

[41] The data are taken from the OSHPD Birth Cohort File for 2003. All summary statistics are very similar to those of our main dataset.

is 1 minus the discount negotiated at the hospital-insurer level so that $\delta_{\pi,h} lp(c_i, h)$ is the insurer's expected payment for the hospital's services

- s_i is a measure of the severity of the patient's illness and $q_h(s)$ is a vector of perceived qualities of hospital h for different patient sickness levels s so that $g_\pi(q_h(s), s_i)$ is an interaction term which differs with the quality of the hospital's services for different sickness levels,

- l_i is patient i's location and l_h is hospital h's location while $d(.)$ provides the distance between the two, and

- ε is a disturbance term not observed by the econometrician which is assumed mean independent of the included right hand side variables.

There is no outside option: we assume that the women in the discharge data only consider giving birth at a hospital.

In much of the analysis that follows we approximate $\delta_{\pi,h}$ with its average at the hospital level δ_h. In Section 7.3 we address the specification error generated by this assumption; until then we ignore it. The discount times the list price determines the first term in the utility function and it derives solely from the physician's preferences; the other terms may be affected by both patient and physician's preferences. Notice that the function $g_\pi(.)$ is allowed to differ arbitrarily among sickness levels for a given hospital and across hospitals. It therefore allows particular hospitals to have higher quality for some sickness levels than for others, and permits physicians to differ in their intensity of preferences for quality when considering patients of different sickness levels. For some specifications we will have to constrain $g_\pi(\cdot)$ to be a parametric function of patient and hospital characterisitcs. To the extent that the parametric assumption does not capture all the variance in $g_\pi(\cdot)$ the residual variance will create an additional unobservable that may bias the other parameters of interest. In particular if the "unobserved quality" represented by this residual is correlated with price we would expect it to cause a positive bias in the price coefficient. Finally note that when we allow $g_\pi(\cdot)$ to differ by insurer (by π), we allow different insurers to assess both different hospitals and the tradeoffs between hospitals and costs, differently, and we allow consumers to respond to these differences and select across insurers accordingly.[42]

6 Logit Analysis

We begin with a multinomial logit model of hospital choice, as it provides a familiar way of investigating the patterns in the data. The logit model makes the following assumptions.

$$\delta_{\pi,h} lp(c_i, h) = \delta_h^o lp^o(c_i, h) \tag{2}$$

$$\theta_{d1,\pi} = \theta_{d1};\ \theta_{d2,\pi} = \theta_{d2} \tag{3}$$

[42] This term is also useful in allowing PPO enrollees in Blue Shield and Blue Cross to have different preferences, or more discretion over choice of hospital, than enrollees in other insurers.

$$g_\pi(q_h(s), s_i) = q_h + \beta z_h x(s_i) \tag{4}$$

We make three different assumptions regarding the price coefficient $\theta_{p,\pi}$:

$$\begin{aligned} (a)\ \theta_{p,\pi} &= \theta_p; \\ (b)\ \theta_{p,\pi} &= \theta_{p,\pi}; \\ (c)\ \theta_{p,\pi} &= \theta_0 + \theta_1.pcap_\pi \end{aligned} \tag{5}$$

Finally we assume that $\varepsilon_{i,\pi,h}$ is known to our composite agent at the time decision is made, and has a distribution, conditional on the other right hand side variables, which is i.i.d. Type 1 extreme value. Note that this implicitly ignores measurement error. We estimate the model using maximum likelihood.

Equations (2) - (3) state that the price is equal to the expected list price (which is our "observed price") multiplied by one minus the observed average discount and that the distance coefficients are assumed to be fixed across insurers. Equation (4) restricts the $g_\pi(.)$ term in a way consistent with the previous literature: we assume it is determined by a hospital fixed effect plus interactions between hospital characteristics and patient characteristics that are known on admission and expected to be correlated with severity. In the inequalities analysis below we define over 100 patient severity groups and allow these to freely interact with hospital fixed effects. We can not do this in the logit analysis because it would imply estimating almost 20,000 coefficients and a similar number of expected price terms (without error). So we assume the interaction terms are determined by linear interactions between hospital and consumer diagnostic characteristics. Included in z_h are the number of nurses per bed and indicators for teaching hospitals, for-profit hospitals and hospitals that offer transplant services (a proxy for high-tech hospitals). We also include a measure of the quality of labor and birth services: hospitals were rated on a scale from 0 to 1, where 0 indicated that no labor/birth services were provided and a higher rating indicated that a less common (assumed to be higher-tech) service was offered. The patient characteristics in x_i are the expected probabilities of death in hospital and of transfer to acute care setting or special nursing facility given the patient's age group, principal diagnosis and Charlson score[43]. While these interactions, like those used in the previous literature, are sensible given the constraints imposed by the methodology, we do not expect them to be sufficient to fully address the price endogeneity issues noted above, and to the extent they do not, we expect an upward bias on the estimated price coefficient.

Of course there may be other biases in the price coefficient which emanate from errors in our expected price variable. We define the expected list price to be the average list price for the particular hospital over patients with the same age (categories 11-19, 20-39, 40-49 and 50-64), principal diagnosis (21 categories for women in labor including, for example, "normal delivery", "previous Cesarean Section" and "early labor"), Charlson score and diagnosis generating the Charlson score. Both principal diagnosis and Charlson score are based only on diagnoses known on admission. We

[43] We cannot include the higher-probability outcomes in Table 3 because the patient identifiers in the Birth Cohort data are not linked to our primary dataset.

are constrained to using these fairly broad definitions of similar patients because we encounter small sample problems when we define narrower groups[44]. To the extent that the aggregation generates measurement error in our price measure we expect it to attenuate the estimated price coefficient.

The equations in (5) note that we begin by assuming a common price coefficient across all insurers. We then allow this to differ across insurers and investigate whether there is a significant relationship between the percent of the insurer's payments to primary physicians that are capitated and the price coefficient. After presenting these results, we provide another set of results which control for variation in patient severity by restricting our attention to the least sick patients in the data. These are defined as women in labor who are aged 20-39, have a Charlson score of 0, and whose principal diagnosis and comorbidities are defined by obstetrical experts to be "routine". Our sample contains 43,742 of these patients. We then repeat the estimation using only the sickest patients in the data, defined as all women in labor other than those "least sick". This, however, is a group with a more diverse set of severity conditions, so we expect the simultanaeity bias to be more evident in this subsample. The inequalities analysis below addresses both the price endogeneity and the measurement error issues more directly.

6.1 Logit Results

A summary of the results is reported in Table 4. The price coefficients, price interaction terms and distance coefficients are reported, together with the sample size, for each specification. In each case the distance coefficient is negative and highly significant, with a magnitude that is consistent with estimates from the previous literature.[45] The price coefficient is positive and significant with a t value of approximately 5. Not unexpectedly the price coefficient seems to be biased upwards in the specification using the full sample of labor/birth discharges. I.e. high price hospitals are likely to be high quality hospitals, there is a preference for high quality hospitals, and part of the relevant quality differences are not picked up by the observables we have been able to put into our equation.

When we restrict the sample to the least-sick women the coefficient becomes negative (magnitude -0.017) and marginally significant (standard error 0.009). Including interactions between price and insurer fixed effects yields interesting results. Insurers in the table are sorted by declining proportion of capitated payments to primary physicians. Blue Cross and Blue Shield, which have the lowest proportions of capitated payments, have small, positive and insignificant price coefficents. All four of the remaining HMOs have price coefficients less than 0 even though we have not fully controlled for severity-hospital interactions. The negative price coefficients are significant for Pacificare and Health Net, two of the three carriers that favor capitation the most (97% of payments for Pacificare and 80% for Health Net). The remaining carriers, Aetna and Cigna, have relatively small

[44] If the set of patients to be used to determine a patient's price in a particular hospital is empty, we expand the group of "similar" patients to include women in the same age category and with the same Charlson score and principal diagnosis. If this is also empty we expand it to include all same-age category same-principal diagnosis patients, then all same-principal diagnosis women. If this group is also empty we take the mean of the non-missing prices already calculated for the particular patient.

[45] See, for example, Gaynor and Vogt (2003) and Ho (2006).

sample sizes (6291 and 8097 labor discharges respectively, compared to 15,479 for Pacificare and 16,950 for Health Net), which helps explain the larger standard errors on their price coefficients. When we remove the price-insurer interactions and instead include an interaction between price and the percent capitation in the insurer, the price coefficient is positive and the interaction term negative with almost twice the magnitude of the price coefficient. Both are significant at p=0.05.

We interpret the magnitudes of the coefficients by considering the average effects of changes in hospital characteristics on demand. Consider first the distance coefficient. We calculate the impact of a one mile increase in distance for hospital h, holding all else fixed, on the probability that a particular patient i visits that hospital. We then take the average over patients and a weighted average over hospitals. The average effect of the one mile distance increase is a 13.7% reduction in the probability that the hospital is chosen.[46] We conduct a similar exercise to evaluate the magnitude of the price effect. Consider Pacificare, the insurer with the most negative estimated price coefficient. The average effect of a \$1000 increase in a hospital's price, holding all other prices constant, is a 5.2% reduction in the probability that the hospital is chosen.[47] Finally we evaluate the trade-offs made between price and distance. We find the average of $\eta_i = \frac{\partial d_i}{\partial p_i}\frac{p_i}{d_i}$, or the distance reduction required to compensate for an incremental price increase at fixed utility. The cross-patient average of η_i for Pacificare is 0.33. Keep in mind that we still expect both simultaneity and attenuation bias in these numbers.

The results for the sickest population are, as expected, quite different. The price coefficient is now positive and significant, consistent with the hypothesis that unobserved within-hospital variation in quality (probably at the hospital-severity level) is positively correlated with price and affects choices more for sicker than for less-sick patients. When we add price-insurer interaction terms the interaction is again negative for Pacificare; although insignificant at p=0.05 and smaller in magnitude than for the healthier population. All other insurers' price coefficients are positive and three out of five are statistically significant. The third specification, including a price-percent capitation interaction, is consistent with prior results. Again we estimate a positive price coefficient and a negative interaction term (implying that insurers that favor capitated payments generate physician referrals that are more price-based than those of other physicians). However, the magnitudes are much more similar than for the healthier population and the implied overall price coefficient is positive even for insurers with 100% capitated payments to primary physicians.

The difference in results for the sick compared to the less-sick populations is suggestive of a more substantial endogeneity issue for the sicker population[48]. We conducted several robustness tests. First we investigated the importance of capitation payments to hospitals (rather than physicians) by

[46] The average distance to the chosen hospital for the less-sick patients included in the sample is 6.45 miles; the standard deviation is 10.11 miles. The weighted average probability that a particular hospital is chosen is 2.7%, where the weight is the number of discharges.

[47] The average price for the less-sick patients in the sample is \$3380; the standard deviation is \$1870.

[48] Of course it could also be that choices are made for sicker patients with a smaller price elasticity of demand but this is unlikely since the insurer pays the price (not the patient). Moreover when we more fully addressed endogeneity issues using the inequalities analysis described below and then split the sample between sicker and less sick patients we found little difference between the two sets of estimates.

interacting our price measure with 1 - the percent of hospital payments that are capitated. This had very little effect on the overall results. Second we added interactions between price and hospital characteristics such as indicators for teaching hospitals, hospitals providing transplant services and for profit hospitals and with the number of nurses per bed at the hospital. The estimated coefficients were almost always insignificantly different from zero. Finally we consider whether the hospital fixed effects estimated in the logit analyses are consistent with our interpretation of the results. These are jointly significantly different from zero in every specification and we look to see whether those estimated for the less sick patients are related to those estimated for the sick patients. The correlation between the coefficients from the analysis of less-sick and sicker patients is 0.71: that is, hospitals that are attractive to physicians referring less-sick women for their labor episodes tend also to be attractive options for sicker women.[49] This despite the fact that when we regress either set of the fixed effects onto hospital characteristics (indicators for teaching hospitals, for profit hospitals and hospitals that offer transplants, the number of nurses per bed and the quality of labor services) we get very low R^2's (.02 to .05), and even when market effects are added the R^2 only rises to .20[50].

7 Inequalities-Based Methodology

7.1 Definitions of Severity and Price

The results of the logit analysis indicate that the price paid by the insurer does matter in determining patient referrals to hospital, at least for the least sick patients. However, the logit methodology does not fully control for variation in quality, or in preferences for quality, at the hospital-severity level; a fact which might explain the positive price coefficient for relatively sick patients. In addition we are compelled to use average prices within quite broadly-defined patient groups because narrower groups would mean that we obtain our expected price by averaging a smaller number of realized prices, and this would generate a measurement error with larger variance in our expected price measure. We now develop an estimation method based on inequalities that addresses these issues.[51]

To do so we create an inequality for each patient and for each feasible alternative hospital that was not chosen. We then sum the inequalities of two same-insurer, same-severity patients whose chosen and alternative hospitals are switched. The severity-hospital interaction terms will be differenced out and it will be relatively straightforward to place bounds on the remaining terms. Since we have removed the interaction terms we no longer need to estimate their coefficients and can define them at a much more detailed level than was possible in the logit analysis. Moreover,

[49] We use the specification that includes price and price interacted with the percent capitation in the insurer.

[50] The characteristics include: whether it is a teaching hospital, nurses per bed, whether it is for profit, whether it offers transplants, and a measure of the quality of labor services.

[51] We address more structural problems with the price variable, problems related to the fact that different HMOs can obtain different discounts at the same hospital, in section 7.3 below.

the averaging should eliminate the effects of classical measurement error.[52]

This methodology relies on the assumption that the price measure varies within a hospital across patients who have the same insurer and the same severity level; otherwise the price terms would be differenced out along with the interaction terms. Severities are assumed to be defined in sufficient detail that the severity-hospital interactions absorb all unobserved variation that affects choices and might be correlated with price. The additional variation across patients in different price groups conditional on severity is therefore assumed not to affect choices except through the price variable itself. We now provide details of our severity and price definitions and consider whether these requirements are satisfied. Our definitions follow the advice of obstetrical experts at Columbia Presbyterian Hospital. As one input to the definitions, these experts assessed the list of principal diagnoses and co-morbidities in our data, assigning each a rank from 1 to 3 where 1 indicated a routine diagnosis (such as normal birth or immunization of the newborn) and 3 indicated something more serious. See Appendix 3 for a complete list.

We use much narrower definitions of severity and price than were used in the logit analysis. Severity groups are now defined by the interaction between age, principal diagnosis, Charlson score, diagnosis generating the Charlson score and a sub-category defined by the rank of the most serious co-morbidity, other than principal diagnosis, that is listed in the discharge record. Prices are now averages for women with the same severity (as just defined) who also have the same number of most seriously-ranked co-morbidities. These definitions generate many more groups than those used in the logit analysis. For example, for the first insurer in our data, there are 9 populated severity groups and 63 groups defining prices using the logit-based categories; there are 106 severities and 272 price groups under the more detailed definitions.

The obstetrical experts we interviewed advised us that these detailed price groups, conditional on severity, were unlikely to be important in terms of hospital choice. The price groupings are more detailed than those used for severity only in that they break out patients by the number of comorbidities of the highest rank as well as the identity of that rank. The number of similarly-ranked comorbidities is viewed as unimportant in determining referrals. While a physician might refer a pregnant woman with a comorbidity of rank 2 (such as hepatitis or a thyroid disorder) to a different hospital from a patient with only rank-1 comorbidities, this would be a hospital well-equipped to deal with high-risk pregnancies rather than the specific comorbidity, and the presence of two rather than one rank-2 comorbidities would not affect the referral decision. In contrast, our experts agreed that the number of comorbidities of a particular rank would be likely to affect the tests performed and drugs prescribed and therefore the price.

We test our assumptions by using an Analysis of Variance to consider whether price groups conditional on severity help explain variance in outcomes. We hypothesize that, if outcomes are not affected by this additional variation, it may be reasonable to assume that it also does not affect choices. We use the outcome measures set out in Table 3: the probabilities of discharge to a

[52] Given particular distributional assumptions, the first of these advantages can be achieved using a conditional logit model like that in Chamberlain (1980) but the second cannot. We plan to estimate a conditional logit model as a robustness test.

location other than home and of readmission within 12 months for both mother and infant. The results indicate that, under our definitions, moving from severity to price groupings significantly increases the proportion of the variance in price that is absorbed in hospital-patient type groups but does not significantly increase the proportion of the variance in outcomes that is absorbed in these groups. That is, we can hold outcomes fixed while allowing price to vary across groups of patients within a severity category. Details of the test are given in Appendix 1. We also note that the Analysis of Variance indicates reasonable price variation across price groups conditional on severity. Moving from severity to price groupings explains an additional 12% of the variance in price (moving from 50% to 62% of the total variance). We take this to be sufficient evidence that our proposed definitions of severity and price groups are well-suited to our model.[53] We now continue to the formal analysis.

7.2 The Inequalities Methodology

The choice model is still defined by the structural part of the utility function in equation (1), i.e. the part that is observed up to the parameter vector being estimated or,

$$W_{i,\pi,h} = \theta_{p,\pi}(\delta_{\pi,h} lp(c_i, h)) + g_\pi(q_h(s), s_i) + \theta_{d,\pi} d(l_i, l_h) \tag{6}$$

where for simplicity we have dropped the squared term in distance since it was very close to zero in all the logit results and dropping it did not affect those estimates. However now we explicitly distinguish between our constructs of price and those determining the physician's choice. We treat the difference between our measures of these variables and the variables actually used by the physician as mean zero measurement error; and it becomes the disturbance in the choice equation.

More formally we have

$$W_{i,\pi,h}(x, h, \theta) = W^o_{i,\pi,h}(x^o, h, \theta) + \varepsilon_{i,\pi,h} \tag{7}$$

where $W^o(\cdot)$ is the model we obtain after substituting

$$\delta^o_h lp^o(c_i, h) = \delta_{\pi,h} lp(c_i, h) - \varepsilon^p_{i,\pi,h} \tag{8}$$

for $\delta_{\pi,h} lp(c_i, h)$ in equation (6), and, if "a" indexes the severity groupings of patients and "c" their groupings for price so that $s_i = s\,(a\,(c_i))$, substituting

$$g_\pi(q_h(s), s(a(c_i)) = g_\pi(q_h(s), s_i) - \varepsilon^g_{i,\pi,h}. \tag{9}$$

for $g_\pi(q_h(s), s_i)$ in that equation, so that

$$\varepsilon_{i,\pi,h} \equiv \theta_{p,\pi}\varepsilon^p_{i,\pi,h} + \varepsilon^g_{i,\pi,h},$$

[53] We have obtained more detailed outcomes measures, including mortality and readmissions data for infants as well as their mothers, and are working on repeating the Analysis of Variance calculations using the new data.

and is assumed to be mean zero measurement error. In the models that use instruments (see below), we will need the stronger assumption that $\varepsilon_{i,\pi,h}$ is mean zero conditional on those instruments.

We allow the price coefficient to differ across insurers as $\theta_{p,\pi}$: this is assumption (b) in equations (5). Since, in contrast to the logit model which assumes an explicit distribution for $\varepsilon_{i,\pi,h}$, we will only be using averages, we have a free normalization. As a result we divide through by the absolute value of the distance coefficient (which is assumed to be negative), incorporating its magnitude into $\theta_{p,\pi}$ and $g_\pi(.)$.

We conduct the analysis for each insurer separately. We begin by ordering the hospitals from the highest to the lowest average price. For every hospital h we consider every patient i_h who is admitted to h in our data and every other hospital h' in her choice set. Our model implies the following inequality:

$$\begin{aligned} \Delta W_{i_h,\pi,h,h'} &= W_{i_h,\pi,h}(x,h,\theta) - W_{i_h,\pi,h'}(x,h',\theta) \\ &= \theta_{p,\pi}(\delta_h^o lp^o(c_{i_h},h) - \delta_{h'}^o lp^o(c_{i_h},h')) + g_\pi(q_h(s), s(a(c_{i_h}))) - g_\pi(q_{h'}(s), s(a(c_{i_h}))) \\ &\quad -(d(l_{i_h},l_h) - d(l_{i_h},l_{h'})) + \left(\varepsilon_{i_h,\pi,h} - \varepsilon_{i_h,\pi,h'}\right) \\ &\geq 0 \end{aligned} \tag{10}$$

For that (i_h, h, h') triple we find every patient $i_{h'}$ who is admitted to hospital h', whose choice set includes h and who has the same severity a, the same insurer π and a different group defining price c. We sum the inequalities of the two patients to difference out the $g_\pi(.)$ terms. Writing $\delta_h^o lp^o(c_{i_h},h) - \delta_{h'}^o lp^o(c_{i_h},h') = p^o(i_h,h,h')$, $d(l_{i_h},l_h) - d(l_{i_h},l_{h'}) = d(i_h,h,h')$ and $\varepsilon_{i_h,\pi,h} - \varepsilon_{i_h,\pi,h'} = \varepsilon(i_h,h,h')$:

$$\begin{aligned} & \Delta W_{i_h,\pi,h,h'} + \Delta W_{i_{h'},\pi,h',h} \\ = \;& \theta_{p,\pi}\left[p^o(i_h,h,h') + p^o(i_{h'},h',h)\right] - \left[d(i_h,h,h') + d\left(i_{h'},h',h\right)\right] + \left[\varepsilon(i_h,h,h') + \varepsilon\left(i_{h'},h',h\right)\right] \\ \geq \;& 0. \end{aligned} \tag{11}$$

Finally we take expectations on the data generating process to construct an inequality that relates the price coefficient to differences in prices and differences in distances:

$$E\left[\theta_{p,\pi}(p^o(i_h,h,h') + p^o(i_{h'},h',h)) \mid I_{i,\pi}\right] \geq E\left[d(i_h,h,h') + d\left(i_{h'},h',h\right) \mid I_{i,\pi}\right] \tag{12}$$

We sum over alternative hospitals $h' > h$ for each h and over severities a to obtain the inequality:

$$\theta_{p,\pi}\sum_a \sum_{h'>h} \sum_{i_h,i_{h'}} (p^o(i_h,h,h') + p^o(i_{h'},h',h)) \geq \sum_a \sum_{h'>h} \sum_{i_h,i_{h'}} \left(d(i_h,h,h') + d\left(i_{h'},h',h\right)\right) \tag{13}$$

for each $h = 1,...,H$. We also sum over h that have less than 1000 patient switches with other hospitals in our data, generating a separate moment for each large hospital and another that includes all smaller hospitals. Finally we divide each moment by an estimate of its standard error, generated using an estimate $\hat{\theta}_{p,\pi}$ of $\theta_{p,\pi}$ implied by the inequalities excluding weights. We denote this standard error $\hat{\sigma}_h(\hat{\theta}_{p,\pi})$. Our first inequality for estimation for hospital h is therefore:

$$\theta_{p,\pi}\frac{\sum_{a,h'}\sum_{i_h,i_{h'}}(p^o(i_h,h,h')+p^o(i_{h'},h',h))}{\hat{\sigma}_h(\hat{\theta}_{p,\pi})} \geq \frac{\sum_{a,h'}\sum_{i_h,i_{h'}}(d(i_h,h,h')+d(i_{h'},h',h))}{\hat{\sigma}_h(\hat{\theta}_{p,\pi})} \quad (14)$$

This generates a lower bound for $\theta_{p,\pi}$ if the price term is positive and an upper bound if the price term is negative.[54] We then add analogous bounds by interacting the inequalities in (11) with an instrument of consistent sign. Any instrument which is observed by the agent when decisions are made and is mean independent of the measurement error will generate consistent bounds. Our instruments are the positive and negative parts, respectively, of the distance difference terms defined above: $d(i_h,h,h')_+, d(i_h,h,h')_-,\ d(i_{h'},h',h)_+, d(i_{h'},h',h)_-$. Overall we have between 73 and 283 moments per insurer: one for each combination of an instrument and a major hospital and an additional moment per instrument that includes hospitals with fewer patients.[55]

We generate 95% confidence intervals for the estimates using the method developed in Pakes, Porter, Ho and Ishii (2011).

7.3 Estimating the Variation in Discounts Across Insurers

The inequalities methodology addresses price endogeneity problems inherent in the logit analysis and also removes mean-zero price measurement error by summing over patients and hospitals. However, specification error in price caused by the unobserved variation in discounts across insurers has not yet been addressed.[56] We now introduce a method for correcting for the variation in hospital discounts across plans.

We begin with the average negotiated discount at the hospital level, d_h.[57] We introduce additional data from the OSHPD hospital discharge and financial records for 2003 that indicate the share of each hospital's total revenues that come from each insurer. We use this information together with some functional form assumptions to estimate an equation for $d_{\pi,h}$, the discount at the hospital-insurer level, as a function of hospital, insurer and market characteristics. We specify a logistic functional form so that $d_{\pi,h} \in [0,1]$ and derive an equation that can be estimated using non-linear least squares. Explanatory variables include, for example, indicators for for-profit hospitals and hospitals that are members of systems (groups of providers that bargain jointly with insurers), indicators for teaching hospitals, insurer fixed effects and either market fixed effects or market

[54] We exclude from the analysis hospitals that have fewer than 50 switches with any other hospital in the analysis. When instruments are included, each pair of hospitals is required to have at least 50 switches whose value of the instrument is non-zero. In addition to reducing noise in the expected price variable, this has the benefit of removing the 8% of Blue Shield's hospitals, and the 5% of Blue Cross's hospitals that receive the fewest patients.

[55] We also tried estimating the coefficients keeping the smaller-hospital moments separate. The estimated coefficients were almost always smaller in magnitude than our baseline results, consistent with small hospitals introducing measurement error, but in qualitative terms the story did not change.

[56] Specification error at the hospital-severity level will be absorbed into the $g_\pi(.)$ term; variation across c_i groups within hospital-severity will be in $\varepsilon_{i_h,\pi,h}$.

[57] We conduct this analysis using the discount d_h rather than one minus the discount, which is defined above as $\delta^o_h = 1 - d_h$.

characteristics.[58] Details of our methodology and results are set out in Appendix 2. The results are intuitive: we find that variables likely to be positively correlated with hospital bargaining power are negatively related to hospital discounts, while those positively related to insurer bargaining power are positively correlated with discounts. For example, the coefficient on a variable measuring the hospital's share of beds in the market, a potential measure of hospital bargaining power, is negative as expected. The coefficient on HMO market share is positive consistent with a bargaining power story.

In additional specifications (results available from the authors) we investigate whether the proportion of the insurer's patients sent to a particular hospital is correlated with the discount. This relationship between the "channeling" of patients to a particular provider and the prices negotiated with that provider is analyzed in Sorensen (2003). When we exclude market fixed effects we estimate a significant positive relationship between patient channeling and discounts (a negative relationship between channeling and prices) for just one insurer, Blue Shield. The coefficient becomes insignificant when we add market fixed effects[59]. In further specifications we repeat our analyses but replace the insurer fixed effects with the plan percent capitation. This coefficient is positive but insignificant in every specification. The other coefficient estimates are qualitatively unaffected by this change. This lack of a significant relationship between discounts and the insurer's percent capitated payments to physicians suggests that accounting for variation in discounts across insurers will not change the results of our inequalities analysis. We confirm this idea in the results section below.

The final step is to use these estimates to generate a prediction for $d_{\pi,h}$. There are two possibilities. First we can use the model's prediction directly; we denote this $\hat{d}^1_{\pi,h}$. Alternatively we can subtract the predicted discounts of other insurers (appropriately weighted) from the observed d_h to generate a second prediction $\hat{d}^2_{\pi,h}$. Details are provided in Appendix 2. We use the predictions to define price measures $p^1(.) = (1 - \hat{d}^1_{\pi,h})lp^o(c_i, h)$ and $p^2(.) = (1 - \hat{d}^2_{\pi,h})lp^o(c_i, h)$ and use these in the inequalities analysis that follows. The two predictions incur different errors. These, together with estimation error from this step and measurement error from the expected list price calculation, will be inputs into the error term $\varepsilon_{i_h,\pi,h}$ defined in Section 7.2.

While use of $p^1(.)$ and/or $p^2(.)$ as our price variable mitigates the problems that could arise from using a price variable that does not account for insurer-specific discounts, it probably does not eliminate them. To the extent that doctors know the unobservables from our discount regression and select hospitals based on their values there will still be a selection bias in both of these price variables. It is therefore clear that all of our price measures are imperfect. Our approach to this problem is to provide several sets of results based on different measures of price. The different measures should be differentially related to the biases discussed above, so by looking across results

[58] The specification that we use to predict $\delta_{\pi,h}$ for the inequalities analysis includes market fixed effects. We estimate other specifications that include market characteristics to check that our results are consistent with previous papers analyzing the impact of market characteristics on hospital prices.

[59] We repeated the inequalities analysis for Blue Shield using this discount specification. The results changed very little: the price coefficient was positive and statistically insignificant as in the primary specification discussed below.

we should be able to tell something about the likely magnitude of the resulting biases.

7.4 Inequality Results

Table 5 reports the results of the inequalities analysis. In each case we use the detailed definitions of severity and price described in Section 7.1. In the first column of results we use the price measure defined using the observed hospital-level discount: $p(.) = \delta_h^o lp^o(c_i, h) = (1 - d_h) lp^o(c_i, h)$. In the second and third columns we address price specification error by using the hospital-insurer level discounts estimated in the previous section: $p^1(.) = (1-\hat{d}^1_{\pi,h}) lp^o(c_i, h)$and $p^2(.) = (1-\hat{d}^2_{\pi,h}) lp^o(c_i, h)$. In every case our estimate of $\theta_{p,\pi}$ is a singleton. That is, there is no parameter vector that satisfies all the inequality constraints. As noted in Pakes, Porter, Ho and Ishii (2011) this does not imply that we should reject the specification. In particular the finding is likely due to small sample bias resulting from taking the highest of the lower bounds and the lowest of the upper bounds generated by the moments. The highest of the lower bounds will have a positive bias and the lowest of the upper bounds will have a negative bias; this can easily cause an intersection of the bounds due to sampling error. When we formally tested for misspecification we could not reject the null (that the result was not due to misspecification) in a test of size 0.01. We follow the methodology outlined in Pakes, Porter, Ho and Ishii (2011) to identify the point estimate that minimizes the amount by which the inequalities are violated.

We estimate the t-statistic of each moment. This is equal to the value of the moment at the estimated $\theta_{p,\pi}$ since we divide by estimated standard errors in generating the moments. Summary statistics are given in Table 6. Recall that under our model the moments should be non-negative. For four out of six insurers no more than 6% of moments have t-statistics with negative values (for example 9 out of 161 moments for Pacificare and 2 out of 90 moments for Cigna) and none have a value less than -1.1. However, Health Net has 11 out of 177 moments with negative t-statistics, two with a value less than -2. Blue Cross has 36 out of 283 moments with negative t-statistics; seven are less than -2. For these two insurers we conduct the robustness test of dropping the moments with t-statistics less than -2 and repeating the estimation procedure. The results are reported in the rows of Table 5 labeled "Drop $t < -2$".

In the first column of Table 5 the price coefficients for all insurers other than Blue Shield are negative and statistically significant at p=0.05. That for Blue Shield is small, positive and statistically insignificant. In Figure 1 we plot the estimated upper and lower bounds of the confidence intervals for $\theta_{p,\pi}$ for each insurer. We exclude Aetna, whose price coefficient is large and negative, so that the others can be compared more easily. The coefficients for Pacificare, Aetna and Cigna are significantly more negative than those for Blue Shield and Blue Cross (that is, the upper bounds of their confidence intervals are above the lower bounds for Blue Shield and Blue Cross). The picture is less clear for Health Net although the lower bound of its confidence interval is much lower than those of the lower-capitation insurers.

The results in columns 2 and 3 of the table are quite consistent with those in column 1. The magnitudes of the estimates are similar except that Aetna's price coefficient, which was negative and very large in column 1 (at -4.34), halves in magnitude in the other columns. The confidence intervals are tighter than in column 1 in several cases, although they are much looser for Blue Shield (the only not for profit insurer in our data)[60]. So the discount information does seems helpful. The similarity across the columns suggests that specification error in the discount variable is of limited importance.[61]

These results suggest that insurers with weakly more than 75% capitated payments to primary physicians have hospital referral processes that place a more negative weight on prices than the other insurers in our data. In contrast to the logit analysis, we did not need to subset by sickness level to generate this result.

As expected, the price coefficients are more negative than those estimated in the logit analysis. Again we interpret magnitudes by finding the cross-patient average of $\eta_i = \frac{\partial d_i}{\partial p_i}\frac{p_i}{d_i}$, or the distance reduction required to compensate the patient for an incremental price increase at fixed utility, considering Pacificare in particular. The logits implied an average η_i of 0.33. When we repeat the calculation using the estimates from the inequalities methodology the average value for Pacificare is 9.90. That is, a 9.9 percent reduction in distance to hospital is required to hold utility fixed when price increases by 1 percent.[62]

7.5 Robustness Tests and Alternative Explanations

We conduct several robustness tests. First, our discount analysis made the assumption that discounts were fixed across diagnoses within a hospital-insurer pair. Our estimates may be biased if in reality discounts differ across diagnoses; interview evidence indicates that this is likely for some hospital-insurer pairs. We address this possibility by estimating a more detailed model of discounts that permits the discount for labor and birth episodes to differ from the average for other diagnoses by a factor γ to be estimated:

$$d_{\pi,h} = \left(1 - s^{birth}_{\pi,h}\right) d_{\pi,h} + s^{birth}_{\pi,h} \gamma d_{\pi,h.}$$

Under analogous assumptions to those made in the baseline discount analysis we can derive an equation for estimation. Using a nonlinear least squares methodology we estimate the magnitude of γ to be 1.058 (S.E. 0.233). That is, births are estimated to have a 6% higher discount than the average for other diagnoses. The other coefficients differ very little from the baseline specification. We repeat the inequalities analysis using the prices for labor and birth episodes implied by this specification. The effect on the final results is very small.[63]

[60] As a not for profit insurer, Blue Shield may have a different process of negotiating prices from other insurers.

[61] In particular the two price estimates incur different errors $e^1_{\pi,h}$ and $e^2_{\pi,h}$, so if the errors were important we would expect columns 2 and 3 to generate different results.

[62] We repeat this analysis for Cigna, which has a lower proportion of capitated payments than Pacificare. Under the logit analysis the average η_i is 0.104; under the inequalities the value implied by the mid-point of the price coefficient interval is 5.65.

[63] We also investigate specifications where γ is defined as a linear function of hospital characteristics but none of the coefficients other than the constant term are statistically significant.

Our second robustness test relates to potentially endogenous quantity. Since the referring physician has some input into the quantity and type of care provided in the hospital, our results could be caused by physicians responding to incentives by providing less care to capitated patients, rather than by referring them to lower-priced hospitals. We investigate this issue by regressing a severity-adjusted price measure on the insurer's percent capitation payments to physicians and market or hospital fixed effects.[64] When we include market fixed effects we estimate a negative and significant coefficient on the capitation variable (coefficient -0.056, standard error 0.012), consistent with physicians either referring capitated patients to lower-priced hospitals or providing less care to them than to other similar patients. When we use hospital fixed effects the capitation coefficient becomes very small and statistically insignificant (coefficient estimate -0.0002, standard error 0.011), suggesting that capitated patients do not receive less care than other same-severity patients in the same hospital. We conclude that our results are related to hospital referrals rather than to treatment decisions conditional on referral.

We also repeat the inequalities analysis using the list price rather than its interaction with the discount as our price measure. The pattern of results is unchanged in that high-capitation insurers have more negative price coefficients in general than other insurers. However all price coefficients are closer to zero than those in Table 5, consistent with our expectation that measurement error should affect these results.

Finally, given that we observe only a cross-section of data with no physician identifiers, it is possible that our results are due to unobserved differences between the insurers in our data. As noted above, our data for Blue Shield and Blue Cross (the carriers with the lowest percent capitation) contain both HMO and PPO enrollees. We drop hospitals that attract small numbers of patients for these insurers, expecting thereby to exclude out-of-network hospitals.[65] The $g_\pi(.)$ terms in the utility equation, which are permitted to vary freely across insurers in the inequalities analysis, allow Blue Shield and Blue Cross enrollees to have different preferences or greater discretion over hospital choice than those in other insurers without affecting the price coefficient. The final issue is the different pricing scheme in PPO plans. Our interpretation of the estimated price coefficient is affected by some PPO enrollees paying a coinsurance rate rather than a fixed copay for hospital services. This pricing scheme affects only around 15% of PPO enrollees so the impact on our results is likely to be small. In addition this issue biases our results towards finding no difference between high- and low-capitation insurers. It implies that our estimated price coefficient $\theta_{p,\pi}$, which we interpret as physician price sensitivity, in fact represents a weighted average of physician and consumer price coefficients:

$$\theta_{p,\pi} = \lambda\theta_{p,\pi}^{phys} + (1-\lambda)\theta_{p,\pi}^{cons}$$

[64] We control for patient severity by using a price ratio measure like that used in Ho and Pakes (2011): $p_i^{ratio} = \frac{p_i}{p_{s_i}}$, where p_i is the observed price (the list price interacted with 1 minus the hospital-level discount) for patient i and p_{s_i} is the average price for same-severity patients.

[65] The inequalities analysis drops hospitals with fewer than 50 switches with other hospitals in the data. This implies dropping 8% of hospitals for Blue Shield and 5% for Blue Cross. Given that on average 83% of the hospitals in the market are included in each insurer's network (Ho (2006)), this is likely to be sufficient to exclude out-of-network hospitals.

where λ is the weight on physician preferences. If, as seems likely, patients are more price-sensitive than physicians, this equation implies that $\theta_{p,\pi}$ is an over-estimate of physician price sensitivity in Blue Shield and Blue Cross.[66]

Most other cross-sectional differences between insurers seem unlikely to be important. While Blue Shield and Blue Cross were historically different from other insurers, these differences have decreased over time. The data in Table 2 indicate that the "Blue" plans were no longer major providers of Medicare services in California by 2003. Blue Cross was a for-profit organization which, while it did provide Medi-Cal coverage (the California equivalent of Medicaid) in 2002, had 3.5 million out of 4.8 million enrollees in commercial plans. Blue Shield, as a not-for-profit firm, was still somewhat different from the other insurers we consider. This may explain the large confidence intervals on its estimated price coefficient in our analysis. In general, however, our assumption is that while historical differences between insurers may be partly responsible for the variation in capitation payments used to identify our model, they do not generate differences in physician referral patterns directly. Similarly, we assume that our results are not caused by physicians who are affiliated with high-cost hospitals selecting into low-capitation insurers. We explain above the reasons why the alternative causal explanation seems just as likely.

8 Outcomes

We now consider whether the outcome measures summarized in Table 3 (the probabilities of discharge to a location other than home and of readmission within 12 months, for both mother and infant) differ significantly across insurers. For every outcome measure and every severity group we conduct a χ^2 test of the null that the proportion of patients experiencing the adverse outcome does not differ across insurers. The results are shown in Table 7 for the most populated severity group (mothers aged 20-39 with a normal pregnancy and no co-morbidities ranked above 1). The p-value of the χ^2 test is reported for each pair of insurers. A p-value below 0.05 implies that the two insurers have significantly different outcomes. This is the case for 15 out of 60 or 25% of the insurer pairs across the four outcomes measures. However there is no clear pattern regarding whether high-capitation or low-capitation insurers have better outcomes; in fact different insurers perform relatively well for different outcome measures[67]. The rankings also differ across severities. We aggregate these analyses by conducting a χ^2 test that includes all outcome measures and severities. We consider each pair of insurers in turn. We can never reject the null hypothesis that the vector of outcomes (for all severities) is independent of the insurer in a test of size 0.001[68]. We conclude

[66] If patients are less price-sensitive than physicians, our estimated physician price coefficient is biased down, but given the high value of λ the bias is small.

[67] Consider for example the insurer ranking implied by the highest-probability adverse event, infant readmission within 12 months. Only 11% of insurer pairs across the remaining three adverse outcomes have significantly different outcomes that have the same rank order as for infant readmission.

[68] The test statistic is defined as follows. If $O_{s,p}$ is the outcome vector (across our 4 outcome measures) for severity s and insurer p, the null hypothesis is that the population means are the same. Then under the null hypothesis, the statistic $\sum_s [O_{s,1} - O_{s,2}]' [\Sigma_{s,1}/n_{s,1} + \Sigma_{s,2}/n_{s,2}]^{-1} [O_{s,1} - O_{s,2}]$ is distributed $\chi^2_{4s_{1,2}}$ where $\Sigma_{s,p}$ is the variance-covariance matrix. The number of degrees of freedom is $4s_{1,2}$ where $s_{1,2}$ is the number of severities.

that, while patients in high-capitation insurers are referred to lower-priced hospitals, they do not have significantly worse outcomes than other similar patients in California.

9 Conclusion

We have estimated the price coefficient in the hospital utility equation using two methodologies: a multinomial logit analysis and an analysis based on inequalities. The inequalities method has several advantages compared to the logit. It enables us to control more fully for price endogeneity and price measurement issues and also implies no distributional assumptions on the unobservables. The latter is a potentially important benefit in discrete choice settings. Finally we are able to allow for selection of unobservably different types of patients into different insurers in a flexible way that is not possible using logit techniques.

Both methodologies indicate that the price coefficient is significantly more negative for patients whose insurers make predominately capitation-based payments to physicians than for other patients. The inequalities analysis indicates that these results hold on average for all women in labor, not just the least sick. The results have important implications for the coming health reforms, which introduce similar financial incentives for physicians providing Medicare and Medicaid services.

Our analysis of the available data on outcomes indicates no consistent difference, conditional on severity, between high- and low-capitation insurers. Of course we would need much more information on the quality of services provided in different hospitals and the link between quality and outcomes to fully understand whether differences in price elasticities across insurers has any effect on patient outcomes. However the data suggest that, for relatively healthy patients such as women in labor, capitation payment arrangements could lead to cost reductions through changes in hospital referrals without substantial implications for outcomes.

We note that in addition to generating cost-control incentives the financial arrangements introduced by the 2010 health reforms, like those used in California, introduce some risk of physician group bankruptcy. During the late 1990s many Californian physician groups that accepted capitation payments, some of which were well-established and well-known to patients, went out of business. Reports in the press and the health policy literature noted that these groups often encountered problems with managing a significant amount of insurance risk.[69] In response to this wave of failures the California Legislature passed several managed care bills in 1999 which required physician groups to maintain positive working capital and positive tangible net equity and established a Department of Managed Health Care to oversee the financial condition of physician groups. Since then the financial stability of physician groups has improved (although the number of capitated HMO patients has fallen as some patients switched to other types of insurance). If the accountable care organizations and bundling arrangements set up by the current health reforms are to be successful, policy makers need to fully understand the issues that caused these problems in California. We leave this as a topic for future research.

[69] See Baumgarten (2004), Bodenheimer (2000) and Robinson (2001) for details.

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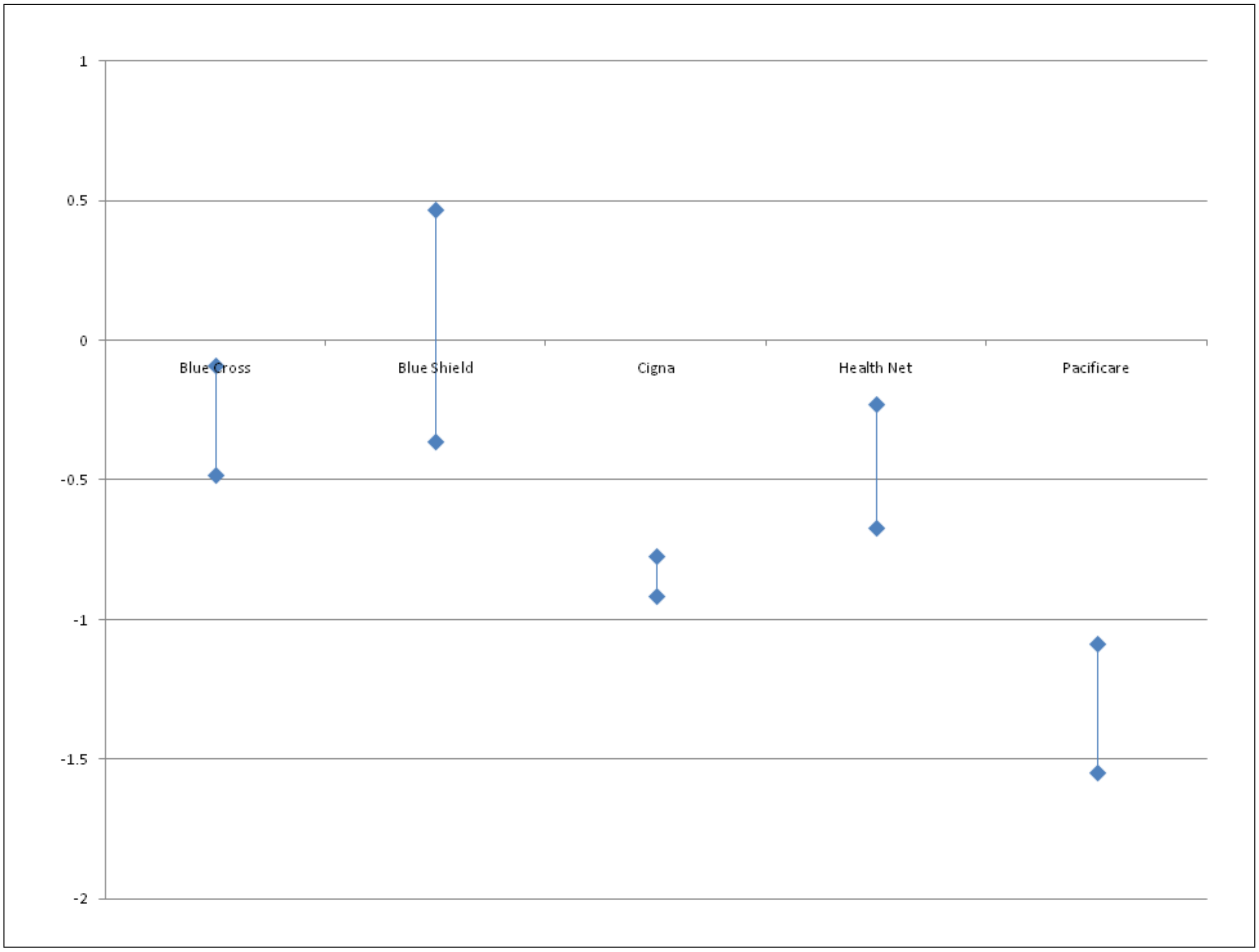
Figure 1: Correlation of Estimated Price Coefficient with Insurer's Percent Capitation Payments



Notes: Graph to illustrate confidence intervals for insurer price coefficients, reported in Table 7. Estimates are from model where $p(.) = (1 - d_h)lp(c_i, h)$. Estimates for Aetna are excluded to enable easier comparisons between the other insurers. Aetna's confidence interval is [-5.97, -4.19].

Table 1: Summary Statistics by Insurer

	2002 enrollment			Labor	% Prim	Tax	Premium	Admin	Medical	Inpatient utilizn		Prescrip
	Commerc	Medicare	Medi-Cal	discharg	Capitn	Status	pmpm	expense	loss ratio	discha	days	drugs
Aetna	485,787	37,312	0	6,291	0.91	FP	152.42	19.33	86.2%	38.4	139.8	23.15
Blue Cross	3,486,358	251,299	1,099,044	25,038	0.38	FP	186.86	21.22	78.9%	38.4	142.4	20.92
Blue Shield	2,231,350	67,049	0	16,302	0.57	NFP	146.33	22.72	83.5%	50.3	176.4	20.51
Cigna	634,568	0	0	8,097	0.75	FP	-	27.07	84.6%	39.8	137.1	15.63
Health Net	1,665,221	101,317	349,826	16,950	0.80	FP	184.92	18.60	86.3%	39.0	137.8	21.08
Pacificare	1,543,000	386,076	0	15,479	0.97	FP	149.92	24.51	88.4%	44.5	156.5	20.48
Kaiser	5,790,348	671,858	104,844	0		NFP	163.44	5.23	97.7%	49.1	158.1	0.44

Notes: Data on the six insurers included in our analysis and on Kaiser Permanente; the latter is excluded from our later analysis because the prices paid to hospitals are not reported. Source for all fields except Labor discharges and % primary capitation: Baumgarten (2004). 2002 enrollment provided separately for commercial plans, Medicare plans and Medi-Cal/Healthy Families plans. "Labor discharg" is the number of discharges in the data sample used in our analyses. "% Prim Capitn" is the percent of payments to primary providers made on a capitated basis in 2003 (source: State of California Department of Managed Health Care Annual Financial Reporting Forms, 2003). "Admin expense" is per member per month administrative expenses for entire insurer in 2002, "Medical loss ratio" is medical and hospital expenses divided by premium revenues for entire insurer in 2002. Inpatient utilization and prescription drug data are for commercial plan only in 2002: "discha" is dicharges per 1000 members, "days" is acute days per 1000 members and "Prescrip drugs" is outpatient prescription drug expenses per member per month.

Table 2: Summary Statistics by Discharge

	Labor only	
	Mean	Std. Devn.
Number of patients	88,157	
Number of hospitals	195	
Number of insurers	6	
Hospitals per patient choice set	38	
Teaching hospital	0.27	
Distance to all hospitals (miles)	24.6	25.6
Distance to chosen hospital	6.7	10.3
List price	$13,312	$13,213
Discounted price	$4,317	$4,596
Length of stay	2.54	2.39
Died	0.01%	0.004%
Acute transfer	0.3%	0.02%
Special Nursing Transfer	1.5%	0.04%

Notes: Summary statistics for dataset comprising private enrollees of the six largest HMOs excluding Kaiser who are admitted for labor-related diagnoses. "Discounted price" is list price*(1-discount). "Died" is the probability of death while in hospital, "Acute Transfer" the probability of transfer to an acute care setting (in this or a different hospital) and "Special Nursing Transfer" the probability of transfer to a special nursing facility (again at this or a different hospital). "Std Devn" for "Died", "Acute transfer" and "Special Nursing Transfer" are calculated under the assumption that the 0/1 variable is binomially distributed.

Table 3: Prices and Outcomes by Patient Type

		Mother			Infant		
	N	Price*(1-disc)	Readmission	Not Home	Price*(1-disc)	Readmission	Not Home
Overall	73117	4291 (4373)	2.39% (0.06%)	1.62% (0.05%)	2679 (18545)	9.42% (0.1%)	6.60% (0.1%)
Age							
<40	71073	4259 (4329)	2.36% (0.1%)	1.60% (0.1%)	2632 (18322)	9.41% (0.1%)	6.50% (0.1%)
>40	2044	5420 (5571)	3.52% (0.4%)	2.10% (0.3%)	4347 (25062)	9.64% (0.6%)	9.88% (0.7%)
Signif diff		0.000	0.000	0.038	0.000	0.365	0.000
Charlson							
0	71803	4256 (4265)	2.33% (0.1%)	1.58% (0.1%)	2624 (18331)	9.36% (0.1%)	6.53% (0.1%)
>0	1314	6227 (8135)	5.78% (0.6%)	3.42% (0.5%)	5714 (27667)	12.3% (0.9%)	10.5% (0.9%)
Signif diff		0.000	0.000	0.000	0.000	0000	0.000

Notes: Data taken from OSHPD Birth Cohort 2003. "Readmission" is percent of patients readmitted to hospital within 12 months of birth episode. "Not Home" is percent of patients discharged somewhere other than home; this includes transfer to acute care setting, transfer to special nursing facility, discharge against medical advice and death. Standard deviations in parentheses; for Readmission and Not Home these are standard errors calculated assuming that the 0/1 variables are binomially distributed. Charlson scores assign weights to comorbidities (known on admission to hospital) other than principal diagnosis where higher weight indicates higher severity. Value 0-6 are observed in the data. "Signif diff" states significance level at which we cannot reject the hypothesis that the means in the two samples are the same; these are the results of a t-test for price*(1-discount) and a z-test assuming two binomial distributions for Readmission and Not Home.

Table 4: Logit Analysis Results

	All labor patients	Least sick patients			Sickest patients		
Price	0.010** (0.002)	-0.017* (0.009)		0.069** (0.014)	0.012** (0.002)		0.028** (0.006)
Price interactions:							
% Capitated				-0.127** (0.016)			-0.025** (0.008)
Pacificare			-0.077** (0.01)			-0.006 (0.006)	
Aetna			-0.011 (0.016)			0.021** (0.008)	
Health Net			-0.038** (0.01)			0.007 (0.005)	
Cigna			-0.021 (0.014)			0.004 (0.007)	
Blue Shield			0.018 (0.011)			0.024** (0.004)	
Blue Cross			0.008 (0.011)			0.014** (0.003)	
Distance	-0.215** (0.001)	-0.215** (0.002)	-0.215** (0.002)	-0.215** (0.002)	-0.217** (0002)	-0.216** (0.002)	-0.216** (0.002)
Distance squared	0.001** (0.000)	0.001** (0.000)	0.001** (0.000)	0.001** (0.000)	0.001** (0.000)	0.001** (0.000)	0.001** (0.000)
$z_h x_i$ controls (15 coeffts)	Y	Y	Y	Y	Y	Y	Y
Hospital F.E.s (194 coeffts)	Y	Y	Y	Y	Y	Y	Y
N	88,157	43,742	43,742	43,742	44,059	44,059	44,059

Notes: Results of multinomial logit demand analysis. N = number of patients. Least sick patients are aged 20-39 with zero Charlson scores and principal diagnoses and comorbidities defined by obstetrical experts to be "routine" (see Appendix 1 for details). Sickest patients are all other patients. $z_h x_i$ are interactions between observed hospital characteristics (indicators for teaching hospitals, for profit hospitals and hospitals that offer transplants, the number of nurses per bed and a variable summarizing the quality of labor services provided) and patient characteristics (probabilities of death while in hospital, transfer to an acute care facility and transfer to a special nursing facility conditional on principal diagnosis, age category and Charlson score, all of which are known ex ante).

Table 5: Results of Inequalities Analysis

		percent	$p(.) = (1-d_h)lp(c_i,h)$			$p(.) = (1-\hat{d}^1_{\pi,h})lp(c_i,h)$			$p(.) = (1-\hat{d}^2_{\pi,h})lp(c_i,h)$		
		capitated	$\hat{\theta}$	$[CI_{LB},$	$CI_{UB}]$	$\hat{\theta}$	$[CI_{LB},$	$CI_{UB}]$	$\hat{\theta}$	$[CI_{LB},$	$CI_{UB}]$
Pacificare		0.97	-1.34**	[-1.55,	-1.09]	-0.98**	[-1.48,	-0.76]	-1.35**	[-1.55,	-1.07]
Aetna		0.91	-4.34**	[-5.97,	-4.19]	-2.38**	[-2.72,	-2.01]	-2.54**	[-2.96,	-2.45]
Health Net	Baseline	0.80	-0.23**	[-0.32,	-0.18]	-0.26**	[-0.36,	-0.19]	-0.27**	[-0.37,	-0.22]
	Drop t<-2		-0.37**	[-0.67,	-0.23]	-0.27**	[-0.62,	-0.12]	-0.43**	[-0.71,	-0.27]
Cigna		0.75	-0.80**	[-0.92,	-0.77]	-0.56**	[-0.62,	-0.53]	-0.61**	[-0.68,	-0.58]
Blue Shield		0.57	0.04	[-0.36,	0.47]	0.28	[-0.71,	0.94]	0.30	[-2.24,	0.96]
Blue Cross	Baseline	0.38	-0.15**	[-0.23,	-0.11]	-0.04**	[-0.17,	-0.02]	-0.15**	[-0.22,	-0.11]
	Drop t<-2		-0.47**	[-0.48,	-0.10]	-0.31**	[-0.36,	-0.24]	-0.50**	[-0.51,	-0.15]

Notes: Results of inequalities analysis. We include 157 hospitals in total. Estimated coefficient is the ratio of the price coefficient to the distance coefficient in the utility equation, where prices are measured in $000 and distance in tens of miles. Three price measures are used; they are calculated using the observed average hospital discount, and the two estimated hospital-insurer level discounts discussed in Section 7.3, respectively. Specification includes four distance-based instruments (positive and negative parts of $d(i_h,h) - d(i_h,h')$ for each patient) plus a constant in the instrument set. The rows labeled "drop t<-2" report results when we dropped moments whose t-statistic values were less than -2 (2 out of 177 for Health Net; 7 out of 283 for Blue Cross) and repeated the estimation process.

Table 6: Summary of t-statistics from Inequalities Analysis

	Pacificare	Aetna	Health Net	Cigna	Blue Shield	Blue Cross
Summary of t-statistics						
Number positive	152	71	166	88	162	247
Ave value of positive	11.1	19.4	14.5	17.0	17.0	20.1
Number negative	9	2	11	2	7	36
Number t $<$ -2	0	0	2	0	0	7

Notes: Summary of estimated t-statistics of the moments used in inequalities analysis. T-statistic = value of the moment at the estimated $\theta_{\pi,p}$ (for specification where $p(.) = (1 - d_h)lp(c_i, h)$). Under the model all moments should be non-negative.

Table 7: Tests of Outcome Differences Across Insurers

	Percent	Pacificare	Aetna	Health Net	Cigna	Blue Shield
Mother Readmission						
Pacificare	1.82%					
Aetna	1.33%	0.32				
Health Net	1.91%	0.82	0.24			
Cigna	2.08%	0.60	0.18	0.74		
Blue Shield	1.84%	0.96	0.30	0.86	0.62	
Blue Cross	1.99%	0.65	0.17	0.83	0.84	0.68
Mother Discharge "not home"						
Pacificare	1.03%					
Aetna	0.92%	0.76				
Health Net	2.01%	0.01**	0.03**			
Cigna	0.67%	0.29	0.51	0.00**		
Blue Shield	1.48%	0.19	0.20	0.18	0.04**	
Blue Cross	0.66%	0.12	0.38	0.00**	0.97	0.00**
Infant Readmission						
Pacificare	9.64%					
Aetna	7.14%	0.02**				
Health Net	7.34%	0.01**	0.84			
Cigna	7.50%	0.04**	0.75	0.86		
Blue Shield	9.02%	0.49	0.08	0.04**	0.13	
Blue Cross	7.47%	0.00**	0.72	0.85	0.97	0.03**
Infant Discharge "not home"						
Pacificare	3.69%					
Aetna	3.36%	0.65				
Health Net	4.83%	0.07	0.06			
Cigna	2.67%	0.12	0.34	0.00**		
Blue Shield	4.80%	0.07	0.07	0.96	0.00**	
Blue Cross	2.96%	0.13	0.51	0.00**	0.59	0.00**

Notes: Results of χ^2 tests of null hypothesis "no difference in outcomes" for each pair of insurers. Only the most populated severity is included: mothers aged 20-39 with a normal pregnancy and no co-morbidities of rank above 1. Each panel reports tests for a different outcome measure; see notes to Table 3 for definitions. Column 1 reports percent of patients in the insurer with this severity who experienced the adverse outcome. Columns 2-7 report p-value for the test that the two insurers have different outcomes; a value < 0.05 indicates a significant difference.

Appendix 1: Analysis of Variance

This appendix defines the test statistics for the Analysis of Variance referenced in Section 7.1.

Continuous Variables

For continuous variables (e.g. price) we define x_{ish} for individual i, severity s and hospital h. We wish to test the null hypothesis that the price groups used in the inequalities analysis do not explain variation in x better than our severity groups. The within hospital severity variation is

$$S = \sum_{i,s,h} (x_{i,s,h} - \bar{x}_{s,h})^2$$

where $\bar{x}_{s,h}$ is the average of x within a severity group and hospital across individuals i. Given two severity measures (our severity and price groups; denote them $s1$ and $s2$), we calculate S_{s1} and S_{s2}. Using severity groups $s1$ as the reference severity definition and price groups $s2$ as the refinement, the F-statistic is calculated as

$$F = \frac{(S_{s1} - S_{s2})/(df_{s1} - df_{s2})}{S_{s2}/df_{s2}}.$$

The degrees of freedom are

$$\begin{aligned} df_{s1} &= N_{total} - C_{s1,h} \\ df_{s2} &= N_{total} - C_{s2,h} \end{aligned}$$

where N_{total} is the number of individuals in the sample and $C_{s,h}$ is the number of severity-hospital combinations with the relevant severity definition. When we consider the list price, or the list price interacted with the discount, we reject the null hypothesis in a test of size 0.001.

Binary Variables

For binary variables x such as discharge other than home and readmission within 12 months, define $\bar{x}_{s1,h}$ and $\bar{x}_{s2,h}$ to be the average of x for a given severity-hospital combination with the relevant severity definition. Define the χ^2-statistic as

$$\chi^2 = \sum_{s,h} \frac{(\bar{x}_{s2,h} - \bar{x}_{s1,h})^2 N_{s2,h}^2}{\bar{x}_{s1,h} N_{s1,h}}.$$

Here $N_{s2,h}$ and $N_{s1,h}$ are the number of individuals in the severity-hospital combination. Under the null hypothesis this statistic has a χ^2 distribution with $df_{s1} - df_{s2}$ degrees of freedom. When we consider readmission within 12 months or discharge to a location other than home, we cannot reject the null hypothesis that price groups do not explain variation in x better than severity groups in a test of size 0.001.

Appendix 2: Estimation of the Discount Variation Across Insurers

This appendix provides details of the method discussed in Section 7.3 that was used to estimate the variation in discounts across insurers. We begin with the average negotiated discount at the hospital level, d_h.[70] This is a weighted average of the discounts for both inpatient and outpatient services to both Knox Keene and Point of Service (POS) insurers. We assume for the moment that the discount at the hospital-insurer level, $d_{\pi,h}$, does not differ across diagnoses for a given (π, h) pair; we relax this assumption in the section on robustness tests. We use data from the OSHPD hospital discharge and financial records for 2003 that are not used in the main analysis. First, we have discharge data covering all Knox Keene inpatient events in the year 2003, including diagnoses other than labor and births. We observe a list price for every discharge. Second, the hospital financial reports include data on hospital h's total charges (sum of list prices) for managed care (Knox Keene and POS) inpatient services and separately for managed care outpatient services.

If $s_{\pi,h}$ ($s^o_{\pi,h}$) is the share of Knox Keene π's inpatient (outpatient plus POS inpatient) charges in hospital h we know that:

$$d_h = \sum_\pi s_{\pi,h} d_{\pi,h} + \sum_\pi s^o_{\pi,h} d^o_{\pi,h} \tag{15}$$

where $\sum_\pi (s_{\pi,h} + s^o_{\pi,h}) = 1$. We are constrained by lack of data on $s^o_{\pi,h}$. We therefore assume that $d^o_{\pi,h} = d_{\pi,h}$. We can always write $s^o_{\pi,h} = s_h s_{\pi,h} + e_{\pi,h}$ where $s_h \equiv \sum_\pi s^o_{\pi,h} / \sum_\pi s_{\pi,h}$, and can be calculated from the observed data, and $\sum_\pi e_{\pi,h} = 0$. Substituting we have:

$$d_h = \sum_\pi (1 + s_h) s_{\pi,h} d_{\pi,h} + \tilde{e}_h \tag{16}$$

where $\tilde{e}_h = \sum_\pi e_{\pi,h} d_{\pi,h}$.

To proceed we need a specification for HMO inpatient discounts at different hospitals. We begin by writing

$$d_{\pi,h} = d_0 + \tilde{d}_h + \tilde{d}_{\pi,h}$$

where $\forall h,\ \sum_\pi \tilde{d}_{\pi,h} = 0$, so that $d_0 + d_h$ is the mean hospital discount, and $\sum_h \tilde{d}_h = 0$ so that d_0 is the mean of the (mean) hospital discount (across hospitals). Our reduced form model for the mean hospital discount is

$$\tilde{d}_h = \left(\frac{\exp(X_{h,m}\beta^h)}{1 + \exp(X_{h,m}\beta^h)} - d_0 \right) + v_h \equiv \Big(f(X_{h,m}, \beta^h) - d_0 \Big) + v_h \tag{17}$$

where $X_{h,m}$ are hospital characteristics or their interactions with market characteristics and v_h is mean independent of $X_{h,m}$. The reduced form model for an insurer's deviation from the mean

[70] We conduct this analysis using the discount d_h rather than one minus the discount, which is defined above as $\delta^o_h = 1 - d_h$.

discount is

$$\tilde{d}_{\pi,h} = \frac{\exp(X_{\pi,h,m}\beta^{\pi}) - \frac{1}{N_{\pi,h}}\sum_{\pi}\exp(X_{\pi,h,m}\beta^{\pi})}{\frac{1}{N_{\pi,h}}\sum_{\pi}\exp(X_{\pi,h,m}\beta^{\pi})} + v_{\pi,h} \equiv f(X_{\pi,h,m}\beta^{\pi}) + v_{\pi,h} \tag{18}$$

where $X_{\pi,h,m}$ are insurer characteristics and their interactions with market and hospital characteristics and $N_{\pi,h}$ is the number of insurers contracting with hospital h and where $v_{\pi,h}$ is mean independent of $X_{\pi,h,m}$ and $\forall h$, $\sum_{\pi} v_{\pi,h} = 0$ (since $\sum_{\pi} \tilde{d}_{\pi,h} = 0$).

Substituting these specifications into equation (16) generates the following equation which can be estimated using nonlinear least squares:

$$d_h = f(X_{h,m}, \beta^h) + \sum_{\pi} (1 + s_h)\, s_{\pi,h} f(X_{\pi,h,m}\beta^{\pi}) + e_h \tag{19}$$

where $e_h = \sum_{\pi} (1 + s_h)\, s_{\pi,h} v_{\pi,h} + v_h + \tilde{e}_h$.

The estimates, set out in Tables 1 and 2 of this Appendix, are intuitive. Table 1 sets out the results when $X_{h,m}$ includes both hospital characteristics and market fixed effects. Model 1 includes insurer fixed effects; in Model 2 we collapse these into a fixed effect for high-capitation insurers (Pacificare together with Aetna, Health Net and Cigna), a fixed effect for Blue Cross and a continuous variable defined as the insurer's share of HMO enrollment in California.[71] In both cases we find that for profit hospitals and hospitals that are members of systems (groups of providers that bargain jointly with insurers) have significantly higher discounts than other hospitals. At first sight this is surprising since a higher discount implies a lower price paid to the hospital. However, this is likely to be explained by the substantial variation in list prices across hospitals. We show in Table 6 of Ho and Pakes (2011) that for profit hospitals have higher prices net of discounts than not-for-profit hospitals. If we add an indicator for hospitals in systems to the regression we find that system hospitals, too, have significantly higher prices than other hospitals.[72] These results indicate that, while discounts are high for system and for profit hospitals, list prices are higher, so that the net price paid conditional on severity is also relatively high for these providers.

Other hospital characteristics such as indicators for teaching hospitals and hospitals that provide transplants (a measure of high-tech hospitals) are not significant in our analyses. The coefficient on a variable measuring the hospital's share of beds in the market, a potential measure of hospital bargaining power, is negative as expected but not significant at p=0.05. The insurer fixed effects in Model 1 are all statistically insignificant and the magnitudes demonstrate no particular correlation between insurer capitation levels and discounts. In Model 2 the coefficient for high-capitation insurers is slightly negative, and that for Blue Cross is somewhat more negative compared to the

[71] We use the share of enrollment at the state level rather than the market level to help avoid endogeneity problems due to insurers with high discounts in a particular market attracting high enrollment in that market.

[72] The analysis controls for patient severity by using as a price measure the price ratio $p_i^{ratio} = \frac{p_i}{\bar{p}_{s_i}}$ where p_i is the price (list price multiplied by δ_h) for patient i and $\bar{p}_{s_i}$ is the average price for same-severity patients across all hospitals in the sample. The results of these regressions are excluded from this paper to conserve space. They are available from the authors on request.

excluded plan (Blue Shield) although neither coefficient is significant at p=0.05. The coefficient on HMO market share is positive (although again insignificant), consistent with a bargaining power story. We use the results in Model 2 to calculate the predicted $\hat{\delta}_{\pi,h}$ that are used in the inequalities analysis since they provide a somewhat smoother prediction of the variation in discounts across insurers than the results in Model 1. The hypothesis that Model 2 fits the data as well as Model 1 cannot be rejected in an F-test of size 0.05.[73]

In Table 2 we replace the market fixed effects with market characteristics. We view this as an exploratory exercise to check that our results are consistent with the previous literature on the impact of hospital and insurer concentration on prices. Our results are similar to those in previous papers: we find that variables likely to be positively correlated with hospital bargaining power are negatively related to hospital discounts, while those positively related to insurer bargaining power are positively correlated with discounts. For example, in Model 3 we find that when market fixed effects are removed the positive coefficient on the insurer market share variable and the negative coefficient on hospital market share both become significant at p=0.05. Models 4-5 demonstrate that discounts are significantly higher in markets with more hospitals per thousand population and lower in markets with more insurers per 1000 population.

The final step is to use these estimates to generate a prediction for $d_{\pi,h}$. There are two possibilities. First, since:

$$d_{\pi,h} \approx f(X_{h,m}, \hat{\beta}^{h}) + f(X_{\pi,h,m}\hat{\beta}^{\pi}) + (v_{\pi,h} + v_h)$$

we define

$$\hat{d}^{1}_{\pi,h} = f(X_{h,m}, \hat{\beta}^{h}) + f(X_{\pi,h,m}\hat{\beta}^{\pi}) \tag{20}$$

and incur the error $e^{1}_{\pi,h} = v_{\pi,h} + v_h$. Second, since

$$d_{\pi,h} \approx d_h - \sum_{\pi} (1 + s_h)\, s_{\pi,h} f(X_{\pi,h,m}\hat{\beta}^{\pi}) + f(X_{\pi,h,m}\hat{\beta}^{\pi}) + \left(v_{\pi,h} - \tilde{e}_h - \sum_{\pi} (1 + s_h)\, s_{\pi,h} v_{\pi,h} \right)$$

we define

$$\hat{d}^{2}_{\pi,h} = d_h - \sum_{\pi} (1 + s_h)\, s_{\pi,h} f(X_{\pi,h,m}\hat{\beta}^{\pi}) + f(X_{\pi,h,m}\hat{\beta}^{\pi}) \tag{21}$$

and incur the error $e^{2}_{\pi,h} = v_{\pi,h} - \tilde{e}_h - \sum_{\pi} (1 + s_h)\, s_{\pi,h} v_{\pi,h}$. We use the predictions to define price measures $p^1(.) = (1 - \hat{d}^{1}_{\pi,h}) lp^{o}(c_i, h)$ and $p^2(.) = (1 - \hat{d}^{2}_{\pi,h}) lp^{o}(c_i, h)$ and use these in the inequalities analysis. The errors $(1 - e^{1}_{\pi,h}) lp^{o}(c_i, h)$ and $(1 - e^{2}_{\pi,h}) lp^{o}(c_i, h)$, together with estimation error from this step and measurement error from the expected list price calculation, will be inputs into the error term $\varepsilon_{i_h,\pi,h}$ defined in Section 7.2.

While use of $p^1(.)$ and/or $p^2(.)$ as our price variable mitigates the problems that could arise from using a price variable that does not account for insurer-specific discounts, it probably does not eliminate them. To the extent that doctors know $\nu_{\pi,h}$ and select hospitals based on its value

[73] We also estimated the inequalities analysis using the discounts predicted by Model 1; the results were very similar to the main analyses reported below.

there will still be a selection bias in both of these price variables[74], and if doctors know ν_h and select based on its value there will be an additional source of selection bias in $p^1(\cdot)$[75].

[74] Only the component of $(1 - e_{\pi,h})lp^o(c_i, h)$ that differs across c_i groups within a hospital-severity pair will be absorbed into the error term rather than into $g_\pi(.)$. However, the interaction with the list price implies that there will be some such variation and if decision-makers observe it this will cause endogeneity bias. We assume that $\tilde{e}_h$ is unrelated to discounts and therefore not problematic here.

[75] We did investigate the magnitude of the errors through a regression analysis. Note from equation (19) that

$$H^{-1} \sum_h e_h^2 \rightarrow_P \sigma_{\tilde{e}}^2 + \sigma_h^2 + \sum_\pi (1 + s_h)^2 s_{\pi,h}^2 \sigma_{\pi,h}^2$$

where $\sigma_{\tilde{e}}^2$ is the variance of $\tilde{e}_h$ and similarly for σ_h^2 and $\sigma_{\pi,h}^2$. We regress e_h^2 on a constant term and $\sum_\pi (1 + s_h)^2 s_{\pi,h}^2$ and estimate a constant term of 0.0037 (standard error 0.0034) and an estimate of the coefficient on the X variable of 0.0286 (standard error 0.0107). We compare these numbers to the variance in d_h, a lower bound on the unobserved variance in $d_{\pi,h}$, which is 0.022. We conclude that the variance in $v_{\pi,h}$ is likely larger in magnitude that that of v_h.

Appendix 2, Table 1: NLLS Analysis of Discount Variation

	percent	Model 1		Model 2	
	capitated	Coefft	(S.E.)	Coefft	(S.E.)
Hospital Characteristics					
Constant		-0.07	(0.30)	-0.14	(0.29)
Teaching hospital		-0.03	(0.11)	-0.06	(0.11)
Cost per admission		-0.01	(0.01)	-0.01	(0.01)
For profit		0.44**	(0.12)	0.43**	(0.12)
Offers transplants		-0.05	(0.17)	-0.03	(0.17)
System hospital		0.26**	(0.11)	0.26**	(0.12)
Share of beds in mkt		-12.32	(7.83)	-11.46	(8.16)
Insurer Characteristics					
Pcare/Aetna/HN/Cigna				-0.11	(0.07)
Pacificare	0.97	-0.04	(0.13)		
Aetna	0.91	0.09	(0.20)		
Health Net	0.80	0.12	(0.15)		
Cigna	0.75	-0.42	(0.23)		
Blue Shield	0.57	0.11	(0.15)		
Blue Cross	0.38	0.00	(0.12)	-0.36	(0.22)
Share in CA				1.77	(1.32)
Market FEs?		Yes		Yes	
pseudo-R^2		0.46		0.45	
Number hospitals		144		144	

Notes: NLLS analysis of variation in hospital discounts d_h across hospitals, insurers and markets. Equation for estimation is $d_h = f(X_{h,m}, \beta^h) + \sum_\pi (1 + s_h)\, s_{\pi,h} f(X_{\pi,h,m}\beta^\pi) + e_h$ where $f(X_{h,m}, \beta^h) = \frac{\exp(X_{h,m}\beta^h)}{1+\exp(X_{h,m}\beta^h)}$ and $f(X_{\pi,h,m}\beta^\pi) = \frac{\exp(X_{\pi,h,m}\beta^\pi) - \frac{1}{N_{\pi,h}}\sum_\pi \exp(X_{\pi,h,m}\beta^\pi)}{\frac{1}{N_{\pi,h}}\sum_\pi \exp(X_{\pi,h,m}\beta^\pi)}$. "Cost per admission" is average hospital cost per admission in \$000. "Share in CA" is insurer's share of HMO enrollment in California. pseudo-R^2 is 1 - (SSR from full model / SSR from model including only a constant). ** = significant at p=0.05; *=significant at p=0.10.

Appendix 2, Table 2: NLLS Analysis of Discount Variation: Market Characteristics

		Model 3		Model 4		Model 5	
		Coefft	(S.E.)	Coefft	(S.E.)	Coefft	(S.E.)
Hospital Charas:	Constant	0.54**	(0.20)	0.13	(0.30)	-0.26	(0.32)
	Teaching hospital	0.05	(0.09)	0.08	(0.09)	0.01	(0.10)
	Cost per admission	-0.03**	(0.01)	-0.02**	(0.01)	-0.02**	(0.01)
	For profit	0.50**	(0.12)	0.53**	(0.12)	0.52**	(0.11)
	Offers transplants	0.03	(0.15)	0.03	(0.15)	-0.01	(0.15)
	System hospital	0.20**	(0.12)	0.20**	(0.12)	0.21**	(0.12)
	Share of beds in mkt	-10.17**	(4.55)	-13.87**	(4.69)	-7.56	(6.22)
Market Charas:	Hosps per 1000 pop			69.06**	(39.60)	172.39**	(53.61)
	Plans per 1000 popln					-81.83**	(36.32)
Insurer Charas:	Pcare/Aetna/HN/Cigna	-0.11	(0.07)	-0.07	(0.07)	-0.06	(0.07)
	Blue Cross	-0.55**	(0.22)	-0.48**	(0.24)	-0.45	(0.24)
	Share in CA	3.48**	(1.45)	3.14**	(1.51)	2.92**	(1.55)
pseudo-R^2		0.33		0.34		0.36	
Number hospitals		144		144		144	

Notes: NLLS analysis of variation in hospital discounts d_h across hospitals, insurers and markets. See notes to Table 5 for details. ** = significant at p=0.05; *=significant at p=0.10.

Appendix 3: Categorization of Co-Morbidities by Severity

We asked obstetrical experts at Columbia Presbyterian Hospital to assign a rank to each co-morbidity listed in our discharge data covering privately insured patients admitted for a labor/birth episode in California in 2003. Ranks were numbered from 1 to 3, where 1 indicated a routine diagnosis that would not affect patient treatment in any significant way, 2 indicated a more severe diagnosis and 3 indicated the most severe conditions that would have a substantial effect on the patient's treatment during the labor/birth admission. The list of diagnoses and their assigned ranks is given below. The number of patients with each co-morbidity is also provided. (A single patient may have more than one co-morbidity.)

Diagnosis	# patients	% patients	Rank (1-3)
1. Tuberculosis	9	0	3
2. Septicemia (except in labor)	42	0.02	2
3. Bacterial infection; unspecified sit	668	0.32	2
4. Mycoses	28	0.01	2
6. Hepatitis	119	0.06	2
7. Viral infection	643	0.3	2
8. Other infections; including parasiti	70	0.03	2
9. Sexually transmitted infections (not	19	0.01	2
10. Immunizations and screening for inf	12,523	5.93	1
22. Melanomas of skin	10	0	3
23. Other non-epithelial cancer of skin	6	0	3
24. Cancer of breast	18	0.01	3
26. Cancer of cervix	14	0.01	3
28. Cancer of other female genital orga	2	0	3
32. Cancer of bladder	1	0	3
33. Cancer of kidney and renal pelvis	2	0	3
35. Cancer of brain and nervous system	5	0	3
36. Cancer of thyroid	24	0.01	3
37. Hodgkins disease	8	0	3
38. Non-Hodgkins lymphoma	5	0	3
39. Leukemias	3	0	3
41. Cancer; other and unspecified prima	4	0	3
44. Neoplasms of unspecified nature or	14	0.01	3
46. Benign neoplasm of uterus	1,110	0.53	1
47. Other and unspecified benign neopla	275	0.13	1
48. Thyroid disorders	1,266	0.6	2
49. Diabetes mellitus without complicat	9	0	2
50. Diabetes mellitus with complication	35	0.02	3
51. Other endocrine disorders	81	0.04	2
52. Nutritional deficiencies	22	0.01	1
53. Disorders of lipid metabolism	11	0.01	2
55. Fluid and electrolyte disorders	554	0.26	2
56. Cystic fibrosis	1	0	3
57. Immunity disorders	8	0	2
58. Other nutritional; endocrine; and m	703	0.33	2
59. Deficiency and other anemia	1,542	0.73	1
60. Acute posthemorrhagic anemia	215	0.1	2
61. Sickle cell anemia	59	0.03	3
62. Coagulation and hemorrhagic disorde	338	0.16	2
63. Diseases of white blood cells	37	0.02	2
64. Other hematologic conditions	9	0	2
76. Meningitis (except that caused by t	9	0	3
77. Encephalitis (except that caused by	1	0	3

Diagnosis	# patients	% patients	Rank (1-3)
78. Other CNS infection and poliomyelit	3	0	3
79. Parkinsons disease	2	0	3
80. Multiple sclerosis	28	0.01	3
81. Other hereditary and degenerative n	10	0	3
82. Paralysis	8	0	3
83. Epilepsy; convulsions	146	0.07	3
84. Headache; including migraine	174	0.08	1
85. Coma; stupor; and brain damage	6	0	3
87. Retinal detachments; defects; vascu	5	0	2
88. Glaucoma	3	0	2
89. Blindness and vision defects	17	0.01	2
90. Inflammation; infection of eye (exc	10	0	1
91. Other eye disorders	4	0	1
92. Otitis media and related conditions	16	0.01	1
93. Conditions associated with dizzines	27	0.01	1
94. Other ear and sense organ disorders	21	0.01	1
95. Other nervous system disorders	103	0.05	2
96. Heart valve disorders	540	0.26	3
97. Peri-; endo-; and myocarditis; card	19	0.01	3
98. Essential hypertension	581	0.27	2
99. Hypertension with complications and	18	0.01	3
101. Coronary atherosclerosis and other	1	0	3
102. Nonspecific chest pain	21	0.01	2
103. Pulmonary heart disease	7	0	3
104. Other and ill-defined heart diseas	12	0.01	3
105. Conduction disorders	28	0.01	3
106. Cardiac dysrhythmias	193	0.09	3
107. Cardiac arrest and ventricular fib	2	0	3
108. Congestive heart failure; nonhyper	1	0	3
114. Peripheral and visceral atheroscle	3	0	3
117. Other circulatory disease	187	0.09	2
118. Phlebitis; thrombophlebitis and th	74	0.04	2
119. Varicose veins of lower extremity	4	0	1
120. Hemorrhoids	186	0.09	1
121. ther diseases of veins and lymphat	18	0.01	2
122. Pneumonia (except that caused by t	66	0.03	2
123. Influenza	21	0.01	1
125. Acute bronchitis	13	0.01	1
126. Other upper respiratory infections	190	0.09	1
129. Aspiration pneumonitis; food/vomit	6	0	2
130. Pleurisy; pneumothorax; pulmonary	42	0.02	3
131. Respiratory failure; insufficiency	12	0.01	3
133. Other lower respiratory disease	79	0.04	2
134. Other upper respiratory disease	19	0.01	2
135. Intestinal infection	37	0.02	1
136. Disorders of teeth and jaw	5	0	1
138. Esophageal disorders	101	0.05	2
139. Gastroduodenal ulcer (except hemor	1	0	2
140. Gastritis and duodenitis	24	0.01	1
141. Other disorders of stomach and duo	13	0.01	1
142. Appendicitis and other appendiceal	67	0.03	2
143. Abdominal hernia	94	0.04	1

Diagnosis	# patients	% patients	Rank (1-3)
144. Regional enteritis and ulcerative	55	0.03	2
145. Intestinal obstruction without her	41	0.02	2
146. Diverticulosis and diverticulitis	2	0	2
147. Anal and rectal conditions	16	0.01	1
148. Peritonitis and intestinal abscess	8	0	3
149. Biliary tract disease	401	0.19	2
151. Other liver diseases	84	0.04	2
152. Pancreatic disorders (not diabetes	41	0.02	2
153. Gastrointestinal hemorrhage	12	0.01	3
154. Noninfectious gastroenteritis	61	0.03	1
155. Other gastrointestinal disorders	390	0.18	2
156. Nephritis; nephrosis; renal sclero	11	0.01	2
157. Acute and unspecified renal failur	8	0	3
158. Chronic renal failure	2	0	3
159. Urinary tract infections	838	0.4	1
160. Calculus of urinary tract	216	0.1	1
161. Other diseases of kidney and urete	191	0.09	2
162. Other diseases of bladder and uret	15	0.01	2
163. Genitourinary symptoms and ill-def	97	0.05	1
167. Nonmalignant breast conditions	14	0.01	1
168. Inflammatory diseases of female pe	837	0.4	1
169. Endometriosis	94	0.04	1
170. Prolapse of female genital organs	3	0	1
171. Menstrual disorders	5	0	1
172. Ovarian cyst	297	0.14	1
173. Menopausal disorders	3	0	1
174. Female infertility	6	0	1
175. Other female genital disorders	448	0.21	1
176. Contraceptive and procreative mana	5,442	2.58	1
177. Spontaneous abortion	20	0.01	1
178. Induced abortion	9	0	1
179. Postabortion complications	98	0.05	2
180. Ectopic pregnancy	11	0.01	2
181. Other complications of pregnancy	16,871	7.99	2
182. Hemorrhage during pregnancy; abrup	755	0.36	3
183. Hypertension complicating pregnanc	2,388	1.13	2
184. Early or threatened labor	3,223	1.53	2
185. Prolonged pregnancy	5,103	2.42	1
186. Diabetes or abnormal glucose toler	3,501	1.66	2
187. Malposition; malpresentation	3,375	1.6	1
188. Fetopelvic disproportion; obstruct	3,061	1.45	2
189. Previous C-section	2,592	1.23	1
190. Fetal distress and abnormal forces	2,586	1.22	1
191. Polyhydramnios and other problems	5,086	2.41	2
192. Umbilical cord complication	10,393	4.92	1
193. OB-related trauma to perineum and	3,157	1.49	1
194. Forceps delivery	273	0.13	1
195. Other complications of birth; puer	26,576	12.58	1
196. Normal pregnancy and/or delivery	83,408	39.48	1
197. Skin and subcutaneous tissue infec	66	0.03	1
198. Other inflammatory condition of sk	92	0.04	1
200. Other skin disorders	182	0.09	1

Diagnosis	# patients	% patients	Rank (1-3)
201. Infective arthritis and osteomyeli	2	0	2
202. Rheumatoid arthritis and related d	5	0	2
203. Osteoarthritis	2	0	1
204. Other non-traumatic joint disorder	23	0.01	1
205. Spondylosis; intervertebral disc d	212	0.1	1
206. Osteoporosis	3	0	2
208. Acquired foot deformities	3	0	1
209. Other acquired deformities	6	0	1
210. Systemic lupus erythematosus and c	7	0	2
211. Other connective tissue disease	93	0.04	2
212. Other bone disease and musculoskel	35	0.02	2
213. Cardiac and circulatory congenital	42	0.02	2
214. Digestive congenital anomalies	2	0	2
215. Genitourinary congenital anomalies	240	0.11	2
216. Nervous system congenital anomalie	5	0	2
217. Other congenital anomalies	47	0.02	2
218. Liveborn	1	0	1
219. Short gestation; low birth weight;	2	0	2
224. Other perinatal conditions	6	0	2
225. Joint disorders and dislocations;	5	0	2
226. Fracture of neck of femur (hip)	2	0	2
228. Skull and face fractures	3	0	2
229. Fracture of upper limb	9	0	2
230. Fracture of lower limb	8	0	2
231. Other fractures	15	0.01	2
232. Sprains and strains	21	0.01	1
233. Intracranial injury	6	0	3
234. Crushing injury or internal injury	6	0	3
235. Open wounds of head; neck; and tru	5	0	2
236. Open wounds of extremities	3	0	2
237. Complication of device; implant or	21	0.01	2
238. Complications of surgical procedur	138	0.07	2
239. Superficial injury; contusion	55	0.03	1
240. Burns	2	0	2
242. Poisoning by other medications and	5	0	2
244. Other injuries and conditions due	45	0.02	2
245. Syncope	27	0.01	2
246. Fever of unknown origin	58	0.03	2
247. Lymphadenitis	5	0	2
249. Shock	3	0	3
250. Nausea and vomiting	32	0.02	1
251. Abdominal pain	185	0.09	1
252. Malaise and fatigue	15	0.01	1
253. Allergic reactions	194	0.09	2
255. Administrative/social admission	13	0.01	1
256. Medical examination/evaluation	1	0	1
257. Other aftercare	37	0.02	1
259. Residual codes; unclassified	1,537	0.73	1
650. Adjustment disorders	11	0.01	1
651. Anxiety disorders	129	0.06	1
652. Attention-deficit, conduct, and di	3	0	1
654. Developmental disorders	2	0	1

Diagnosis	# patients	% patients	Rank (1-3)
655. Disorders usually diagnosed in inf	1	0	1
657. Mood disorders	397	0.19	2
658. Personality disorders	5	0	2
659. Schizophrenia and other psychotic	8	0	2
660. Alcohol-related disorders	13	0.01	2
661. Substance-related disorders	164	0.08	2
663. Screening and history of mental he	410	0.19	1
670. Miscellaneous disorders	684	0.32	2