Physician Practice Organization and Negotiated Prices: Evidence from State Law Changes*

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August 31, 2019

Abstract

We study the relationship between the organization of physician practices and prices negotiated with private insurers. Using variation from state-level judicial decisions, we show that changes in the enforceability of non-compete agreements (NCAs) in physician employment contracts alter the organization of physician practices and the service prices they charge. The effects of these NCA decisions are economically meaningful: an increase in NCA enforceability of 10% of the observed policy spectrum causes a 4.3% increase in average physician prices. Using two databases containing the universe of physician establishments and firms in the US between 1996 and 2007, linked to prices negotiated with private insurance companies, we show that this price effect is associated with reductions in practice sizes and market concentration that increase prices for services with high practice overhead costs. Using these judicial decisions as instruments, we estimate that a 100 point increase in the establishment-based Herfindahl Index (HHI) causes a 1.4% to 1.9% decline in prices, consistent with insurers extracting efficiency gains from larger establishments. In contrast, the same change in concentration caused by physically-distinct establishments negotiating jointly as a firm leads to price increases of 1.7% to 2.1%.

*We are grateful to Jay Bhattacharya, Jeff Clemens, Leemore Dafny, Michael Dickstein, Will Dow, Alon Eizenberg, Randy Ellis, Josh Gottlieb, Arthur Lewbel, Ian McCarthy, Jesse Rothstein, Ashley Swanson, and seminar participants at the AEA Annual Meeting, ASHE, Berkeley, Chicago Booth Junior Economics Summit, DOJ, Hebrew University, IDC, LSE, MIT, NBER Productivity Lunch, NBER Summer Institute, Northwestern Kellogg, NYU, Stanford, Tel Aviv University, and UGA for helpful comments, to Norman Bishara for sharing legal data, and to Eric Auerbach, Richard Braun, Akina Ikudo, and David Krosin for research assistance. This research was conducted while Lavetti was a Robert Wood Johnson Foundation Scholar in Health Policy at UC Berkeley, and their support is gratefully acknowledged. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. Correspondence: lavetti.1@osu.edu

1 Introduction

At 17.2% of GDP, the share of income devoted to healthcare in the US is over 90% higher than the OECD average.[1] Many studies, including Pauly (1993) and Anderson et al. (2003), have shown that this difference in spending is primarily due to differences in prices rather than quantities, which has led researchers to try to understand why prices are so much higher in the US. Much of this attention has focused on how competition affects prices in health insurance markets (Dafny (2010); Dafny et al. (2012); Ericson and Starc (2012); Ho and Lee (2016)) and in hospital markets (Gowrisankaran et al. (2014); Gaynor and Vogt (2003)). There is relatively less evidence on the determinants of physician prices, even though physician services account for a large and rising share (20%) of total U.S. medical spending.[2] Previous research has found evidence that prices of physician services are higher in more concentrated markets, based on across-market comparisons of specialist prices (Dunn and Shapiro (2014); Kleiner et al. (2015)), and within-market changes in prices over time (Baker et al. (2014)).

This paper provides new evidence on the impacts of physician practice organization and market concentration on prices negotiated with private insurers. We quantify the extent to which organizational changes at the establishment and firm levels may have different effects on prices.[3] These differences could occur, for example, due to the importance of efficiency gains relative to improvements in bargaining position when physician practice growth occurs within versus across locations. To provide evidence on these organizational dynamics, our research design builds upon and extends previous studies in two primary ways.

First, we develop the most comprehensive known database to date on physician practices and negotiated prices, including two complete censuses of practices in all specialties and geographic markets in the US between 1996-2007. The Medicare Physician Identification and Eligibility Registry (MPIER) from the Center for Medicare and Medicaid Services (CMS), which contains all practicing physicians in the US, allows us to aggregate physicians by practice location and measure establishment sizes by specialty and geography. In addition, we use confidential Census Bureau data from the Longitudinal Business Database (LBD), Economic Censuses (EC), and Business Register (SSEL) to observe firm-level linkages across establishments based on IRS tax IDs, and to measure total payroll and sales from all sources for each firm. We link these databases to Truven

[1]See OECD Health Statistics 2017

[2]National Health Expenditure Fact Sheet 2013, CMS.

[3]We define an establishment as a specific physical practice location, differentiated by mailing addresses. In contrast, firms may own multiple establishments, and we identify firms by IRS tax IDs.

Health Analytics MarketScan data on ambulatory care (non-hospital) prices negotiated between physicians and a large sample of private commercial insurance companies covering every state in the US. Together, these data provide a uniquely comprehensive panel of virtually every physician market nationwide over twelve years.

Second, we address a fundamental challenge of potentially endogenous practice organization choices. We do this by constructing a new panel database of state-level law changes that affect physicians' organizational incentives and practice sizes. The database quantifies judicial decisions that change the enforceability of non-compete agreements (NCAs), which restrict an employee's ability to leave a firm and compete against it. As documented by Bishara (2011), NCA laws vary along seven dimensions across states and over time. Following Bishara's methodology, we measure each of these legal dimensions for every state-year during the sample period. We then trace the effects of these judicial decisions through changes in organizational incentives, practice structures, and market concentration to measure the impacts of these practice characteristics on negotiated prices.[4]

To build intuition for the effects of NCAs on physician practice organization, we present a stylized framework in which practices can use NCAs to encourage efficient within-firm patient referrals, which increases productivity but also increases average costs because physicians must be compensated for accepting an NCA. In this framework, increases in NCA enforcement policies affect establishments and firms differently. At the establishment level, an increase in enforceability can decrease efficient establishment sizes and increase average costs. At the firm level, greater enforceability can potentially reduce merger frictions by preventing physicians from responding to a proposed merger by spinning-off and poaching patients from the practice. In our framework, multi-establishment firms provide greater convenience to consumers, but the risk of spinoffs makes such a union difficult to create. Thus changes in NCA enforcement can provide differential incentives for growth at the establishment versus firm levels.

We provide a variety of evidence on the effects of NCA law changes on physician practice organization. Changes in NCA enforceability significantly affect the rate of physician-establishment job separations and the creation of new establishments, which in turn affects the distribution of establishment sizes. Our controlled event-study estimates suggest that an average law change increasing NCA enforceability causes a 168 point decline in the

[4]NCA law has been used previously as a source of variation in important work by Fallick et al. (2006), Marx et al. (2009), and Garmaise (2009). These papers focus on a few specific law changes (in Michigan, Texas, Florida, and Louisiana) or cross sectional differences (Massachusetts vs. California) rather than using the full panel of judicial law changes on all seven legal dimensions and in all U.S. states, as we do. Lavetti et al. (2018) provide evidence from survey data that the use of NCAs in physician employment contracts is very common, with about 45% of primary care physicians in group practices bound by NCAs.

establishment-based HHI within 2 years and a slightly smaller increase in the firm-based HHI.

We use these law changes, which alter the organization and concentration of physician markets without directly affecting insurers, as IVs to estimate the effect of concentration on prices. The richness of our data allows us to control for unobserved heterogeneity across geographic markets as well as for census-division-by-year effects, medical specialty effects, service procedure code effects, and medical facility type effects. In addition, our unique ability to observe both establishments and firms enables us to estimate the marginal effect on prices of increasing establishment concentration conditional on firm concentration, and vice versa.

We find that changes in concentration have heterogeneous effects on negotiated prices that depend on the structural nature of the changes. Increases in concentration caused by the growth of physician establishments lead to negative price effects, while increases in concentration due to the growth of firms that may have physically distinct establishments cause prices to rise. Specifically, we find that a 100 point increase in the *establishment*-based HHI causes a reduction in negotiated prices of about 1.4% to 1.9% on average. In contrast, the same increase in concentration caused by firm-level consolidation holding fixed establishment concentration causes prices to increase by 1.7% to 2.1%. OLS specifications imply very small (but statistically significant) positive price effects of 0.02% or less, consistent with within-state evidence from Baker et al. (2014).[5]

Taken together, these results suggest that the effects of consolidation on prices depend on a tradeoff between the efficiency gains of larger establishments and the improved negotiating position associated with bargaining as a larger organization. To the extent that larger establishments have better bargaining position, any consequent positive effect on prices is outweighed by insurers extracting cost reductions due to economies of scale, resulting in a net negative price effect. These economies of scale could be due, for example, to shared nursing, laboratory, technological, and administrative resources among more physicians. However, when practices grow larger through multi-establishment expansion, the net effect on prices is positive, implying that any economies of scale from mergers of physically-distinct practices have smaller effects on prices than does the associated change in bargaining position. Although the variation in practice organization (caused by NCA law changes) underlying our estimated local average treatment effects may differ to some extent from the margin of variation occurring more broadly in physician markets, such as hospital acquisitions of physician practices, our estimates indicate that price effects

[5] Baker et al. (2014) use Marketscan price data and estimate market concentration using Medicare data.

come predominantly from the channel of establishment-level growth. The negative net relationship between concentration and prices suggests there may be important efficiency gains from physical consolidation of practices.

Identifying the effects of physician practice organization on service prices is a challenge because of the many features of medical care markets that are co-determined with concentration and prices. The use of a new set of instrumental variables which affect physician organization without directly affecting prices brings a new identification approach to this important but challenging question. To further assess the instruments, we evaluate several alternative mechanisms—in addition to physician practice organization and associated costs—through which judicial decisions on NCAs could potentially generate the effects we find. Survey data from Lavetti et al. (2018), which links information on whether physicians have signed NCAs to data on service prices and quality measures, provides evidence against the hypothesis that changes in NCA enforceability might generate quality differences between firms due to physician sorting. We also test whether changes in NCA enforceability affect the total number of physicians in a market through entry or exit and find no such evidence. Since health insurers do not tend to use NCAs, it is unlikely that changes in insurer organizational structure would be affected by these legal changes. We corroborate this non-responsiveness empirically and also control for insurer concentration in our main model specifications.

Previous research that has addressed this topic suggests that physician organizational structures can have effects on prices that vary by context. Kleiner et al. (2015) and Dunn and Shapiro (2014) find evidence consistent with market power among specialist physicians when they compare concentration levels across markets, while Baker et al. (2014) estimate that a 1000-point increase in HHI over time, within market increases office visit prices by about 1%-2%.[6] More recently, Clemens et al. (2017) show that the extent to which privately negotiated prices track changes in Medicare prices depends on both practice size and the cost structure of procedures. Specifically, they find that among capital-intensive procedures, for which average costs are more likely to differ from marginal costs, insurers appear to extract a share of the economies of scale from larger physician practices. Taken together, these studies suggest that physician organizational structures can have important effects on prices—potentially through cost efficiencies that may counteract the effects of improvements in bargaining position relative to insurers.

Our findings support the importance of physician organizational structures and highlight a significant role of state NCA policies in affecting healthcare markets. We show that

[6]Clemens and Gottlieb (2016) also find evidence consistent with the presence physician market power, although they do not directly estimate the magnitude of the effect of market structure on prices.

a judicial decision decreasing NCA enforceability by 10% of the observed policy spectrum (about 0.39 standard deviations) causes physician prices to fall on average by 4.3%. This estimate suggests that such a policy change at the national level would reduce aggregate medical spending by over $25 billion annually. Despite the important role of NCAs, 39 states have never comprehensively reviewed and legislated NCA policies, and instead the law itself is defined by the set of case-specific judicial decisions.

The paper is structured as follows. Section 2 provides background on non-compete laws and their usage by physicians. Section 3 presents a conceptual framework of the effects of NCAs on practice organization. Section 4 describes the data sources. Section 5 presents evidence on the impacts of NCA laws on practice organization. Section 6 describes our main empirical model, IV results, and a variety of robustness tests. Section 7 concludes and discusses the policy implications of our findings.

2 Background: Non-Compete Laws and Physicians

NCA Laws and Changes: Non-compete agreements are clauses of employment contracts that prohibit an employee from leaving a firm and competing against it. In the case of physicians, who compete in local geographic markets, NCAs prohibit practicing medicine within a specified geographic area and fixed period of time. Physicians bound by an NCA who leave their firm must either exit the geographic market, wait until the NCA has expired, or take a job outside of medicine.[7] Common physician NCAs restrict competition within 10-15 mile radii for 1-2 years. Allowable radii depend in part on how far patients generally travel to see a doctor, which can vary across urban and rural markets, and by physician specialty. However, since the enforceability of NCAs is determined by state law, there is also a large degree of variation across states in how restrictive these contracts can be. For example, some states do not allow employment-based NCAs to be enforced at all, while other states allow them to be easily enforced with broad market definitions and/or long durations.

The permissibility of NCAs dates back to at least 1621 under English common law, and 39 US states still follow common law in determining the enforceability of NCAs. Thus, historical precedent is the main determinant of NCA policies in most states. However, states that follow the same common law origins have diverged dramatically in their enforcement of NCAs. For example, Kansas has the second highest NCA enforceability

[7] In some states contracts with NCAs are required to specify a buyout option. For example, Sorrel, AL (2008) describes a case in Kansas in which a physician had a buyout option of paying her former practice 25% of her earnings during the NCA restriction period.

measure while North Dakota has the lowest measure, despite the fact that both states follow legal traditions that were heavily influenced by English common law.

Common law requires judges to consider three specific questions when evaluating NCA contracts. First, does the firm have a legitimate business interest that is capable of being protected by an NCA? Second, does the NCA cause an undue burden on the worker? And third, is the NCA contrary to the public interest? Changes in the interpretation and relative importance of these questions have caused judicial decisions to break from precedent. Under common law, a judge's decision to deviate from precedent has the effect of changing the law going forward.

The vast majority of these policy changes involve legal cases unrelated to healthcare markets. For example, in Shreveport Bossier v. Bond (2001) a Louisiana construction company attempted to enforce an NCA against a carpenter. The state Supreme Court ruled that the NCA could only prevent the carpenter from establishing a new business, but not from joining a pre-existing firm. This decision abruptly changed the law in the state, allowing all workers, including employed physicians, who had previously signed NCAs to escape the restrictions and move to other firms.

To take advantage of the rich variation in the relevant legal environments, we quantify variation in NCA laws across states and 52 law change events during our study period (28 that strengthen NCA enforceability, and 24 that weaken it) using the methodology developed by Bishara (2011). These data are described in detail in Section 4.4.

Physician Markets and the Use of NCAs: In order to understand the mechanism behind these instruments, it is useful to know what motivates physician practices to use NCAs. Lavetti, Simon, and White (2019) study this question and conclude that physician practices use NCAs primarily to deter physicians who exit a group practice from taking clients with them to another firm. In firms that provide skilled services, information asymmetries between clients and service providers make it costly for clients to search for new providers, generating loyalty towards providers. The loyalty of patients to their doctors is arguably the most valuable asset of most physician practices—the stock of patients is often the basis for determining a price when practices are sold—but firms have no direct property rights or control over these valuable assets. They are threatened by the possibility that steering patients to a new physician who joins the practice could lead to losing the patients if the physician were to exit the practice and the patients were to follow.[8] NCAs can prevent this type of loss.

Lavetti et al. (2019) find that about 45% of primary care physicians in group practices

[8] Sabety (2019) provides evidence for the strength of patient loyalty to their physicians using variation from physician exits.

are bound by NCAs on average, where use ranges in a five state sample from about 30% in California, a low enforceability state, to 66% in Pennsylvania. They also show that NCAs are used more frequently in practice settings where ongoing patient relationships are more valuable, such as in office-based practices as opposed to in hospitals, and in metro or micropolitan markets where the supply of physicians is larger relative to the population, making patient stocks more valuable.

Our empirical analyses suggest that NCA enforceability is generally negatively correlated with physician practice sizes and market concentration. Although explaining the nuances of all of the legal dimensions of NCAs is beyond our space constraints (we provide a brief overview in Appendix Table A2), an example of one dimension of the law called the 'Employer Termination Index' measures the extent to which state law allows a firm to fire a worker and still enforce the NCA. In some states this action would be legal, while in other states NCAs can only be enforced if the worker quits. An increase in this component of the law causes a spike in job separations and a significant decrease in establishment concentration as it becomes less costly for firms to fire workers, and as workers tend to move to smaller practices or start new practices. In contrast, another component of the law called the 'Blue Pencil Index' measures the extent to which NCA clauses that are overly restrictive to workers can be modified by judges ex post and thus still enforced. This dimension of the law is the only one that is positively correlated with concentration; this correlation could occur if increases in this dimension make it harder for physicians to escape pre-existing NCA agreements, leading practices to grow larger over time by deterring exits. Each of the seven dimensions of NCA law undergoes a number of state level judicial changes during our sample period (1996-2007), generating exogenously timed variation in physician concentration in the affected state relative to nearby states. In Sections 6.2 and 6.7 we present evidence supporting the exogeneity of the law changes, including a lack of pre-trends in either concentration or prices, and we show that there is no clear correlation between law changes and state-level economic or political measures.

3 NCA Laws and Practice Organization

To build intuition behind the first-stage effect of NCAs on practice organization, we consider a simple model of physician practice organization with NCAs. A fixed number of physician practices split demand in a market, with each practice receiving D patient visits. To reflect that fact that most consumers have insurance for the product being purchased, prices are assumed to be fixed ex-ante by contract with insurers, and consumers pay nothing out-of-pocket.[9] For simplicity, we consider a symmetric uniform price p.

The production of patient visits requires physician labor, L, and a fixed amount of capital, $\overline{K}$. Each practice either requires all physicians to sign NCAs or doesn't use NCAs at all. As Lavetti, Simon and White (2019) discuss, one effect of NCAs is that they can facilitate intra-firm patient referrals by reducing the risk that a referred patient will be poached. This ability to refer leads to a more balanced distribution of patients across physicians within the practice and can increase the average product of labor at the firm. To incorporate this feature, we assume that output in the firm using NCAs is given by:

$$g(L,1) = L^{\alpha_1}\overline{K}^{\beta}$$

Firms without NCAs have output

$$g(L,0) = L^{\alpha_0}\overline{K}^{\beta}$$

where $1 > \alpha_1 > \alpha_0 > 0$. We first consider perfectly-enforceable NCAs and then relax the assumption.

For output to meet quantity demanded, firms require labor inputs:

$$L_1^\star = \left(\frac{D}{\overline{K}^{\beta}}\right)^{1/\alpha_1} \qquad L_0^\star = \left(\frac{D}{\overline{K}^{\beta}}\right)^{1/\alpha_0}$$

If $\alpha_1 > \alpha_0$ then $L_1^\star < L_0^\star$. The higher productive efficiency of labor in firms that use NCAs reduces conditional factor demand for labor.

In addition to affecting α, NCAs also affect costs by increasing wages. Since workers require a compensating wage difference to accept a restriction on their post-employment options, $w_1 > w_0$. The derivative of costs with respect to the decision to use NCAs equals:

$$\frac{dC}{dNCA} = \frac{\partial w}{\partial NCA} L_0^\star + w_1\left[L_1^\star - L_0^\star\right]$$

The cost of production is greater when NCAs are used if:

$$\left(\frac{D}{\overline{K}^{\beta}}\right)^{\frac{\alpha_0-\alpha_1}{\alpha_0\alpha_1}} + \frac{\frac{\partial w}{\partial NCA}}{w_1} > 1 \tag{1}$$

Why would a physician practice choose to use NCAs if they increase the cost of labor? As Lavetti, Simon and White (2019) discuss, although NCAs increase wages they provide

[9] Note that this model focuses on the first-stage impacts of NCAs on organizational structure, and says nothing about equilibrium prices or bargaining.

firms with a form of insurance against the risk that physicians may exit the practice and take patients with them. We introduce this feature of NCAs below. Condition 1 says that NCAs will increase average costs if their effect on wages is sufficiently large relative to their effect on α.

3.1 Changes in NCA Enforceability

Now suppose that NCAs are imperfectly enforceable, and the ex ante probability that an NCA will be enforced is determined by a policy parameter θ. Output is given by a mixture of the NCA and non-NCA production functions:

$$g(L,\theta) = L^{[\theta\alpha_1+(1-\theta)\alpha_0]}\overline{K}^{\beta}$$

Firms with NCAs will hire

$$L_\theta^\star = \left(\frac{D}{\overline{K}^{\beta}}\right)^{1/[\theta\alpha_1+(1-\theta)\alpha_0]}$$

units of labor. A policy change that increases θ leads to a decrease in the quantity of labor demanded:

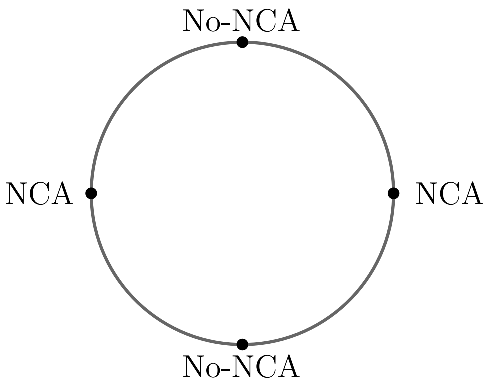
$$\frac{\partial L_\theta^\star}{\partial\theta} = \ln\left(\frac{D}{\overline{K}^{\beta}}\right)\left(\frac{D}{\overline{K}^{\beta}}\right)^{\frac{1}{[\theta\alpha_1+(1-\theta)\alpha_0]}}\left[\frac{-\alpha_1+\alpha_0}{[\theta\alpha_1+(1-\theta)\alpha_0]^2}\right] < 0$$

The policy change also increases average costs if condition 1 holds.

Numerical Example: Consider the example of a primary care physician practice in which the annual number of patient visits produced is given by $g(L,0) = 5000 * L^{0.6}K^{0.4}$. Suppose the annual cost of hiring one physician is \$200,000, and the annual cost of capital and other practice overhead is \$1,000,000. Suppose the use of NCAs increases physician salaries by 10% and α by 2% (that is, $g(L,1) = 5000 * L^{0.612}K^{0.4}$.) For practices without NCAs, the average cost per patient visit (\$149.26) is minimized when there are 7.5 physicians in the practice ($L_0^\star = 7.5$). For practices with NCAs, the average cost per patient visit (\$154.40) is minimized when $L_0^\star = 7.2$. Average costs are higher in practices that use NCAs as long as the number of physicians per practice is below 115, many times larger than the size that minimizes average costs.

3.2 Practice Mergers

To consider the potential for mergers, suppose practices compete in the circular city model of Salop (1979), an extension of Hotelling (1929). D consumers with unit demand are uniformly distributed on a circle with perimeter length one. Four firms begin with maximum differentiation along the circle. Two firms use NCAs and two do not.



Consumers pay travel cost dx, where x is the distance to the firm from which they purchase. Any pair of practices can attempt to merge by paying a fixed cost M. If the merger is successful, travel costs to both practices in the merged firm decrease to $dx/2$. With probability ϵ the merger attempt will fail and one of the physicians from the practice will separate and open a new practice at the same location, poaching some of the practice's patients. NCAs can prevent this type of separation from occurring, increasing the likelihood that the merger will succeed. For practices that use NCAs, the probability of a spin-off is $\epsilon(1-\theta)$.

Suppose an NCA and a no-NCA practice attempt to merge. There are two components to the expected change in the flow of profits to the NCA practice. With probability $(1-\epsilon)(1-\epsilon(1-\theta))$ the merger will be successful, and the practice's share of the total demand will increase from 1/4 to 5/16, increasing profits. With probability $\epsilon(1-\theta)$ the NCA practice will have a spin-off, and the practice's share of the total demand will decrease from 1/4 to 1/8. If the merger fails because the no-NCA practice has a spin-off, then there is no change to the flow of profits of the NCA practice, but there is a loss of M, the fixed cost of attempting to merge.[10] The NCA practice's expected flow of profits from merging is strictly increasing in θ:

$$\frac{\partial \Delta\pi_1}{\partial\theta} = (1-\epsilon)\epsilon\left[\frac{5pD}{16} - \frac{pD}{4} - w_\theta\left(\frac{5t}{16\overline{K}^\beta}\right)^{1/\alpha_\theta} + w_\theta\left(\frac{t}{4\overline{K}^\beta}\right)^{1/\alpha_\theta}\right]$$

$$+\epsilon\left[\frac{pD}{4} - \frac{pD}{8} - w_\theta\left(\frac{t}{4\overline{K}^\beta}\right)^{1/\alpha_\theta} + w_\theta\left(\frac{t}{8\overline{K}^\beta}\right)^{1/\alpha_\theta}\right] > 0$$

[10]See appendix for additional steps in this calculation.

Similarly, the flow of profits to the practice without NCAs is also increasing in θ.[11] Therefore, as θ increases, a merger is more likely to generate an increase in profits sufficiently large to justify the cost of attempting the merger, M. That is, for any increase in θ, there always exists a range of fixed costs M in which the change in θ will cause an attempted merger.

If an NCA practice attempts to merge with another NCA practice, the probability of a spin-off disrupting the merger is lower. In expectation, the merger will increase the flow of profits by more in this case. Moreover, the expected effect of an attempted merger on profits is more responsive to a change in θ. Therefore, a merger between two practices that use NCAs, all else equal, is more likely to be attempted.

Although this model is heavily stylized and considers only one, crude measure of NCA enforceability, it provides a useful example of how NCAs can affect organizational incentives differently at the establishment and firm levels. At the establishment level, the model demonstrates how an increase in NCA enforceability may reduce the sizes of physician establishments and raise prices, consistent with an increase in average costs. At the firm level, however, NCAs can also affect the ease of mergers and other types of organizational consolidation. Because major organizational changes like mergers tend to increase the risk of employee separations, while employees in this case control the valuable patient relationships that generate profits, the merger's success can be jeopardized by these separations. NCAs can increase the ability of practices to merge without disruptions caused by the departure of key employees. Empirically, the relationships between the seven dimensions of NCA enforceability and the size of physician firms are more nuanced, of course. This model is intended to provide an example of a key channel through which NCAs can affect firm-level organizational incentives differently than they affect establishment-level incentives.

4 Data

We use data from a variety of sources to construct a longitudinal database that includes physician market concentration measures, negotiated prices, and our 7 instrumental variables. The main sample, during which all of the data components are available, spans 1996-2007.

[11] This derivative equals the first term from $\frac{\partial \Delta \pi_1}{\partial \theta}$ with w_θ and α_θ replaced by w_0 and α_0.

4.1 MPIER Physician Panel

The Medicare Physician Identification and Eligibility Registry (MPIER) is a database collected by the Center for Medicare and Medicaid Services (CMS). The database began in 1989 when the Health Care Financing Administration assigned unique identifying numbers to all physicians associated with Medicare. In 1996 the physician identification requirement was strengthened under HIPAA, which mandated every physician to receive an identifying number and be included in the MPIER regardless of their association with Medicare. The coding system used in MPIER was in place through 2007.

The MPIER data provide each physician's name, identifying number, the number of practices that the physician is associated with, the dates of any changes in practice affiliations, physician specialties, a group practice indicator, the practice billing address, and the practice's business location street address. Physicians can have multiple practice affiliations at the same time, and each location at which a physician treats patients is required to be recorded. Using the `soundex` fuzzy matching algorithm[12] we construct a longitudinal database of establishments by matching physicians to establishment street addresses. We allow slight differences that may be due to typographical errors in street addresses, but we require exact matches on street numbers and office numbers. In the appendix we examine the sensitivity of our results to the fuzzy matching tolerance parameter.

There are two limitations with this database. First, we cannot observe connections between establishments, which could be important to the extent that multi-establishment firms negotiate as a single entity with insurers. Second, we cannot observe revenues or allocations of time for physicians that work in multiple establishments. To calculate concentration measures from these data we use the shares of the number of physicians in a given market. Each physician with multiple establishment associations is allocated in equal proportions to each of the establishments for as long as each establishment continues, so that each physician contributes exactly one to the total physician headcount at any time. Although it has some limitations, this dataset is to the best of our knowledge the only complete national census of individual physicians during our study period.

4.2 Longitudinal Business Database

Several of these data limitations can be overcome with data from the Census Bureau's confidential Longitudinal Business Database (LBD), which contains data on all non-farm employer establishments in the US and is available from 1976 to (nearly) the present. The LBD contains establishment employment, payroll, industry codes, and county loca-

[12] See R. Russell US Patent 1261167 (1918).

tions with firm linkages via IRS Employer Identification Numbers (EINs). These IRS EINs enable us to calculate firm-level measures of practice organization, including HHIs. Physician practices are identified by NAICS code 621111, described as 'Offices of Physicians (Except Mental Health Specialists).' While the LBD solves the problem of observing firm-level information, it too has limitations since it does not contain the medical specialties of the physicians at each firm.

We use the LBD first to construct measures of firm-based physician market concentration by county and year using the firm linkages indicated by EINs. We calculate two alternative HHI measures: one based on physician employment shares, as in the MPIER, and another based on sales shares; analyses are presented using both. We also use the LBD to construct longitudinal measures of health insurance market concentration using data on sales from firms in NAICS 524114, 'Direct Health and Medical Insurance Carriers'. We control for insurer concentration in our main specifications.

Because the LBD is subject to disclosure review of results and associated standards, we have less flexibility in analyses using these data. We cannot, for example, present results from many different underlying samples. As a consequence, we make our case with several types of results: (1) main results that simultaneously contain MPIER and LBD data; (2) main results with MPIER data only, as a benchmark for robustness tests; (3) robustness tests using MPIER data only.

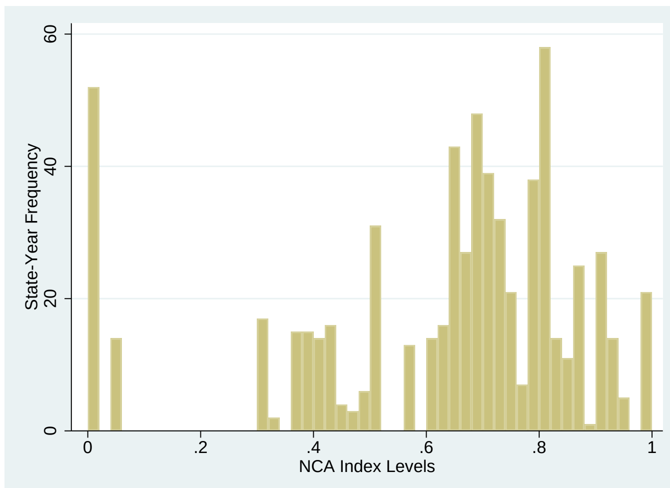
4.3 MarketScan Negotiated Prices Data

Data on prices negotiated between physicians and private commercial insurers come from the Truven Health Analytics Marketscan database. The database includes the medical claims for all active employees and their dependents from a sample of large firms. We use data between 1996-2007 on average negotiated prices, counts, and variances of negotiated prices by county, year, physician specialty, Current Procedural Terminology (CPT) code, and medical facility type (for example, physician office, urgent care facility, end-stage renal disease facility). This combination of dimensions gives about 10 million average negotiated prices, based on prices from about 550 million procedure claims, covering every state-year and nearly every county-year during the study period. Note that since the average is taken over claim level observations, it reflects the empirical distribution of prices, which naturally weights practices according to their sample market shares in each cell. Our analyses use the top 35 most common procedure codes to reduce imbalance across cells caused by infrequently used procedure codes. The sample contains only prices for ambulatory services that are not hospital-based.

4.4 NCA Law Data

We develop a new database quantifying the variation in state-level NCA laws systematically over time, following the measurement system developed by Bishara (2011). Bishara (2011) analyzes case law in each state and scores states along 7 different dimensions, following the framework from a series of legal texts by Malsberger (1991, 1996, 1997, 2000, 2001, 2003, 2004, 2006, 2008, 2009, and 2011). Each of the dimensions is assigned a weight, based on legal knowledge of their relative importance, to create a weighted index score. The 7 components and the scoring system are described in detail in Table A2.

Figure 1: Distribution of NCA Index Levels



Notes: Data points underlying the histogram are state-year observations of the NCA Index, a weighted sum of the 7 NCA law dimensions. The Index is scaled to range from 0 to 1, where 0 is the least restrictive state-year in the sample and 1 is the most restrictive.

The analysis by Bishara (2011) quantifies laws in 1991 and 2009. Using the same methodology, we code the timing and degree of the law changes, creating an annually-measured longitudinal dataset that spans the period 1991-2009 and matches the endpoint measures of Bishara (2011).[13] During the period we study, there were 52 law change events. Each event moved one or more of the seven legal dimensions. Previous work using NCA law changes for variation in organizational incentives in non-physician markets examined specific events in Michigan (Marx et al. (2009)) and in Texas, Florida, and Louisiana (Garmaise (2009)).

[13]We are grateful for legal expertise from Richard Braun, J.D., and for research assistance from Akina Ikudo, and David Krosin in the creation of this dataset.

In the Bishara (2011) data, the weighted sum of scores for all seven components ranges from 0 to 470, where 470 (Florida) corresponds to policies under which NCAs are easiest to enforce, and 0 means that NCAs cannot be enforced in employment contracts. In our analyses we normalize the measures by dividing each component by its maximum value to create continuous measures that range from 0 to 1, representing the observed spectrum of each policy dimension, where 1 corresponds to the state-year policy in which NCAs are easiest to enforce. Figure 1 shows the frequencies of these NCA index values in all state-year pairs in our sample, and Table 1 presents summary statistics on the changes in legal indices by Census region, indicating that changes are geographically dispersed and move in both directions within each region. The average magnitude of law changes in our sample is 0.08 in absolute value, which is about one-third of a standard deviation of the overall policy variation.

Table 1: NCA Law Components: Descriptive Statistics by Census Region

Region	Northeast	Midwest	South	West	Total
Average Index	0.66	0.72	0.64	0.51	0.63
Standard Deviation of Index	0.28	0.25	0.22	0.27	0.26
Maximum Index	1.00	1.00	0.96	0.88	1.00
Minimum Index	0.00	0.00	0.00	0.00	0.00
Number of Law Changes	10	11	22	9	52
Number of States in Region	9	12	17	13	51
Number of Index Increases	7	7	9	5	28
Number of Index Decreases	3	4	13	4	24
Average Magnitude Positive Index Change	0.04	0.12	0.06	0.08	0.08
Maximum Positive Index Change	0.09	0.26	0.14	0.16	0.26
Average Magnitude Negative Index Change	–0.07	–0.07	–0.15	–0.05	–0.09
Maximum Negative Index Change	–0.09	–0.10	–0.63	–0.07	–0.63

Notes: Statistics in the table represent data from 1994-2007 for each state-year in which a legal precedent exists, and uses physician-specific laws whenever applicable. States that forbid NCAs either generally or for physicians specifically are CO, DE, MA, and ND. The minimum of each component is 0 and the maximum of each component is normalized to 1.

5 Effects of Law Changes on Practice Organization and Market Concentration

Changes in these NCA laws caused by judicial decisions can alter the incentives of physician practices and affect their organizational form. Section 3 provides a conceptual framework with intuition for how and why organizational form may be affected. This section presents a variety of evidence that practice organization is empirically affected by NCA enforcement changes.

We begin by estimating the effect of NCA policies on organizational outcomes and market concentration using the following equation:

$$Org_{mct} = \alpha + \beta NCA_{s(c),t-1} + \eta_m + \gamma_c + \nu_{d(c)t} + \varepsilon_{mct} \tag{2}$$

Org_{mct} represents a variety of organizational outcome variables, including physician-establishment separation rate, establishment size, establishment births and deaths, and HHI, in medical specialty m, county c, and year t. The fixed effects specification controls for specialty effects, county effects, and census division by year effects ($\nu_{d(c)t}$). The coefficient β therefore identifies the extent to which the organizational outcome moves differentially in counties in states with law changes relative to those in other states in the same census division (there are 4.6 within-division comparison states, on average). This specification, which we use in lieu of imposing functional form restrictions on time trends, allows the prices in each census division to have any arbitrary unobserved idiosyncratic variation over time. Since prices may not be renegotiated immediately following an organizational change, we follow previous studies of negotiated healthcare prices (Dafny et al. (2012), Dunn and Shapiro (2014), and Baker et al. (2014)) in using a lagged specification.

In Appendix Figure A2 we show estimates from an event study model which suggests that an average increase in NCA enforceability leads to a 15 percentage point drop in the rate of job separations in the year of the law change. This evidence suggests that NCA laws constrain physicians' choices over practices, consistent with broader evidence that NCA laws have effects on practice organizational structures. Still, it is not obvious that even an exogenous event causing separations should change establishment sizes or market concentration. Separating physicians could start new small practices, reducing the average practice size, or join larger established practices, increasing establishment sizes. We also show in Appendix Table A4 that the law changes significantly affect the rates of new establishment births and incumbent establishment deaths.

Table 2 shows that these changes in separation rates and establishment counts also lead to changes in the average sizes of establishments. The dependent variable in this model is the log of the number of full-time equivalent physicians per establishment, where full-time equivalence is calculated by assigning equal fractions of each physician to every establishment location at which they treat patients during the year. The independent variables include one-year lags of each legal dimension, as well as fixed county effects and census-division-by-year effects. Since many practices contain multiple physicians with

Table 2: Fixed Effects Models of Establishment Sizes

Dependent Variable: Log FTE Physicians per Establishment	By Component	Combined
	(1)	(2)
Statutory Index$_{t-1}$	–0.169*	–0.141
	(0.075)	(0.086)
Protectible Interest Index$_{t-1}$	–0.026	–0.178
	(0.094)	(0.139)
Burden of Proof Index$_{t-1}$	–0.048	–0.262
	(0.088)	(0.193)
Consideration Index Inception$_{t-1}$	–0.121*	0.081
	(0.046)	(0.191)
Consideration Index Post-Inception$_{t-1}$	0.044	0.099
	(0.062)	(0.052)
Blue Pencil Index$_{t-1}$	–0.151*	–0.163*
	(0.037)	(0.028)
Employer Termination Index$_{t-1}$	–0.159*	–0.103
	(0.065)	(0.080)

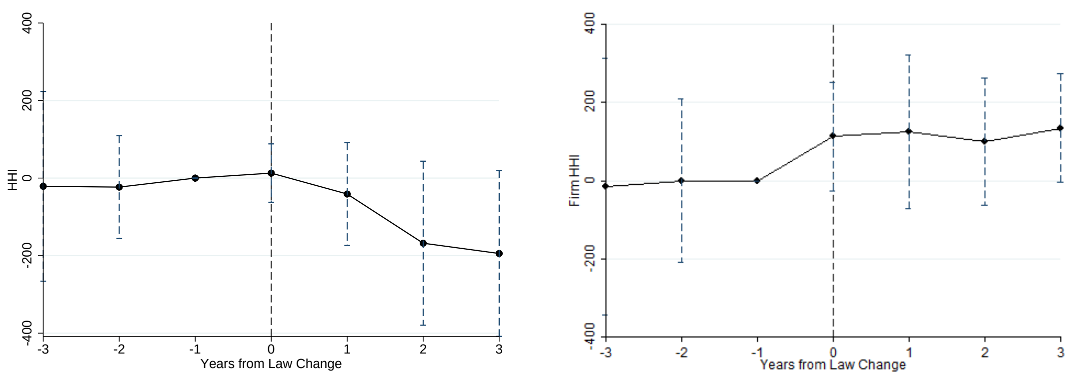
Notes: Column 1 reports estimates from separate regressions on each law component, and column 2 reports estimates from a regression including all 7 components. Dependent variable is the log number of FTE physicians per establishment in a county-year. All specifications include controls for the aggregate supply of physicians in the county and fixed effects for county and census division by year. FTE establishment sizes are estimated by assigning equal partial shares (summing to one) to all establishments at which a physician is active. All standard errors are clustered by state. * indicates significance at the 0.05 level.

different specialties, we do not condition on specialty in these specifications. Column 1 presents estimates from seven separate regressions, each containing one of the instruments. Four of the law dimensions have statistically significant negative effects, ranging from a reduction in establishment sizes of 12.1% to a reduction of 16.9% per unit change in each index, or about –3.6% to –5.1% per standard deviation change in each index. Column 2 presents estimates from a single regression on all 7 coefficients, in which the coefficients differ somewhat because each judicial decision can cause correlated changes in multiple indices at once. These estimates are again generally consistent with the negative relationship between NCA enforceability and practice sizes, as discussed in the conceptual framework in Section 3.

Figure 2 depicts results from an event study model estimated on treatment states with only one law change within the event window (to prevent contamination from multiple overlapping event windows), and control states in the same census division with no law changes. The dependent variable in Figure 2a is the county-specialty establishment-based

HHI, and in Figure 2b it is the firm-based HHI (from the LBD). The figure shows that an average increase in NCA enforceability decreases establishment-based HHIs by about 168 points within 2 years after the law change, with very little evidence of a differential pre-trend in treatment states. The p-value of an F-test that all three pre-period coefficients are equal to each other is 0.87, consistent with the common trends assumption. Meanwhile, Figure 2b shows that firm-based HHIs increase in response to an average increase in NCA enforceability, again by about 150 points.

Figure 2: Event Study Plots: Concentration Before and After Law Changes



(a) MPIER Establishment HHI (b) LBD Firm HHI

Notes: Sample includes treatment states with only one law change within the event window, and control states in the same Census division as the treatment state that had no law changes during the corresponding event window. Estimates are from fixed effects regressions including county effects, census division by year effects, and specialty effects. Subfigure (a) uses establishment-based HHIs from MPIER, while subfigure (b) uses firm-based HHIs from the LBD. The MPIER sample includes primary care and non-surgical specialists. Dashed lines represent 95% confidence intervals based on standard errors clustered by state. Year 0 is the calendar year during which the law change occurred, and the -1 event year effect is normalized to zero.

6 Measuring Price Effects

The fact that changes in NCA laws seem to have important effects on physician practice organization and market concentration suggests the possibility that we could use these variables formally as instruments in estimating the effects of physician market concentration on service prices. In this section, we describe the IV model specification and evaluate the identifying assumptions required to interpret the estimates as local average treatment effects (LATEs). We subsequently estimate reduced-form price effects, providing additional evidence in support of the instruments as both having important effects on prices and affecting prices via organizational form. We then present first and second stage IV estimates, followed by a brief overview of a wide range of sensitivity analyses.

6.1 IV Model and Assumptions

We estimate a fixed effects two-stage least squares model, instrumenting for potentially endogenous variation in market concentration using the seven different dimensions of NCA laws as instruments. To differentiate between the effects of increases in concentration driven by larger firms as opposed to larger establishments, we allow both firm and establishment concentration to be endogenous regressors, the effects of which are overidentified by the seven instruments.[14] The first-stage equations for the two endogenous regressors are:

$$\begin{aligned} EC_{mc(t-1)} &= \alpha_1 + \beta_1 NCA'_{s(c)(t-1)} + \beta_2 InsC_{s(c)(t-1)} + \eta_m + \pi_f + \theta_p + \gamma_c + \nu_{d(c)t} + \epsilon_{mc(t-1)} \\ FC_{c(t-1)} &= \alpha_2 + \beta_3 NCA'_{s(c)(t-1)} + \beta_4 InsC_{s(c)(t-1)} + \eta_m + \pi_f + \theta_p + \gamma_c + \nu_{d(c)t} + \epsilon_{mc(t-1)} \end{aligned}$$

and the second-stage equation is:

$$\begin{aligned} \ln(P_{mfpct}) &= \alpha_3 + \beta_5 \widehat{EC}_{mc(t-1)} + \beta_6 \widehat{FC}_{c(t-1)} + \beta_7 InsC_{s(c)(t-1)} + \eta_m + \pi_f \\ &+ \theta_p + \gamma_c + \nu_{d(c)t} + \varepsilon_{mfpct} \end{aligned} \tag{3}$$

where η_m, π_f, θ_p, γ_c, and $\nu_{d(c)t}$ are fixed effects for medical specialties, facility types, procedure codes, counties, and census division-by-years, respectively. $\ln(P_{mfpct})$ is the log negotiated price. $NCA'_{s(c)t}$ is a vector of the seven law instruments, measured at the state-year level, where $s(c)$ denotes the state in which county c is located. EC_{mct} is the establishment-based measure of market concentration, in contrast to FC_{ct}, the firm-based concentration measure.[15] $InsC_{s(c)t}$ is the concentration of health insurance firms in the state. Our main specifications use HHIs as concentration measures, though we also present results using a range of alternative concentration measures including average practice size, the negative log HHI transformation, and the four and eight-firm concentration ratios.[16]

The ability to distinctly observe both firms and establishments is a relatively unique feature of the data, and it allows us to estimate the marginal effect of each concentration measure on prices. The intuition behind this specification follows Dranove and Lindrooth (2003), who study the effects of hospital consolidation on operating costs. They find

[14]See Malsberger 1991-2011 and Bishara 2011 for thorough discussions, and Appendix Table A2 for a brief overview, of the differences between these seven aspects of non-compete agreements in employment law.

[15]Firm-based concentration by county and year is calculated in the LBD data using EINs to link establishments of the same firm and using firm-level employment and sales shares to calculate two alternative measures of FC. Results are presented with each.

[16]In Appendix Table A14 we consider models in which practice size can have nonlinear effects on prices. However, in these analyses we are unable to detect significant evidence of nonlinearities.

that when a hospital is acquired by another system there are no significant cost savings unless the acquisition leads to a physical consolidation of establishments, in which case median costs decline by about 14%. Building on this idea, we hypothesize that increases in establishment concentration, EC, conditional on firm concentration, FC, are likely to create cost efficiencies that may be partially extracted by insurers in negotiations, reducing prices. Conversely, evidence from the hospital setting suggests that increases in FC conditional on EC are less likely to result in cost savings, though they may improve bargaining position in negotiations with insurers, increasing prices. In Appendix Section C, we more formally develop these hypotheses and derive the linkage between our estimands and more primitive underlying theoretical parameters in a model of simultaneous bilateral bargaining that builds on Ho and Lee (2014). Distinguishing between these two distinct forms of organizational consolidation can potentially provide useful insights into the factors that drive physician prices in the US.[17]

Because our concentration measures are at the geographic level of the county and we include county and census-division-by-year fixed effects, our estimates identify the effects of changes in bargaining position in local markets but do not incorporate potential bargaining power effects of multi-market physician systems, a distinction discussed in the context of large hospital systems by Lewis and Pflum (2015, 2017).

The model specifications use lagged concentration measures in the second stage, consistent with the literature as well as with the event studies, and the instruments affect concentration in the contemporaneous year. Since the dependent variable in the first stage is lagged, the IVs include first lagged laws.[18]

In Section 6.7, we evaluate a wide range of alternative specification assumptions, including alternative market definitions, assumptions about the treatment of multi-specialty practices in calculating HHIs, alternative measures of market concentration and firm sizes,

[17]Our main specifications include both FC, measured in the LBD, and EC, measured in MPIER, as specified in the model above. However, because of Census Bureau disclosure rules concerning complementary estimation samples, robustness checks that slightly alter samples cannot be released. As a result, in addition to our main estimates with both FC and EC, we also present specifications analogous to those in the main results but with MPIER data only, as a benchmark for robustness tests, and then we present the robustness tests with MPIER data only. Under the assumption that Equation 3 is properly specified, estimates in our robustness analyses that omit firm-level concentration should be interpreted with caution as the combined impact of establishment concentration and the portion of the error term that is correlated with establishment concentration.

[18]The lag structure of the specification is chosen with the goal of being conservative. While some physician practices may negotiate their prices at a less-than-annual frequency, such that it may take some time for any given change in market concentration to have its full GE effects on prices in the market, measuring prices with a longer lag risks confounding the concentration effect with other changes in the market. The tightest identification allows just enough time for at least some firms (including spinoffs and mergers induced by NCA changes) to renegotiate prices, while excluding potentially endogenous changes that occur as the market evolves in the years after a law change.

omission of outlier law changes, and interaction effects between physician and insurer concentration.

6.1.1 Structure, Conduct, and Performance Assumptions

Our modeling approach follows the general structure-conduct-performance (SCP) framework for estimating effects of market structure on prices, which has several well-known limitations. One important class of concerns about SCP models described by Gaynor, Ho, and Town (2015) in their review of this literature is that measures of market structure are generally endogenous in pricing equations. A key difficulty in resolving this endogeneity is that there are many potential forms to consider. For example, latent variation in demand, costs, bargaining ability, or quality—all of which may affect prices—could be correlated with market structure, causing bias. Moreover, these bias components could oppose each other, creating ambiguity about the net direction of bias.

For example, consider the case of unobserved heterogeneity in practice cost functions. Since a high cost practice will negotiate higher prices in a standard bargaining model, the error term will contain some of this latent variation in practice costs. To the extent that insurers can steer patients towards low cost providers, the market share of high cost practices will be lower. The negative correlation between latent average cost and market share, which determines HHI, may cause downward bias in $\widehat{\beta}_5$.

On the other hand, a practice with high quality, unobserved to the researcher, is likely to have high market share. The error term contains the component of price variation caused by quality differences, and this error component is positively correlated with market share, possibly causing an upward bias in $\widehat{\beta}_5$.

In addition to being ambiguous, the sign of the net bias could depend on whether changes in practice size are motivated primarily by average costs or by bargaining position. Our empirical findings suggest that OLS estimates of β_5 and β_6 are attenuated towards zero. Our results generally support the conclusion that endogeneity of market structure in Equation 3 causes substantial bias. Previous empirical research on healthcare markets has also used instruments to address this endogeneity, as in the case of Dafny et al. (2012), which uses the merger of two large healthcare insurers as an instrument for concentration in local insurance markets. One contribution of our study is to develop new instrumental variables to overcome these biases in a variety of markets, including markets outside of healthcare in which NCAs are used frequently.

A second class of concerns described by Gaynor, Ho, and Town (2015) is that estimates can be sensitive to assumptions about market definition, conduct, and performance. We evaluate the sensitivity of our estimates across a range of potential market definitions and find the conclusions to be robust to this assumption. Perhaps more fundamentally, however, without estimating both conduct and performance, the choice of market structure measures can be arbitrary and potentially inconsistent with firm conduct. For example, choosing HHI as a market structure measure to estimate performance implies specific implicit assumptions about conduct: homogeneous goods and Cournot competition. These assumptions are appropriately regarded with skepticism in many markets.

We make two points about firm conduct in our estimates. First, without a national panel of claims data covering our study period, we do not attempt to estimate firm conduct directly. Instead we take the approach that, using a variety of market structure measures, we identify patterns in negotiated prices under a broad conceptual framework. Each of these measures has underlying it a specific, and different, assumption about firm conduct. We show that the qualitative conclusions are identical regardless of our measure of market structure, suggesting that the assumptions of firm conduct do not substantially alter the findings once we correct for several other estimation challenges. We find the most important estimation challenge to be the endogeneity of these measures.

Second, there may be reasons to be less concerned about the implicit assumptions of homogeneous goods and Cournot competition in the case of physician practices, at least relative to hospitals. Hospitals often have observable (to the patient and econometrician) objective measures of quality, such as mortality rates, that vary substantially. In addition, consumers tend to have strong perceptions of quality differences. For example, research hospitals affiliated with prominent universities may be perceived to have sufficiently higher quality such that consumers are willing to pay higher premiums for insurer networks that include them (see Capps, Dranove, and Satterthwaite, 2003). Although some large physician groups have similar brand affiliations with prominent research hospitals, there is frequently no clear analogue among physicians to the dominant hospital phenomenon. There are few, if any, objective measures of physician-level quality outside of hospitals. Although consumers may have preferences for visiting a doctor that they personally know well, loyalty to a doctor is very different than a commonly-shared perception of quality, and it does not necessarily lead to correlation in willingness to pay across consumers.[19] We also condition on physician specialty, medical procedures, and geography, making the services closer to being conditionally homogeneous. Still, there is very little empirical evidence from the literature on measures of either objective heterogeneity in physician quality (outside of hospitals) or consumers' perceptions of differences in quality, and we

[19] For example, if homogeneous consumers are uniformly distributed across doctors, even if each consumer is willing to pay more for an insurance network that includes their own doctor, the average willingness to pay for any particular doctor is the same, since willingness to pay is not correlated across consumers in the market.

have nothing concrete to add to the dearth of evidence on this question.

There is direct empirical evidence in support of the assumption of Cournot competition in the market for physician services. Gunning and Sickles (2013) estimate a structural model of conduct among physician practices that builds on the approach developed by Bresnahan (1989). Using data from the American Medical Association, they estimate firm price elasticities and reject the null hypothesis of perfect competition, but they fail to reject the hypothesis of Cournot conduct, suggesting that using HHI as a market structure measure is consistent with firm conduct for physicians.

6.1.2 IV Assumptions

Interpretation of our estimates as local average treatment effects (LATE) requires several assumptions. Adding to our discussion above, we formally discuss instrument strength in Section 6.5 and show that our instruments exceed typical power thresholds, supporting the relevance assumption.

The exclusion restriction necessary for the validity of the IVs requires that changes in NCA laws affect physician service prices only through physician market concentration. That is, changes in NCA laws must not be correlated with the error term in the second stage equation. In our structural equation, negotiated prices depend on market concentration and fixed specialty effects, county effects, medical facility type effects, procedure effects, and census-division-by-year effects. Given that we condition on this set of covariates, law changes can mechanically only be correlated with the structural error if NCA laws affect negotiated prices across practices *within* a given market, defined by geography and medical specialty, and through some mechanism other than market concentration.

Although an exclusion restriction is not formally testable, we provide evidence supporting its validity in this setting. Using survey data from about 2,000 physicians with information on whether each physician has signed an NCA, linked to negotiated prices with private insurers for the most common office visit procedures, Lavetti et al. (2019) find that the use of NCAs has precisely no effect on negotiated prices conditional on the market and practice size. They find that there is substantial variation in prices within geographic markets—the within-market standard deviation of office visit prices is about 39% of the mean price. However, the average price difference associated with NCA use is only 2% of the mean, and is not statistically significant. In addition, the price difference between NCA users and non-users is no different in higher versus lower NCA enforcement states. To the extent that NCAs affect prices, this evidence suggests that the effect occurs either across markets or through practice size and concentration measures, which is consistent with the requirements of the exclusion restriction.

The evidence presented in Section 6.2, below, on heterogeneity in reduced form price effects across procedures is also helpful for considering whether the exclusion restriction assumption is reasonable. One potential concern with the assumption is that changes in NCA laws could directly affect prices by causing labor market frictions that lead to a divergence between the earnings and marginal value products of physicians. However, Figure 4 shows that procedures using primarily physician labor as inputs have little systematic change in prices in response to changes in NCA laws. In contrast, the instruments cause large changes in the prices of procedures that use relatively high amounts of equipment, office space, and non-physician labor inputs. This evidence suggests that the instruments affect prices primarily through a mechanism outside of physicians' labor supply decisions, alleviating concern about this form of violation of the exclusion restriction assumption.

We also consider the possibility that changes in NCA laws could affect prices via the aggregate relative supply of physicians. As the law changes may affect physicians' option sets within the local market, they could potentially affect flows of physicians across geographical markets and impact prices through changes in aggregate supply. We investigate this possibility and show in Appendix Table A18 that NCA laws have no significant effect on the number of physicians per capita in a market.

Another mechanism through which NCAs could potentially affect prices is through physician sorting on the basis of quality. Survey data from Lavetti et al. (2019) indicate that there is no relationship between physician quality and the use NCAs. Physician quality is measured by asking a series of vignette-based questions designed by clinical experts to elicit knowledge of best practices, diagnostic skill, treatment patters, and clinical recommendations. Finally, physician experience—which is strongly correlated with measures of patient satisfaction and perceived quality (Choudhry et al., 2005)—does not vary with the use of NCAs.

The conditional exogeneity of law changes is supported by three pieces of evidence. First, the event studies indicate the absence of pre-treatment trends, which supports the notion that judicial decisions were not made in response to trends in physician concentration or prices. Second, a direct analysis of the opinions written by judges, describing the rationales that led them to their decisions, enables us to identify the judicial decisions that were related to physicians and verify that our findings are not sensitive to excluding these events. Third, in Section 6.7, we provide evidence that changes in NCA laws are not systematically related to other state-level political and economic factors that could also affect prices.

The final IV assumption is monotonicity. The monotonicity condition in our case requires that a change in any particular law dimension affects HHIs in all states in (weakly) the same direction. To evaluate this condition we estimate the model using samples split along several dimensions, including metro and non-metro counties, physician specialties, states with positive and negative law changes, and markets with high or low HHI. The first stage results for these tests are generally consistent with the monotonicity assumption, showing that six out of seven law dimensions always affect the endogenous regressor in the same direction (Appendix Table A11). Appendix Table A12 shows results are not sensitive to excluding the seventh dimension, the Blue Pencil Index.

Under these IV assumptions, each instrument identifies a separate LATE, and our second-stage estimand is an average of these LATEs. As we will show, the seven LATEs are all similar to each other, so this average is informative.

6.2 Reduced-Form Effects

Before presenting the IV results, we estimate the effect of NCA policies on negotiated prices. The model specification is similar to that in Equation 3:

$$\ln(P_{mfpct}) = \alpha + \beta NCA_{s(c),(t-1)} + \eta_m + \pi_f + \theta_p + \gamma_c + \nu_{dt} + \varepsilon_{mfpct} \quad (4)$$

The dependent variable is the log of the negotiated price of procedure code p performed by a physician with medical specialty m in facility type f, county c, and year t. As in the main specification, the model includes fixed procedure code effects, specialty effects, facility type effects, county effects, and census division by year effects.

Table 3 presents estimates from Equation 4. In the first row we use the weighted average NCA enforceability index created by Bishara (2011), and in the rows below we show estimates from separate regressions for each of the seven legal indices. The results suggest that prices increase by 4.3% in the year following a 0.1 unit increase in the weighted average NCA index. Four of the seven individual indices have significant effects on prices, and six have positive coefficients ranging from 0.4% to 2.7% per 0.1 unit increase in the corresponding index.

6.3 Reduced Form Event Study Analyses of Price Trends

To evaluate whether these results may be affected by differential trends in states with changes in NCA laws, Figure 3 presents event study estimates of Equation 4. While the regressions report estimates from the full sample, the graphs depict event studies from a sample that is limited to treatment states with only one law change within the event window and control states in the same census division with no law changes. The subfigures

Table 3: Reduced-Form Price Effects, by NCA Index

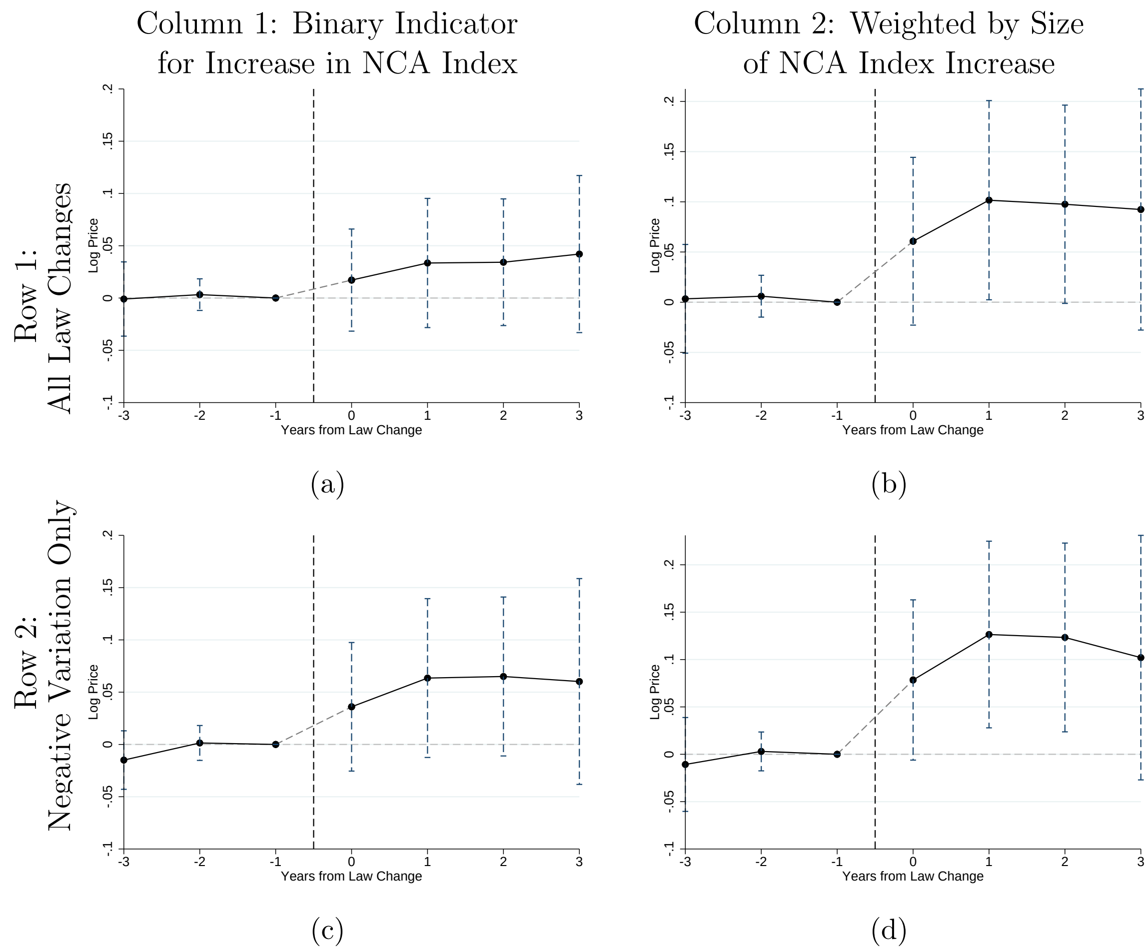
Dependent Variable:	$\ln(Price)_t$
NCA Index (Weighted Average)$_{(t-1)}$	0.428*
	(0.110)
Statutory Index$_{(t-1)}$	0.043
	(0.084)
Protectible Interest Index$_{(t-1)}$	0.037
	(0.130)
Consideration Index Inception$_{(t-1)}$	0.241*
	(0.009)
Consideration Index Post-Inception$_{(t-1)}$	0.056
	(0.062)
Burden of Proof Index$_{(t-1)}$	0.216*
	(0.008)
Blue Pencil Index$_{(t-1)}$	–0.057*
	(0.005)
Employer Termination Index$_{(t-1)}$	0.272*
	(0.027)

Notes: Each coefficient comes from a separate regression of log prices on the first lag of the corresponding legal index. Each legal index is scaled to range from 0 to 1, where 1 corresponds to the highest observed enforceability measure for that index. All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. All standard errors, in parentheses, are clustered by state. * indicates significance at the 0.05 level.

in column 1 are estimated using binary indicators of an increase (+1) or decrease (-1) in NCA enforceability, while estimates in column 2 use the continuous magnitude of the law changes. Row 1 uses all law changes, while row 2 uses only variation from decreases in NCA enforceability.

There are several notable conclusions from these event studies. First, increasing NCA enforceability leads to higher prices on average. Figure 3b, for example, suggests that a 0.1 unit increase in NCA enforceability leads to about 10% higher prices on average within 2 years, a larger effect than is observed in the full sample in Table 3. Second, there is very little evidence of differential pre-period trends in states with law changes. We also test the common trends assumption for a broader set of law changes that includes the first law change in each state plus any subsequent law changes that occurred at least three years after the previous change, providing an uncontaminated three-year pre-period. The parallel trends assumption is also satisfied in this broader sample.Third, decreases in enforceability have (negative) price effects that are similar to the overall estimates,

Figure 3: Event Study Plots: Reduced-Form Price Effects



Notes: Sample includes treatment states with only one law change within the event window, and control states in the same Census division as the treatment state that had no law changes during the corresponding event window. Estimates are from fixed effects regressions including county effects, census division by year effects, procedure code effects, facility type effects, and specialty effects. Specialties included in sample are primary care and non-surgical specialists. Dashed lines represent 95% confidence intervals based on standard errors clustered by state-year. Year 0 is the calendar year during which the law change occurred, and the dependent variable is normalized to zero in year -1.

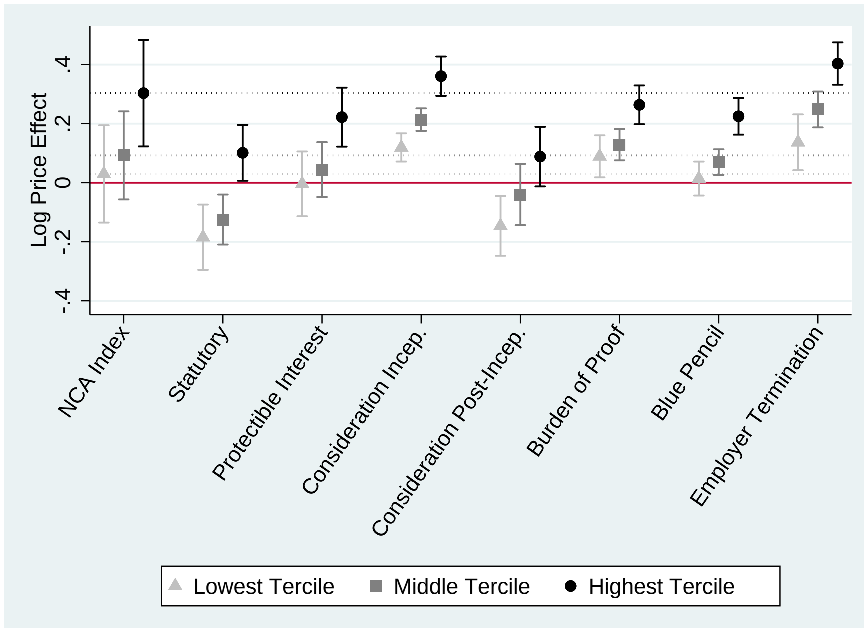
suggesting that the effects of positive and negative law changes are symmetric. Finally, the price effects appear to flatten after about two years, suggesting that the law changes primarily impact price levels as opposed to rates of growth, and that the effects occur fairly quickly.

6.4 Heterogeneity in Reduced Form Price Effects and Potential Mechanisms

To help clarify what types of mechanisms might be driving these price effects, we investigate whether there is systematic heterogeneity across different types of medical procedures. Our test is motivated by the analyses of Clemens, Gottlieb, and Molnar (2017), who study the extent to which physician practices negotiate price schedules with private insurers that are benchmarked to Medicare prices. They find that in smaller physician groups, about 90% of procedure prices are negotiated relative to Medicare prices, while in larger practices (with at least $1 million in billings) only 40% of procedure prices are benchmarked to Medicare. In addition, they show that deviations from Medicare benchmarks are most likely to occur for procedures that use capital-intensive inputs, as opposed to labor-intensive procedures. Finally, they show that deviations from the Medicare price schedule tend to be negative for capital intensive procedures, potentially narrowing the gap between marginal costs and the average cost estimates used to set Medicare payments. This result suggests that insurers may extract a portion of the cost savings associated with larger physician practices to bring prices closer to marginal costs. In contrast, to the extent that prices of labor-intensive services deviate from the Medicare schedule, the deviations tend to be positive. This supports the notion that for at least some procedures physicians are not entirely price-takers, consistent with evidence from Kleiner et al. (2015) and Dunn and Shapiro (2014).

To get a sense of whether NCA laws may be affecting prices through practice size or through some other mechanism, we follow this intuition and test for differential effects of NCA laws on prices of procedures that have high versus low overhead costs and high versus low use of physician labor. We link the Marketscan price data to Medicare data containing the resource-based Relative Value Units (RVUs) for each procedure code. RVUs, which are used in calculating Medicare payments, are divided into three categories: 'Work RVUs' capture the amount of physician labor typically used in the procedure; 'Facility Practice Expense RVUs' capture the average use of equipment, office space, supplies, and non-physician labor expenses; and 'Malpractice RVUs' are designed to cover the costs associated with malpractice insurance for the procedure. For each procedure, we calculate the ratio of Facility Practice Expense RVUs to physician Work RVUs, and group procedures by tercile of this ratio. The top tercile, for example, contains procedures that use primarily capital and other practice costs, while using relatively less physician labor. We then interact these tercile indicators with the NCA variable in Equation 4, and re-estimate the model.

Figure 4: Price Effects by Tercile of the Ratio of Medicare
Facility Practice RVUs to Physician Labor RVUs



Notes: Estimates are similar to those in Table 3, with NCA laws interacted with terciles of Medicare facility practice expense RVUs divided by physician work RVUs. Error bars are 95% confidence intervals, based on standard errors clustered by state.

Figure 4 depicts the coefficient estimates and 95% confidence intervals from these regressions, which are estimated separately by NCA index, as in Table 3. The figure shows that, for procedures in the highest tercile of the ratio of practice RVUs to physician labor RVUs, six of the indices have significant and positive price effects, ranging from 1.0% to 4.0% per 0.1 unit increase in the NCA index. Among procedures in the bottom tercile—those with a relatively intensive use of physician labor relative other inputs—there is no clear pattern of price effects: two of the indices have insignificant coefficients, and the remaining five range between -1.8% to +1.4% per 0.1 change in the law index. Using the weighted average NCA index, the coefficient in the bottom tercile is slightly positive, 0.3%, and statistically insignificant, compared to 3.0% in the top tercile of procedures. Moreover, for every dimension of the NCA indices, the effect on prices in the top tercile is significantly greater than the effect on those in the bottom tercile.

These results help narrow the scope of potential mechanisms that could produce this pattern. Consistent with the evidence on negotiated prices from Clemens et al. (2017), the findings suggest that effects of NCAs on prices appear to occur through a mechanism related to practice organization and overhead costs, rather than through one related to physician labor costs. We evaluate the possibility that NCAs affect prices through alternative (non-organizational) channels in section 6.1.2.

6.5 IV First-Stage Effects of NCA Laws on HHI

First-stage regression results corroborate the evidence from Section 5 that increases in NCA enforceability lead to reductions in physician market concentration. Table 4 presents estimates from the first-stage models based on employment. The first column shows results from seven separate regressions of establishment-level concentration on each of the instruments. Five of the seven legal indices are statistically significant, and all but one of these have negative coefficients. The dependent variable, HHI, is scaled to range from 0 to 100, so the coefficient on the Burden of Proof Index, for example, suggests that a one unit increase in the index decreases the HHI by 443 points on a 10,000 point scale. Scaling by the standard deviation of the Burden of Proof Index (0.27) implies that a one standard deviation increase reduces the concentration by about 119 points.

Column 2 presents estimates from a similar specification that includes all 7 seven instruments. The Cragg-Donald excluded instrument F-statistic is 232, and six of the instruments are statistically significant at the 0.05 level. By comparison, the Stock and Yogo (1997) critical F-statistics thresholds range from about 9 to 12 for achieving 10% relative bias under 2SLS with one endogenous regressor and 3 to 14 instruments. The fixed effects and excluded instruments explain about 60% of the variation in county-specialty-year concentration.

The main first stage IV results using Census data are presented in columns 3 and 4, which correspond to the two jointly-estimated first-stage equations from Section 6.1. Column 3 shows estimates from the establishment concentration first-stage equation, and column 4 from the firm concentration equation. There are three main points to note about these estimates. First, regarding instrument power, the main limitation of the models estimated using Census data relative to the MPIER estimates is that specialties are not observed, which weakens the first-stage power, though the instruments still have enough power to satisfy typical relative bias thresholds. Since these models have two endogenous regressors, we report the jointly-estimated Kleinbergen-Paap F-statistic (24.7), which is comparable to the Cragg-Donald F-statistic (298.0) suggested by Stock and Yogo (1997) but is robust to non-independent errors.

The first stage parameter estimates themselves are not strongly affected by the controls for firm and insurer concentration (comparing column 3 to column 2). The only clear exceptions are the coefficient on the Blue Pencil Index, which is the only instrument with a significant positive coefficient in just-identified specifications, and the coefficient on the

Table 4: IV First Stage Estimates: Effect of NCA Laws on Employment-Based HHI

Dependent Variable:	Establishment HHI_{t-1} (1)	Establishment HHI_{t-1} (2)	Estab. HHI_{t-1} (3)	Firm HHI_{t-1} (4)
Statutory $Index_{t-1}$	–4.97	–3.28	0.55	–5.72*
	(4.23)	(2.69)	(2.37)	(2.46)
Protectible Interest $Index_{t-1}$	1.65	11.61*	14.72*	4.17
	(5.35)	(1.47)	(4.40)	(3.94)
Consideration Index $Inception_{t-1}$	–4.53*	27.90*	17.58*	13.76*
	(0.14)	(1.01)	(6.01)	(5.40)
Consideration Index $Post\text{-}Inception_{t-1}$	–3.14*	–2.75*	–2.38*	1.65*
	(0.52)	(0.20)	(0.41)	(0.63)
Burden of Proof $Index_{t-1}$	–4.43*	–28.66*	–16.47*	–11.50*
	(0.12)	(0.93)	(5.95)	(4.44)
Blue Pencil $Index_{t-1}$	5.90*	5.55*	–0.21	3.89
	(0.63)	(0.63)	(3.31)	(3.53)
Employer Termination $Index_{t-1}$	–9.22*	–16.68*	–24.80*	–8.73*
	(0.28)	(1.50)	(4.49)	(4.33)
Insurer HHI_{t-1}			0.00	0.01
			(0.01)	(0.01)
MPIER Data Used	Yes	Yes	Yes	Yes
Census Data Used	No	No	Yes	Yes
N	3,226,388	3,226,388	6,509,000	
CD F-Statistic		232.4	298.0	
KP F-Statistic		1090.3	24.7	

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Column 1 reports estimates from separate regressions on each law index, and columns 2-4 report estimates from a single regression with all 7 components. All independent variables are scaled to range between 0 and 1, where 1 is the strongest observed measure of the variable in any state and year in the data. HHIs are all based on employment levels, with establishment HHIs from the CMS MPIER file and firm HHIs from the Census LBD. HHIs are scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. Cragg-Donald F-Statistic and Kleibergen-Paap F-Statistic reported. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Burden of Proof Index, which becomes more strongly negative.

Finally, the table shows that the legal indices have different effects on the establishment and firm concentration measures, as can be seen by comparing column 3 to column 4. For example, an increase in the Consideration Index Post-Inception has a positive effect on firm concentration of 1.65 but a negative effect on establishment concentration of –2.38. This pattern suggests the presence of heterogeneity in the features of the legal indices that affect firm organizational incentives, with some laws having more impact on multi-establishment firm incentives and while others appear to impact the sizes of each establishment.

In Appendix Table A6, we also present first-stage estimates using concentration measures based on sales data (rather than employment) from the Census LBD, Economic Census, and SSEL. In this specification, all seven instruments have the same first-stage sign as do the estimates in column 4 of Table 4, suggesting consistency between the two different concentration measures. The instruments also pass conventional power thresholds in this specification.

6.6 The Effect of HHI on Negotiated Prices

Our main estimates are reported in Table 5. The top panel of the table presents results using the sales-based firm concentration measure. The IV coefficient on firm concentration of 0.02 implies that a 100 point increase in firm HHI, holding fixed both the establishment concentration and insurer concentration, causes a 2% increase in negotiated prices on average. This result is consistent with multi-establishment growth improving bargaining position relative to insurers. In contrast, the coefficient on the establishment concentration, –0.014, implies that holding firm concentration fixed but increasing the establishment HHI by 100 points leads to 1.4% lower prices. These estimates suggest that the efficiency gains of larger group practices at a given location outweigh any effects of practice size on the bargaining position of physicians. However, consolidation of multi-site physician groups increases the insurance network value of the firm as a whole, and more than offsets any impacts of economies of scale.

The coefficient on insurer concentration is modest, 0.0007, although to be clear, the law change events do not affect this variable because insurers do not tend to use NCAs, and the coefficient is identified only by the small intertemporal changes in insurer concentration that are not absorbed by county effects and census division by year effects. In contrast, previous studies that use more substantial sources of variation in insurer HHIs suggest that insurance market concentration plays an important role in affecting prices (Dafny et al. (2012)). We include this term only as a control variable, and we caution against the interpretation that insurance market concentration does not affect negotiated prices, since our identifying variation for this coefficient is potentially too small to be salient for bargaining, and since we do not have an instrument for insurance market concentration.[20]

To highlight the importance of addressing endogeneity in physician concentration,

[20]To provide evidence that NCA laws do not directly affect insurer concentration, we estimate our baseline first stage but with our LBD insurer concentration measure on the left-hand side. Appendix Table A9 presents the results of this regression in two alternative lag specifications; both indicate no correlation between NCA law changes and insurer concentration.

Table 5: Main Estimates: Effect of Market Concentration on Negotiated Prices

	Dependent Variable: ln($Price$)	
	IV	OLS
	(1)	(2)
Physician Firm HHI, Sales-Based	0.0212*	0.0001
	(0.0107)	(0.0001)
Physician Establishment HHI	–0.0139*	0.0001
	(0.0064)	(0.0001)
Insurer HHI	0.0007	–0.0001
	(0.0006)	(0.0004)
N	6,509,000	6,509,000
F-Stat (Cragg-Donald)	270.3	
F-Stat (Kleinbergen-Paap)	52.20	
Physician Firm HHI, Employment-Based	0.0172*	0.0001
	(0.0076)	(0.0001)
Physician Establishment HHI	–0.0142*	0.0001
	(0.0076)	(0.0001)
Insurer HHI	0.0000	0.0000
	(0.0002)	(0.0004)
N	6,509,000	6,509,000
F-Stat (Cragg-Donald)	298.0	
F-Stat (Kleinbergen-Paap)	24.72	
Physician Establishment HHI (MPIER)	–0.019*	0.0001*
	(0.006)	(0.0000)
N	3,226,388	3,226,388
F-Stat (Cragg-Donald)	232.4	
F-Stat (Kleinbergen-Paap)	1090.3	

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Data for physician firm and insurer HHIs in these regressions come from the Census' LBD (employment) and SSEL (sales). Physician establishment HHIs are from MPIER. HHIs are scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. Insurer HHIs are calculated from firm-level in-state sales. Medical specialties are observed in price data but not in Census data used to calculate physician HHIs. All standard errors are clustered by state. * indicates significance at the 0.05 level.

we also report the starkly different OLS estimates from the same sample: 0.0001 for both concentration measures. OLS estimates close to zero are consistent with evidence from previous studies using either cross-sectional variation or panel variation in an OLS specification (Dunn and Shapiro (2014) and Baker et al. (2014)).

The middle panel of Table 5 presents corroborating evidence using employment-based concentration measures (from both LBD and MPIER data). The estimates are again statistically significant and imply that a 100 point increase in firm HHI, conditional on establishment and insurer concentration, increases negotiated prices by about 1.7%, while the same size increase in establishment concentration decreases prices by 1.4%.

Since Census Bureau confidentiality restrictions impose cell size restrictions for every combination of subsamples across any analyses conducted, the disclosure requirements grow exponentially with the number of samples used, making it impossible for us to use Census data to conduct a wide range of robustness analyses. Instead we rely on the MPIER data to conduct these analyses. The associated models therefore have one first-stage equation corresponding to EC, excluding FC and $InsC$ from Equation 3.[21]

Panel 3 of Table 5 presents IV estimates using only the MPIER data, where firm concentration and insurer concentration are not observable. The results suggest that a 100 point increase in the establishment concentration leads to a 1.9% reduction in average negotiated prices, somewhat larger than in the estimates with Census data that control for firm and insurer concentration.

Since the majority of our robustness analyses can only be conducted using MPIER data, we first seek to understand why the MPIER estimates differ somewhat from the Census estimates. To that end, we collapse the MPIER HHI measures as though physician specialties were unobserved, and we re-estimate the IV models. The results, shown in Appendix Table A8, suggest that the effect of establishment concentration is –0.018 when the data structure is made more comparable to that underlying the Census estimates. We also estimate our main MPIER concentration specifications including the Census insurer concentration control but no firm concentration, and we find that it does not substantively alter those estimates either. These results provide some reassurance that the robustness analyses using MPIER data are relevant to our main estimates.

Returning to the discussion of the exclusion restriction from Section 6.1.2, one additional piece of evidence in support of this restriction comes from the consistency of estimates when we estimate the IV model using only one legal index at a time, shown in Table 6. The table presents second-stage estimates from 7 separate just-identified IV regressions. Six out of seven models yield negative coefficients on establishment concentration, and four are statistically significant.

This result is reassuring because if the exclusion restriction were violated due to a direct effect of the instruments on practice cost functions conditional on practice size, the differences in the legal nature of the instruments would likely cause substantial hetero-

[21] As discussed in more detail in Section 6.1, the estimates with MPIER data only should not be interpreted causally, but rather as supporting the robustness of the main findings.

Table 6: IV Results Estimated Separately by Law Component

Dependent Variable:	$\ln(Price)$
Statutory Index	–0.009 (0.025) [1.26]
Protectible Interest Index	0.023 (0.147) [0.10]
Consideration Index Inception	–0.053* (0.003) [1032.15]
Consideration Index Post-Inception	–0.018 (0.019) [37.19]
Burden of Proof Index	–0.049* (0.002) [1465.06]
Blue Pencil Index	–0.010* (0.001) [88.59]
Employer Termination Index	–0.030* (0.003) [1116.64]

Notes: Each cell shows the second stage IV estimate of the effect of lagged HHI on log prices using a single legal component as the instrument. The first column displays just-identified models using the first lag of each index. The second column includes both the first and second lags of the legal component as instruments. All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors, in parentheses, are clustered by state. First-stage Kleibergen-Paap F-statistics are shown in brackets. * indicates significance at the 0.05 level.

geneity across instruments in the second-stage estimates. For example, whereas the Consideration Index changes the way employment contracts are written by affecting whether compensation for NCAs must be explicit, it is less clear that law dimensions such as the Burden of Proof Index or the Blue Pencil Index would impact practice cost functions. Both of these dimensions relate to the specific procedures used during the litigation of NCA contracts and become relevant when a job ends and litigation occurs. The Burden of Proof index could violate the exclusion restriction if, for example, the cost to the firm of producing evidence for the litigation affected prices negotiated with insurers. Similarly, the Blue Pencil Index could lead to a violation if the ability of a judge to adjust, ex-post, the terms of a contract that was operable throughout an employment spell had a direct effect on prices. The consistency of estimates over a range of instruments, each of which has unique legal mechanisms for affecting organizational incentives, provides some reassurance that a potential violation of the exclusion restriction for any one legal measure is unlikely to drive the overall pattern of results.

Taken together, our results from MPIER and Census data suggest that the effects of consolidation on prices depend on a tradeoff between the efficiency gains of larger establishments and the increased negotiating power associated with bargaining as a larger organization. Larger establishments facilitate efficiency gains via economies of scale that appear to dominate any effects of bargaining position, leading negotiated prices to fall. The contrasting estimates from the firm-level component of the variation, however, suggest that consolidation of multi-establishment firms increases the bargaining position of firms by more than any efficiency gains, leading to higher negotiated prices.

6.7 Heterogeneity and Robustness

In this section we provide a concise overview of many supplemental analyses conducted to assess the robustness of our results to model assumptions and to potential measurement concerns. Due to the privacy restrictions in releasing results derived from the LBD, these analyses use measures of EC only (from MPIER data) and omit FC. Since these models omit FC and insurer concentration, the estimates should be interpreted with caution as suggestive, corroborating evidence. The coefficients on EC represent the combined impact of establishment concentration and the portion of the error term that is correlated with establishment concentration.

Market Structure Measure: Although our main estimates rely on HHIs, the most commonly used measure of market concentration in the literature (Gaynor et al. (2015)), interpreting these estimates as elasticities of demand requires the potentially undesirable assumptions that goods are homogeneous and firms engage in Cournot competition, as discussed in Section 6.1.2. Since we cannot estimate firm conduct directly without detailed claims data, we test the sensitivity of our estimates to these assumptions by re-estimating the model using the negative log HHI transformation, average establishment size, 4-firm market share, and 8-firm market share.

Table 7 shows that the qualitative conclusions are identical for all of these choices of market structure. In the negative log HHI specification, the sign is positive (which is consistent since the measure is negated), and the adjustment relative to the OLS specification goes in the same direction. When average establishment size is used, we find

Table 7: Alternative Measures of Market Concentration (Establishment-Based)

	Dependent Variable: $\ln(Price)_t$	
	IV	OLS
Negative Log $\text{HHI}_{(t-1)}$	0.336*	0.004*
	(0.120)	(0.002)
1st Stage F-Stat	[358.8]	
Mean Establishment $\text{Size}_{(t-1)}$	–0.060*	0.0003*
	(0.027)	(0.0001)
1st Stage F-Stat	[184.8]	
4-Firm Market $\text{Share}_{(t-1)}$	–0.037*	–0.0001
	(0.005)	(0.0002)
1st Stage F-Stat	[223.0]	
8-Firm Market $\text{Share}_{(t-1)}$	–0.054*	–0.0000
	(0.006)	(0.0002)
1st Stage F-Stat	[167.6]	

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. All concentration measures are calculated from establishment sizes in MPIER data, provided by CMS. 4-Firm and 8-Firm Market Shares are measured from 0 to 100. Cragg-Donald F-Statistics reported in brackets. All standard errors are clustered by state. * indicates significance at the 0.05 level.

that increasing the average number of physicians in a practice by one reduces negotiated prices by about 6%. Similarly, in markets that become more concentrated in terms of the market shares of the 4 largest or 8 largest establishments, average negotiated prices fall significantly. Across the variety of market structure measures, we conclude that there is a statistically significant negative relationship between establishment concentration and negotiated prices.

Geographic Market Definition: Although county is a commonly used market definition (See Baker et al. (2015), Schneider et al. (2008),) we also test whether the results are sensitive to this choice. Table 8 presents estimates of the main specification using counties, hospital service areas (HSAs), and primary care service areas (PCSAs) as potential market definitions.[22] HSAs are defined by the Dartmouth Atlas of Healthcare using data on patient locations and their choices between hospitals. We chose HSAs as a plausible upper bound on the size of markets since patients tend to travel farther on average to

[22]PCSAs and HSAs are likely preferred to counties as market definitions because they are based on patient flows, with PCSAs at the lower end of the relevant range and HSAs at the upper end of the range (we use only non-hospital based procedures, and patients tend to travel farther to hospitals than to physicians). In practice, our main specifications use counties to define markets because that is the feasible market definition in the Census data. Results in Table 8 indicate that estimates are comparable under each of these market definitions.

hospitals than they do for ambulatory physician visits. PCSAs are similarly defined by the Dartmouth Atlas but are based on choices of primary care physicians only. Since patients tend to travel farther to visit specialists than they do to visit primary care physicians, PCSAs are likely to be smaller on average than the appropriate overall market definition for physicians.

Table 8: Sensitivity of MPIER IV Estimates to Market Definition

	Dependent Variable: ln($Price$)				
	County	Metro County	Non-Metro County	HSA	PCSA
$\text{HHI}_{(t-1)}$	–0.019*	–0.031*	–0.005*	–0.032*	–0.037*
	(0.006)	(0.011)	(0.002)	(0.007)	(0.003)
1st Stage F-Stat	[232.4]	[144.3]	[182.8]	[420.4]	[582.8]

Notes: All specifications include fixed effects for the corresponding geographic market, census division by year, procedure code (CPT), physician specialty, and facility type. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHIs are scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. In each specification the instruments include all lagged law components, as in column (1) of Table 5. Standard errors, in parentheses, are clustered by state. First-stage Cragg-Donald F-statistics are reported in brackets. * indicates significance at the 0.05 level.

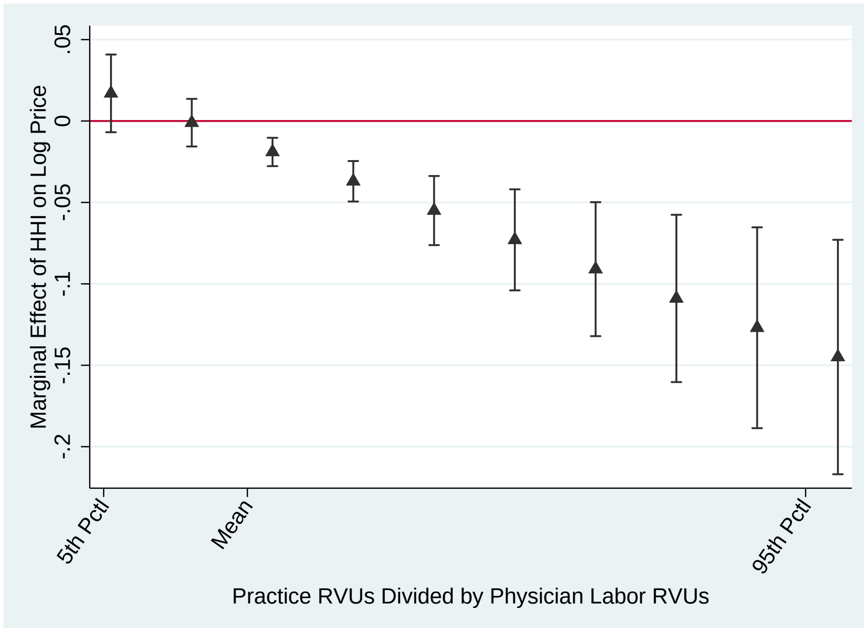
The estimates are similar, ranging from –1.9% in counties to –3.7% in PCSAs. Although appropriately defining markets is very important to evaluating price effects caused by differences in concentration levels, one potential reason why our estimates are fairly stable across market definitions is because we rely on changes in concentration within markets, thus differencing out many dimensions of unobservables.

Table 8 also includes heterogeneity in price effects in metropolitan and rural areas. These estimates come from splitting the sample into metro and non-metro counties. We find that a 100 point increase in establishment HHI causes a 3.1% decline in negotiated prices in metro areas. In non-metro counties the effect is small, –0.5%, though still statistically significant. This pattern is potentially consistent with greater economies of scale in metro markets, where input factor prices such as nursing and staff labor, rent, and equipment costs tend to be higher.

Heterogeneity by Procedure Type:

Returning to the analysis in Section 6.2 showing that reduced-form price effects were largest for procedures with high facility practice RVUs relative to physician labor RVUs, we estimate the corresponding IV model. Consistent with the reduced-form evidence, Figure 5 shows that the IV estimates are driven primarily by high-ratio procedures, with

Figure 5: Price Effects by Percentile of the Ratio of Medicare Facility Practice RVUs to Physician Labor RVUs



Notes: Points on the graph represent IV estimates of HHI on log price for procedure types at different points in the distribution of Medicare facility practice expense RVUs divided by physician work RVUs. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change on the typical 10,000 point scale. Error bars are 95% confidence intervals, based on standard errors clustered by state.

no significant effects of concentration on prices for procedures with low capital-to-labor RVU ratios.

Interactions between Physician and Insurer Concentration: Our main results in Table 5 (Panels 1 and 2) control for insurer HHI, which has little effect on our estimates. This result is surprising given previous work, such as Dafny et al. (2012), which shows that insurer concentration is an important determinant of market outcomes. One limitation to our research design, which includes county effects and census division by year effects, is that the impact of insurer concentration on prices is identified only off of year-to-year, within-state changes in insurer concentration relative to the census division average. These fixed effects may leave only a small share of the overall variation in insurance concentration, in part because insurance markets tend to be much larger than physician markets.

We re-estimate the MPIER model specification (shown in Table 5, Panel 3) while including interactions between physician establishment HHI and categories of insurer HHI using 2007 data on insurers from the American Medical Association. These estimates, presented in Appendix Table A13, show that the negative relationship between establishment concentration and prices remains, but we do not have enough power to measure heterogeneity based on interactions with insurer concentration. This question remains an interesting one for future research, potentially using other sources of variation in insurer concentration.

Exogeneity of NCA Law Changes: Using law changes as a source of identification generally raises the concern that the laws may not be exogenous to the outcome being investigated. The inclusion of county effects in our specifications removes average differences that may affect both NCA laws and outcomes, so our concern is limited to covariation within states over time. This could occur, for example, if political or economic environment that generated the law changes also affected the outcome of interest, potentially through other correlated laws or through intermediate factors other than physician market concentration.

We test for evidence that NCA law changes are correlated with a variety of economic outcomes or with state residents' subjective views from the Generalized Social Survey (GSS) on a variety of political, economic, and cultural topics. Appendix Table A20 shows that log payroll per worker, unemployment rates, and population are all uncorrelated with the law changes (columns 1-3). The share of votes for Republican presidential and congressional candidates is also uncorrelated with the law changes (column 4).

Appendix Table A21 presents tests of correlations between law changes and GSS survey responses. The first five columns relate to the respondent's views on size of government and spending on social issues, such as cities, welfare, and medical care. The last two columns reflect the respondent's political identification and financial satisfaction, respectively. The law changes appear uncorrelated with views captured in the GSS; only one of 49 coefficients in the table is significant at the 5% level, suggesting that NCA laws are not systematically driven by or correlated with important changes in the local political or economic climate.

Finally, we test whether NCA laws are correlated with state managed care penetration rates. Appendix Table A22 shows that HMO penetration rates in 2004 (the one year of our sample period for which data are publicly available) are not significantly associated with NCA laws in first lag or first lead. There doesn't seem to be evidence that the laws predict HMO penetration or that penetration leads judges to reconsider NCA policy if and when a case appears before them.

7 Discussion

This paper makes three main contributions towards understanding the relationships between physician practice organization and negotiated prices with private insurers in the US. First, we address several important data limitations that have impeded research on this topic. We build on existing work on physician markets by employing two comprehensive longitudinal data sets on physicians: one from CMS covering all physicians and practices in the US, and a second confidential database from the Census Bureau containing firm linkages for all multi-establishment practices using IRS tax IDs, and providing sales and payroll for every physician firm in the US. By linking these sources to a longitudinal database of negotiated prices between physicians and private insurers, we create a comprehensive new database with which to study physician markets, spanning virtually all markets in the country over 12 years. In addition to its breadth, this database has the advantage that it includes total sales of physician firms from all sources.

Second, we develop a new set of instruments to address the longstanding identification challenge in estimating the effects of practice organization on prices in physician markets. We evaluate the validity of using judicial decisions that change NCA policies as instruments for the potentially endogenous variation in physician practice organization and market concentration. After presenting evidence consistent with the IV assumptions, we use these instruments to estimate the effect of physician market concentration on negotiated prices. Our results highlight an important distinction between economies of scale in physician practices and the effect of larger practices on bargaining position. We find that when physician *establishments* grow larger, economies of scale dominate the effect of bargaining position on prices, leading to a net reduction in prices of about 1.4% to 1.9% per 100 unit increase in HHI. However, when physician *firms* grow larger conditional on establishment concentration, the opposite is true—a 100 point increase in HHI increases prices by about 1.7% to 2.1%, suggesting that any associated economies of scale are outweighed by the effects of firm consolidation on bargaining position. These results have important implications for policies aimed at protecting competition in physician markets, suggesting that practice mergers that coincide with physical consolidation may be more likely to lead to lower prices. They also suggest the importance of measuring both establishment and firm sizes for understanding the impacts of practice organization on prices.

Finally, these findings highlight the important role states play in affecting physician service prices through NCA policies. We show that even modest increases in NCA enforceability lead to meaningful increases in physician prices. As a rough back-of-the-envelope calculation, and abstracting from general equilibrium effects, our estimates suggest that if NCA enforceability decreased nationally by 0.1 units of the NCA Index, total physician spending would fall by about 4.2%—over $25 billion annually based on 2015 spending levels.[23] Yet 39 states have never legislatively chosen an NCA policy and instead leave the decisions to the judicial branch, in which common law traditions shape current policies. Our findings suggest that substantial value may arise from states conducting comprehensive assessments of NCA laws and actively legislating policies, drawing on the expanding research studying the impacts of NCAs.

As a matter of interpretation, one question that we cannot fully address in our analysis is whether the estimated changes in concentration and prices are good or bad for consumers. Consolidation of multi-establishment practices may improve geographic access or other aspects of medical care that consumers value. As such, if multi-establishment consolidation causes price increases by affecting the bargaining weights of physicians relative to insurers, these price increases may be of less concern to antitrust regulators than if they were caused by changes in bargaining threat points. Interpretation of our estimates further depends on the margin of variation we use, which may be unique relative to patterns of consolidation in physician markets more generally. Our estimates are local average treatment effects driven by responses to changes in NCA enforceability, and the margin around which we identify effects on prices may differ from the margin that has prompted the recent trend of hospital acquisitions of physician groups, for example. More research is necessary to extend our findings before drawing conclusions about welfare effects.

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Appendix for Online Publication

A Appendix Tables and Figures

Table A1: NCA Law Change Frequencies by Census Division

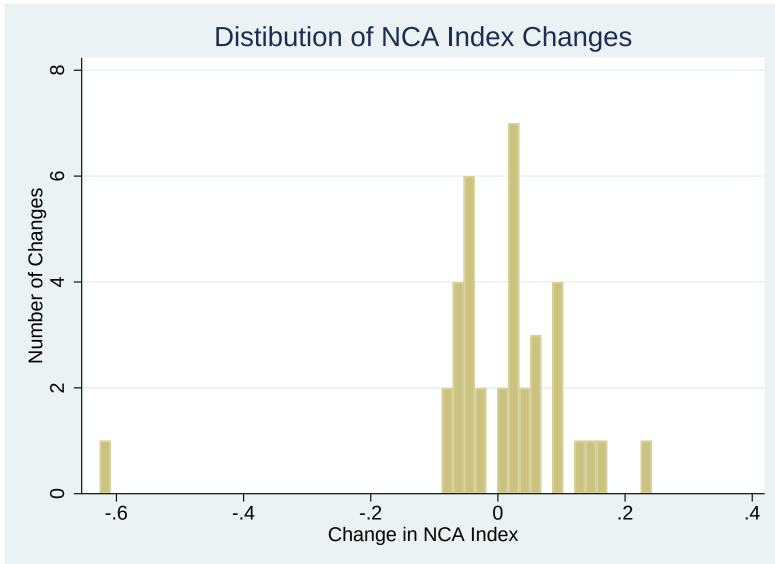
	East North Central	East South Central	Middle Atlantic	Mountain	New England	Pacific	South Atlantic	West North Central	West South Central	Total
					Positive Changes					
Statutory Index	0	1	0	0	0	0	0	1	0	2
Protectible Interest Index	0	1	1	2	1	1	1	2	2	11
Burden of Proof Index	1	1	1	2	1	0	1	0	0	7
Consideration Index Inception	0	0	0	0	0	0	0	1	0	1
Consideration Index Post-Inception	1	0	0	0	1	0	0	0	2	4
Blue Pencil Index	0	0	0	0	0	0	0	1	0	1
Employer Termination Index	0	0	2	0	0	0	0	0	0	2
					Negative Changes					
Statutory Index	1	1	0	2	0	0	1	0	2	7
Protectible Interest Index	1	1	0	0	0	1	0	0	0	3
Burden of Proof Index	0	1	1	0	0	0	1	1	0	4
Consideration Index Inception	0	1	1	0	0	0	0	0	0	2
Consideration Index Post-Inception	0	1	0	0	0	1	2	0	0	4
Blue Pencil Index	0	1	0	0	1	0	1	1	0	4
Employer Termination Index	0	0	0	0	0	0	0	0	0	0
Total All Dimensions	4	9	6	6	4	3	7	7	6	52

Table A2: Bishara (2011) Rating of the Restrictiveness of Non-Compete Agreements

Question #	Question	Criteria	Question Weight
Q1	Is there a state statute that governs the enforceability of covenants not to compete?	10 = Yes, favors strong enforcement 5 = Yes or no, in either case neutral on enforcement 0 = Yes, statute that disfavors enforcement	10
Q2	What is an employer's protectable interest and how is that defined?	10 = Broadly defined protectable interest 5 = Balanced approach to protectable interest 0 = Strictly defined, limiting the protectable interest of the employer	10
Q3	What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?	10 = Weak burden of proof on plaintiff (employer) 5 = Balanced burden of proof on plaintiff 0 = Strong burden of proof on plaintiff	5
Q3a	Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?	10 = Yes, start of employment always sufficient to support any CNC 5 = Sometimes sufficient to support CNC 0 = Never sufficient as consideration to support CNC	5
Q3b	Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?	10 = Continued employment always sufficient to support any CNC 5 = Only change in terms sufficient to support CNC 0 = Neither continued employment nor change in terms sufficient to support CNC	5
Q3c	Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?	10 = Continued employment always sufficient to support any CNC 5 = Only change in terms sufficient to support CNC 0 = Neither continued employment nor change in terms sufficient to support CNC	5
Q4	If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenant enforceable? If so, under what circumstances will the courts allow reduction and what form of reduction will the courts permit?	10 = Judicial modification allowed, broad circumstances and restrictions to maximum enforcement allowed 5 = Blue pencil allowed, balanced circumstances and restrictions to middle ground of allowed enforcement 0 = Blue pencil or modification not allowed	10
Q8	If the employer terminates the employment relationship, is the covenant enforceable?	10 = Enforceable if employer terminates 5 = Enforceable in some circumstances 0 = Not enforceable if employer terminates	10

Source: Bishara (2011). Notes: The questions in the table correspond to the NCA law components used in the IV estimates throughout the paper. In the paper and tables, we refer to Q1 as the 'Statutory Index', to Q2 as the 'Protectible Interest Index', to Q3 as the 'Burden of Proof Index', to Q3a as 'Consideration Index Inception', to Q3b and Q3c together as 'Consideration Index Post-Inception', to Q4 as 'Blue Pencil Index', and to Q8 as 'Employer Termination Index'. In the raw data, the laws are scaled in each state-year from 0 to 10, as indicated by this table. In the estimations, each component is rescaled to range from 0 to 1, where 0 is the least restrictive observation in the data and 1 is the most.

Figure A1: Distribution of NCA Index Changes



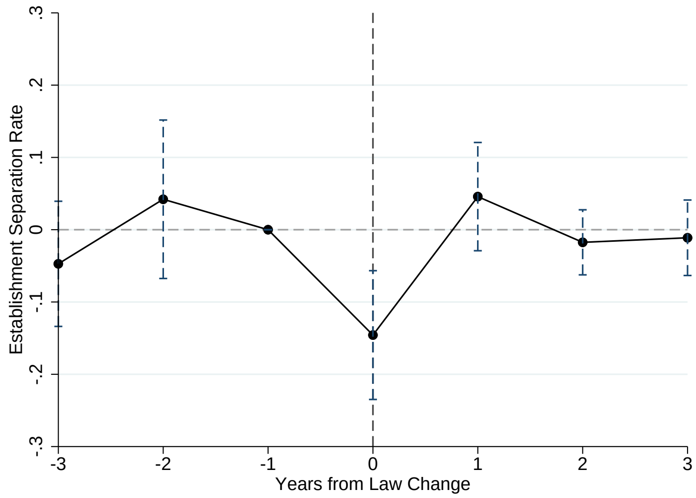
Notes: Data points underlying the histogram are state-year observations of year-to-year changes in the NCA Index, which is a weighted sum of the 7 NCA law dimensions. The Index is scaled to range from 0 to 1, where 0 is the least restrictive state-year in the sample and 1 is the most restrictive. Changes in the Index can thus range from -1 to 1.

Table A3: NCA Law Components: Descriptive Statistics

	Mean	SD	N (State-Years)
Statutory Index	0.55	0.24	612
Protectible Interest Index	0.59	0.24	604
Burden of Proof Index	0.56	0.27	599
Consideration Index Inception	0.85	0.29	562
Consideration Index Post-Inception	0.70	0.33	524
Blue Pencil Index	0.53	0.34	538
Employer Termination Index	0.62	0.29	407

Notes: Statistics in the table represent data from 1995-2007 for each state-year in which a legal precedent exists. The minimum of each component is 0 and the maximum of each component is normalized to 1.

Figure A2: Event Study: Physician-Establishment Separation Rates
Before and After Decrease in Enforceability



Notes: Sample includes treatment states with only one law change within the event window, and control states in the same Census division as the treatment state that had no law changes during the corresponding event window. Estimates are from fixed effects regressions including county effects, census division by year effects, and specialty effects. Specialties included in sample are primary care and non-surgical specialists. Dashed lines represent 95% confidence intervals based on standard errors clustered by state. Year 0 is the calendar year during which the law change occurred, and the dependent variable is normalized to zero in year -1.

Table A4: Fixed Effects Models of Establishment Births and Deaths

Dependent Variable:	Births		Deaths	
	Univar.	Multivar.	Univar.	Multivar.
	(1)	(2)	(3)	(4)
Statutory Index$_{t-1}$	–1.277*	–0.608*	–1.347*	–0.733*
	(0.091)	(0.092)	(0.121)	(0.124)
Protectible Interest Index$_{t-1}$	0.582*	1.258*	0.609*	1.205*
	(0.066)	(0.158)	(0.089)	(0.177)
Burden of Proof Index$_{t-1}$	–0.633*	–3.689*	–0.531*	–3.657*
	(0.117)	(0.270)	(0.136)	(0.329)
Consideration Index Inception$_{t-1}$	0.023	3.392*	–0.354*	2.039*
	(0.088)	(0.299)	(0.090)	(0.265)
Consideration Index Post-Inception$_{t-1}$	–0.293*	–0.848*	0.081*	–0.459*
	(0.050)	(0.093)	(0.038)	(0.074)
Blue Pencil Index$_{t-1}$	0.234*	0.286*	–0.197*	–0.306*
	(0.041)	(0.060)	(0.048)	(0.065)
Employer Termination Index$_{t-1}$	–4.020*	–4.677*	–4.424*	–4.529*
	(0.513)	(0.630)	(0.682)	(0.780)
Number of Physicians in County	0.070*	0.070*	0.123*	0.123*
	(0.012)	(0.012)	(0.019)	(0.019)
N		599,975		599,975
R-Sq		0.43		0.34

Notes: Columns 1 and 3 report estimates from separate univariate regressions, and columns 2 and 4 report estimates from mutivariate regressions of the number of establishment births and deaths (MPIER) on the 7 NCA law indices, controlling for the aggregate supply of physicians, and including fixed effects for county by medical specialty, and census division by year. Huber-White standard errors reported in parentheses. * indicates significance at the 0.05 level.

Table A5: Lasso IV: Effect of Establishment-Based Market Concentration on Prices

	Dependent Variable: $\ln(Price)_t$
$\text{HHI}_{(t-1)}$	–0.027*
	(0.012)

First Stage: Effect of NCA Laws on Establishment-Based Market Concentration

	Dependent Variable: HHI_{t-1}
Consideration Index Post-Inception$_{t-1}$	–3.14*
	(0.52)
Burden of Proof Index$_{t-1}$	–3.35*
	(0.09)
Employer Termination Index$_{t-1}$	–4.49*
	(0.15)
N	3,226,388
R-Sq	0.60
CD F-Stat	329.9
KP F-Stat	511.2

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. All independent variables are scaled to range between 0 and 1, where 1 is the strongest observed measure of the variable in any state and year in the data. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Table A6: IV First Stage Estimates: Effect of NCA Laws on Sales-Based HHI

Dependent Variable:	Estab. HHI_{t-1} (1)	Firm HHI_{t-1} (2)
Statutory $Index_{t-1}$	–0.25	–3.09
	(2.16)	(2.31)
Protectible Interest $Index_{t-1}$	14.11*	7.23
	(4.45)	(4.10)
Consideration Index $Inception_{t-1}$	17.26*	22.56*
	(6.20)	(10.57)
Consideration Index $Post\text{-}Inception_{t-1}$	–2.19*	2.79*
	(0.41)	(1.45)
Burden of Proof $Index_{t-1}$	–16.15*	–19.49*
	(6.08)	(9.22)
Blue Pencil $Index_{t-1}$	–0.75	0.53
	(3.27)	(4.61)
Employer Termination $Index_{t-1}$	–24.06*	–10.28*
	(4.53)	(4.44)
Insurer HHI_{t-1}	0.00	–0.04
	(0.01)	(0.02)
MPIER Data Used	Yes	Yes
Census Data Used	Yes	Yes
N	6,330,000	
R-Sq	0.83	
CD F-Statistic	270.27	
KP F-Statistic	52.2	

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Legal indices are scaled to range between 0 and 1, where 1 is the strongest observed measure of the variable in any state and year in the data. Firm HHIs are based on sales from the Census LBD and SSEL, and establishment HHIs are based on employment levels from MPIER. HHIs are scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors are clustered by state. Cragg-Donald F-statistic and Kleinbergen-Paap F-statistic reported. * indicates significance at the 0.05 level.

Table A7: Sensitivity to IV Estimator

	Dependent Variable: $\ln(Price)_t$		
Estimator:	2SLS	LIML	2SGMM
HHI_{t-1}	–0.019*	–0.028*	–0.020*
	(0.006)	(0.008)	(0.0004)
N	3,226,388	3,226,388	3,226,388
1st Stage CD F-Stat	232.4	232.4	232.4
1st Stage KP F-Stat	1090.3	1090.3	1090.3

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Table A8: IV Second Stage Estimates: MPIER HHIs, Markets defined by county only

	Dependent Variable: ln($Price$)	
	IV	OLS
$\text{HHI}_{(t-1)}$	–0.018*	–0.000
	(0.004)	(0.000)
N	3,226,388	3,226,388
1st Stage CD F-Stat	200.6	
1st Stage KP F-Stat	2562.4	

Notes:All specifications include fixed effects for county, census division by year, procedure code (CPT), and facility type. Markets are defined by county only, and are not differentiated by physician specialty. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Table A9: Fixed Effects Models of Insurer Concentration (10,000 Scale)

Dependent Variable: Insurer HHI (LBD)	Lags 1 and 2 (1)	Lag 1 (2)
Statutory Index$_{t-1}$	–8.63	28.44
	(30.73)	(14.81)
Protectible Interest Index$_{t-1}$	1.65	–6.00
	(17.31)	(12.56)
Burden of Proof Index$_{t-1}$	30.87	74.99
	(62.21)	(72.22)
Consideration Index Inception$_{t-1}$	9.97	2.56
	(5.90)	(3.01)
Consideration Index Post-Inception$_{t-1}$	–38.24	-82.91
	(63.16)	(71.09)
Blue Pencil Index$_{t-1}$	21.24	-18.68
	(22.92)	(16.20)
Employer Termination Index$_{t-1}$	–13.12	–4.86
	(17.13)	(13.10)
Statutory Index$_{t-2}$	37.33	
	(24.03)	
Protectible Interest Index$_{t-2}$	1.44	
	(15.64)	
Burden of Proof Index$_{t-2}$	46.75	
	(31.59)	
Consideration Index Inception$_{t-2}$	–10.11	
	(7.30)	
Consideration Index Post-Inception$_{t-2}$	–50.81	
	(27.77)	
Blue Pencil Index$_{t-2}$	–56.72	
	(39.52)	
Employer Termination Index$_{t-2}$	6.50	
	(16.10)	
N	6,509,000	6,509,000
R-Sq	0.903	0.898

Notes: Column 1 reports estimates from a regression of Insurer HHI, calculated at the state level from LBD data, on first- and second-lagged NCA laws, while Column 2 reports estimates from the analogous regression with only first-lagged laws. All specifications include fixed effects for county and census division by year. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Robustness to Treatment of Multi-Specialty Practices: Defining markets by specialty involves assumptions about how to treat physicians in multi-specialty practices. For example, when defining a market for orthopedists, how should one treat practices that contain orthopedists as well as radiologists? One approach is to ignore radiologists altogether and only consider the market shares of orthopedists in the geographic market. However, an insurer concerned about the negative consequences of failing to reach an agreement with such a practice may care about the consequences of losing both the orthopedists and the radiologists. Our main specifications calculate HHIs using all physicians in any practice containing at least one physician in a given specialty. In Appendix Table A10, we consider four different possible sets of assumptions about the treatment of multispecialty practices in measuring concentration. The estimates are similar under each alternative assumption tested, though in some cases alternative assumptions increase the coefficient estimates and decrease precision.

Table A10: IV Second Stage Estimates for Alternative MPIER HHI Measures

	Dependent Variable: ln(*Price*)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$HHI_{(t-1)}$	–0.019*	–0.018*	–0.018*	–0.017*	–0.026	–0.027	–0.023	–0.023
	(0.008)	(0.004)	(0.008)	(0.007)	(0.021)	(0.019)	(0.015)	(0.015)
N	3,226,388	2,981,709	2,894,189	2,894,189	2,981,709	2,981,709	2,894,189	2,894,189
CD F-Stat	210.2	600.5	400.4	467.5	98.5	112.8	172.3	172.3
KP F-Stat	382.0	739.1	1298.2	1238.4	246.2	350.1	990.8	990.8

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. HHI is calculated from establishment sizes in MPIER data, provided by CMS. In column (1) the HHI is measured including all physicians in any group that has at least one member in a given specialty, and assumes physicians with missing addresses are solo establishments. The HHI in column (1) is the one used throughout the paper. The HHI in column (2) is similar to that in column (1), but assumes all physicians in a given market with missing addresses are in the same establishment. In column (3) the HHI is measured including all physicians in any group that has at least one member in a given specialty, drops observations with missing addresses if the same physician has another known address in the same zip code, and assumes all remaining missing addresses are solo establishments. The HHI in column (4) is similar to that in column (3), but assumes all remaining missing addresses in a given market are a single establishment. In column (5) the HHI is measured including only physicians in the given specialty within the market, and assumes physicians with missing addresses are solo establishments. The HHI in column (6) is similar to that in column (5), but assumes all physicians in a given market with missing addresses are in the same establishment. In column (7) the HHI is measured including only physicians in the given specialty within the market, drops observations with missing addresses if the same physician has another known address in the same zip code, and assumes all remaining missing addresses are solo establishments. The HHI in column (8) is similar to that in column (7), but assumes all remaining missing addresses in a given market are a single establishment. All HHIs are scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. In each specification, the instruments include all first-lagged (corresponding to the specification in Panel 3 of Table 5). All standard errors clustered by state. * indicates significance at the 0.05 level.

Table A11: First Stage IV Signs by Subsample

	Dependent Variable: ln($Price$)								
	All Counties (1)	Metro Counties (2)	Non-Metro Counties (3)	Primary Care (4)	Non-Surg. Specialists (5)	Positive Law Changes (6)	Negative Law Changes (7)	High HHI (8)	Low HHI (9)
Statutory Index$_{t-1}$				-			-		
Protectible Interest Index$_{t-1}$	+	+	+		+			+	+
Burden of Proof Index$_{t-1}$	+	+		+	+	+		+	+
Consideration Index Inception$_{t-1}$	-	-	-	-	-			-	
Consideration Index Post-Inception$_{t-1}$	-	-		-	-	-		-	-
Blue Pencil Index$_{t-1}$	+	+		+	-			+	-
Employer Termination Index$_{t-1}$	-	-		-	-	-		-	-

Notes: All estimates are based on the main specification, including fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. '+' and '-' indicate that the coefficient is positive (negative) and statistically significant at the 0.05 level, based on standard errors clustered by state. 'Positive (Negative) Law Changes' includes all observations in states that ever have an increase (decrease) in NCA enforceability Missing indicates non-identified or insignificant coefficients. 'High (Low) HHI' refers to observations with HHI levels above (below) the median level.

Table A12: IV Estimates Excluding Blue Pencil Index

	Dependent Variable: $\ln(Price)_t$
HHI_{t-1}	–0.020*
	(0.006)
N	3,226,388
1st Stage CD F-Stat	266.8
1st Stage KP F-Stat	627.4

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Instruments do not include Blue Pencil Index, which is the only index with a positive coefficient in the univariate just-identified first-stage model, as shown in Table 6. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Table A13: Interactions between Physician and Insurer Concentration

	Dependent Variable: $\ln(Price)_t$					
	(1)	(2)	(3)	(4)	(5)	(6)
		All Counties			Metro Counties	
$Phys\ HHI_{t-1}$	–0.026*	–0.024*	–0.019*	–0.036*	–0.034*	–0.030*
	(0.007)	(0.009)	(0.007)	(0.005)	(0.004)	(0.012)
$Phys\ HHI_{t-1} \times I(Ins\ HHI > 25thPctl)$	0.015			0.015		
	(0.024)			(0.025)		
$Phys\ HHI_{t-1} \times I(Ins\ HHI > 50thPctl)$		0.009			0.010	
		(0.024)			(0.024)	
$Phys\ HHI_{t-1} \times I(Ins\ HHI > 75thPctl)$			0.003			0.007
			(0.023)			(0.026)
N	3,226,388	3,226,388	3,226,388	2,196,226	2,196,226	2,196,226
1st Stage CD F-Stat	114.3	152.3	70.9	80.1	117.8	43.4
1st Stage KP F-Stat	390.9	269.2	51.9	54.5	19.3	4.9

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Physician HHIs are calculated from establishment sizes in MPIER data, insurer HHIs are state-level measures in 2007 from the AMA. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change on the typical 10,000 point scale. '25th Pctl' refers to the 25th percentile of the distribution of insurer concentration. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Table A14: Economies of Scale

	Dependent Variable: $\ln(Price)_t$			
	(1)	(2)	(3)	(4)
	All Counties		High Phys. Supply	Low Phys. Supply
Establishment Size	–0.0602*	–0.0900	–0.0954*	–0.0242
	(0.0270)	(0.0656)	(0.0436)	(0.0369)
Establishment Size Sq.		0.0009	0.0006	-0.0004
		(0.0017)	(0.0007)	(0.0007)
N	3,309,701	3,309,701	626,858	2,682,843
1st Stage CD F-Stat	184.8	1.5	1.8	14.0
1st Stage KP F-Stat	3321.8	70.9	752.1	397.2

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Establishment sizes are means by county, specialty, year, and facility type. 'Low (High) Phys. Supply' denotes counties with below (above) the mean number of physicians. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Table A15: Effect of Concentration on Prices, by Medical Specialty and Urban Status

	Dependent Variable: $\ln(Price)_t$		
	All Counties (1)	Metro Counties (2)	Non-Metro Counties (3)
	All Physicians		
HHI_{t-1}	–0.019*	–0.031*	–0.005*
	(0.006)	(0.011)	(0.002)
N	3,226,388	2,196,226	1,030,162
1st Stage CD F-Stat	232.4	144.3	182.8
	Primary Care Physicians		
HHI_{t-1}	–0.013*	–0.024*	–0.001
	(0.004)	(0.005)	(0.004)
N	486,004	312,859	173,145
1st Stage CD F-Stat	96.8	55.44	79.3
	Non-Surgical Specialists		
HHI_{t-1}	–0.010	–0.020*	0.001
	(0.005)	(0.006)	(0.002)
N	317,654	243,308	74,346
1st Stage CD F-Stat	99.5	56.3	43.5
	Surgical Specialists		
HHI_{t-1}	–0.002	–0.008	0.004
	(0.016)	(0.015)	(0.014)
N	262,029	183,485	78,544
1st Stage CD F-Stat	36.9	45.8	5.3

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All estimates represent the second stage coefficient on HHI in 2SLS models corresponding to that in Table 5 Panel 3, here for all counties, metro counties, and non-metro counties. The first column of the first panel reproduces the second stage results for all physicians in Table 5, Panel 3. The 'Primary Care Physicians' sample includes primary care MDs (excluding DOs), Internal Medicine, Family Practice, Geriatric Medicine, and Pediatric specialists. The 'Non-Surgical Specialist' sample includes specialists in Proctology, Urology, Dermatology, Cardiovascular Dis/Cardiology, Neurology, Gastroenterology, and Hematology. The 'Surgical Specialist' sample includes specialists in General Surgery, Neurological Surgery, Orthopaedic Surgery, Thoracic Surgery, Anesthesiology, and Radiology. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Robustness to Balanced Panel Restriction: The sample size of the MarketScan price data increases over time. To test whether the imbalance in our panel caused by sample growth affects our baseline results, we re-estimate the model using only the subset of county-specialty pairs for which we have price data in all years of our panel. The IV estimates, shown in Appendix Table A16, are similar in the balanced panel.

Table A16: IV Estimates on Balanced Panel

	Dependent Variable: $\ln(Price)$
$\text{HHI}_{(t-1)}$	–0.018*
	(0.005)
N	2,418,152
1st Stage CD F-Stat	435.0
1st Stage KP F-Stat	1239.9

Notes: All specifications are the same as in Table 5 panel 3, except the sample includes only observations corresponding to a county-specialty pair that is observed in all 12 years of the panel. All standard errors clustered by state. * indicates significance at the .05 level.

Robustness to Fuzzy Matching Algorithm and Measurement Error:

The association of addresses to practices requires an assumption about the tolerance in the fuzzy matching algorithm. The algorithm allows characters in the addresses to be slightly different, allowing for typographic errors and abbreviations, while forcing numerical elements of the addresses to be exactly identical (that is, street numbers and office numbers must match exactly.) We use the normalized Levenshtein distance as a metric for the distance between all combinations of character subsets of addresses in the same zip code. Appendix Table A17 presents estimates from the main specification by re-calculating HHIs under alternative fuzzy matching thresholds that allow for stricter or more flexible matching of addresses. Smaller distance thresholds result in smaller average establishment sizes by forcing addresses to almost exactly match, while the opposite is true for larger thresholds. The results have very little sensitivity to this tolerance parameter, ranging from –0.019 to –0.020 in all nine specifications.

Table A17: Sensitivity of MPIER Second Stage IV Estimates to Fuzzy Matching Algorithm Parameter

Normalized Levenshtein Distance Threshold	IV Estimate	First Stage KP F-Stat.
0.01	–0.020* (0.006)	213.9
0.05	–0.019* (0.006)	244.6
0.10	–0.019* (0.006)	239.4
0.15	–0.019* (0.006)	229.8
0.20	–0.019* (0.006)	232.4
0.25	–0.020* (0.006)	221.3
0.30	–0.019* (0.005)	233.9
0.35	–0.020* (0.006)	240.2
0.40	–0.019* (0.005)	241.6

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. IVs are the full set of first and second lags of law components. The normalized Levenshtein Distance equals the minimum number of character insertions, deletions, or substitutions necessary to make two strings equal, divided by the length of the shorter string. The threshold value is the value of the normalized Levenshtein distance below which the character elements of two addresses in the MPIER are assumed to be equivalent. A larger threshold value results in overestimating the size of establishments, while too low a value in the presence of typographical errors may lead to an underestimate of establishment sizes. The main estimates in the paper are based on a threshold value of 0.20. HHI is calculated from establishment sizes in MPIER data, provided by CMS. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors are clustered by state. * indicates significance at the 0.05 level.

Table A18: Fixed Effects Models of Aggregate Physician Supply

Dependent Variable:	Log Physicians per 100,000 Population	
NCA Index$_t$	-0.027	
	(0.041)	
NCA Index$_{t-1}$	–0.022	–0.043
	(0.045)	(0.030)
Log Per Capita Income	0.156*	0.156*
	(0.030)	(0.030)
N	48,807	48,807
Adj. R Sq.	0.87	0.87

Notes: All specifications are fixed effects models and include county effects and census division by year effects. * indicates significance at the 0.05 level.

Table A19: IV Estimates with Alternative Lag Assumptions

	Dependent Variable: ln($Price$)		
	First Lags	Second Lags	First and Second Lags
	(1)	(2)	(3)
HHI$_{t-1}$	–0.019*	–0.017*	–0.017*
	(0.006)	(0.006)	(0.006)
N	3,226,388	3,226,388	3,226,388

Notes: All specifications are fixed effects models and include county effects, year effects, census division by year effects, procedure code (CPT) effects, physician specialty effects, and facility type effects. HHI is scaled to range from 0 to 100, so that a 1 unit change in HHI corresponds to a 100 point change in the typical 10,000 point scale. All standard errors clustered by state. * Significant at the .05 level.

Table A20: Correlation of Law Changes with State Political and Economic Outcomes

Dependent Variable:	Log Payroll per Worker (1)	Unemployment Rate (2)	Population (3)	Republican Vote Share (4)
Statutory Index$_{t-1}$	–0.010	1.148*	–2183.276*	0.050
	(0.022)	(0.559)	(1073.492)	(0.033)
Protectible Interest Index$_{t-1}$	0.060	–0.636	724.974	–0.037
	(0.078)	(0.785)	(584.068)	(0.044)
Burden of Proof Index$_{t-1}$	0.051	0.762	–139.085	–0.034
	(0.040)	(0.859)	(520.628)	(0.061)
Consideration Index Inception$_{t-1}$	–0.056	0.328	678.706	–0.012
	(0.057)	(1.151)	(970.078)	(0.098)
Consideration Index Post-Inception$_{t-1}$	–0.038	–0.345	–367.454	0.035
	(0.023)	(0.599)	(252.488)	(0.035)
Blue Pencil Index$_{t-1}$	0.009	–0.702	–1485.250*	0.024
	(0.034)	(0.528)	(735.155)	(0.039)
Employer Termination Index$_{t-1}$	–0.119	–0.612	–567.853	–0.057
	(0.061)	(0.778)	(481.806)	(0.065)
N	969	969	969	510
N Clusters	51	51	51	51

Notes: An observation in these regressions is a state-year, and regressions are estimated by OLS with state and year fixed effects. All independent variables are scaled to range from 0 to 1, where 1 is the strongest observed measure of the variable in any state and year in the data. Standard errors are clustered by state. Data are from the Bureau of Labor Statistics (cols. 1 and 2), the Census Bureau (col. 3), and the Federal Election Commission (col. 4: presidential and congressional elections – every two years). Population is measured in thousands. Unemployment rate is measured in percentage points. * indicates significance at the 0.05 level.

Table A21: Correlation of Law Changes with Political and Economic Views in the GSS

Dependent Variable:	Respondent Thinks The Government Should Do Less:		Respondent Thinks We are Spending too Much On:			Respondent Considers Himself:	
	In General	To Help Pay for Medical Care	Urban Issues	Welfare	Nation's Health	A Republican	Satisfied With His Finacial Situation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Statutory Index$_{t-1}$	0.316	0.031	–0.166	–0.102	–0.121	–0.009	–0.297
	(0.177)	(0.120)	(0.257)	(0.322)	(0.155)	(0.169)	(0.216)
Protectible Interest Index$_{t-1}$	–0.026	0.074	–0.427	–0.513	0.021	–0.331	–0.074
	(0.376)	(0.196)	(0.372)	(0.462)	(0.210)	(0.365)	(0.363)
Burden of Proof Index$_{t-1}$	–0.103	–0.031	–0.685	0.394	0.215	–0.141	–0.454
	(0.383)	(0.360)	(0.515)	(0.745)	(0.343)	(0.502)	(0.317)
Consideration Index Inception$_{t-1}$	0.029	–0.092	0.819	–0.164	–0.453	0.463	0.527
	(0.438)	(0.340)	(0.558)	(0.758)	(0.347)	(0.502)	(0.422)
Consideration Index Post-Inception$_{t-1}$	0.144	–0.131	–0.034	0.151	0.546*	–0.062	0.001
	(0.123)	(0.086)	(0.407)	(0.244)	(0.208)	(0.271)	(0.237)
Blue Pencil Index$_{t-1}$	–0.297	0.365	–0.026	–0.121	0.268	–0.297	0.228
	(0.339)	(0.240)	(0.468)	(0.474)	(0.317)	(0.405)	(0.535)
Employer Termination Index$_{t-1}$	0.817	0.738	–0.974	–0.197	0.237	–0.325	0.631
	(0.532)	(0.568)	(0.484)	(0.791)	(0.590)	(0.538)	(1.006)
N	1,026	1,026	1,026	1,026	1,026	1,026	1,026
N Clusters	28	28	28	28	28	28	28

Notes: Regressions are linear probability models in which an observation is a survey respondent in a given year and a positive outcome represents the respondent's agreement with the statement presented in each column. All regressions include state, year, occupation, and industry fixed effects, as well as controls for age, education, marital status, and employment status. All independent variables are scaled to range from 0 to 1, where 1 is the strongest observed measure of the variable in any state and year in the data. Standard errors are clustered by state. Data on political and economic views are taken from the Generalized Social Survey for the years 1993-2010, where data exist (approximately every other year and in only 28 states). * indicates significance at the 0.05 level.

Table A22: NCA Laws and Managed Care Penetration Rates

Dependent Variable: HMO Penetration Rate, 2004	First Lagged Laws (1)	First Lead Laws (2)
Statutory Index	0.188	0.350
	(1.020)	(1.010)
Protectible Interest Index	1.112	1.299
	(1.009)	(1.161)
Burden of Proof Index	–1.050	–1.307
	(0.935)	(1.040)
Consideration Index Inception	0.267	0.219
	(1.066)	(1.042)
Consideration Index Post-Inception	–0.411	–0.287
	(0.635)	(0.604)
Blue Pencil Index	0.445	0.319
	(0.435)	(0.429)
Employer Termination Index	–0.883	–0.942
	(1.191)	(1.252)
N	328	329
R-Sq	0.07	0.07

Notes: Column 1 reports estimates from a regression of HMO penetration rates in 2004 (the only year in our sample period for which data is publicly available by state) on first-lagged NCA laws. Column 2 reports estimates from the same regression but with laws included as a one-period lead. Standard errors are clustered at the state level. * indicates significance at the 0.05 level.

B Appendix Notes to Conceptual Framework: NCA Laws and Practice Organization

These notes elaborate on the mechanics of the conceptual framework presented in Section 3.

Case 1: NCA practice attempts to merge with no-NCA practice.

First, note that if the merger attempt is successful, the location of the indifferent consumer will change.

$$dx/2 = d(\frac{1}{4} - x)$$

$$x = \frac{1}{4(1.5)} = 1/6$$

That is, the indifferent point between the NCA practice and the no-NCA practice (that is not involved in the merger) moves from 1/8 away from the NCA firm to 1/6 away. Therefore the NCA practice gains 1/16 of the total demand along the circle.

If the no-NCA firm has a spinoff, the NCA firm has the same demand as it did before the merger. If the NCA firm has a spinoff, demand is reduced. The expected change in the flow of profits for the NCA practice:

$$\Delta\pi_N = [(1-\epsilon)(1-\epsilon(1-\theta))]\left[\frac{5pD}{16} - \frac{pD}{4} - w_1\left(\frac{5t}{16\overline{K}^{\beta}}\right)^{1/\alpha_1} + w_1\left(\frac{t}{4\overline{K}^{\beta}}\right)^{1/\alpha_1}\right]$$

$$+\epsilon^2(1-\epsilon)\theta) * 0 + [\epsilon(1-\theta)]\left[\frac{pD}{8} - \frac{pD}{4} - w_1\left(\frac{t}{8\overline{K}^{\beta}}\right)^{1/\alpha_1} + w_1\left(\frac{t}{4\overline{K}^{\beta}}\right)^{1/\alpha_1}\right]$$

$$\Delta\pi_N = \frac{pD}{16}\left[1 - 4\epsilon + 3\epsilon\theta + \epsilon^2 - \epsilon^2\theta\right] + [(1-\epsilon)(1-\epsilon(1-\theta))]\, w_1\left(\frac{t}{16\overline{K}^{\beta}}\right)^{1/\alpha_1}\left[4^{1/\alpha_1} - 5^{1/\alpha_1}\right]$$

$$+ [\epsilon(1-\theta)]\, w_1\left(\frac{t}{16\overline{K}^{\beta}}\right)^{1/\alpha_1}\left[4^{1/\alpha_1} - 2^{1/\alpha_1}\right]$$

$$\Delta\pi_N = \frac{pD}{16}\left[1 - 4\epsilon + 3\epsilon\theta + \epsilon^2 - \epsilon^2\theta\right]$$

$$+w_1\left(\frac{t}{16\overline{K}^{\beta}}\right)^{1/\alpha_1}\left[4^{1/\alpha_1}\left(1 - \epsilon + \epsilon^2 - \epsilon^2\theta\right) - 5^{1/\alpha_1}\left(1 - 2\epsilon + \epsilon\theta + \epsilon^2 - \epsilon^2\theta\right) - 2^{1/\alpha_1}\left(\epsilon - \epsilon\theta\right)\right]$$

The expected change in the flow of profits for the no-NCA practice:

$$\Delta\pi_C = [(1-\epsilon)(1-\epsilon(1-\theta))]\left[\frac{5pD}{16} - \frac{pD}{4} - w_0\left(\frac{5t}{16\overline{K}^{\beta}}\right)^{1/\alpha_0} + w_0\left(\frac{t}{4\overline{K}^{\beta}}\right)^{1/\alpha_0}\right]$$

$$+(1-\epsilon)\epsilon(1-\theta)*0+\epsilon\left[\frac{pD}{8}-\frac{pD}{4}-w_0\left(\frac{t}{8\overline{K}^{\beta}}\right)^{1/\alpha_0}+w_0\left(\frac{t}{4\overline{K}^{\beta}}\right)^{1/\alpha_0}\right]$$

$$\frac{\partial\Delta\pi_C}{\partial\theta}>0$$

Case 2: The two practices with NCAs merge
The problem is symmetric, so we just need to solve one practice's change in profits.

If the merger is successful, the indifferent consumer will again be located $x = 1/6$ away from the NCA practice, this time on each side of the NCA practice. The total gain in market share for each of the NCA practices is 1/8 of the total demand along the circle.

The expected change in the flow of profits is:

$$\Delta\pi_N=\left[(1-\epsilon+\epsilon\theta)^2\right]\left[\frac{3pD}{8}-\frac{pD}{4}-w_1\left(\frac{3t}{8\overline{K}^{\beta}}\right)^{1/\alpha_1}+w_1\left(\frac{t}{4\overline{K}^{\beta}}\right)^{1/\alpha_1}\right]$$

$$+(1-\epsilon+\epsilon\theta)(\epsilon(1-\theta))*0+[\epsilon(1-\theta)]\left[\frac{pD}{8}-\frac{pD}{4}-w_1\left(\frac{t}{8\overline{K}^{\beta}}\right)^{1/\alpha_1}+w_1\left(\frac{t}{4\overline{K}^{\beta}}\right)^{1/\alpha_1}\right]$$

The expected increase in profits from the merger is larger, so the additional profits from the merger are more likely to exceed M. Again, $\frac{\partial\Delta\pi_N}{\partial\theta}>0$, so there is a range of values for M and times t for which an increase in θ will cause practices to merge. The merger can still happen even if only one practice uses NCAs, but it is more likely to occur between practices that both use NCAs.

C Bargaining Model

We model bargaining between physician groups and insurers following the setup of Ho and Lee (2016). The purpose of the model is to derive a relationship between negotiated prices and firm sizes under a set of plausible assumptions, and clarify how our empirical estimates can provide bounds on the underlying theoretical parameters. The market consists of a set of physician groups j and insurers i. Enrollees in insurance plan i can only visit a physician that is in the insurer's network, where the network is denoted by $\mathcal{G}_i \subseteq \{0,1\}^{i \times j}$. Similarly, $\mathcal{G}_j$ is the set of insurers with whom physician group j has contracted to be included in the network.

In each period of the model the following events take place. First, insurers and physician groups conduct simultaneous bilateral bargains over capitated prices p_{ij}, which are private knowledge of the negotiating parties.[24] Simultaneously with bargaining, insurers set profit-maximizing uniform premiums ϕ_i. Next, consumers form willingnesses to pay for insurance plans based on premiums and physician access in the network, measured by the amount of time a patient has to wait to get an appointment, $w_i(\phi_i, \mathcal{G})$, which depends on plan enrollment (and therefore plan premiums) and the size of the provider network. Finally, consumers probabilistically get sick and derive utility from being treated by a physician, and disutility from waiting for an appointment.

There are several simplifying assumptions about consumer choices. First, consumers are assumed to be incapable of discerning physician quality; they view physicians as homogeneous and value networks insofar as they differ in access. This assumption is made due to data limitations. In the hospital setting it is possible to obtain data on input choices for each hospital in a given market, which can allow researchers to estimate cost functions directly and model latent quality differences through fixed hospital effects (see Ho and Lee, 2016.) In physician markets there are no known similar data on the input choices of every physician office in a market, so the same estimation approach cannot be used. Second, we assume that insurers set uniform copayments. As a result, consumers are not directly affected by negotiated prices between physicians and insurers, although prices may have indirect effects on consumers through premiums or wait times. We abstract from specialties, but in the empirical estimates we consider each physician specialty to be a distinct market. The remaining model assumptions are similar to those made in models of hospital bargaining, such as Ho and Lee (2016) and Gowrisankaran et al. (2013).

The profit function of insurer i is:

$$\pi_i(\mathbf{p}, \mathcal{G}) = D_i(w_i, \phi)\phi_i - \sum_{r \in \mathcal{G}_i} D_{ir}(w_i, \phi) p_{ir}$$

where D_i represents the number of enrollees in insurance plan i, which depends on wait times $w_i(\phi_i, \mathcal{G})$ in network i, and D_{ij} is the number of enrollees in plan i who visit physician

[24] In reality many contracts are capitated, but for other contracts a capitated payment is conceptually similar to an average price for an expected bundle of services.

group j.[25] The profits of physician group j are similarly:

$$\pi_j(\mathbf{p}, \mathcal{G}) = \sum_{s \in \mathcal{G}_j} D_{sj}(w_i, \phi)(p_{sj} - c_j)$$

which equals the sum of enrollees D_{sj} over all insurers in the network of physician group j times the negotiated price p_{sj} minus c_j, the average per-patient cost for physician group j.

Prices are negotiated through simultaneous bilateral Nash bargains, where p_{ij} solves:

$$p_{ij} = \arg\max_{p_{ij}} \left[\pi_i(\mathbf{p}, \mathcal{G}) - \pi_i(\mathbf{p}_{-ij}, \mathcal{G} \backslash ij)\right]^{\tau_i} \times \left[\pi_j(\mathbf{p}, \mathcal{G}) - \pi_j(\mathbf{p}_{-ij}, \mathcal{G} \backslash ij)\right]^{\tau_j} \quad \forall\, ij \in \mathcal{G}$$

where $\pi_i(\mathbf{p}_{-ij}, \mathcal{G} \backslash ij)$ represents the disagreement profits of insurer i if they fail to reach an agreement over network inclusion with physician group j, and similarly $\pi_j(\mathbf{p}_{-ij}, \mathcal{G} \backslash ij)$ are the disagreement profits of physician group j. τ_i and τ_j are the bargaining power parameters of the insurer and physician group.

The first order condition of the bargaining problem simplifies to:

$$\begin{aligned} \underbrace{p^{\star}_{ij} D_{ij}}_{\text{Physician Group Revenue}} &= \tau_j \left[\underbrace{\phi_i \left(D_i - D_{i-j}\right)}_{\Delta\text{Insurer Revenue}} - \underbrace{\left(\sum_{h \in \mathcal{G}_i \backslash ij} p^{\star}_{ih} \left(D_{ih} - D_{ih-j}\right) \right)}_{\Delta\text{Insurer } i \text{ Payments to Other Physicians}} \right] \\ &+ \tau_i \left[\underbrace{c_j D_{ij}}_{\text{Average Cost}} - \underbrace{\left(\sum_{n \in \mathcal{G}_j \backslash ij} \left(p^{\star}_{nj} - c_j\right) \left(D_{nj} - D_{nj-i}\right) \right)}_{\Delta\text{Physician Group } j \text{ Profits from Other Insurers}} \right] + \varepsilon_{ij} \quad (5) \end{aligned}$$

where D_{i-j} is the number of enrollees in plan i if there is disagreement between i and j. The second term equals the additional payments that the insurer will have to make to other physician groups if group j is not included in the network, which is negative. $D_{ih} - D_{ih-j}$ is the effect of disagreement between insurer i and group j on the number of consumers in plan i who visit another group h, where $h \neq j$. The third term is the average cost to group j of treating an enrollee. The fourth term is the effect of disagreement between plan i and group j on the profits of group j from other insurers, which is negative. And ε_{ij} represents *iid* cost shocks.

Conditional on getting sick, consumer k derives utility from visiting a physician j in network i, which we assume takes the form:

$$u_{kij} = \eta_k + \frac{1}{w_{ij}}$$

[25] More precisely ϕ_i can be thought of as the premium for plan i net of any per-capita non-medical costs of running the plan.

where in equilibrium wait times will be equal within any network, so that $w_{ij} = w_i$. The average wait time for an enrollee who gets sick in network i is:

$$w_i = \beta \frac{\sum_{r \in \mathcal{G}_{i \times j}} \gamma N_i}{\sum_{r \in \mathcal{G}_{i \times j}} |P_j|}$$

where N_i is the number of enrollees in insurance plan i, γ is the probability of getting sick, $|P_j|$ is the size of physician group j, and $\mathcal{G}_{i \times j}$ denotes the connected subset of $\mathcal{G}$ that contains all insurers and physician groups that have any nodes in common with the networks $\mathcal{G}_i$ or $\mathcal{G}_j$. For an insurer i with an exclusive network of physicians that do not participate in other networks, this subset is simply $\mathcal{G}_i$.

As in Capps, Dranove, and Satterthwaite (2003) we consider willingness to pay (WTP) as a measure of the surplus that consumer k would lose if a given physician group were to leave the network. A consumer's change in utility caused by physician group j exiting the network is:

$$\Delta \text{WTP}_{kij} = u_{kij} \mid_{j \in \mathcal{G}_i} - u_{kij} \mid_{j \notin \mathcal{G}_i}$$

Each consumer's ex ante WTP is then $\gamma \Delta u_{kij}$. We express the WTP by the insurer for participation of group j in the network, which affects the premium charged by insurer i, as a constant proportion ξ of the average consumer surplus:

$$\Delta \text{WTP}_{ij} = \frac{\sum_k \Delta \text{WTP}_{kij}}{N_i} \xi = \frac{|P_j|}{\beta \gamma \sum_{r \in \mathcal{G}_{i \times j}} N_i} \xi$$

As a result $\frac{\partial \text{WTP}_{ij}}{\partial |P_j|} > 0$ since premiums reflect consumers' WTP. Also $\frac{\partial p^{\star}_{ih} \left(D_{ih} - D_{ih-j} \right)}{\partial |P_j|} < 0$, so the second term of Equation 5 gets increasingly negative as practice size increases, since the number of consumers who visit other physician groups increases when a larger group exits the network. The fourth term is also increasing with group size. If a plan fails to agree with a larger group, equalization of wait times implies the group will attract more consumers from other plans. Therefore the sum of the first, second, and fourth terms in Equation 5 cause prices to increase with group size. However, the cost function potentially opposes this effect. Without making assumptions, it is plausible that there are economies of scale, and that average costs (the third term) are declining in group size. In this case the sign of the aggregate effect of group size on negotiated prices is ambiguous.

To construct an empirical analogue of the FOC, suppose in disagreement the potential consumers of group j are distributed proportionally among the other physicians in the network. Then:

$$\begin{aligned} p^{\star}_{ij} &= a + |P_j| \tau_j \xi + \sum_{h \in \mathcal{G}_i \setminus ij} \tau_j p^{\star}_{ih} \frac{D_{ih}}{D_{ij}} \left(1 + \frac{|P_h|}{|\mathcal{G}_i| - |P_j|} \right) + \tau_i c_j(|P_j|) \\ &+ \sum_{n \in \mathcal{G}_j \setminus ij} \tau_i \left(p^{\star}_{nj} - c_j \right) \frac{D_{nj}}{D_{ij}} \left(\frac{|P_j|}{|\mathcal{G}_i| - |P_j|} - \frac{|P_j|}{|\mathcal{G}_i|} \right) + \epsilon_{ij} \qquad (6) \end{aligned}$$

This gives the equilibrium negotiated price, plugging the WTP values from the utility function into Equation 5. The negotiated price depends on the bargaining power parameters, physician group sizes, and the number of physicians in insurer i's network, $|\mathcal{G}_i|$, conditional on agreement with group j. Given the theoretical ambiguous effect of $|P_j|$ on $p^{\star}_{ij}$, it is an empirical exercise to determine this relationship.

C.1 Empirical Implementation

In our empirical setting we cannot estimate Equation 6 directly because we do not observe the bargaining parameters or practice-level demand. Instead we consider the combined impact of physician practice sizes on negotiated prices through two aggregated components: the value of including practice j in the network of insurer i, and the cost function of practice j:

$$p^{\star}_{ij} \equiv a + \beta_1 \times \text{Network Value}_j(|P_j|) + \tau_i \times \text{Average Cost}_j(|P_j|) + \epsilon_{ij} \tag{7}$$

where Network $\text{Value}_j(|P_j|)$ is defined by the sum of the first, second, third, and fifth terms in Equation 6, and β_1 captures the average effect of practice size on prices through network value. Average $\text{Cost}_j(|P_j|)$ is the fourth term, which has coefficient τ_i according to Equation 6.

There are several further adjustments to the model that must be made given our empirical setting and data. First, since we do not observe costs, what we can actually identify is an aggregate coefficient that combines β_1 and τ_i. Second, Equation 7 represents a specific market, where markets may be defined by a combination of geography, physician specialty, and time. In our analyses we use data from many markets, while controlling for latent market-specific variation. Finally, we do not observe the negotiated price for each practice; we only know the average price across all practices in a market.

The empirical analogue of the structural model we consider is thus:

$$\overline{p^{\star}_{mpct}} = \alpha + \beta_2 ES_{mct} + \beta_3 FS_{mct} + \eta_m + \pi_p + \gamma_c + \nu_{d(c)t} + \varepsilon_{mpct} \tag{8}$$

where ES_{mct} measures establishment sizes in specialty market m, county c, and year t; FS_{mct} measures firm sizes; and β_2 and β_3 represent effects of changes in each of the practice size measures on average negotiated prices. This specification allows the derivative of costs with respect to firm size to differentially affect prices depending on whether firm growth occurs within or across establishments. The equation includes controls for latent heterogeneity across services through medical specialty effects, η_m, and procedure code effects, π_p; across space through geographic effects, γ_c, for which we consider a variety of potential market definitions; and over time through census-division-by-year effects, $\nu_{d(c)t}$, which nest year effects while allowing prices to change arbitrarily over time across census divisions.

Given the limitations of the empirical model relative to the structural analogue, it is worth questioning whether the parameters are nevertheless useful for understanding the extent to which larger practice sizes may lead to higher prices by increasing the network value of the practice. In general they may not be very informative, since both β_2 and β_3 identify combinations of the effects of changes in average costs and network value, without separately identifying either parameter of interest. However, the estimates turn out to be informative in our setting because we find an important sign difference: $\beta_2 < 0$ while $\beta_3 > 0$. This combination of results implies lower bounds on both the network value parameter β_1 and the cost function parameter τ_i.

To understand why this result is informative, consider a hypothetical merger between two nearby physician practices that remain physically distinct after the merger but minimize costs jointly and negotiate with insurers jointly. The network value of the combined firm cannot decline, because otherwise the firm would prefer to negotiate separately by establishment, an option still within the choice set. Similarly, average costs cannot increase, since minimizing costs separately by establishment is still within the choice set. After the merger, there is no change in ES since the establishments remain distinct, but FS increases. If the merger were to increase negotiated prices, $\beta_3 > 0$, this would imply that the true effect of the merger on network value is at least as large as β_3, since τ_i is non-positive in this case.

Conversely, suppose the same two nearby firms merge and physically consolidate into a single establishment. In this case the change in FS is the same as in the case above, but ES now also increases. In our theoretical model, the network value of the post-merger firm depends on the total number of doctors (not on physical consolidation) and is thus the same as in the case above. A finding of $\beta_3 > 0$, then, suggests the effect of the merger on prices due to network value will also be positive in this case. However, a cost-reducing physical consolidation could put downward pressure on negotiated prices. If this merger were to generate a decrease in prices the implication would be that the average cost effect of τ_i dominates any change in network value, implying that β_2 is a lower bound estimate of τ_i.

In our empirical analyses we estimate an aggregated version of this model using establishment sizes from the MPIER data and firm sizes calculated by linking multi-establishment practices together using IRS tax IDs. Our finding that $\widehat{\beta_2} < 0$ and $\widehat{\beta_3} > 0$ suggests insurers extract the efficiency gains from larger establishments in the form of lower prices, but multi-establishment consolidation yields efficiency gains that are smaller than the effects on network value, causing negotiated prices to increase. This model aims to facilitate the interpretation of these empirical parameters as lower bound estimates of τ_i and β_1, the parameters of interest.