Are Hospital Acquisitions of Physician Practices Anticompetitive?

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Abstract

This paper empirically analyzes the effects of mergers between complementary firms on competition and pricing. As these non-horizontal mergers have become more common, there is increasing interest in evaluating both potential efficiencies such as eliminating double marginalization and potential anticompetitive effects such as foreclosure and recapture. The mergers we study – hospital acquisitions of physician practices – have reshaped the $1 trillion US physician industry, nearly doubling the share of physicians working for hospitals between 2008 and 2016. We combine novel data and machine learning algorithms to identify a large number of integration events, spanning a wide range of markets with different competitive circumstances. We merge the integration events with claims data from a large national insurer to study their effects on prices. Focusing on childbirths, the most ubiquitous admission among the privately insured, we find that, on average, these mergers led to price increases for hospitals and physicians of 3.3% and 15.1%, respectively, with no discernible effects on quality measures. Using demand estimation to characterize substitution patterns for both physicians and hospitals, we construct tests that demonstrate price increases are larger among transactions with greater scope for foreclosure and recapture. Our estimates suggest that the costs of these mergers of hospitals and physicians have been substantial, and our mechanism tests offer guidance in predicting where the anticompetitive effects of non-horizontal mergers are likely to be strongest.

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1 Introduction

Rising markups, increasingly concentrated markets, and a slew of consummated mergers that are widely perceived to be anticompetitive have spurred a renewed interest in antitrust enforcement in the US.[1] Because of the direct harm to competition, academics and antitrust enforcement agencies have primarily focused their attention on "horizontal" mergers—those between firms competing in the same product market. However, there is now increasing interest in non-horizontal mergers, such as vertical transactions and mergers of complements (Economides et al., 2020; Gaynor, 2020; Salop, 2018). The theoretical and empirical literature on non-horizontal mergers is relatively complicated, nuanced, and still evolving. The theoretical literature illustrates the scope for such mergers to be both efficiency enhancing (Cournot 1838; Linnemer 2022; Williamson 1971, 1979, 1985) and anticompetitive (Hart and Tirole 1990; Rey and Tirole 2007; Riordan 2008). The empirical literature has demonstrated, in sectors ranging from carbonated beverages to gasoline to regional sports networks, that non-horizontal mergers can raise integrating firms' prices, eliminate double marginalization, and disadvantage rivals (Crawford et al., 2018; Hastings and Gilbert, 2005; Luco and Marshall, 2020). Moreover, while there are well established methods for prospectively and retrospectively analyzing horizontal transactions, the tools for assessing non-horizontal transactions are comparatively nascent. Collectively, this has made antitrust enforcement against non-horizontal deals extremely challenging for regulators.

The growth in non-horizontal mergers has been pronounced in the US health care industry. Over the last two decades, hospital systems and health insurers have acquired many types of providers, including physician practices, nursing home facilities, and home health agencies. There is a growing consensus in the literature that integration between hospitals and physicians is likely to lead to higher spending (Handel and Ho 2021; see Section 2.2 below). However, there has been virtually no antitrust enforcement against non-horizontal mergers in the health sector at either the federal or state level. Beyond the complexity of predicting and measuring the effects of non-horizontal transactions, the lack of enforcement in health care is likely a function of the fact that many of these transactions have deal values below Hart–Scott–Rodino (HSR) merger reporting thresholds and, therefore, can be challenging for regulators

[1] See, e.g., De Loecker et al. (2020), Philippon (2019), and Wollmann (2020).

to observe (Capps et al., 2017; Wollmann, 2020).[2]

In this paper, we retrospectively examine the competitive effects of a large sample of mergers between hospitals and physicians. These types of transactions—ones in which a hospital system acquires a physician practice—have occurred in large numbers and have transformed the organizational structure of health care delivery throughout the United States (Dranove and Ody, 2019; Handel and Ho, 2021; Nikpay et al., 2018). Our goal is to analyze whether mergers of hospitals and physician practices lead to price increases and whether any observed price increases are a function of a lessening of competition. To do that, we: (1) introduce novel data to carefully characterize the evolution of hospital-physician integration in the United States; (2) describe several theories of how a merger between a hospital and physician practice could affect prices, quantities, and quality via changes in competition; (3) combine our integration measures with detailed claims data covering the privately insured to estimate the impact of mergers on prices (which, we find, increase on average); and (4) present new empirical evidence on the mechanisms through which integration leads to price increases. The economic and social importance of hospitals and physicians—two sub-sectors that collectively account for 52.4% of total health spending and 9.4% of GDP (Hartman et al., 2018)—makes the impact of these acquisitions an important policy issue in its own right. In addition, our analysis of the mechanisms underlying the effects of these non-horizontal mergers may inform analysis of transactions outside the health sector.

Holding all else equal, mergers between complementary suppliers like physicians and hospitals should decrease prices by eliminating double marginalization (Cournot, 1838). Likewise, any merger-induced transaction cost reductions would reinforce such a price effect (Grossman and Hart, 1986; Williamson, 1985). However, there are also several competing theories about how non-horizontal mergers could lessen competition and cause prices to increase. We focus on two mechanisms: foreclosure, in which physicians concentrate their patients and services with the acquiring hospital system (Hart and Tirole, 1990; Ordover et al., 1990; Salinger, 1988; Salop and Scheffman, 1987); and recapture, in which integration with another highly-valued provider followed by an all-or-nothing negotiation with the insurer leads to an im-

[2] The Hart–Scott–Rodino Antitrust Improvements Act of 1976 (Section 7A of the Clayton Act) requires firms to provide pre-merger notifications to antitrust authorities regarding acquisitions exceeding a threshold dollar value. In 2021, that threshold was $92 million (Federal Trade Commission, 2021).

proved outside option for the focal provider (Peters, 2014). We provide more details on these mechanisms in Section 2.2. There is also a third mechanism that we study, via which these transactions could cause prices to increase via a lessening of competition. In some cases, hospital acquisitions of multiple physician practices increase concentration among physicians in the same geographic market (e.g., two physicians now become part of the same practice via a series of non-horizontal transactions, akin to the "roll-ups" studied by Asil et al. (2024)).

Mergers of hospitals and physicians could also lead to higher prices via pro-competitive or competitively neutral forces. First, these merged entities could deliver higher quality care, which could increase demand and therefore the prices of their services. To the extent that integration increases quality, the value created by an integrated provider would be greater and standard models of bargaining would predict higher prices (Grennan and Swanson, 2022). Second, integration could improve the bargaining ability—the actual bargaining skills—of the merging parties. Bargaining ability is typically modeled as distinct from competitive forces because it captures the share of surplus captured by a given negotiating party conditional on parties' next best alternatives.

These competing theoretical mechanisms create an important role for empirical work. Unfortunately, the empirical literature on integration between hospitals and physicians to date has faced challenges with respect to both data and identification. First, integration between physicians and hospitals is not systematically reported in any database. Second, data on market-determined prices are hard to obtain. Given these data constraints, most studies have focused on limited subsets of states or time horizons, or used proxy measures of integration and provider prices. Moreover, these mergers are not randomly allocated, so their effects may be confounded by other factors that influence quantities and prices of health care services.

To date, the empirical literature on hospitals' physician acquisitions suggests that these transactions can raise provider prices and health spending (Baker et al., 2014; Dranove and Ody, 2019; Ho et al., 2019; Koch et al., 2017; Lin et al., 2021b; Neprash et al., 2015). However, it has been difficult to tell the mechanisms at play and whether varying results on the effects of physician–hospital integration are driven by measurement error, different research designs, or distinctions in the economic fundamentals across geographic regions and time horizons.

In what follows, we bring together a combination of new data and empirical tests to address these challenges. We use rich provider- and market-level data with machine learning models to develop and validate a novel, high-quality measure of integration between hospitals and physicians. Our measure shows that virtually every region in the US experienced an increase in physician–hospital integration and that the share of physicians integrated with a hospital increased 71.5 percent from 27.5% to 47.2% from 2008 to 2016. During our sample period, we estimate that the valuations for 99.9% of observed physician–hospital transactions nationwide and across medical specialties were below HSR reporting thresholds under standard valuation multiples. We combine our integration data with administrative claims data from a large US commercial health insurer and focus on labor and deliveries, which account for the plurality of inpatient health spending on the privately insured.[3]

Our empirical strategy uses event studies to compare trends in outcomes for treated providers (those who merged) versus non-merging "control" providers in the years before and after integration events. One novel element of our approach is to use a discrete choice model of provider demand to identify suitable controls: non-merging providers that are not close substitutes with any merging providers. A physician integration event is an acquisition of the physician's practice by a hospital or health system; a hospital integration event is a large one-year increase in the share of physicians practicing at the hospital who are integrated with the hospital. The sample includes 276 physician integration events and 66 hospital integration events. Our ability to precisely examine a large number of mergers in a variety of economic environments allows us to explore heterogeneity in the antecedents, mechanisms, and effects of physician–hospital mergers in a way that is rare in industrial organization studies of market power and antitrust enforcement.

We demonstrate that, on average, mergers of hospitals and obstetricians and gynecologists (OB-GYNs) increase both hospital and physician prices. Two years after an integration event, hospital prices for labor and delivery increase by 3.3% ($475), and physician prices increase by 15.1% ($502). Our results are robust to a wide

[3] A limitation is that our dataset does not include all insurers. Given that all-payer claims data from individual states have shown small variations in price levels across insurers within local markets (Craig et al., 2021a)—and that we have no a priori reason to expect changes in quantities and prices with physician–hospital mergers to vary systematically across insurers—we consider this a small trade-off for the national coverage in our sample and the large number and variety of markets that come with it. Not having all insurers does limit our ability to directly measure some effects like recapture, or to be able to estimate a full structural model of local health care markets as in Ho and Lee (2017).

range of sample and modeling decisions, and to the inclusion of controls for other contemporaneous mergers at the same providers.

Next, we show that the price increases we observe are a function of a lessening of competition and are unlikely to be caused by alternative theories consistent with consumer benefit. To do so, we show that prices for physicians who are *already* integrated at a hospital increase by 9% after their hospital acquires additional physicians within their specialty. Given that these physicians' integration status did not change, it is unlikely that a sudden change in their quality or bargaining ability precipitated the price increase we observed. This suggests that the price increases were the result of a lessening of competition. We also provide direct evidence that, in this setting, there is no improvement in a range of quality measures post-integration that could explain the price increases we observe.

Finally, we leverage the results of our demand estimation for physician and hospital markets to develop empirical evidence for specific theories of harm that could underlie our findings. First, we estimate that hospital price effects are larger for transactions involving greater foreclosure potential (i.e., transactions where acquired physicians were doing business at competing hospitals prior to the merger). Second, we find that physician price treatment effects are significantly larger among physicians acquired by hospitals with a higher *ex ante* value to insurer networks. This is consistent with the recapture mechanism, which predicts that price effects will be larger when a focal provider's merging counterpart has more market power. Third, many hospitals acquire multiple physician practices, either simultaneously or sequentially, such that these mergers can have a "horizontal" component. We find that post-merger price increases are larger for transactions that increase concentration in the physician market. However, we also find that physician price effects are large, positive, and significant among deals with zero or negative effects on physician market concentration, reinforcing our other evidence of non-horizontal theories of harm.

Taken together, our results support three anticompetitive effects of physician–hospital mergers. We find evidence that physician–hospital mergers often increase prices and, when they do, they do so in ways consistent with: (1) greater foreclosure of rivals, (2) improving negotiating parties' outside options through recapture, and (3) increasing concentration in physician markets.

These results pose a challenge for antitrust enforcement agencies. Over the last two decades, enforcement agencies have focused on protecting consumers from mergers of the largest head-to-head competitors. However, our analysis demonstrates that hospital acquisitions of physician practices, transactions that have reshaped the physician industry via many small non-horizontal acquisitions, have harmed consumers. While evaluating all of these acquisitions might be challenging under current processes, our estimates suggest the scale of consumer harm generated by these transactions in aggregate is similar in magnitude to that of horizontal hospital mergers, which have been of great interest to regulators.

This paper provides new evidence on the impact of the recent wave of physician–hospital mergers and highlights the likely mechanisms through which these types of non-horizontal transactions have raised prices. We build off a literature which has measured the association between integration and proxy measures of prices (Baker et al., 2020; Lin et al., 2021b). Our focus on physician markets is most closely related to Capps et al. (2018), who examine the effect of physician–hospital mergers on physician prices, though they place a particular emphasis on the regulatory arbitrage induced by the Medicare payment schedule. In contrast, our study focuses on the net effect of these mergers on physician and hospital prices, both of which rise. We consider pro-competitive explanations and find they lack empirical support. We then evaluate which anti-competitive mechanisms are supported by the data, and analyze how those forces vary across markets.

More generally, this paper contributes to the broader empirical literature on integration of complements, which is relatively scarce despite its importance. Prior studies have documented gains in efficiency from improvements in scale and management (Atalay et al., 2014; Hortaçsu and Syverson, 2007), combinations of efficiency and foreclosure effects that leave consumers either unharmed or better off (Chipty, 2001; Mortimer, 2008), and combinations of foreclosure and price increases that leave consumers worse off (Crawford et al., 2018; Cuesta et al., 2024; Cutler et al., 2020; Hastings and Gilbert, 2005; Luco and Marshall, 2020). By studying the heterogeneity of price effects across transactions, we add to this literature by documenting how price changes induced by non-horizontal mergers can be mediated by three forces: foreclosure, recapture, and increases in horizontal market concentration via roll-ups.

This paper proceeds as follows. In Section 2, we describe the role of physicians in the provision of hospital care and theories of how physician–hospital mergers can influence provider negotiations with private insurers. In Section 3, we describe our approach for measuring physician–hospital integration and trends in integration among physicians 2008–2016. We describe our empirical strategy and present descriptive statistics in Section 4. We discuss our results in Section 5 and conclude in Section 6.

2 Background, Institutions, and Setting

2.1 Physicians and Hospitals

Historically in the US, physicians and hospitals have acted as financially and organizationally independent co-producers of care. As separate economic entities, each submits separate claims for reimbursement and negotiates separate fee schedules with private insurers. However, physician and hospital services are necessarily deeply intertwined, especially in the provision of inpatient care.

Physicians provide crucial inputs in hospital care, and hospitals cannot produce inpatient procedures such as joint replacements, births, and spinal fusions without the labor of physician specialists. Indeed, an influential class of models has positioned hospitals somewhat passively as workshops in which physicians practice medicine (Pauly, 1980). Likewise, physicians are arbiters of demand in the sense that many services that hospitals provide must be ordered by physicians on behalf of patients, hence the oft-repeated framing of the doctor's pen as a particularly expensive piece of medical equipment (Cassel and Guest, 2012). Finally, physicians have substantial influence over the choice of facilities in which patients are treated, and much has been written about physicians' conflicting incentives to manage costs in this role (Chernew et al., 2021; Ho and Pakes, 2014; Munnich et al., 2021).

Regulators, concerned that perverse physician incentives might reduce quality or increase costs in patient care, have introduced various constraints to financial relationships between physicians and hospitals. In particular, the Stark laws and anti-kickback statutes prevent hospitals from paying physicians for referrals. These constraints apply regardless of physician–hospital integration (Szostak 2015). Even when physician–hospital mergers take place, these laws require that transactions be "commercially reasonable," with both the up-front purchase price and post-purchase employment contracts reflecting "fair market value" without any explicit promise of referrals in exchange for remuneration. Nevertheless, an employment relationship and organizational integration may create new ways for hospitals to reward physicians for actions that benefit the hospital, and shared technology may enable greater monitoring and compliance. Therefore, despite the existence of regulations that prohibit profit-maximizing behavior, the possibility of these perverse incentives creates need for further research. Indeed, as an empirical matter, prior literature has found that physicians steer their patients toward the acquiring hospital post merger, and we find the same pattern in our sample (Baker et al., 2016; Koch et al., 2017; Lin et al., 2021a).

2.2 Non-Horizontal Mergers and Competition

The simplest theoretical models predict that mergers of complements may decrease prices by internalizing externalities across firms (Cournot, 1838; Linnemer, 2022) or by internal efficiencies (Grossman and Hart, 1986; Williamson, 1971, 1979, 1985). However, non-horizontal mergers could also lessen competition and cause prices to increase.

First, the merger of an upstream and downstream firm could lessen competition markets via foreclosure—the withholding of an input or access to customers (Hart and Tirole, 1990; Ordover et al., 1990; Salinger, 1988; Salop and Scheffman, 1987). Foreclosure can be partial, leading to higher costs for the foreclosed entity, or total, in which case the rival is shut out of the market entirely. In our setting, physicians could concentrate their patients and services with the acquiring hospital system and therefore foreclose their rival hospital. Moreover, if a hospital acquired a sufficient number of physician practices, the acquiring hospital could narrow physician referrals in their region and hence gain bargaining leverage with insurers over their own prices (Cuellar and Gertler, 2006; Gal-Or, 1999; McCarthy and Huang, 2018).

Second, if a physician integrates with a highly-valued hospital, the merger could result in an improved outside option for the physician in a subsequent negotiation with an insurer. Peters (2014) terms this mechanism "recapture." The outside option for a given supplier may include a recapture effect in which business lost through the focal purchaser can be recaptured via an alternative purchaser. For example, a sought-after physician may reasonably expect to retain some of a given insurer's enrollees as patients even if she is dropped from the insurer's network, as particularly loyal patients will switch insurers to retain access to the physician's in-network services. Providers with higher expectations of recapture are expected to negotiate higher prices with insurers, all else equal. Physician–hospital mergers are generally followed by joint, all-or-nothing negotiations such that dropping a merged physician means also dropping the hospital and vice versa. If patients are more willing to switch insurers to maintain access to the integrated providers after a merger, the merger will improve merging providers' outside options and prices will increase. Intuitively, when physicians are independent and negotiating with the insurer, they know that they will lose many patients if they do not stay in-network. However, if the entire system is excluded by the insurer, more patients will make the effort to change insurers in order to keep both the hospital and the doctors they want. The system will be able to "recapture" these patients, who will be lost to a rival insurer and re-gained by the physicians. If integration increases recapture, the integrated providers can negotiate higher prices.

Third, physician–hospital integration may increase physician market concentration if hospitals acquire multiple physician practices, whether simultaneously or sequentially. Such increased concentration in a physician market could increase prices via the typical mechanism of reduced horizontal competition. Sequential practice acquisitions (or "roll-ups") by private equity firms have been found to sharply increase prices in other health care settings (Asil et al., 2024).

However, physician–hospital mergers could also raise prices through competitively benign or beneficial forces. First, integration could lead to higher quality care, which could increase patients' willingness to pay for integrated providers. Health care market participants and policymakers have long been concerned about the negative impacts of care fragmentation (Cebul et al., 2008). Agha et al. (2023) provide compelling evidence that organizational concentration, in which outpatient visits are concentrated within a small set of firms, leads to lower utilization and improvements in diabetes care. Under standard models of bargaining, higher quality would predict higher prices (Grennan and Swanson, 2022). That said, prior empirical work on physician–hospital integration has not found evidence of quality increases (Koch et al., 2021; Lin et al., 2021b), and we do not find any here.

Second, integration could increase prices by increasing the bargaining ability of the merging parties. Bargaining ability determines the share of surplus captured by a negotiating party, conditional on the competitive forces that shape next best alternatives (which may also change with a merger as described above). Prior research has found that bargaining ability is a key determinant of price increases following horizontal hospital mergers (Lewis and Pflum, 2015). Similarly, physician prices may increase after physician–hospital mergers because a transaction affords them access to the services of hospitals' more informed or more capable negotiators.

3 Measuring Physician–Hospital Integration

3.1 Trends in Physician–Hospital Integration

A lack of comprehensive data on physician–hospital integration has been a significant challenge in the existing literature. Early studies used the American Hospital Association (AHA) survey data to create a binary measure of integration at the hospital level (Baker et al. 2014). Other researchers have used a claims-based approach introduced in Neprash et al. (2015), which defines whether or not a physician is integrated based on the amount billed in Medicare claims with the hospital outpatient department (HOPD) as their place of service.[4] Subsequent studies have incorporated more granular survey data from SK&A (now IQVIA OneKey) to identify integrated physicians (Baker et al. 2016; Lin et al. 2021b). However, SK&A is not a complete survey of physicians and integration status is often reported inaccurately. Finally, some researchers have studied highly publicized acquisitions captured in the Levin Associates M&A data (Koch et al. 2017, 2021). This approach limits misclassification of integration events but is likely to under-represent small acquisitions.

In this paper, we utilize a new method that combines a rich collection of datasets with machine learning methods, allowing us to accurately predict physician–hospital integration for the near universe of physicians between 2008 and 2016. We summarize our classification method here and refer readers to Appendix A for details on data sources, model fitting, and comparisons to existing measures of physician–hospital integration used in the literature.

3.1.1 Measuring Physician–Hospital Integration

Key to our approach is the use of the Medicare Data on Provider Practice and Specialty (MD-PPAS)—administrative data containing taxpayer identification numbers

[4]This measure is likely correlated with integration because of Medicare payment rules that allowed integrated providers to benefit financially from the substitution of place of service codes on claims for identical services. Both Dranove and Ody (2019) and Song et al. (2015) document this relationship between integration and HOPD billing.

(TINs) under which each physician bills Medicare, along with each physician's national provider identifier (NPI). Generally, each physician bills under a unique NPI. TINs roughly correspond to practices, although a single practice may have multiple TINs and locations, particularly among larger practices. The MD-PPAS data include all physicians that participate in Medicare, totaling 791,649 physicians in 233,787 TINs over 2008–2016 (5,393,622 NPI-years). We combine these data with information about health care systems, local provider markets, and physicians' reported practice ownership from the AHA surveys, National Plan and Provider Enumeration System from the Centers for Medicare and Medicaid Services (CMS), Physician Compare data also from CMS, Securities and Exchange Commission (SEC) filings, and SK&A.

We then train and validate machine learning models using a manually verified measure of hospital ownership for a random sample of 916 unique TINs covering 124,725 unique NPIs (543,183 NPI-years, or roughly 10% of those in MD-PPAS). In this training sample, integration status was coded manually using Internal Revenue Service (IRS) and SEC filings, press releases, news articles, and practice websites. One research assistant independently created the training set's integration variables, which were then manually audited in full by a coauthor. Ten percent of the sample was double-audited by another research assistant.

Table 1: Training Sample Prediction Error by Method of Predicting Integration

	Misclassification Rate (%)		
	Overall (1)	Integrated (2)	Non-Integrated (3)
HOPD Billing	44.67	48.76	29.41
SK&A Self-Report	40.99	50.37	6.05
SK&A TIN Groupings	6.38	5.67	9.06
Random Forest	2.80	3.06	1.84
N (NPI-Years)	543,183	428,262	114,921

Notes: This table presents the misclassification rate as a percentage of NPI-years in the training sample among different methods for determining integration. Columns (1)–(3) show the rate for all NPI-years, those integrated with a hospital/system, and those not integrated with a hospital/system, respectively. Out-of-sample error for the SK&A TIN groupings and random forest methods is determined by repeated 5-fold cross validation. The HOPD method classifies integration status only and cannot capture linkage with specific a specific hospital/system—we consider physicians who bill above 25% in a HOPD to be integrated. The SK&A TIN Groupings method pools SK&A responses across NPIs in the same TIN; a practice is integrated when the rate of ownership among physicians in a TIN exceeds a threshold value chosen by a decision tree.

Our novel measure of physician–hospital integration relies on random forest models to flexibly predict each physician's integration with candidate hospitals and hospital systems. Most importantly, we aggregate reported ownership from SK&A to the TIN-year level.[5] Other predictors include summary measures of similarity between business names and geographic distances between physician practices and hospitals. We fit separate models for each calendar year and cross-validate to select random forest tuning parameters in order to avoid overfitting. We leverage our large training sample to fit our preferred machine learning models and assess out-of-sample performance.

Table 1 reports a comparison of the misclassification rate between our random forest, the HOPD billing method, and SK&A, using our hold-out sample to assess performance.[6] It demonstrates that the HOPD billing method incorrectly predicted integration status for 44.67% of NPI-years. Similarly, SK&A self-reporting incorrectly predicted either integration status or the hospital/system owner for 40.99% of NPI-years. Error for the SK&A self-report approach was higher among integrated physicians, suggesting under-reporting of integration in the survey. To highlight the importance of aggregating ownership reporting in SK&A, the third row (labeled "SK&A TIN Groupings") reports misclassification from a simple decision tree as a function of all responses within a TIN-year. Pooling ownership information within a TIN-year reduced the rate of error to 6.38%. The additional predictors and flexibility of the random forest model lowered overall error due to misclassification to 2.8%. Taken together, this exercise demonstrates that the random forest improved the prediction of integration status and the hospital/system that owns each physician's practice relative to previous methods, increasing accuracy from under 60% to over 97%.

3.1.2 Variation in Physician–Hospital Integration

In this section, we use our random forest models to predict integration for the near universe (98%) of physician-years in the MD-PPAS data and describe the evolution

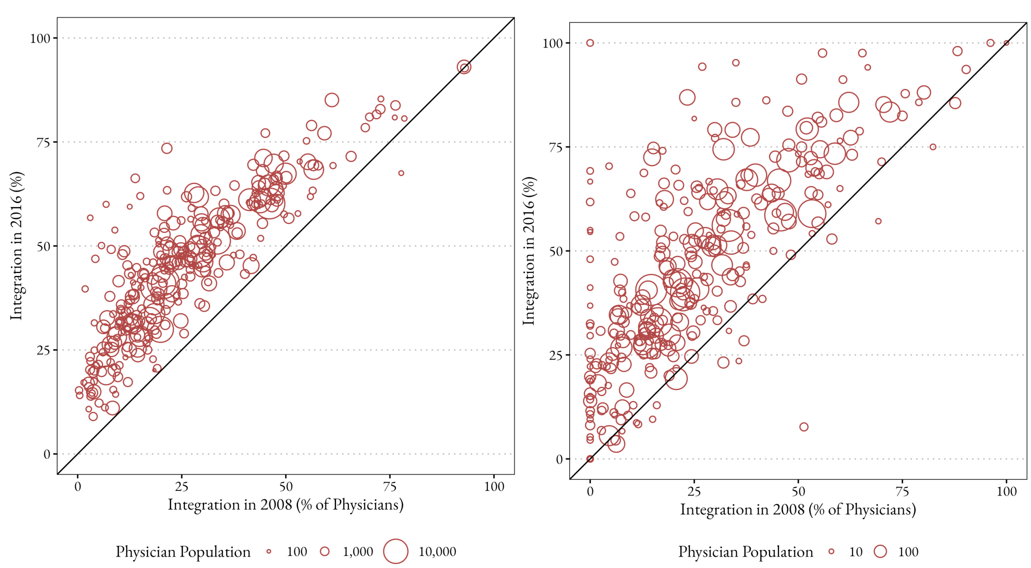
[5]Although ownership reported by individual physicians suffers from significant error, the overall rate of reported ownership in a TIN provides useful information on integration of a practice. This relies on the assumption that physicians who use the same financial identifier for billing have the same integration status. In support of this assumption, Ho et al. (2020) compares TINs reported in MD-PPAS to data from a large commercial payer that tracks physician–hospital integration, finding that these sources overwhelmingly agree on ownership.

[6]Details on the construction of each comparison method can be found in Appendix A.

of hospital-physician integration over time.[7] We find that there is substantial variation in integration across geography, specialty, and time. Overall, from 2008 to 2016, hospital ownership of physician practices grew from 27.5% to 47.2% across all NPIs. Integration increased within all specialties, but at differing rates. Integration increased particularly rapidly for cardiologists (38.4 percentage points) and general surgeons (28.1 percentage points). Integration among primary care physicians was less extreme but still substantial, growing by 18.1 percentage points to 50.5% by 2016. The trend in integration among obstetricians and gynecologists—the primary focus of our analysis—was quite similar to that of physicians overall: OB-GYN integration grew by 20.4 percentage points to 48.0% by 2016. Comparisons of integration trends implied by alternative approaches to integration measurement are presented in Appendix Figure A1, and additional specialty-specific trends in physician-hospital integration are presented in Appendix Figure A2.

Figure 1: Growth in Physician Integration by HRR

(a) All Specialties (b) OB-GYN Only



Notes: This figure presents the percentage of physicians in a hospital referral region (HRR) that are integrated with a hospital/system in 2008 (x-axis) and 2016 (y-axis). Panels (a) and (b) present figures for physicians of all specialties and for OB-GYNs, respectively. Dot size corresponds to the number of total physicians (a) or OB-GYNs (b) in each region.

[7] After excluding NPI-years without available geographic coordinates, NPIs located outside of the continental US, and NPIs that cannot be assigned a hospital referral region (HRR), we assign integration for the remaining 224,325 TINs and 779,018 physicians in the MD-PPAS data (5,303,124 NPI-years) to create a comprehensive physician-year panel.

Figure 1 compares physician integration in 2008 to integration in 2016 for each HRR in the continental US. Virtually all of our observations are above the 45° line, indicating growth in integration that is broadly shared across geography. Of 304 HRRs in the continental US, 302 (99.3%) experienced growth in integration, and 99.8% of physicians practice in an HRR where integration increased. Integration increased among OB-GYNs in 282 HRRs (92.7%) accounting for 96.3% of practicing OB-GYNs. Though most regions experienced increased integration, there is vertical dispersion of bubbles at each value on the horizontal axis of Figure 1, which demonstrates that some health care markets experienced much more growth in integration than others. Appendix Figure A3 presents maps of physician-hospital ownership, which shows substantial variation in integration across regions. For example, 81.4% of physicians in North Dakota were integrated with a hospital system by 2016, compared to only 17.0% in Nevada. Among OB-GYNs, integration reached 80% in Rhode Island and Vermont by 2016, compared to only 17.5% in Nevada.

4 Analytic Sample and Empirical Strategy

In this section, we outline our empirical approach to estimating the causal effects of physician-hospital integration events. We begin by constructing an analytic sample of procedures conducted by OB-GYNs in our commercial claims data. Details on how our analytic sample was constructed can be found in Appendix B. We then combine these data with the integration measures described in Section 3.1, and use event studies to assess what happens when a hospital acquires one or more physician practices. In order to isolate comparison or "control" providers, we estimate a model of patient demand (separately for physicians and hospitals) and use the implied substitution patterns to determine which providers are close competitors and whether a particular provider's competitors expose it to a meaningful increase in integration.

4.1 Analytic Sample of Labor and Delivery Admissions

This analysis uses claims data from 2011–2016 for individuals with employer-sponsored insurance from UnitedHealthcare, a large, national insurer that covers tens of millions of lives annually. We focus specifically on labor and delivery admissions, which constitute the plurality of inpatient admissions (∼28%) in our data. We merge in data on hospital characteristics from the AHA Annual Survey. We also include data on hospitals and physician practices from a range of sources described below.

We analyze physician–hospital mergers that occurred in 2013 and 2014, which allows us to observe hospital and physician prices related to all admissions for at least two years before and two years after an integration event. For an integration event to be included in our regression sample, we also require the relevant provider to perform admissions in every year of the sample period. This timing support restriction ensures that interpretation of the treatment effect estimates is not confounded by changing composition of the treatment group over time. We also show, via looking at single years of mergers, that our results are robust when analyzed over longer time horizons.

As shown in Appendix Table A3, our physician-event regression sample includes 117,906 labor and d performed by 2,024 physicians. Our hospital-event regression sample includes 195,810 baby deliveries performed at 462 hospitals. In both samples, the modal mother was aged approximately 31, and 37 percent of births involved a cesarean section. Our physician-event regression sample captures \$1,200,289,330 in spending on labor and delivery, including \$316,651,413 on physician services and \$883,637,916 in hospital spending. Our hospital-event regression sample captures \$2,082,135,389 in spending on labor and delivery, including \$553,647,181 on physician services and \$1,528,488,207 in hospital spending. The average physician payments for cesarean and vaginal deliveries were \$2,835 and \$2,599, respectively; the average hospital payments for cesarean and vaginal deliveries were \$9,194 and \$6,506, respectively.

4.2 Empirical Specification

We use a difference-in-differences strategy to estimate the causal effects of physician–hospital mergers on prices. We separately analyze price changes for physicians who are acquired by a hospital system, and for hospitals which experience a substantial increase in integration.

Recent work has highlighted a range of issues that arise in difference-in-differences models that estimate effects for multiple treatments with staggered timing (Callaway and Sant'Anna, 2021). In order to properly deal with these concerns, we use a stacked difference-in-differences regression, following several prior studies (Brot-Goldberg et al., 2024a; Cengiz et al., 2019; Craig et al., 2021b). This involves matching treated units to a suitable set of control units, and estimating separate unit and time fixed effects for each match group. This approach addresses potential identification concerns due to staggered treatment timing (Roth et al., 2023), and relies on a weaker, conditional (i.e., within-match group) parallel trends assumption.

The event study regressions we estimate are constructed as follows:

$$Y_{i,j,t} = \sum_{\substack{\tau=-2 \\ \tau\neq-1}}^{2} \beta_\tau D_{j,t,\tau} + \theta_{m(j),d(i),t} + \theta_{m(j),d(i),j} + \varepsilon_{i,j,t}. \tag{1}$$

$Y_{i,j,t}$ denotes the outcome variable associated with the admission of patient i performed by physician or hospital j in year t. $D_{j,t,\tau}$ denotes an event time indicator for provider j experiencing an integration event: $D_{j,t,\tau} = 1\{j \text{ treated}\} \cdot 1\{t - T_j = \tau\}$, where T_j is the year of j's integration event. $m(j)$ and $d(i)$ denote the match group of provider j and the diagnosis-related group (DRG) of admission i, respectively. DRG fixed effects are generally used in empirical health economics studies to control for the severity and intensity of inpatient admissions; in this sample, there are different DRG codes for vaginal and cesarean births, and for births with and without complications. We cluster the standard errors at the match group-provider level.

4.3 Defining Integration Events

Physicians The timing of a physician p's integration event, T_p, is the year in which she integrates with a hospital system. 20.6% (4,374) of the physicians in our data experience an integration event during our sample period (2011–2016).[8] 41.9% (1,831) of these events occur between 2013–2014. Of these, 18.0% (329) perform admissions in every year of the sample period. 84.0% (276) of these satisfy our remaining sample restrictions and constitute our treated group (see top panel of Appendix Table A4).

Hospitals Defining an analogous integration event for a hospital is less straightforward. Some hospitals in our sample become gradually more integrated with physicians as a result of multiple practice acquisitions over time. However, it seems unlikely that a small increase in the share of physicians integrated at a hospital would trigger a price change. As a result, we define hospital-level merger events by applying a simple algorithm which identifies large and sudden increases in physician–hospital integration.

[8] This excludes 789 (15%) of the integrating physicians who later de-integrate.

We later explore the sensitivity of our estimated treatment effects to the threshold values used in this algorithm and find them to be remarkably robust.

We define hospital h's level of integration in year t as the share of admissions at the hospital performed by physicians with whom the hospital is integrated, denoted by $v_{h,t}$. We define a hospital h as experiencing an integration event in year T_h if:

1. Hospital h experiences a large increase in integration over 2011-2016. In our preferred specifications, we require a hospital's "long difference" $v_{h,2016} - v_{h,2011}$ to be 15 percentage points or more.

2. The majority of a hospital's increase in integration over 2011–2016 occurs in a single year: In practice, we require that at least 60% of h's long difference occurs in year T_h.

3. Less than 5% of h's long difference occurs in any single year before T_h.

Of the 1,362 hospitals in our labor and delivery sample (performing admissions and with credible demand estimates in every year of the sample period), 360 (26.4%) had large increases in integration. 66 of those (18.3%) experienced an integration event between 2013-2014 according to our definition (see Appendix Table A5). Among those, 71.2% (47) were acquiring physicians for the first time.

4.4 Measuring Competition Between Providers and Concentration in Provider Markets

Estimating and interpreting the price effects of the mergers in our data requires an understanding of the degree of competition between and substitution across providers. In this section, we describe a model of patients' demand for hospitals and physicians. As a matter of internal validity, the substitution patterns we recover allow us to restrict our matched controls to those not exposed to spillover effects from mergers of rival providers. That is, we use the model estimates to exclude non-merging providers from the pool of potential controls if they compete directly with merging providers. Importantly, we also use the predictions of this model to construct measures of market power and concentration, which we use to test our competitive mechanisms.

We model the utility of patient i, located in region r, in year t selecting provider $j \in \mathcal{S}_{i,t}$ as:

$$u_{i,j,t} = \theta_{r,j,t} + \theta_{r,d,t} \cdot d_{i,j,t} + \varepsilon_{i,j,t},$$

where $\theta_{r,j,t}$ denotes a region-specific provider-by-year fixed effect (with the variation by year accommodating changes with integration status), $d_{i,j,t}$ denotes the distance in miles between patient i's home address and provider j in year t, $\theta_{r,d,t}$ captures the utility weight on distance for patients in region r in year t, and $\varepsilon_{i,j,t}$ is an i.i.d. extreme value error term. The choice set, $\mathcal{S}_{i,t}$, consists of all providers selected by patients from region r in year t.[9]

The use of an extreme value error term in this model implies that the predicted probability consumer i selects provider $j \in \mathcal{S}_{i,t}$ in year t will be

$$P_{i,t}(j) = \frac{\exp\left(\theta_{r,j,t} + \theta_{r,d,t} \cdot d_{i,j,t}\right)}{\sum_{k \in \mathcal{S}_{i,t}} \exp\left(\theta_{r,k,t} + \theta_{r,d,t} \cdot d_{i,k,t}\right)}. \tag{2}$$

We recover the coefficient estimates using maximum likelihood. Appendix Figure A4 presents the distributions of the resulting distance coefficients across regions and years, separately for hospital and physician demand.

This model allows us to quantify the extent to which a provider j competes with another provider g by estimating the *diversion ratio* from g to j. This diversion ratio predicts the share of g's patients who would substitute to provider j if provider g were removed from the patients' choice sets.

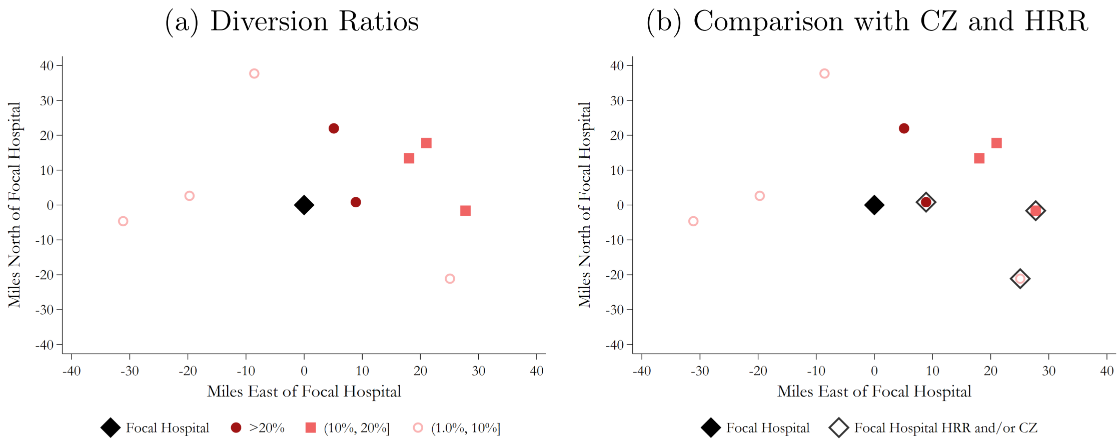
To construct the diversion ratios predicted by our estimated demand model, we first construct the predicted probability consumer i in region $r(i)$ selects provider $j \in \mathcal{S}_{i,t}$ in year t: $\hat{P}_{i,t}(j) = \frac{\exp\left(\hat{\theta}_{r(i),j,t} + \hat{\theta}_{r(i),d,t} \cdot d_{i,j,t}\right)}{\sum_{k \in \mathcal{S}_{i,t}} \exp\left(\hat{\theta}_{r(i),k,t} + \hat{\theta}_{r(i),d,t} \cdot d_{i,k,t}\right)}$. Similarly, the counterfactual probability that consumer i selects provider j in year t conditional on the removal of another provider $g \in \mathcal{S}_{i,t}$ from the choice set, $\hat{P}_{i,t}(j|\neg g)$, is estimated by replacing $\mathcal{S}_{i,t}$ with $\mathcal{S}_{i,t} \backslash \{g\}$.[10] The estimated choice probabilities are then used to calculate the diversion ratio from provider g to provider j in year t: $\hat{R}_t(j|\neg g) = \frac{\sum_i \hat{P}_{i,t}(j|\neg g) - \sum_i \hat{P}_{i,t}(j)}{\sum_i \hat{P}_{i,t}(g)}$. This diversion ratio $\hat{R}_t(j|\neg g) \in [0, 1]$ provides a data-driven measure of the extent to which provider j is a competitor from the perspective of provider g.

Figure 2 demonstrates the important role the estimated demand model plays in identifying which providers compete with each other. Panel (a) presents the diversion ratios from our estimated demand model for potential competitors surrounding a

[9] In practice, we restrict the choice set to providers within 100 miles of a patient's home, regardless of whether the provider is in the patient's region. A given provider may be in the choice sets of patients in multiple regions.

[10] By definition, $\hat{P}_{i,t}(j) > 0$ only if $j \in \mathcal{S}_{i,t}$, and $\hat{P}_{i,t}(j|\neg g) \geq \hat{P}_{i,t}(j)$, with strict inequality only if $\{j, g\} \subseteq \mathcal{S}_{i,t}$.

Figure 2: Implied Competitors from Estimated Demand Model—Example Hospital Market



Notes: This figure presents the diversion ratios from our estimated demand model for potential competitors surrounding a focal hospital.

focal hospital. Panel (b) overlays which hospitals are in the focal hospital's HRR and commuting zone (CZ), two potential market definitions used in the literature. In this example, there are hospitals in the CZ and HRR which are not close competitors; and there are hospitals outside of the CZ and HRR that are close competitors. We find that both of these "error" types are prevalent across geographies—across all hospitals in the first year of our sample, 44.4% percent of hospitals in the same HRR have diversion ratios below one percent, and 24.3% percent of hospitals with diversion ratios above ten percent are not in the same HRR. Thus, demand estimation gives us much sharper measures of who competes with whom than off-the-shelf market definitions. This is important for identifying clean controls that are unaffected by integration. It is also important for our mechanism tests, because scope for foreclosure and roll-up effects of multiple acquisitions both depend on good measures of who competes with whom.

In addition to measuring who competes with whom, the estimated demand model also provides a measure of how important any given provider is to an insurer trying to form a network in the region. We measure this using the change in consumers' *ex ante* willingness to pay (WTP) for the full network of providers versus the network from which a focal provider is removed. This is a crucial input into our mechanism tests related to recapture, where the ability of a hospital to increase the price of an acquired physician via joint negotiation depends on how many patients would leave an insurer that did not include that hospital in its network.

4.5 Defining Controls

Identifying the causal effects of integration events requires constructing valid control groups for treated physicians and hospitals. The key identifying assumption is that, in the absence of the focal integration event, the counterfactual trends in outcomes of treated providers would have been parallel to the observed trends in outcomes of control providers. For this assumption to be plausible, trends in outcomes of control providers must not have been affected by integration events. As a result, we construct a set of control providers for each integration event that did not experience integration events themselves, and were not exposed to spillover effects of integration among their rivals. We also restrict our attention to a set of controls which are observably similar to treated providers, which we select using propensity score matching.

4.5.1 Restricting the Set of Potential Controls

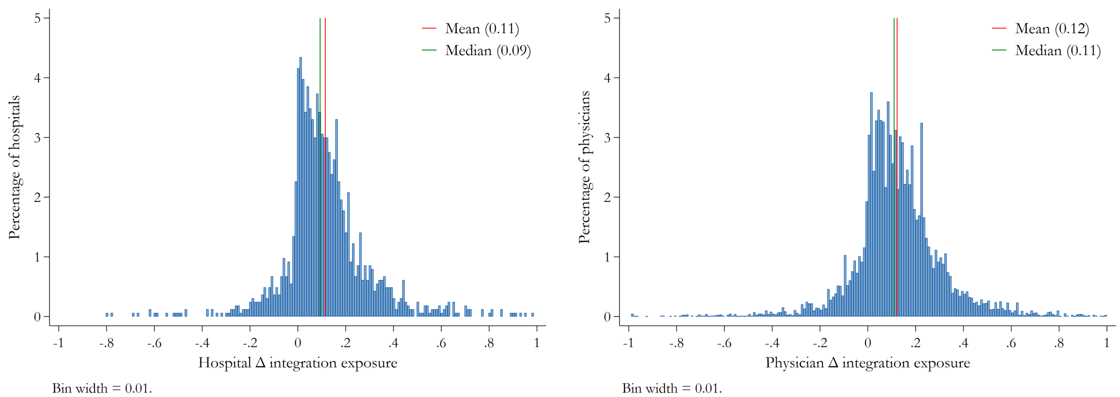
We limit our physician controls to those who do not become integrated with a hospital system over our study period. We limit our hospital controls to those whose integration level does not increase by more than 10 percentage points over the sample period. We further limit to control providers that are not exposed to integration through their rivals. To inform this restriction, we use our demand model to estimate the extent to which each provider is exposed to spillover effects. Using the diversion ratio estimates calculated above, we define the following *integration exposure index* for each provider j in year t: $Exp_{j,t} = \sum_{\{g|g\neq j\}} v_{g,t} \cdot \hat{R}_t(g|\neg j)$.

In the case of hospitals, $v_{g,t}$ is, as defined above, the share of admissions at hospital g in year t performed by physicians who are integrated with g. In the case of physician practices, $v_{g,t} = 1$ if practice g is integrated with a hospital in year t and $v_{g,t} = 0$ otherwise. Thus, $Exp_{j,t}$ estimates the probability that a patient of provider j in year t would substitute to an integrated provider following the removal of j. $Exp_{j,t} \in [0, 1]$ is increasing in the extent to which j is exposed to integration through competition. Figure 3 shows the distributions of $\Delta Exp_j = Exp_{j,2016} - Exp_{j,2011}$ for hospitals and physicians, which we use in implementing our restriction.

Among both hospitals and physicians, we require that j's exposure index changes by at most 10 percentage points over the sample period, ensuring that control providers are not experiencing an increase in exposure to integration among their competitors ($|\Delta Exp_j| \leq 0.1$). This restriction implies that—for patients who choose control provider j—the fraction of their second-best choices that become integrated over our study period cannot increase by more than 10 percentage points. This restriction yields 471 and 2,489 potential control hospitals and physicians, respectively.

Figure 3: Distribution of Long Difference of Integration Exposure Indices

(a) Hospital ΔExp (b) Physician ΔExp



Notes: Figure presents empirical distribution of ΔExp among providers performing admissions in both the first and final year of the sample period.

4.5.2 Constructing Matched Control Groups

We use pre-event observable characteristics to match each treated provider to eligible control providers. This is done by constructing a set of match groups, each containing 15 providers, consisting of a treated provider and their nearest neighbors among the set of eligible controls. The controls selected for each match group are those closest to the focal treated provider in propensity score space, where propensity scores are estimated using a probit regression. Control providers are selected with replacement. Of the 471 hospitals in the pool of eligible controls, 396 unique hospitals are matched to a treated hospital. Of the 2,489 physicians in the pool of eligible controls, 1,748 unique physicians are matched to a treated physician.[11]

[11] Hospitals are matched using number of beds, number of full-time personnel, number of admissions, number of technologies, teaching, non-profit, government-owned, insurer county market share, proportion of all admissions medicaid, proportion of all admissions medicare, local hospital HHI, and local physician HHI. Physicians are matched using practice size, multi-specialty practice, top-50

4.6 Descriptive Patterns

Table 2 compares the means and standard deviations of facility- and region-level characteristics of all hospitals in our labor and delivery data, as well as treated and matched control hospitals in our hospital event regression sample.

Table 2: Baseline Characteristics of Hospitals in Regression Sample

	(1)	(2)	(3)	(4)
Characteristics	All Hospitals	Treated Hospitals	Control Hospitals	Difference
Total Beds	246.24	263.58	263.55	0.03
	(202.87)	(239.69)	(183.10)	(31.46)
Total Full Time Personnel	1,486.22	1,504.98	1,530.58	−25.60
	(1,702.25)	(1,849.90)	(1,340.76)	(240.19)
Total Admissions	11,851.09	12,704.26	12,576.36	127.90
	(10,378.07)	(13,092.96)	(8,635.33)	(1,685.95)
Total Technologies	65.96	66.61	68.35	−1.73
	(21.68)	(20.61)	(20.98)	(2.84)
Teaching	0.37	0.35	0.39	−0.04
	(0.48)	(0.48)	(0.49)	(0.07)
Nonprofit	0.71	0.80	0.76	0.04
	(0.45)	(0.40)	(0.43)	(0.05)
Government-Owned	0.13	0.11	0.13	−0.02
	(0.34)	(0.31)	(0.34)	(0.04)
Proportion Medicaid	0.20	0.21	0.22	−0.01
	(0.09)	(0.10)	(0.10)	(0.01)
Proportion Medicare	0.45	0.46	0.44	0.02
	(0.10)	(0.09)	(0.10)	(0.01)
Patient Distance Travelled	10.55	10.41	10.40	0.01
	(7.15)	(7.96)	(5.44)	(1.04)
C-Section	0.41	0.45	0.41	0.04
	(0.25)	(0.21)	(0.20)	(0.03)
ΔWTP Quantile	0.50	0.53	0.54	−0.01
	(0.29)	(0.26)	(0.27)	(0.04)
Physician Price	2,753.46	2,772.10	2,728.31	43.79
	(705.98)	(687.36)	(674.33)	(92.26)
Facility Price	7,643.13	8,453.81	7,511.74	942.07**
	(3,137.43)	(3,431.99)	(3,203.82)	(468.33)
Total Price	10,396.59	11,225.92	10,240.05	985.86**
	(3,366.03)	(3,617.37)	(3,377.53)	(491.90)
Share Admissions VI	0.28	0.11	0.28	−0.17***
	(0.40)	(0.20)	(0.41)	(0.04)
Local Hospital HHI	4,808.36	4,505.05	4,659.62	−154.57
	(1,909.70)	(1,753.97)	(1,785.18)	(239.34)
Local Physician HHI	2,711.16	2,260.59	2,418.65	−158.07
	(1,922.08)	(1,550.48)	(1,756.93)	(210.93)
N	1799	66	396	

Notes: Summary of the baseline facility- and region-level characteristics of all hospitals (column 1), hospitals experiencing integration events (column 2), and treated hospitals' matched controls (column 3); standard deviations in parentheses. Characteristics are measured in 2011, the first year of our sample period. Column 4 compares columns 2 and 3, presenting the coefficient estimate from a simple regression of the row-level characteristic on treatment status in the treatment/control sample, with standard errors clustered at the hospital level. Stars indicate the statistical significance of the coefficient estimate: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

medical school graduation, female, total medicare amount allowed, medicare outpatient billing share, medicare billing per service, local hospital HHI, and local physician HHI.

Integrating hospitals are quite similar to the average hospital. By design, treated and matched control hospitals have similar capacities (as proxied by bed count, staffing, and technologies), patient panels (as proxied by annual inpatient admissions, Medicare shares, and Medicaid shares), and ownership (as proxied by nonprofit and teaching status). They are also in similar markets in terms of hospital and physician concentration.

There are two key differences between treatment and control hospitals. First, treated hospitals charge significantly higher (∼13%) facility prices for labor and delivery admissions at baseline; this difference leads to a higher total price at baseline as well. Second, treated hospitals have fewer integrated admissions (17 percentage points) at baseline. This difference is to a great extent mechanical; if a hospital is highly integrated at baseline, it is less possible for it to experience an integration event.

The above pattern, wherein treated providers are quite similar to the full sample of providers, is also seen for physicians in Table 3. Integrating physicians are more likely to be in smaller, single-specialty practices than the average physician. As a result, they tend to be located in less concentrated physician markets and have lower prices at baseline. These differences are generally smaller by design when we compare treated physicians to their matched controls. Treated physicians have 11% lower Medicare billing and 4% higher prices at baseline than their matched controls, but are otherwise similar and practice in similar markets. For both physicians and hospitals, even the statistically significant differences are small in magnitude relative to the variation in the data as measured by standard deviations.

To clarify the magnitude of hospital integration events, Figure 4 shows event study estimates of Equation 1, where the dependent variable is measures integration at the hospital.[12] The left (navy squares) series presents estimates where integration is measured as the share of unique labor and delivery physicians integrated at the hospital. Integration levels follow the same trend in treated and control hospitals in the two pre-event years. In the event year, integration at the treated hospital jumps upward by 50 percentage points and remains fairly flat for two more post-event years. The right (red circles) series presents estimates where integration is measured as the share of admissions performed by integrated physicians at the hospital. It is nearly identical to the event study for share of physicians integrated. Thus, the

[12]We present the analogous estimates for non-OB-GYN physicians in Appendix Figure A5.

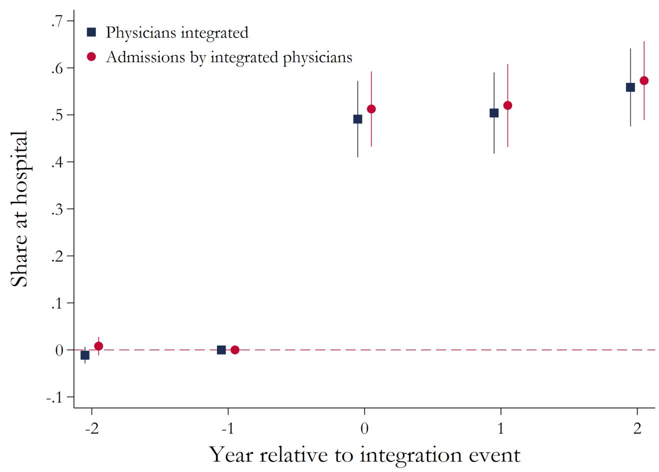
Table 3: Baseline Characteristics of Physicians in Regression Sample

	(1)	(2)	(3)	(4)
Characteristics	All Physicians	Treated Physicians	Control Physicians	Difference
Practice Size	90.22	51.63	49.94	1.68
	(221.67)	(162.86)	(144.77)	(11.15)
Multi-Specialty	0.40	0.25	0.26	−0.01
	(0.49)	(0.43)	(0.44)	(0.03)
Top 50 Medical School	0.31	0.33	0.33	0.01
	(0.46)	(0.47)	(0.47)	(0.03)
Female	0.51	0.56	0.59	−0.03
	(0.50)	(0.50)	(0.49)	(0.03)
Age	48.17	45.92	46.14	−0.22
	(9.70)	(8.91)	(8.54)	(0.59)
Total Medicare Amt Allowed	16,349.38	13,516.68	15,229.61	−1,712.93*
	(21,840.70)	(13,561.09)	(16,514.65)	(924.84)
Medicare Billing Per Service	97.97	91.58	95.30	−3.72*
	(37.72)	(31.74)	(32.22)	(2.13)
Medicare Outpatient Billing Share	0.06	0.04	0.03	0.02*
	(0.19)	(0.15)	(0.12)	(0.01)
C-Section	0.41	0.41	0.41	0.00
	(0.32)	(0.25)	(0.28)	(0.02)
Patient Distance Travelled	10.33	10.33	10.31	0.02
	(7.85)	(6.25)	(6.75)	(0.43)
Physician Price	2,824.56	2,760.85	2,659.28	101.56*
	(828.21)	(821.32)	(668.00)	(52.69)
Local Hospital HHI	5,433.76	5,695.30	5,554.29	141.01
	(2,268.66)	(2,380.13)	(2,204.74)	(155.53)
Local Physician HHI	3,585.06	3,173.13	3,366.76	−193.63**
	(1,555.73)	(1,370.60)	(1,440.10)	(92.15)
N	13,281	276	1,748	

Notes: Table summarizing the baseline characteristics of all physicians (column 1), physicians experiencing integration events (column 2), and treated physicians' matched controls; standard deviations in parentheses. Control physicians are either never integrated or always integrated. Characteristics are measured in 2011, the first year of our sample period. Column 4 compares columns 2 and 3, presenting the coefficient estimate from a simple regression of the row-level characteristic on treatment status in the treatment/control sample, with standard errors clustered at the hospital level. Stars indicate the statistical significance of the coefficient estimate: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

integration events in our sample are driven by sharp changes in physician integration at the hospital, rather than by contemporaneous increases in inpatient volumes among already-integrated physicians.

Figure 4: Event Study: Hospital–OB-GYN Integration Around Hospital Integration Event



Notes: Hospital-level event study comparing hospital–OB-GYN integration levels at treated hospitals to those of matched controls, in the five-year period covering the event. Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors.

5 Results

5.1 Treatment Effects on Prices

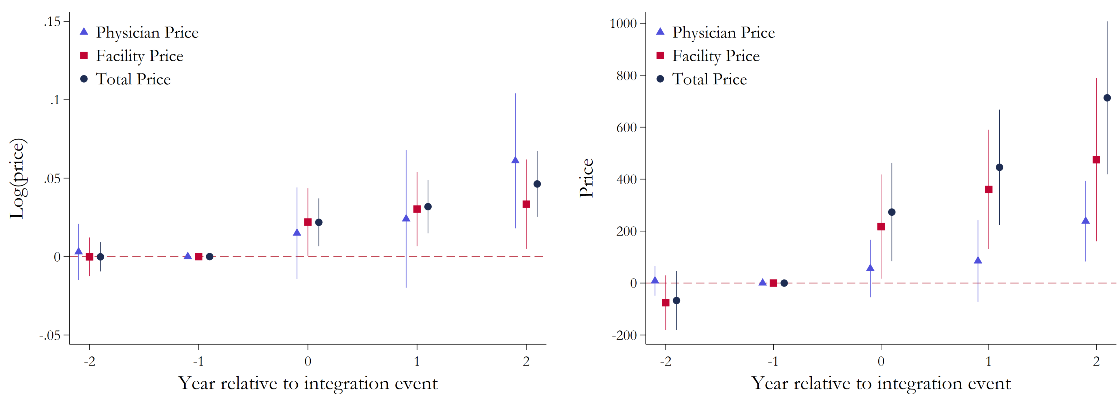
In this section, we present the estimates of Equation 1. Under joint negotiations by an integrated physician and hospital, market power may impact facility prices and/or physician prices. Therefore, we present estimates of the effect of physician-hospital integration on facility prices, physician prices, and the sum total of facility and physician prices for childbirth admissions.

Figure 5 presents the coefficient estimates and 95% confidence intervals for the effects of physician–hospital integration events on hospital-level prices. Each panel shows three series: the leftmost (blue triangles) represents coefficient estimates and confidence intervals where the dependent variable is the average price for OB-GYN physicians at treated hospitals (regardless of integration status), the middle series (red squares) presents the analogous results for facility prices, and the rightmost series (navy circles) presents the results for the sum total of the physician and facility prices.

Panel (a) of Figure 5 presents results for the natural log of each dependent variable. Prices at treated and control hospitals exhibited parallel trends in the years leading up to the integration event. In the year of the integration event, treated hospitals' prices jumped sharply, building to a long-run treatment effect of 3.3% for facility prices, 6.1% for physician prices, and 4.6% for total prices two years post event.

Figure 5: Event Study: Hospital Prices Around Hospital Integration Event

(a) Log Prices (b) Price Levels



Notes: Figures present event studies comparing prices at treated hospitals to those of matched control hospitals, in the five-year period covering hospital-level integration events. The dependent variables are log price in panel (a) and price in panel (b). Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors. See Appendix Tables A6 and A7 for the corresponding regression tables with point estimates and standard errors.

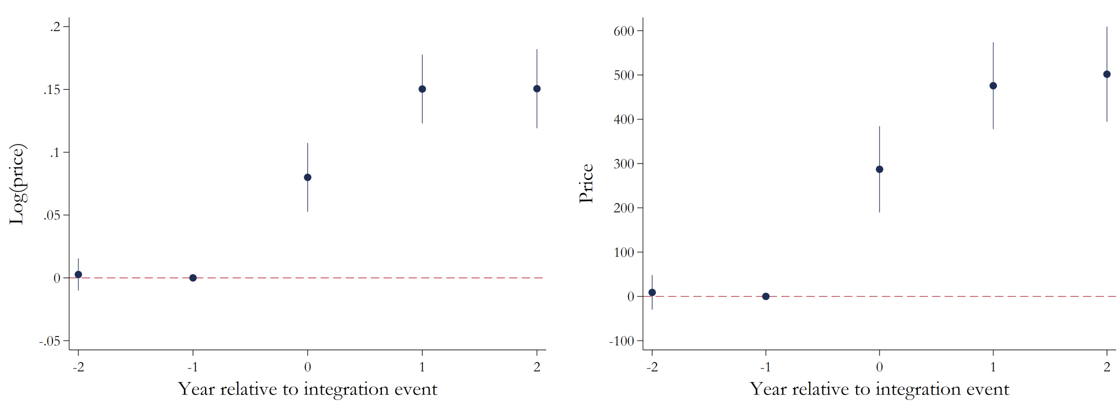
Panel (b) of Figure 5 presents the results of the same regression, but with prices (in dollars) as the dependent variables. The average sample facility price is $7,774, and the treatment effect of the integration event on facility price two years post event is $475. The average sample physician price is $2,820, and the treatment effect of the integration event on physician price two years post event is $239. Thus, physician price treatment effects are smaller than facility price treatment effects in absolute (dollar) terms, but larger in relative (log) terms. In aggregate, we estimate that the average total price for labor and delivery services increased by $713 two years after the integration event.

The above results present treatment effects of *hospital-level* integration events on *physician-level* prices, regardless of whether the physicians on the inpatient admissions were themselves acquired as part of the integration event. Thus, they are informative from the perspective of a patient receiving the same treatment at the same hospital by a random physician practicing at that hospital, but they do not measure the effect on the prices of the merging physicians.

To measure the price effects from the perspective of the merging physicians and their patients, we next focus on the effect of *physician-level* integration events on *physician-level* prices. The event study in Figure 6 presents results for physician price in logs (Panel a) and levels (Panel b). The Figures show that treated physicians (those who were integrating) and control physicians (those who were alread integrated, or never integrated) were on parallel trends in the years prior to integration. We then observe a treatment effect of 8.0% in the year of integration and 15.1% two years post integration, or roughly $502. These estimates are substantially larger than those observed for the hospital-level events, in part due to the hospital event sample including many non-integrating physicians.

Figure 6: Event Study: Physician Prices Around Physician Integration Event

(a) Log Prices (b) Price Levels



Notes: Physician-level event studies comparing prices of treated physicians to those of matched controls, in the five-year period covering the event. The dependent variables are log price in panel (a) and price in panel (b). Vertical lines represent 95% confidence intervals based on physician-match group-clustered standard errors. See Appendix Table A8 for the corresponding regression table with point estimates and standard errors.

The above results suggest that the physician–hospital integration events we study lead to substantial increases in both physician and hospital prices on average. In Appendix E, we present a series of exhibits demonstrating the robustness of these primary treatment effects. Appendix Figures A6 and A7 show difference-in-differences estimates for our preferred specification, as well as for a wide range of alternative samples, thresholds defining treatment and control status, and treatment-control matching algorithms. The results are qualitatively and quantitatively similar to our preferred specification. In Appendix Figures A8 and A9, we present event studies with longer time horizons based on single years of mergers. Using 2013 mergers, we can look three years after the merger events. Using 2014 mergers, we can look three years before the merger events. Our results are robust to looking over longer time horizons.

We also present evidence that our key results are not driven by other contemporaneous investments made by integrating hospitals. In Appendix Figure A10(a), we demonstrate that the results in Figure 5 are unchanged when we control for contemporaneous non-OB-GYN integration at the event hospital. Similarly, Appendix Figure A10(b) demonstrates that our results are unchanged when we exclude the four hospitals in our sample which are party to a horizontal hospital merger.

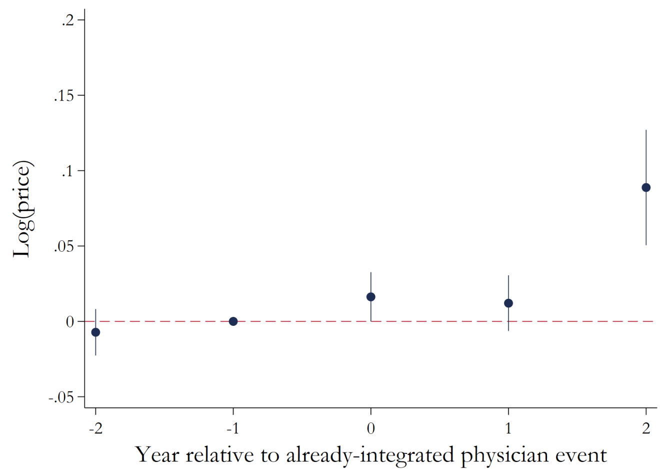
5.1.1 Price Effects, Quality, and Bargaining Ability

The results in Figure 5 and Figure 6 illustrate that hospital integration with OB-GYNs leads to increases in both physician and hospital prices. In this section, we provide evidence that these price increases are driven by, at least in part, a lessening of competition and not exclusively by changes in providers' bargaining ability or quality.

First, we estimate the treatment effects of physician integration events on prices of OB-GYN physicians who were *already integrated* with the acquiring system. These OB-GYNs are unlikely to experience a sudden increase in quality or bargaining ability when additional OB-GYNs integrate with their facility. As a result, to the extent that their prices increase after further integration of other physicians at their facility, it is likely a function of a lessening of competition.

To estimate this effect, we use a difference-in-differences regression similar to Equation 1, but with the "treated" physicians being those OB-GYNs who were already integrated with an acquiring hospital system involved in a physician integration event. We limit the set of treated physicians to those whose diversion ratios to the newly-integrating physicians are above 5%; i.e., we focus on already-integrated physicians whose systems acquire their competitor(s). Figure 7 presents the coefficient estimates and 95% confidence intervals. We estimate that already-integrated physicians at acquiring hospital systems have similar price trends to control physicians before an integration event, but their prices increase by approximately 9% ($354) two years after other physicians are integrated into their owning hospital.

Figure 7: Event Study: Prices of Already-Integrated Physicians



Notes: A physician-level event study comparing prices of already-integrated physicians, whose owning system engages in a physician integration event, to those of matched controls, in the five-year period covering the event. The dependent variable is log price. Vertical lines represent 95% confidence intervals based on physician-match group-clustered standard errors.

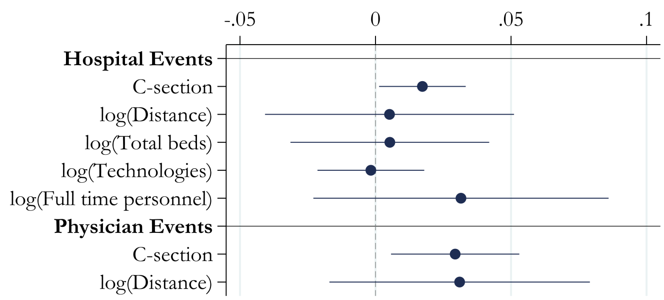
Second, we argue it would be difficult to explain the facility price changes we find via a bargaining ability effect. Bargaining ability is unobserved, but physician–hospital integration always involves a merger between asymmetric parties, with hospitals being economically larger and likely having greater bargaining ability (see Lewis and Pflum (2015) for discussion).

Third, we present direct empirical evidence on the impact of physician–hospital mergers on observable dimensions of quality. Figure 8 presents difference-in-differences estimates of Equation 1 on a range of quality measures relevant to childbirth. These include indicators for cesarean section and travel distance, each of which factors prominently in patient preferences (Ho and Pakes, 2014), as well as readmission rates and measures of investment such as total full-time personnel, beds, and technologies. Most of the estimates show mixed but imprecisely estimated effects of hospital-physician integration on quality.

In the physician-level event studies, integration is associated with a roughly 3.1% increase in patient travel distance, which is suggestive of reduced patient convenience. In the hospital-level event studies, integration is associated with a 3.2% increase in hospital personnel. However, neither result is statistically significant, and the latter may be driven mechanically by the integration event itself.

The most compelling and lone statistically significant evidence on quality is shown in the treatment choice regressions. Childbirth can be vaginal or via cesarean section. C-section deliveries are typically more profitable for providers. We estimate that C-section rates increase by 1.7–2.9 percentage points post integration ($p < 0.05$), which is roughly 4.5–8.0% of baseline. Experts consider cesarean section to be overused, and higher C-section rates are often characterized as resulting from physician-induced demand (Johnson and Rehavi, 2016; La Forgia, 2023). Taken together, our estimates suggest that, if anything, quality declines post-integration, contradicting the argument that price increases are driven by quality improvements.

Figure 8: Event Studies: Quality Following Integration Event



Notes: Pooled two-year post-period coefficients from hospital- and physician-level event studies comparing quality of providers experiencing integration events to matched controls. DRG fixed effects are omitted from event studies where C-Section is the dependent variable. Horizontal lines represent 95% confidence intervals based on provider-match group-clustered standard errors.

5.2 Heterogeneity and Mechanisms

The documented price effects of integration could reflect a range of underlying economic mechanisms. Thus far, we have shown evidence that price increases after integration events are likely a function of a lessening of competition, rather than of an increase in providers' quality or bargaining ability. In this section, we explore the mechanisms through which physician–hospital integration could be leading to price increases via a lessening of competition.

To do so, we estimate a series of triple-differences specifications that explore heterogeneity in the treatment effects of integration events and take the form:

$$Y_{i,j,t} = \sum_{g=1}^{G} \sum_{\substack{\tau=-2 \\ \tau\neq-1}}^{2} \gamma_{\tau,g} X_{j,g} D_{j,t,\tau} + \theta_{m(j),d(i),t} + \theta_{m(j),d(i),j} + \varepsilon_{i,j,t}. \tag{3}$$

where $\gamma_{\tau,g}$ is a group g-specific treatment effect in year τ relative to the integration event. In each specification, treated providers are categorized into G mutually exclusive groups according to a particular characteristic along which we seek to identify treatment effect heterogeneity. $X_{j,g} = 1$ if provider j is in group g and $X_{j,g} = 0$ otherwise. Each of the analyses employs a grouping of providers, using baseline market conditions to identify proxies for foreclosure, recapture, and horizontal consolidation opportunities.

5.2.1 Foreclosure

Acquiring hospital systems may provide integrated physicians with stronger incentives to "steer" patients toward their facilities (Baker et al., 2016). To provide a high-level sense of the steerage possibilities in the labor and delivery context, Table 4 presents a transition matrix where each cell contains the share of integrating physicians' admissions going to their eventual acquirer, pre- and post-acquisition. The majority of acquired physicians have little or no potential for greater steerage: 56% of acquired physicians performed all of their deliveries at the acquiring hospital both pre- and post acquisition. However, of the 32% of integrating physicians that were *not* performing *any* deliveries in the acquiring system pre-event, the majority ($\frac{0.02+0.2}{0.1+0.02+0.2} = 0.69$) increased steerage post event.

Table 4: Physician Steerage Transition Matrix

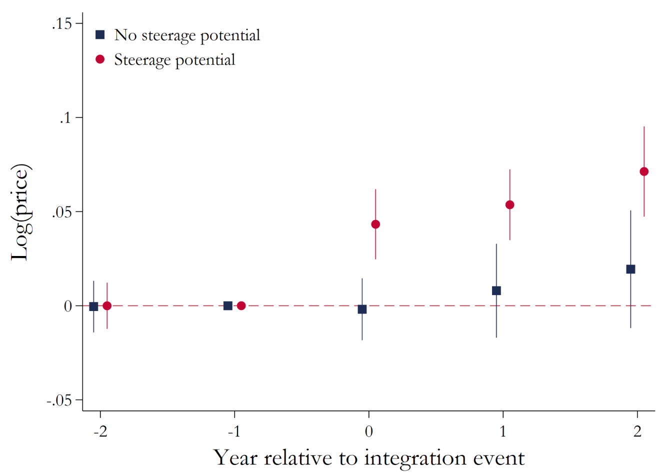
Pre \ Post	**0**	**(0,1)**	**1**
0	0.10	0.02	0.20
(0,1)	0.00	0.04	0.07
1	0.00	0.01	0.56

Notes: Transition matrix showing changes in acquired physicians' share of admissions performed at the acquiring system pre- and post acquisition. The array captures the joint distribution of these pre- and post-acquisition admission shares, such that each number corresponds to a share of the physicians in our sample integrating with a hospital between 2013 and 2014.

Under the foreclosure mechanism, we would predict larger price treatment effects for integration events with greater steerage potential. To construct proxies for foreclosure opportunities, we combine the intuition of the above transition matrix with the estimates from our demand model, focusing on potential steerage from integrating hospitals' local competitors. Specifically, we take a weighted average of the share of competing hospitals' admissions performed by the acquired physicians during the two pre-acquisition years. The weights are equal to the mean diversion ratio from the focal treated hospital in the two pre-event years.[13] We then estimate a triple-difference regression (Equation 3) where the interaction variable is an indicator for the integration event having positive steerage potential.

Figure 9 presents the treatment effect estimates for total hospital prices at hospitals experiencing events with and without steerage potential. We observe larger price increases among acquisitions with steerage potential (7.1% two years post merger) than without (1.7%), consistent with the prediction of the foreclosure model.

Figure 9: Event Study: Hospital Total Prices Around Hospital Integration Event By Steerage Potential



Notes: Price comparison between treated hospitals and matched controls, where hospital events are categorized based on their steerage potential. Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors. See Appendix Figure A12 for facility and physician prices separately.

In complementary analysis, we consider the impact of steerage on realized patient volumes at acquirer hospitals. We estimate a hospital-year level version of Equation 1, with log quantity of admissions as the dependent variable. As shown in Appendix Figure A13, if anything, admission volumes decreased after integration events, suggesting that increased steerage from acquired physicians was more than counterbalanced by

[13]See Appendix Figure A11 for the distribution of this variable. Steerage potential is positive for 25.8% of events.

admission declines from other physicians.

5.2.2 Recapture

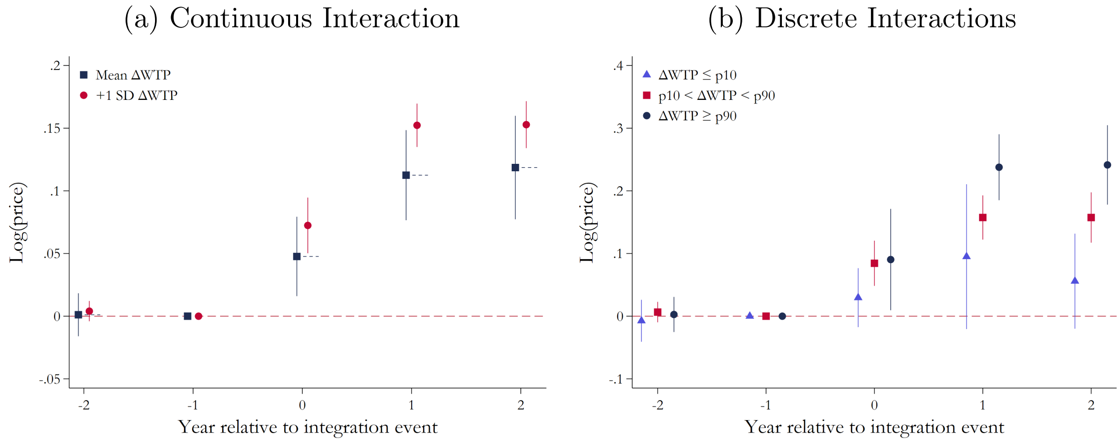
According to the recapture mechanism, the price effects of an integration event are expected to be larger when a focal provider's merging partner is more dominant in the local market. We test this prediction by estimating the correlation between acquired physicians' price effects and a measure of the acquiring hospitals' local market power.

We construct an observable measure of hospital market power using the pre-event estimates of *ex ante* WTP from our demand model. A hospital's WTP is the change in expected consumer utility due to the inclusion of the hospital in the consumer's choice set, integrated over all consumers. This object is a key building block of empirical studies of hospital–insurer bargaining (Capps et al., 2003; Gowrisankaran et al., 2015; Ho and Lee, 2017). For a given hospital h, in year t, we calculate WTP using the standard formula from this literature: $WTP_{h,t} = \sum_i \log\left(\frac{1}{1-\hat{P}_{i,t}(h)}\right)$. For the sake of interpretation, we normalize the empirical distribution of hospital WTP to have mean zero and unit variance. Hospital acquirers tend to have greater market power than non-acquirers, with a mean *ex ante* WTP 0.46 standard deviations higher than the full sample mean.[14]

We estimate triple-difference regressions using physician-level integration events, where the interaction variable is a function of the WTP of the acquiring hospital in the year prior to the event. We estimate one specification using a continuous interaction with ΔWTP, and other specifications using discrete interactions based on quantiles of the empirical distribution of ΔWTP among physician events in our regression sample. The results are shown in Figure 10.

[14] Appendix Figure A14 shows the distribution of normalized WTP among all hospital-years in our sample, and among hospital-years involving a physician integration event.

Figure 10: Event Study: Physician Prices Around Physician Integration Event By Acquiring Hospital ΔWTP



Notes: Figures compare price of treated physicians compared to controls, where physician integration events are categorized by the acquiring hospital's *ex ante* willingness-to-pay. Coefficients are estimated using triple-difference regressions, where the treatment indicators are interacted with a measure of the acquiring hospital ΔWTP. Vertical lines represent 95% confidence intervals based on physician-match group-clustered standard errors. Red confidence intervals in panel (a) quantify the uncertainty associated with the estimated difference between the red and navy coefficients. See Appendix Figure A15 for finer groupings of physicians according to their acquirers' ΔWTP.

Panel (a) of Figure 10 shows that the physician price treatment effect was 11.9% two years post event for acquirers with the mean ΔWTP ($= 0$ by construction) but was statistically significantly higher at 15.3% ($= 11.9\% + 3.4\%$) two years post event for physicians whose acquirers' ΔWTP was 1 SD higher. Panel (b) contrasts the price treatment effects for physicians whose acquirers' ΔWTP was below the 10th percentile, between the 10th and 90th percentile, and above the 90th percentile. Physicians whose acquirers had high ΔWTP experienced price effects of 24.1% two years post event, significantly greater than those of acquired physicians whose acquirers had low ΔWTP (5.6% at two years post-event). Taken together, the results suggest that physician price treatment effects are monotonically increasing in acquirer ΔWTP, consistent with the recapture mechanism.

5.2.3 Horizontal Concentration

Finally, physician–hospital integration can lead to increased horizontal concentration in the physician market, given that hospitals may acquire multiple physician practices in the same specialty over time or all at once. In this sense, these deals that are typically framed as "vertical" may also have a "horizontal" component.

To explore this issue, we begin by estimating the change in horizontal concentration in the physician market at the time of our integration events (see Appendix D for a detailed description of the calculations). For a given physician experiencing an event in year T, we quantify the change in the Herfindahl–Hirschman Index (HHI) in her local market from $T-2$ to T due to mergers and acquisitions carried out by the acquiring hospital system. We calculate these ΔHHI measures holding fixed market shares at their $T-2$ levels in order to isolate the effects of ownership changes.

In our sample, ΔHHI has both positive and negative values, resulting from the net effect of two opposing forces.[15] The force pushing towards higher physician market concentration ($\Delta HHI > 0$) is consolidation of physician practice ownership under local hospital systems. The force pushing towards lower physician market concentration ($\Delta HHI < 0$) comes from partial physician practice acquisitions that split practices into smaller groups. The former force dominates on average, so that the mean change in HHI is 148 and there is a substantial number of events in the ≥ 200 range.[16]

Next, we estimate triple-difference regressions, correlating physician-level price effects with ΔHHI. Panel (a) of Figure 11 presents the results for integrating physicians. It illustrates that there are significant price increases for integrating physicians whose markets become no more (or, indeed, less) concentrated as a result of their integration event, with a treatment effect of 8.6% two years post integration. However, treatment effects are far larger for integrating physicians whose markets become substantially more concentrated ($\Delta HHI \geq 200$), with a treatment effect of 23.1% two years post-event.

Panel (b) presents the results for already-integrated physicians at the integrating physicians' acquiring systems. These estimates illustrate that the spillover effects of physician integration on already-integrated physicians at acquirers are also increasing in ΔHHI. The treatment effect two years post event is economically and statistically insignificant for already-integrated physicians experiencing no increase in integration, but it is large and significant at 15.5% for physicians whose HHI increases by at least 200 due to the integration event. These results suggest that, while hospital acquisition

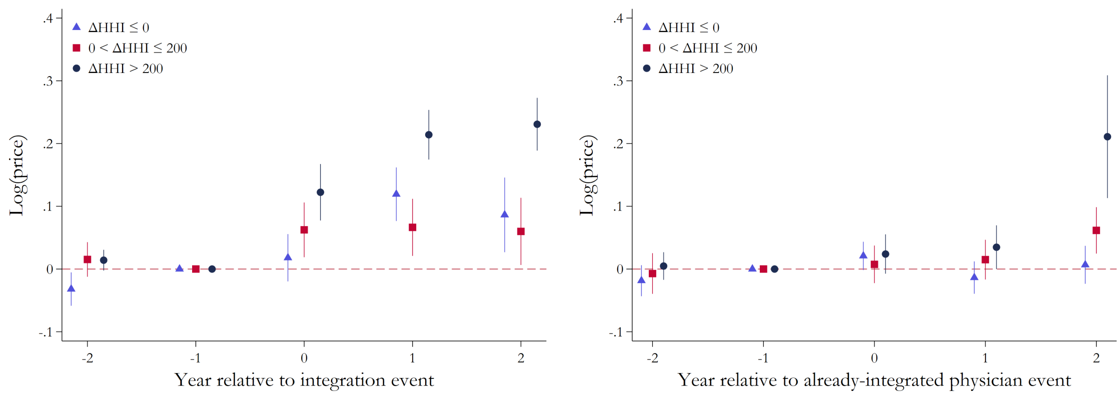
[15] Appendix Figure A16 presents the empirical distribution of ΔHHI in our sample.

[16] For context, the 2023 merger guidelines trigger a presumption of illegality for a post-merger HHI exceeding 1,800 and an HHI increase of more than 100.

of a given physician's practice increases that physician's own price regardless of the impact of acquisition on physician market concentration, the spillover effect of a given acquisition on the practices already owned by the acquirer also depends upon traditional horizontal market power channels.

Figure 11: Event Study: Physician Prices Around Physician Integration Event By Physician Market ΔHHI

(a) Integrating Physicians (b) Already-Integrated Physicians



Notes: Price comparison of treated physicians (panel (a)) and already-integrated physicians (panel (b)) to matched controls, where integration events are categorized by physician ΔHHI. Coefficients are estimated using triple-difference regressions, where the treatment indicators are interacted with a measure of physician ΔHHI in the focal physician's local market due to transactions by their acquiring/owning system. Vertical lines represent 95% confidence intervals based on physician-match group-clustered standard errors.

6 Conclusion

The integration of hospitals and physicians over the last few decades has dramatically reshaped provider markets. As we have illustrated, these types of transactions resulted in a 71.5% increase in the share of physicians employed by hospitals from 2008 to 2016.

To analyze the effect of hospital and physician integration on competition, we examine a large, geographically diverse set of mergers. We find strong evidence that, on average, integration between doctors and hospitals leads to significant and sudden increases in hospital and physician prices. The scale of the price increases that occur after physician–hospital mergers varies with the characteristics of the transactions: deals with greater scope for foreclosure and recapture and deals that induce greater horizontal concentration generate larger price increases. We do not observe any concurrent increases in quality after hospital-physician integrations that could explain the sudden price increases we observe. Collectively, our evidence demonstrates that the price increases we observe are likely a function of a lessening of competition.

Despite our strong evidence that physician–hospital mergers raised prices, we are aware of only a handful of federal investigations of these types of transactions.[17] This likely reflects the fact that 99.9% of the transactions we analyzed fell under HSR guidelines. Likewise, there has been little enforcement by state attorneys general under the competition statutes of their states. Our estimates suggest that taking action against many physician–hospital mergers could help preserve competition in health care markets and keep prices from rising.

Our results also provide guidance on the observed pre-merger characteristics of the transactions that tend to lead to the largest price increases. In our analysis of a large number of OB-GYN–hospital mergers, we find that deals with the most potential to increase either foreclosure (where the acquired physicians are most likely to shift their referral patterns) or recapture (where one of the merging parties already commands significant market power) generate the largest price increases. While federal enforcement in this sector has been limited, the 2023 Merger Guidelines take a useful step forward by combining the analysis of horizontal and non-horizontal mergers in one document, while establishing a guideline specifically covering non-horizontal transactions.[18, 19] It therefore seems possible that harmful transactions of this type could be enforced against, going forward.

We conclude that many of the mergers we observe are likely anticompetitive and harm both consumers and end payors of health care services by increasing the cost of care without generating commensurate increases in quality. The price increases from these transactions were substantial enough that the cumulative impact of these transactions could rival the impact of horizontal hospital mergers, which, in the presence

[17] FTC v St Luke's (2013) is an example of a rare litigated physician–hospital transaction (1:13-CV-00116-BLW).

[18] U.S. Department of Justice and the Federal Trade Commission (2023) ("Merger Guidelines 2023"); Guideline 5.

[19] Likewise, recent FTC activity suggests that the enforcement environment may be changing. The successful FTC challenge of a non-horizontal merger in 2023 was the first in many years and took place in a health care market (gene sequencing). This transaction would have combined the dominant sequencing machine for gene fragments made by Illumina with Grail, one of several competing developers of multi-cancer early detection tests that rely on the machine.

of employer-sponsored health insurance, have been shown to lower the wages and employment of workers outside the health sector (Brot-Goldberg et al., 2024b). We hope that this work motivates further study and investigation of the impact of mergers of complementary businesses both inside and outside of the health care sector.

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Appendices

A Physician–Hospital Integration Data

In this appendix, we document the creation of a new measure of physician–hospital integration that we constructed using machine learning. In the main text, we compared the performance of this method with alternative approaches used in the literature using a large validated sample, finding that it outperforms previous methods by a substantial margin. Here, we provide more detail on those methods as well. Finally, we briefly summarize the implications of our method for the growth in physician–hospital integration over the years 2008–2016.

A.1 Data sources

To determine integration status for each physician national provider identifier (NPI)-year, we combine several detailed datasets: Medicare Data on Provider Practice and Specialty (MD-PPAS), which contains the taxpayer identification numbers (TINs) under which each physician bills Medicare along with specialty and CBSA code; American Hospital Association (AHA) annual survey data on hospital and hospital system characteristics, cleaned up as a complete panel for years in which facilities are open as in Cooper et al. (2018); SK&A (now IQVIA OneKey), which contains physician survey data on practice address, specialty, and self-reported system and hospital ownership for 65.5% of the physician-years in MD-PPAS;[20] Physician Compare data from the Centers for Medicare and Medicaid Services (CMS) with physicians' practice addresses, specialties, and self-reported hospital affiliations for all clinicians enrolled in Medicare; and CMS National Plan and Provider Enumeration System (NPPES), which contains the universe of physician' practice addresses and specialties.

To prepare predictors of integration for physicians in the MD-PPAS data, we take several additional steps:

1. We assign geographic coordinates based on physician practice addresses from SK&A, Physician Compare, and NPPES. When practice addresses were unavailable, we use coordinates of CBSA centroids.

[20]Only 65.5% of physician-years in MD-PPAS are present in SK&A data. However, 94.6% of TIN-years contain at least one physician that is present is SK&A in the same year. These TIN-years account for 98.2% of all physician-years in MD-PPAS.

2. For each NPI-year, we keep only the TIN with the highest allowed amount. 82% of NPI-years have a single TIN. After removing secondary TINs, 96% of total allowed amounts remain in the sample.
3. We match each combination of SK&A ownership code, year, and HRR to a hospital/system observation in the AHA survey in the same year.[21] We used the following match procedure:
    - We manually matched each SK&A system ownership code to its appropriate AHA system.
    - For each SK&A hospital ownership code, we matched to AHA using geography and string matching (according to Jaro–Winkler distance) on names. Candidate matches were generated by iteratively matching and relaxing string and geographic distances. Unique SK&A-AHA matches generated by this procedure were accepted without further validation if (a) the median physician-hospital distance (across physicians in the TIN) is less than 50 miles; or b) the hospital is geographically closest and has string distance less than 0.15. All other matches were validated manually.
4. Using SEC filings, we collect subsidiary names of large, public, for profit health systems (those with greater than 6 hospitals). Specifically, we collect all subsidiary names from exhibit 21 of hospital system 10-K filings available from the SEC's Electronic Data Gathering, Analysis, and Retrieval system (EDGAR).

A.2 Training Sample and Model Details

We train models and evaluate various measures of physician–hospital integration using a training sample of 916 unique TINs. The training data was built from a random sample of 572 TINs, a random sample of 214 TINs associated with mergers between hospitals and physician practices identified by Irving Levin Associates, and a random sample of 131 TINs with at least one cardiologist in the Philadelphia and Miami HRRs.[22] The training sample was assembled using publicly available sources such as press releases, news articles, practice websites, and IRS and SEC filings. Our manual

[21] A single SK&A code might refer to multiple similarly-named entities hundreds of miles apart.

[22] We originally pursued cardiologists as a particularly interesting specialty, and the Philadelphia and Miami markets as ones with which we had previous familiarity. This last sample is therefore a sample of convenience. Our performance results are unchanged when we exclude this sample of convenience from the training set.

validation of integration variables is performed at the TIN-year level. One research assistant independently created the training set integration variables, which was then manually audited in full by one coauthor and, for 10% of the sample, double-audited by a research assistant. All discrepancies were resolved manually. Integration status was confirmed in IRS/SEC filings for 5,006 TIN-years (408,257 NPI-years) of our total sample of 6,498 total TIN-years (543,183 unique NPI-years). We follow all sample TINs for the full time horizon 2008–2016. The number of NPIs in our sample grew over time due to additional physician practices joining our sample TINs over time. When this occurred, we did not track down the integration status of those NPIs' previous TINs and did not include any of their previous TIN-years in our training sample.

Using this training sample, we trained random forest algorithms to identify integration between a given NPI and a given hospital system. Our random forest models predict integration at TIN-year level and are based on measures at the practice level. Predictors in this model include the similarity between physician's TIN legal name and hospital, system, and system subsidiary names; geographic distance between the physician's practice and the hospitals in the system (and similarly for all physicians in the same TIN); and the current, one-year lag, and one-year lead of SK&A reported ownership of the physician's practice by the system (and similarly for all physicians in the same TIN). We fit separate models in each calendar year and cross-validate to select random forest tuning parameters and avoid overfitting.

A.3 Alternative Integration Prediction Methods

To assess performance of the HOPD billing method, we follow Neprash et al. (2015) and use MD-PPAS to compute the percent of outpatient billing provided at a hospital outpatient department, considering physicians integrated if the percent of hospital outpatient billing is greater than 25%.[23] Across all training sample physicians, 45% of physician-years were misclassified using this approach, and misclassification rates were similar in the full sample and in the subsample of NPIs involved in group practice mergers. Misclassification rates were higher for integrated physician-years, indicating that this approach misses many physicians who are truly integrated with hospitals but do not do much outpatient billing.

[23] As in Neprash et al. (2015), we also tried using cutoffs at 50, 75, 95, and 99.9% outpatient billing; results were worse (higher overall error rate) with these stricter thresholds.

We then assess the performance of measuring integration by relying directly on self-reporting of hospital/system ownership in SK&A survey data, following the the approach in Baker et al. (2016). We assign each physician to her reported system owner (or, as appropriate, to the system of her reported hospital owner) in SK&A. If a doctor reports ownership in more than one system, we use the Jaro–Winkler distance to assign her to the system whose name is most similar to the physician's TIN legal name. For hospitals that are not a part of a system, we compare hospital and TIN legal names. Any ties are broken at random. SK&A self-report performs slightly better than hospital outpatient billing, with an error rate of 41% in the full sample and 23% in the subsample of physicians involved in group practice mergers. A significant driver of the still-high error rate is survey nonresponse, which disproportionately affects integrated physicians, 50% of whom are erroneously classified as non-integrated.

An alternative way of using the SK&A data is to pool survey responses within each TIN-year. We pool responses by fitting a decision tree to predict integration status, where the only input is the share of physicians in each TIN-year reporting ownership by a given hospital/system. Cutoffs used for share of physicians are determined by a decision tree with one split; the split in the classification tree is determined by the largest decrease in Gini impurity. Results are qualitatively similar when we instead use a logistic regression with the same single regressor. Since the training sample is used to train the model and to assess performance, error rates below are out-of-sample error rates from repeated (three repeats) five-fold cross-validation. Pooling data across NPIs within a TIN improves performance substantially: full sample error rates drop from 41% to 6% and the "Group Practice Merger" subsample error rates drop from 23% to 14%.

In our preferred approach to assigning integration, we use a random forest model to predict each NPI's integration status with each candidate hospital/system. We tune the random forest parameters (number of trees and number of variables tried at each node of each tree) to reduce overall misclassification in repeated five-fold cross-validation. Ties are broken by the vote share (over trees) in the random forest; remaining ties are broken at random using a fixed seed. This algorithm improves significantly upon the SK&A-by-TIN model above, reducing the full sample error rate to 3% and the "Group Practice Merger" subsample error rate to 10%. Performance is significantly better in 2010–2016 than in 2008–2009 because of improvements in SK&A reporting in later years.

Several features of our analysis bear noting. First, we use a number of both geographic distance and string distance variables to fit the model. The latter is more difficult to interpret than the former, so we also fit models using no string distance variables; This causes out-of-sample error rates to increase from 2.7% to 3.6% for the full sample. Second, we use our full training sample to fit the model, which involves combining a pure random sample of TINs with the Levin sample, which explicitly upsamples TINs changing integration status, and the convenience cardiology sample, which explicitly overweights two particular MSAs. However, we have also trained and fit each model (outpatient billing, SK&A individual, SK&A-by-TIN, and random forest) on the random sample of TINs only and found the performance of each approach to be nearly identical.

A.4 Consistency of Predictions

Table A1 below displays the extensive margin concordance between different approaches we have implemented, for our full sample of NPI-years. Each column compares an alternative approach to the baseline, full sample random forest model. For example, the value of 0.975 under "No String" indicates that 97.5% of NPI-years had the same integration vs. non-integration status predicted in our full sample random forest model, whether or not string distance variables were employed in the model. Similarly, the value of 0.980 under "Random Only" indicates that 98% of NPI-years had the same integration status predicted in the random forest model, whether or not we restrict the training sample to the random sample of TINs. As expected, concordance is high for all random forest models, and lower for the SK&A and HOPD models. There is no clear pattern in which specific specialties have higher concordance rates for most comparisons, with the exception of HOPD. In the last column, we observe that concordance rates are lowest for hospital-based specialties like anesthesiology and radiology, where high outpatient billing rates would lead the HOPD approach to erroneously flag integration where none exists.

We have also compared the static predictions of our model to a large validated sample made available by the Agency for Healthcare Research and Quality (AHRQ). In 2015, AHRQ created an initiative to study health systems, in collaboration with researchers at Dartmouth College, the National Bureau of Economic Research, the RAND Corporation, and Mathematica Policy Research. AHRQ has since published

Table A1: Comparing Integration Predictions: Share of Physician-Years with Matching Integration Status

	No String	Random Only	Grouped SK&A	Individual SK&A	HOPD Billing
Overall	0.975	0.980	0.918	0.758	0.671
Anesthesia	0.972	0.981	0.923	0.783	0.401
Cardiology	0.980	0.975	0.914	0.763	0.702
Nephrology	0.985	0.986	0.917	0.822	0.807
Neurosurgery	0.978	0.979	0.908	0.737	0.688
Ophthalmology	0.990	0.991	0.932	0.876	0.891
Orthopedics	0.979	0.982	0.913	0.826	0.773
Otolaryngology	0.984	0.985	0.921	0.819	0.804
Plastic Surgery	0.987	0.989	0.925	0.842	0.627
Primary Care	0.979	0.982	0.927	0.751	0.675
Radiology	0.957	0.967	0.893	0.718	0.491
Urology	0.982	0.985	0.914	0.816	0.802

Notes: Each column compares a possible approach to determining integration status with the baseline random forest specification. Specifically, each column displays the share of physicians whose integration status matches that in the baseline random forest. "Cardiology" includes cardiologists and cardiac surgeons. "Anesthesia" includes anesthesiologists and pain management physicians.

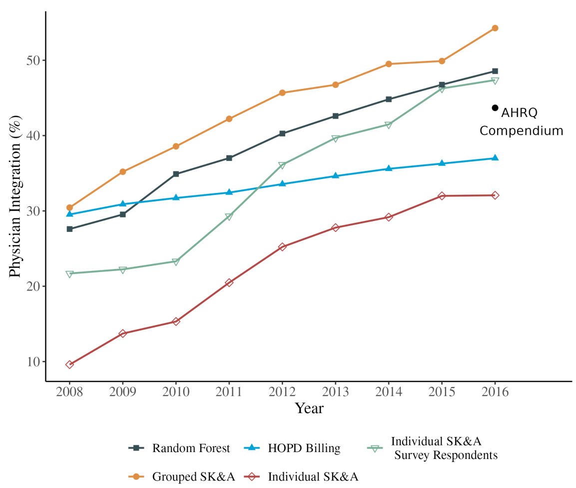
lists of US health systems for 2016 and 2018, including indicators for system ownership and provider affiliations with systems. By AHRQ's definition, "a health system includes at least one hospital and at least one group of physicians that provides comprehensive care (including primary and specialty care) who are connected with each other and with the hospital through common ownership or joint management." AHRQ's list explicitly excludes candidate systems without at least one general acute care hospital, 50 total physicians, or 10 primary care physicians. These and other exclusions imply that AHRQ's list is not fully comparable with our database, which is meant to detect all instances of hospital/health system ownership of physician practices.

Our full random forest specification disagrees with AHRQ for only 9% of NPIs. Disagreement is concentrated in four hospital-based specialties: pathology, radiology, emergency medicine, and anesthesiology. We performed further investigation of a random sample of TINs for which predictions between our algorithm and AHRQ differed. This analysis revealed two key patterns of disagreement. First, we found a number of instances where we were able to confirm ownership using SEC/IRS data, but no "health system" was flagged by AHRQ. This may be driven by those instances not meeting AHRQ's standard of health system due to type of hospital or number of physicians. Second, we observe that AHRQ occasionally flags integration for certain large TINs (e.g., hospital staffing companies) with tight affiliations to hospitals, but with physician or private equity ownership, rather than hospital/health system ownership.

A.5 Integration Prediction Methods and National Trends

For physicians in the continental United States, Figure A1 shows the national trends in integration predicted by each of the above algorithms, with the AHRQ prediction indicated for 2016 only.[24] Each algorithm predicts steady growth in integration between 2008 and 2016. Growth is flattest for the outpatient billing algorithm. By 2016, predicted integration is similar for the "Random Forest" and "Individual SK&A Survey Respondents" algorithms, a bit higher for "Grouped SK&A," and a bit lower for "AHRQ Compendium."

Figure A1: Comparing Trends in Integration Across Methods



Notes: This figure presents national trends in the percentage of physicians integrated with a hospital/system. Each series represents a different approach to predicting integration. The HOPD billing method (Neprash et al., 2015) uses a 25% billing threshold. The Grouped SK&A approach corresponds to "SK&A TIN Groupings" in Table 1. Level of integration based on the AHRQ Compendium is presented for 2016 alone (the only year it is available).

B Identifying Births in Claims Data

We construct a dataset of hospital-based births by first collecting all of the patients from 2011–2016 with inpatient claims containing one or more clinical codes indicating

[24]The Figure has two separate lines for "Individual SK&A Survey Respondents" (self-reported ownership among NPIs responding to the SK&A survey) and "Individual SK&A" (defaults to non-integration for all non-respondents). Unsurprisingly, the former is shifted upward relative to the latter.

a birth took place. We consider these the "focal" claims because they define the hospital admission as a relevant birth for our analysis. We also collect all claims with overlapping service dates, taking the first and last service data as the admission or discharge data.

We identify hospitals in the claims data using their names, addresses, and NPIs. An AHA hospital ID may correspond to multiple NPIs in the claims data. We consider the AHA ID a unique identifier for each hospital. Using the NPIs in the claims data, we match the physicians to our integration panel, which also allows us to observe each physician's specialty.

We define prices for both the physician and facility by summing the allowed amounts across all of the claims between admission and discharge separately for each provider. In practice, a patient may receive some aspects of care from multiple physicians within an admission. To focus on the most relevant physician, we restrict our attention to the OB-GYN with the highest allowed amounts associated with the admission. We also remove a small number of admissions for which the OBGYN performs births in facilities that are not general-surgical hospitals.

In order to ensure that we capture admissions with interpretable prices, we focus on admissions for which the patient was between 18 and 64 years old at admission. We then remove admissions defined by denied claims, as well as admissions in which the claims were subject to "coordination of benefits," meaning that another payer was considered as part of the payment determination. We also require all admissions to have at least one facility and at least one physician claim within the admission window. In order to avoid measurement complications from situations in which a patient is transferred between two facilities, we restrict to admissions in which the patient only has claims from a single facility. Finally, we require each patient to have a valid ZIP code of residence.

We categorize admissions based on the DRG code. DRG codes capture the severity and procedural intensity of an admission (i.e. whether the admission was for a vaginal delivery or cesarean section, whether the admission included major complications or patient comorbidities). For admissions with multiple DRGs, we privilege the DRG associated with the claims containing the largest allowed amounts. A small number of admissions do not contain a valid DRG. We classify all of these admissions into a "remainder" DRG, representing less than 2 percent of the admissions in the data.

Finally, we restrict to admissions with positive allowed amounts for both facility and physician services. We then exclude physicians below the 5th and above the 99th percentile of the price distribution for either physician or facility amounts. The resulting extract contains 696,802 admissions, performed by 22,811 physicians, at 2,304 unique hospitals.

C Demand Estimation

To carry out the demand estimation procedure, we require each patient's coordinate location at the time of their admission. We drop a small number of observations where this information is missing. We then drop observations if the distance between the patient and admission location exceeds 100 miles.

In a minority of patient HRR-years where the data are relatively sparse, the demand estimation has proven computationally infeasible. In such cases, the maximum likelihood algorithm either fails to converge, or nominally converges while producing imprecise coefficient estimates with abnormally large magnitudes.

We keep all demand estimates for a particular HRR-year if the estimation algorithm has successfully converged *and* if the distance coefficients are above the 1^{st} percentile and below the 99^{th} percentile of their respective empirical distributions. We then keep all provider-years for which we have demand estimates (for both hospital and physician demand models) for $\geq 99\%$ of their patients. We also require the providers to perform admissions in all years of the sample period, ensuring the composition of the regression sample does not change over time.

Table A2 shows how sequential cleaning restrictions affect the number of admissions and providers which are eligible to be used in the analytic regression sample.

Table A2: Data Cleaning for Demand Estimation and Regression Analysis

	(1)	(2)	(3)	(4)
	Hospital Regression Sample		Physician Regression Sample	
	Admissions	Hospitals	Admissions	Physicians
None	696,802	2,304	696,802	22,811
Patients with HRR identified from zip code	695,749	2,304	695,749	22,807
Patients with coordinates identified from zip code	695,745	2,304	695,745	22,807
Patients within 100 miles of admission location	683,709	2,293	683,709	22,697
Provider-years with demand estimation convergence for >99% of patients	681,980	2,291	681,602	22,658
Provider-years with convergence & well-behaved demand estimates for >99% of patients	675,529	2,285	673,542	22,585
Provider with full timing support	633,818	1,363	487,126	7,663
Well-defined pre-period HHIs	633,808	1,362	483,303	7,562

Notes: Sample size following sequential data cleaning restrictions imposed in preparation for, and following, demand estimation.

D ΔHHI Calculations

Consider a provider j experiencing an integration event in year T_j. j can be either a hospital or a physician. We calculate the change in (hospital or physician practice) HHI in j's market from $T_j - 2$ to T_j using the steps outlined below. Essentially, we calculate the change in HHI in the markets where j competes, focusing on j's competitors

1. Using the predicted patient choice probabilities (Equation 2), aggregate every provider's patient volume to the patient zip-code level in year $T_j - 2$, $\hat{P}_{z,T_j-2}(k) = \sum_{\{i:z(i)=z\}} \hat{P}_{i,T_j-2}(k)$, where $z(i)$ denotes the zip code of patient i.

2. Identify the set of patient zip codes where focal provider j has positive market share in year $T_j - 2$, $\mathcal{Z}_{j,T_j-2} = \left\{z : \hat{P}_{z,T_j-2}(j) > 0\right\}$.

3. Identify the set of provider j's competitors, consisting of all providers with a diversion ratio from j of at least 5 percent in $T_j - 2$, $\mathcal{C}_{j,T_j-2} := \left\{k : \hat{R}_{T_j-2}(k|\neg j) \geq 0.05\right\}$.

4. Among all providers in $\mathcal{C}_{j,T_j-2}$, aggregate predicted patient volumes from zip codes in $\mathcal{Z}_{j,T_j-2}$ to the firm level. This will depend on a (factual or counterfactual) ownership structure $\mathcal{W}$. If provider p represents a physician practice,

the firm is either: (1) the owning system if the practice is integrated; or (2) the practice itself if the practice is not integrated. If provider p represents a hospital, the firm is the owning hospital system. The aggregate patient volume of firm f in j's market in year $T_j - 2$ is:

$$\hat{P}^{(j)}_{f,T_j-2}\left(\mathcal{W}\right) = \sum_{k \in \mathcal{C}_{j,T_j-2}} w_{k,f}\left(\mathcal{W}\right) \sum_{z \in \mathcal{Z}_{j,T_j-2}} \hat{P}_{z,T_j-2}(k), \tag{4}$$

where $w_{k,f}(\mathcal{W})$ denotes the share of provider k owned by f under *ownership structure* $\mathcal{W}$. If k is a hospital, then $w_{k,f} \in \{0,1\}$. If k is a physician practice, then $w_{k,f} \in [0,1]$, which may be between 0 and 1 when a fraction of physicians initially in k are then acquired by system f. Let $\mathcal{F}^{(j)}_{T_j-2}(\mathcal{W}) := \{f : \hat{P}^{(j)}_{f,T_j-2}(\mathcal{W}) > 0\}$ denote the corresponding set of firms in focal provider (j)'s market in year $T_j - 2$ under ownership structure $\mathcal{W}$.

5. Calculate the HHI in focal provider j's market in year $T_j - 2$ under ownership structure $\mathcal{W}$ as follows:

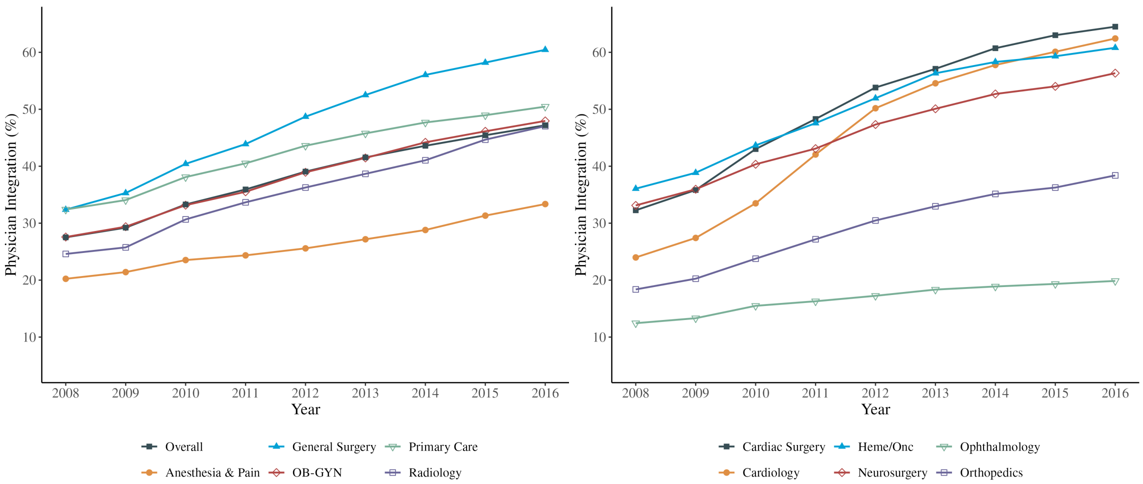
$$HHI^{(j)}_{T_j-2}\left(\mathcal{W}\right) = 10^4 \times \sum_{f \in \mathcal{F}^{(j)}_{T_j-2}(\mathcal{W})} \left(\frac{\hat{P}^{(j)}_{f,T_j-2}\left(\mathcal{W}\right)}{\sum_{f' \in \mathcal{F}^{(j)}_{T_j-2}(\mathcal{W})} \hat{P}^{(j)}_{f',T_j-2}\left(\mathcal{W}\right)} \right)^2.$$

6. Calculate ΔHHI in focal provider j's market due to transactions by j's T_j-owning system between $T_j - 2$ and T_j:

$$\Delta HHI^{(j)} = HHI^{(j)}_{T_j-2}(\mathcal{W}_{T_j}) - HHI^{(j)}_{T_j-2}(\mathcal{W}_{T_j-2}).$$

E Additional Figures

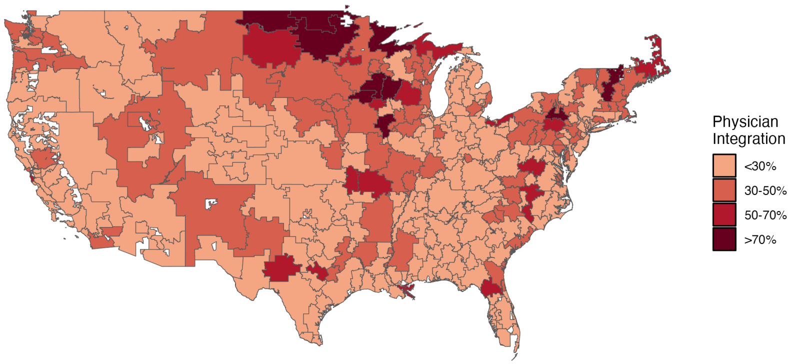
Figure A2: National Trends in Integration by Physician Specialty



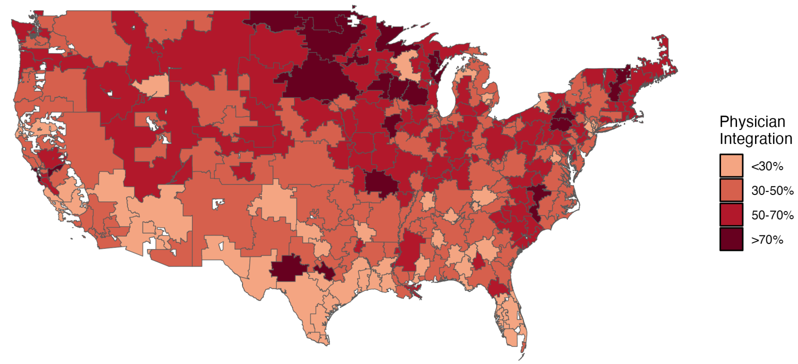
Notes: Physician integration separately by specialty. Physician integration is measured as the percentage of physicians that are integrated with a hospital/system in each year.

Figure A3: Geographic Distribution of Physician–Hospital Integration

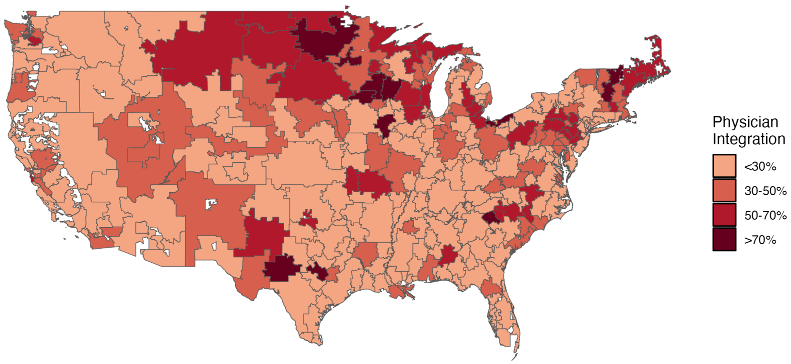
(a) All Specialties, 2008



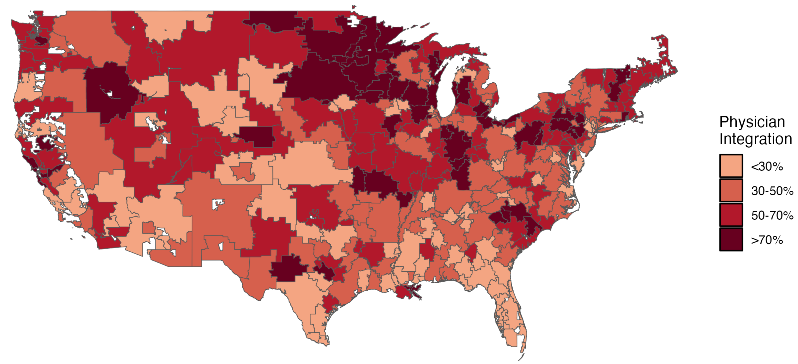
(b) All Specialties 2016



(c) OB-GYN, 2008



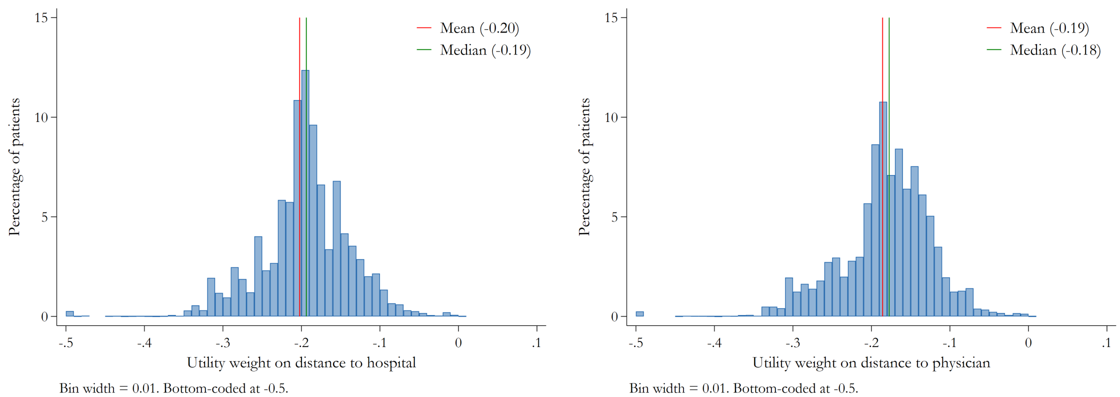
(d) OB-GYN, 2016



Notes: Maps of the contiguous US demonstrating the percentage of physicians in each hospital referral region (HRR) that are integrated with a hospital/system. Panels (a) and (b) show total physician integration in 2008 and 2016, respectively. Panels (c) and (d) show OB-GYN integration in 2008 and 2016, respectively. Data is presented for HRRs with at least 5 total physicians/OB-GYNs only.

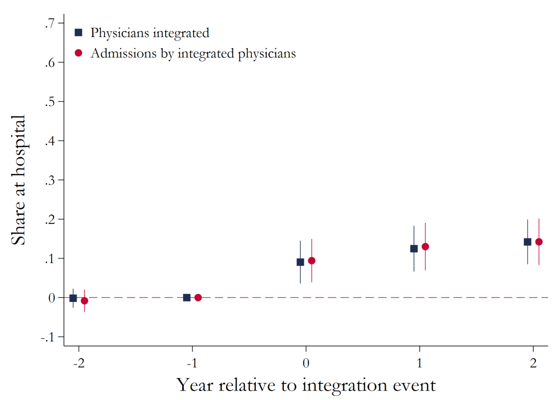
Figure A4: Distribution of Demand Model Distance Coefficients

(a) Hospital Demand Model Coefficients (b) Physician Demand Model Coefficients



Notes: Empirical distribution of distance coefficients resulting from demand estimation. Unit of observation is the patient.

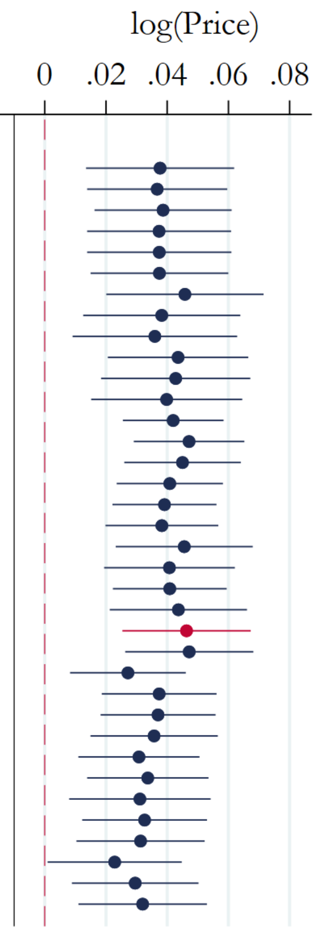
Figure A5: Event Study: Hospital Non-OB-GYN Integration Levels Around Hospital Integration Event



Notes: Hospital-level event study comparing hospital non-OB-GYN integration levels at treated hospitals to those of matched controls, in the five-year period covering the event. Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors.

Figure A6: Event Studies: Alternative Estimates of Hospital Total Price Effects Following Hospital Integration Event

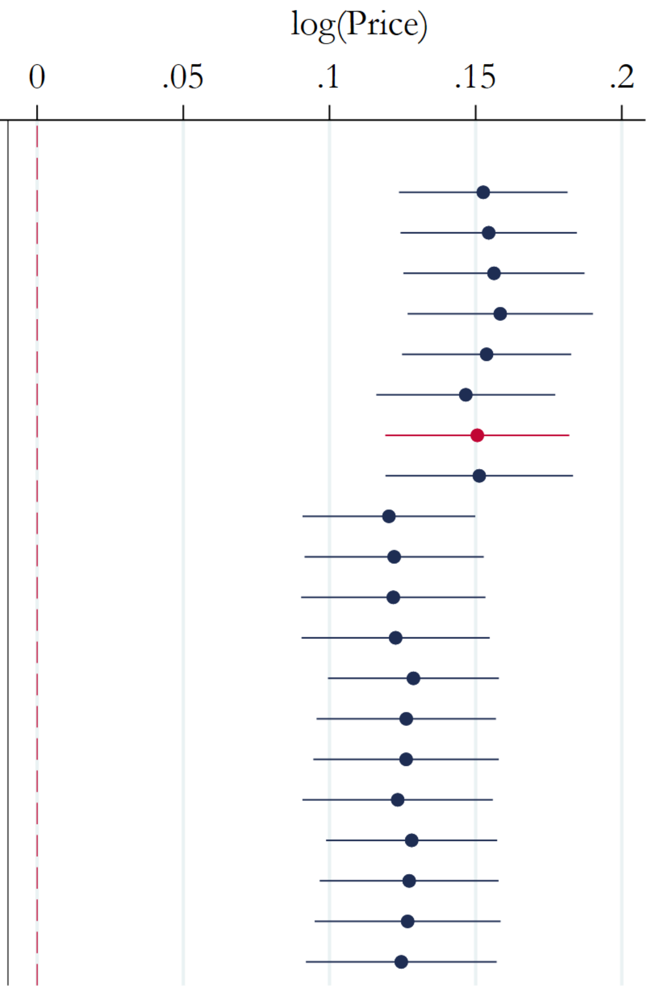
Integration LD	Integration SD	Max ΔExposure	Neighbors	Treated	Controls	Observations	Mean Price ($)
0.10	0.50	0.05	10	85	238	376,954	10,070
0.10	0.50	0.05	15	85	258	574,422	10,016
0.10	0.50	0.05	20	85	267	731,607	10,016
0.10	0.50	0.10	10	85	349	335,961	10,517
0.10	0.50	0.10	15	85	412	514,230	10,590
0.10	0.50	0.10	20	85	433	698,860	10,544
0.10	0.60	0.05	10	78	228	306,001	10,228
0.10	0.60	0.05	15	78	256	487,137	10,242
0.10	0.60	0.05	20	78	265	656,072	10,229
0.10	0.60	0.10	10	78	339	284,374	10,763
0.10	0.60	0.10	15	78	394	419,737	10,701
0.10	0.60	0.10	20	78	428	564,609	10,795
0.15	0.50	0.05	10	73	236	306,881	10,247
0.15	0.50	0.05	15	73	255	444,846	10,144
0.15	0.50	0.05	20	73	267	590,192	10,149
0.15	0.50	0.10	10	73	335	270,116	10,650
0.15	0.50	0.10	15	73	399	418,137	10,689
0.15	0.50	0.10	20	73	433	549,957	10,652
0.15	0.60	0.05	10	66	228	253,663	10,203
0.15	0.60	0.05	15	66	263	374,794	10,194
0.15	0.60	0.05	20	66	271	523,379	10,207
0.15	0.60	0.10	10	66	325	221,289	10,693
0.15	0.60	0.10	15	66	396	335,698	10,594
0.15	0.60	0.10	20	66	438	453,310	10,671
0.20	0.50	0.05	10	61	218	235,825	10,229
0.20	0.50	0.05	15	61	238	355,583	10,141
0.20	0.50	0.05	20	61	254	465,905	10,048
0.20	0.50	0.10	10	61	299	215,381	10,641
0.20	0.50	0.10	15	61	370	320,875	10,637
0.20	0.50	0.10	20	61	403	428,271	10,669
0.20	0.60	0.05	10	55	183	200,492	10,299
0.20	0.60	0.05	15	55	215	305,857	10,268
0.20	0.60	0.05	20	55	233	423,663	10,239
0.20	0.60	0.10	10	55	264	170,422	10,777
0.20	0.60	0.10	15	55	316	269,696	10,782
0.20	0.60	0.10	20	55	364	362,705	10,949



Notes: Hospital-level event study coefficients comparing price at treated hospitals to matched controls, in the two-year period after the event. Each coefficient is estimated using a different combination of thresholds defining treatment and control status, and treatment-control matching algorithm. Horizontal lines represent 95% confidence intervals based on hospital-match group-clustered standard errors. The red coefficient corresponds to our preferred specification presented in the main body of the paper. Integration LD ("long-difference") is the minimum percentage point increase in annual integrated admission share over the entire sample period required to be treated. Integration SD ("short-difference") is the minimum fraction of Integration LD which must occur in a single year for it to be considered an event. Max ΔExposure is the maximum percentage point increase in integration exposure allowed for controls.

Figure A7: Event Studies: Alternative Estimates of Physician Price Effects Following Physician Integration Event

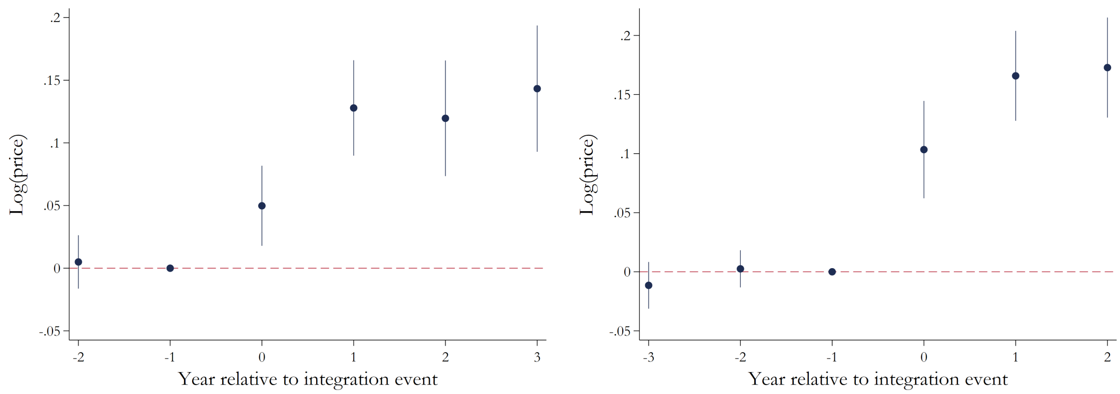
Max ΔExposure	Neighbors	Treated	Controls	Observations	Mean Price ($)
0.05	5	276	654	77,816	2,615
0.05	10	276	990	155,238	2,595
0.05	15	276	1,121	236,933	2,588
0.05	20	276	1,192	319,321	2,590
0.10	5	276	801	73,427	2,695
0.10	10	276	1,387	149,083	2,647
0.10	15	276	1,748	225,193	2,649
0.10	20	276	1,951	302,944	2,649
0.20	5	276	915	77,268	2,880
0.20	10	276	1,732	156,445	2,856
0.20	15	276	2,321	233,365	2,845
0.20	20	276	2,792	308,902	2,837
0.50	5	276	959	74,840	2,846
0.50	10	276	1,871	154,642	2,810
0.50	15	276	2,585	234,032	2,817
0.50	20	276	3,165	311,968	2,811
1.00	5	276	963	74,149	2,830
1.00	10	276	1,880	153,838	2,810
1.00	15	276	2,606	232,622	2,817
1.00	20	276	3,202	310,967	2,808



Notes: Physician-level event study coefficients comparing price at treated physicians to matched controls, in the two-year period after the event. Each coefficient is estimated using a different combination of thresholds defining control status, and treatment-control matching algorithm. Horizontal lines represent 95% confidence intervals based on physician-match group-clustered standard errors. The red coefficient corresponds to our preferred specification presented in the main body of the paper. Max ΔExposure is the maximum percentage point increase in integration exposure allowed for controls.

Figure A8: Event Studies: Physician Prices Around Physician Integration Event By Treatment Year with Longer Time Horizons

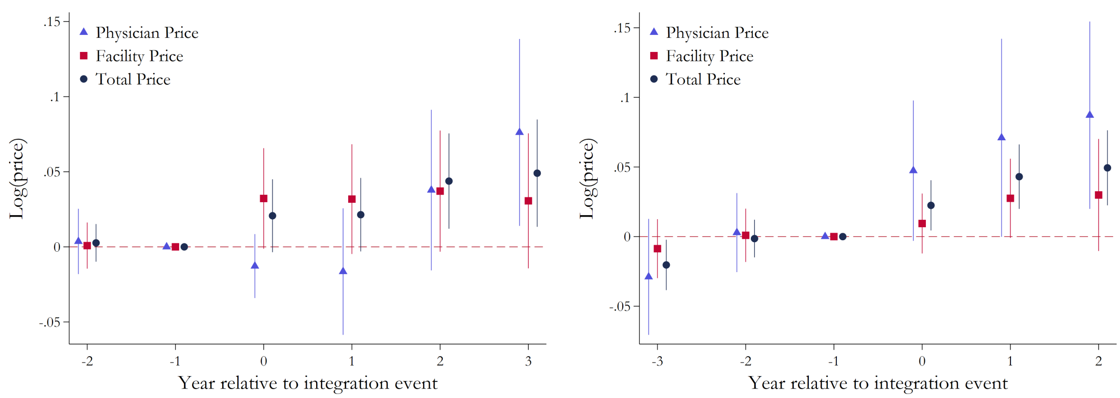
(a) 2013 Physician Integration Events　　(b) 2014 Physician Integration Events



Notes: Physician-level event studies comparing price of treated physicians to matched controls, in a six-year period covering the event. In panel (a), the coefficients are based on integration events in 2013. In panel (b), the coefficients are based on integration events in 2014. Vertical lines represent 95% confidence intervals based on physician-match group-clustered standard errors.

Figure A9: Event Studies: Hospital Prices Around Hospital Integration Event By Treatment Year with Longer Time Horizons

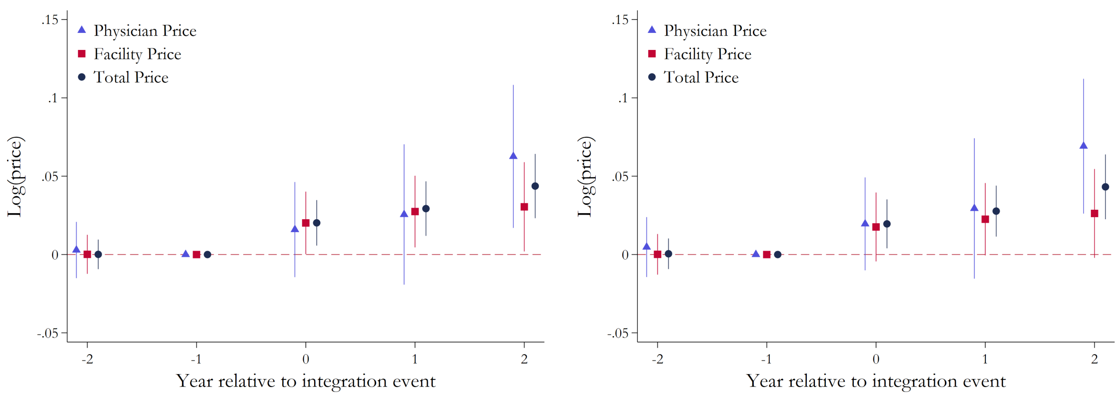
(a) 2013 Hospital Integration Events　　(b) 2014 Hospital Integration Events



Notes: Hospital-level event studies comparing price at treated hospitals to matched controls, in a six-year period covering the event. In panel (a), the coefficients are based on integration events in 2013. In panel (b), the coefficients are based on integration events in 2014. Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors.

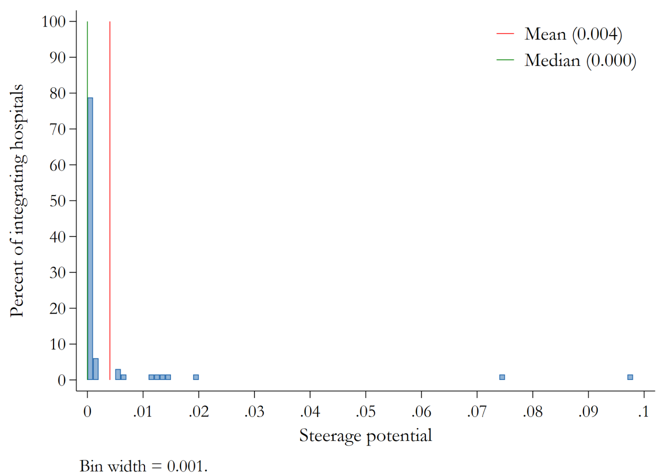
Figure A10: Event Studies: Hospital Prices Around Hospital Integration Event, Controlling for Other Contemporaneous Investments

(a) Non-OB-GYN Mergers (b) Horizontal Hospital Mergers



Notes: Hospital-level event studies comparing price at treated hospitals to matched controls, in the five-year period covering the event. In panel (a), hospital-year non-OB-GYN integration level (share of non-OB-GYN admissions performed by integrated physicians) is included as a covariate. In panel (b), hospitals experiencing an integration event in year T are dropped from the sample if hospital mergers/acquisitions by their owning system induce an increase in their local hospital HHI between $T-2$ and T. Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors.

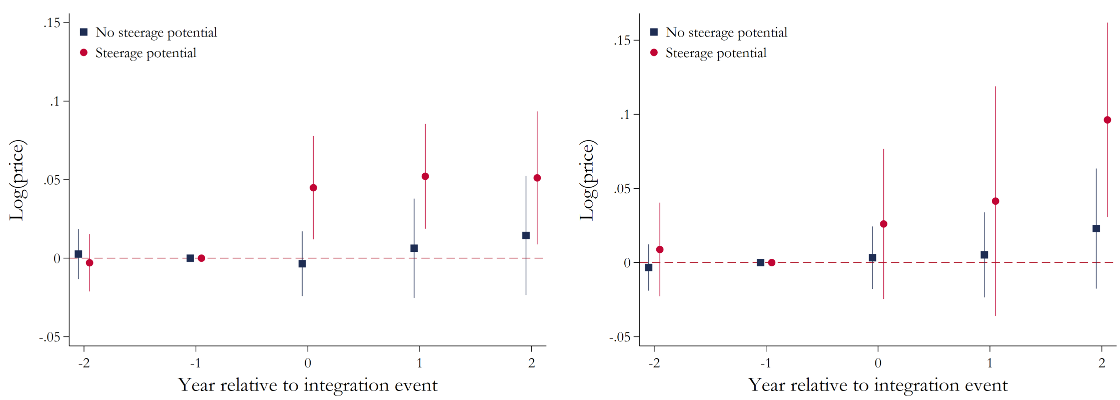
Figure A11: Steerage Potential of Hospital Integration Events



Notes: The unit of observation in this histogram is a hospital experiencing an integration event. The histogram presents the empirical distribution of a weighted average of the share of admissions at the integrating hospital's competitors performed by physicians acquired during the event. The shares are computed using data from the two pre-event years. The weights are equal to the mean diversion ratio from the focal integrating hospital in the two pre-event years. We say the hospital integration event has steerage potential if the resulting quantity is positive.

Figure A12: Event Study: Facility and Physician Prices Around Hospital Integration Event By Steerage Potential

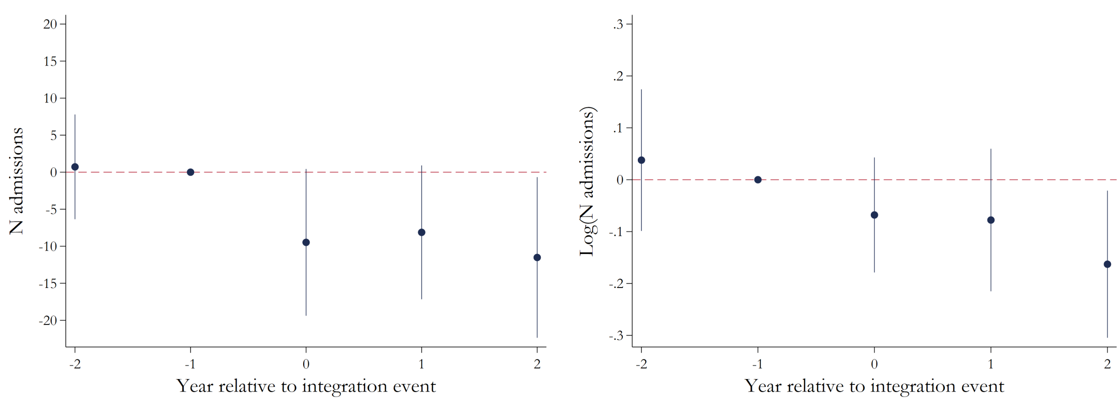
(a) Log Facility Prices (b) Log Physician Prices



Notes: Hospital-level event studies comparing prices at hospitals experiencing integration events, with and without steerage potential, to matched controls. Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors.

Figure A13: Event Study: Admission Volume Around Hospital Integration Event

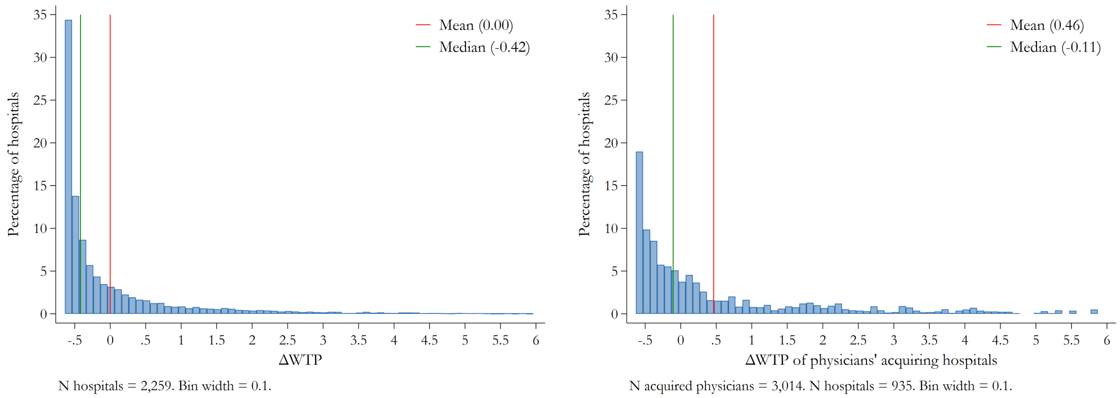
(a) Admission Volume (b) Log Admission Volume



Notes: Hospital-level event studies comparing the number of admissions at treated hospitals to matched controls. Vertical lines represent 95% confidence intervals based on hospital-match group-clustered standard errors.

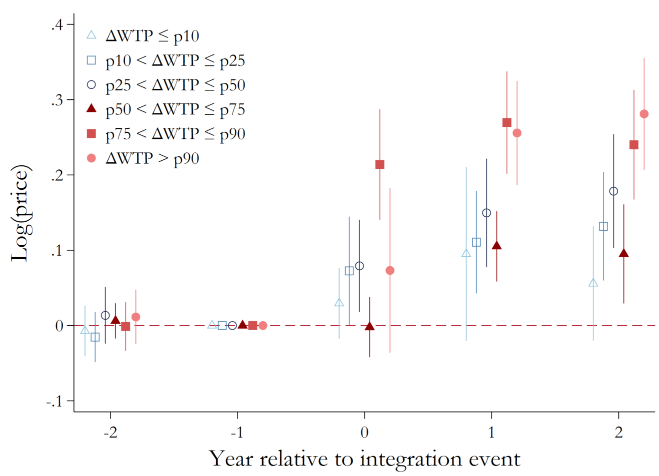
Figure A14: Distribution of Hospital Willingness to Pay

(a) All Hospitals (b) Hospitals Involved in Physician Events



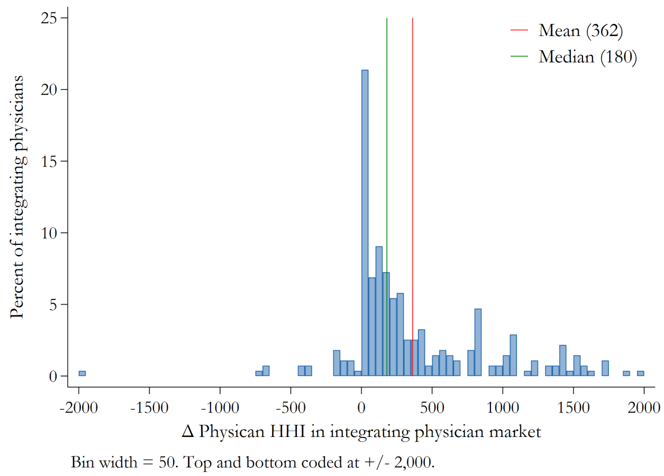
Notes: Figure presents empirical distributions of aggregate consumer WTP for a hospital. As discussed in Appendix Section C, we limit to the set of provider-years for which we have demand estimates for at least 99% of patients. To remove outliers, we truncate the ΔWTP distribution at the 99th percentile. Panel (a) shows the distribution among all hospital-years in the sample, subject to our sample restrictions. Panel (b) shows the distribution among hospitals involved in physician events analyzed in our main regression sample, measured in the year prior to the event (conditional on the hospital performing admissions in this year).

Figure A15: Event Study: Physician Prices Around Physician Integration Event By Acquiring Hospital ΔWTP



Notes: Price of treated physicians compared to controls, where physician integration events are categorized by the acquiring hospital *ex ante* willingness to pay. Coefficients are estimated using triple-difference regressions, where the treatment indicators are interacted with a measure of the acquiring hospital ΔWTP. Vertical lines represent 95% confidence intervals based on physician-match group-clustered standard errors.

Figure A16: Distribution of Physician ΔHHI in Markets with Physicians Integration Events



Notes: Empirical distribution of physician ΔHHI in the markets of physicians experiencing integration events. ΔHHI is calculated from two years before the focal event until the year of the event. These ΔHHI measures account for changes of ownership due to transactions carried out by the integrating physicians acquiring system during this time period, holding fixed pre-event market shares. For details, see Appendix Section D.

F Additional Tables

Table A3: Summary Statistics of Admissions in Analytic Regression Samples

	(1)	(2)
	Hospital Regression Sample	Physician Regression Sample
Providers	462	2,024
Admissions	195,810	117,906
Mean Patient Age	31.34	31.30
Mean Patient Distance Traveled	10.11	10.14
Mean C-Section	0.38	0.37
Mean Price Per Vaginal Delivery	9,586.68	9,104.46
Mean Facility Price Per Vaginal Delivery	6,834.98	6,505.68
Mean Physician Price Per Vaginal Delivery	2,751.71	2,598.78
Mean Price Per C-Section	12,366.80	12,029.31
Mean Facility Price Per C-Section	9,413.87	9,194.38
Mean Physician Price Per C-Section	2,952.93	2,834.94
Total Prices	2,082,135,389.36	1,200,289,329.94
Total Facility Prices	1,528,488,207.44	883,637,916.49
Total Physician Prices	553,647,181.16	316,651,413.04

Notes: Summary statistics of admissions in analytic regression samples.

Table A4: Construction of Physician Treatment and Control Groups

	(1)	(2)
Restriction following Table A2	Admissions	Physicians
Treated physicians		
None	483,303	7,562
Pre-period characteristics for matching	463,748	7,285
Integrating during sample period (2011–2016)	46,480	799
Integrating in 2013 or 2014	14,404	276
Admissions in five-year window around event	12,142	276
Control physicians		
None	483,303	7,562
Pre-period characteristics for matching	463,748	7,285
Never or always integrated	402,917	6,242
$<$10pp integration exposure long-difference	162,246	2,489
15 nearest neighbors to treated physicians	116,445	1,748
Admissions in five-year window around event	105,764	1,748

Notes: Sample size during construction of physician treatment and controls groups.

Table A5: Construction of Hospital Treatment and Control Groups

	(1)	(2)
Restriction following Table A2	Admissions	Hospitals
Treated hospitals		
None	633,808	1,362
$>$15pp integration long-difference	169,472	360
$\geq$60% of integration long-difference occurring in 2013 or 2014	48,787	99
$\leq$5% of LD occurring before potential event	31,532	66
Admissions in five-year window around event	26,520	66
Control hospitals		
None	633,808	1,362
$<$10pp integration long-difference	423,338	945
$<$10pp integration exposure long-difference	234,309	471
15 nearest neighbors to treated hospitals	191,092	396
Admissions in five-year window around event	169,290	396

Notes: Sample size during construction of hospital treatment and controls groups

Table A6: Event Study: Log Hospital Prices Around Hospital Integration Event

	(1)	(2)	(3)
	log(total price)	log(facility price)	log(physician price)
T-2	–0.000	–0.000	0.003
	(0.005)	(0.006)	(0.009)
T	0.022***	0.022**	0.015
	(0.008)	(0.011)	(0.015)
T+1	0.032***	0.030**	0.024
	(0.009)	(0.012)	(0.022)
T+2	0.046***	0.033**	0.061***
	(0.011)	(0.015)	(0.022)
Observations	335,698	335,698	335,698
Treated	66	66	66
Controls	396	396	396
Clusters	990	990	990
Mean Price	10,593.56	7,773.91	2,819.65
R-Squared	0.766	0.777	0.507

Notes: Table presents hospital-level event study comparing log price at treated hospitals to matched controls, in the five-year period covering the event. Hospital-match group-clustered standard errors are shown in parentheses. Stars indicate the statistical significance of the coefficient estimate: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Event Study: Hospital Prices Around Hospital Integration Event

	(1)	(2)	(3)
	total price	facility price	physician price
T-2	–67.184	–75.466	8.282
	(57.745)	(53.622)	(29.076)
T	273.309***	217.416**	55.893
	(96.504)	(102.374)	(56.425)
T+1	445.621***	360.567***	85.053
	(113.333)	(117.149)	(80.172)
T+2	713.155***	474.865***	238.290***
	(150.229)	(160.227)	(79.195)
Observations	335,698	335,698	335,698
Treated	66	66	66
Controls	396	396	396
Clusters	990	990	990
Mean Price	10,593.56	7,773.91	2,819.65
R-Squared	0.744	0.743	0.517

Notes: Table presents hospital-level event study comparing price at treated hospitals to matched controls, in the five-year period covering the event. Hospital-match group-clustered standard errors are shown in parentheses. Stars indicate the statistical significance of the coefficient estimate: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Event Study: Physician Prices Around Physician Integration Event

	(1)	(2)
	log(physician price)	physician price
T-2	0.003	8.951
	(0.007)	(19.875)
T	0.080***	287.000***
	(0.014)	(49.705)
T+1	0.150***	475.757***
	(0.014)	(50.080)
T+2	0.151***	501.776***
	(0.016)	(54.828)
Observations	225,193	225,193
Treated	276	276
Controls	1,748	1,748
Clusters	4,140	4,140
Mean Price	2,649.47	2,649.47
R-Squared	0.720	0.751

Notes: Table presents physician-level event study comparing prices of treated physicians to matched controls, in the five-year period covering the event. Physician-clustered standard errors are shown in parentheses. Stars indicate the statistical significance of the coefficient estimate: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.