Physician Concentration and Negotiated Prices: Evidence from State Law Changes*

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November 13, 2017

Abstract

We study the relationship between physician market concentration and prices negotiated between physician practices and private insurers. We develop new instrumental variables for changes in concentration using state-level judicial decisions that change the enforceability of non-compete clauses in physician employment contracts. These law changes alter the organizational incentives of physicians, causing shocks to the concentration of physician markets. 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 prices fall when physician establishments grow larger but rise when physician firms grow larger conditional on establishment concentration. Our results imply that a 100 point increase in the establishment-based Herfindahl Index (HHI) causes a 1.3% to 1.7% decline in prices, suggesting that insurers extract some efficiency gains from larger establishments. In contrast, the same change in concentration caused by physically distinct establishments negotiating jointly leads to price increases of 1.0% to 2.0%. The overall effect of a one standard deviation increase in state non-compete enforceability is a 9.6% increase in average physician prices.

JEL Codes: I11, I18, K31

*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, and seminar participants at 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

Physician services account for 20% of all U.S. medical spending, and this component grew even faster than overall medical spending since 1980.[1] Anecdotal evidence suggests that physician practices have consolidated substantially during the past decade. Rising healthcare spending and concern over high service prices have led numerous researchers to study the effects of market concentration on prices, both 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)). However, there is relatively limited causal evidence on the extent to which competition among physicians affects prices negotiated with insurers. Kleiner et al. (2015) and Dunn and Shapiro (2014) find evidence consistent with market power, but focus on two specialties and use primarily cross-sectional variation.[2]

Two empirical challenges have hindered research on physician prices relative to other segments of healthcare markets. First, longitudinal data on physician practice sizes linked to prices in private markets are very difficult to obtain, in contrast to more accessible hospital and insurer data. Second, there is a basic endogeneity challenge—that market structure may be correlated with unobserved variation in quality, costs, and demand, for example—which may be further compounded by data limitations. Empirical methods developed in hospital settings with few providers, such as Ho and Lee (2016) and Gowrisankaran et al. (2014), require data on costs for each provider in a market, but similar data are not available for the large number of physician practices in most markets.

In this paper we provide comprehensive evidence on the effects of physician market concentration on negotiated prices with private insurers, addressing both of these empirical challenges. We employ two complementary data sets containing the universe of all physician practices in the US between 1996-2007 to construct measures of physician concentration in a variety of ways. 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 calculate establishment-based and medical specialty-specific concentration measures. 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 based on IRS tax IDs and to calculate concentration measures using payroll and sales data in addition to employment data. We link these concentration measures to Truven 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 picture of virtually every physician market nationwide from 1996-2007.

To overcome problems associated with endogenous market structure, we construct new instrumental variables using judicial decisions that cause changes to state laws governing the enforcea-

[1] National Health Expenditure Fact Sheet 2013, CMS

[2] 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.

bility 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 quantifiable dimensions across states and over time. We construct a panel of law changes for each of these legal dimensions for every state between 1991-2009 and trace the effects of these law changes through changes in organizational incentives, organizational structure, physician market concentration, and finally to average prices.[3]

We provide a variety of evidence on the mechanisms through which NCA law changes affect negotiated prices. We show that the law changes have significant effects on the rate of physician-establishment job separations. These separations affect the rates of new establishment births and incumbent establishment deaths, leading to changes in the distribution of establishment sizes. Our controlled event-study estimates suggest that an average law change increasing NCA enforceability causes a 165 point decline in the HHI within 2 years. Differences in the nonparametric density of annual HHI changes in the years following law changes suggest that most of our identifying variation comes from reductions in concentration in the range of about 100 to 400 HHI points.

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. Our fixed effects specifications control for unobserved heterogeneity across geographic markets as well as census-division-by-year effects, medical specialty effects, service procedure code effects, and medical facility type effects. Most importantly, the unique ability in our data to observe both establishments and firms allows us to estimate the marginal effect on prices of increasing establishment concentration conditional on firm concentration, and vice versa.

The estimates suggest that changes in HHI have heterogeneous effects on negotiated prices that depend on the structural nature of the concentration changes. Increases in HHI caused by the growth of physician establishments lead to negative price effects, while increases in HHI 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 1.3% to 1.7% on average. In contrast, the same increase in concentration caused by firm-level consolidation holding fixed establishment concentration causes prices to increase by 1.0% to 2.0%.[4] OLS specifications imply very small (but statistically significant) positive price effects of 0.02% or less, suggesting that our instruments may reduce substantial endogeneity bias. This fixed effects OLS estimate is consistent with results from Baker et al. (2014), who find that a 100 point increase in HHI is associated with 0.08% higher prices on average.[5]

Taken together, these results suggest that the effects of consolidation on prices depend on a

[3]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. (2016) 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.

[4]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.

[5]Baker et al. (2014) use Marketscan price data but estimate market structure using Medicare beneficiaries.

tradeoff between the efficiency gains of larger establishments and the increased negotiating power associated with bargaining as a larger organization. To the extent that larger establishments have greater bargaining leverage, 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 bargaining leverage. The estimate provides a lower bound of the effect of physician firm size on the ability to negotiate higher prices. Although the changes in consolidation from NCA laws underlying the local average treatment effect we estimate may differ to some extent from the margin of variation occurring more broadly in physician markets, our estimates suggest that price effects come predominantly from the channel of establishment level growth, generating a net negative relationship between concentration and prices on average.

Our approach to studying this question follows the general structure-conduct-performance (SCP) approach to estimating effects of market structure on prices (Gaynor et al., (2015)), which has several well-known concerns. The first is that estimates can be sensitive to assumptions about market definition, which we address by showing that results are consistent across a range of potential market definitions. A second, but perhaps more fundamental, concern is that without a structural model to estimate 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 very specific implicit assumptions about conduct: homogeneous goods and Cournot competition. These assumptions may not be reasonable in many markets.

Previous studies on the effects of provider consolidation in medical care markets have generally taken a structural approach to modeling bargaining between hospitals and insurers.[6] This approach allows researchers to identify fundamental parameters like Nash bargaining weights and consumer willingness to pay, and to evaluate counterfactual scenarios like hypothetical mergers or entry. However, in the physician setting the same general methodology cannot be applied due to differences in available data on hospitals versus physicians. In addition, this approach requires claims data to estimate demand and willingness to pay in a manner that allows for unobserved quality heterogeneity, but these data are unavailable to us at a national level covering our 12-year study period.

Our approach is instead to show that the patterns in our results are robust to a wide variety of market definitions and at least five different measures of market structure, each of which has different assumptions about firm conduct. The similarity of estimates across these models suggests that, in our setting, assumptions about firm conduct and market definition are less important than the endogeneity of market structure measures. Although the parameters we are able to identify

[6]See Capps et al. (2003), Ho and Lee (2016), and Gowrisankaran et al. (2014).

are combinations of the more primitive structural parameters, they still provide meaningful and intuitive answers to important policy questions.

To facilitate the interpretation of our estimand relative to the underlying theoretical parameters, we derive a linkage between our empirical model and one particular structural model that assumes Nash-Bertrand conduct, adapted from the Ho and Lee (2016) bargaining model. The connection to this model provides a framework for understanding why price effects might be positive at the firm-level but negative at the establishment level, and why our empirical parameters can be interpreted as lower bound estimates of the rate at which average costs fall with practice sizes and of the effect of practice size on network value.

The use of new instruments as a source of exogenous variation in market structure requires careful attention to the exclusion restriction. A potential concern with our IVs could arise if practices using NCAs have different cost functions, which could directly alter negotiated prices. In addition, there could be selection on physician quality into practices that choose to impose NCAs. We present several pieces of evidence against these concerns based on survey data from Lavetti et al. (2016), which links information on whether physicians have signed NCA contracts to their negotiated service prices and a variety of quality measures. We show that there is no statistically or economically significant difference in the prices negotiated between insurers and physician practices that use NCAs relative to practices of the same size in the same geographic market that do not (the decision to impose NCAs is made at the firm level, not the physician level).[7] Second, there is no evidence of quality differences associated with the use of NCAs. In addition to a lack of difference in negotiated prices, which suggests no difference in average quality, physicians with NCAs respond identically to vignette-based questions designed by clinical experts to elicit knowledge of best-practices, diagnostic skill, treatment patterns, and clinical recommendations. There is also no difference in the amount of prior experience that physicians have when entering NCA vs. non-NCA practices, which is informative since physician experience tends to be strongly correlated with patient satisfaction and perceived quality (Choudhry et al. (2005)) Moreover, our seven law-based instruments affect practice organization incentives in distinct ways, such that potential violations of the exclusion restriction should be unique to each instrument. Yet all seven instruments yield similar negative coefficients on establishment concentration when used one at a time.

Our estimates of the effect of physician market structure on prices are highly relevant for policy. At 16.9% of GDP, the share of income devoted to healthcare in the US is about 82% higher than the OECD average.[8] 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 driven researchers to try to understand why prices are so much higher in the US. Though provider consolidation is a commonly considered explanation, available evidence on the effect of physician market structure on prices is either limited in scope (small number of

[7] For example, within an MSA the standard deviation in negotiated prices for a basic office visit (CPT 99213) is 39% of the mean price, while the average difference in negotiated prices between practices that use NCAs and those that do not is only 2% of the mean (both unconditionally and conditional on specialty and practice size) and not statistically significant.

[8] See OECD Health Statistics 2014

specialties or geographic markets), or does not address the potential endogeneity of variation in market structure. Our results also highlight the importance of NCA laws in affecting healthcare markets. Our findings suggest that if NCA enforceability decreased nationally by 10% of the observed policy spectrum (about 0.39 standard deviations), physician prices would fall by 3.7%, reducing aggregate spending by over $20 billion annually. Despite the important role of NCAs, 39 states have never comprehensively reviewed and legislated NCA policies and instead rely on case-specific common law traditions.

The paper is structured as follows. Section 2 provides background on non-compete laws and their usage by physicians. Section 3 includes a stylized bargaining model of physician firms negotiating prices with insurers and motivates the empirical research design. Section 4 describes the multiple data sources we use, and Section 5 elaborates on the instrumental variables we develop, including evidence on the mechanisms and instrument validity. Section 6 describes our main empirical model. Section 7 describes our main results, reduced-form estimates, and several robustness tests. Section 8 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.[9] 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 only narrow market definitions or brief durations.

The permissibility of NCAs dates back at least 1621 under English common law, and 39 US states still follow common law in determining the enforceability of NCAs, making historical precedent the main determinant of enforceability 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 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

[9] 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.

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.

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 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 our instruments, it is useful to know what motivates physician practices to use NCAs. Lavetti, Simon, and White (2016) 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. NCAs can prevent this type of loss.

Our empirical analyses suggest that most of the components of NCA laws are negatively correlated with physician 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 HHIs as it becomes less costly for firms to fire workers, who 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 HHIs in our just-identified IV estimates, which 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 exogenous variation in physician concentration measures. In Sections 5 and 7.4 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.

Physicians do, in fact, frequently and systematically use NCAs, and they do so at higher rates where NCAs are more strictly enforced. Lavetti et al. (2016) find that about 45% of primary care physicians in group practices 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 office-based practices as opposed to hospitals, and in metro or micropolitan markets where the supply of physicians is larger relative to the population, making patient stocks more valuable.

3 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.[10] 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

[10] 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.

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 group j.[11] 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:

$$\begin{aligned}
\underbrace{p^{\star}_{ij} D_{ij}}_{\text{Physician Group Revenue}} &= \tau_j \left[\underbrace{\phi_i \left(D_i - D_{i-j}\right)}_{\Delta\text{Insurer Revenue}} - \underbrace{\left(\sum_{h \in \mathcal{G}_i \backslash ij} p^{\star}_{ih} \left(D_{ih} - D_{ih-j}\right) \right)}_{\Delta\text{Insurer } i \text{ Payments to Other Physicians}} \right] \\
&+ \tau_i \left[\underbrace{c_j D_{ij}}_{\text{Average Cost}} - \underbrace{\left(\sum_{n \in \mathcal{G}_j \backslash ij} \left(p^{\star}_{nj} - c_j\right) \left(D_{nj} - D_{nj-i}\right) \right)}_{\Delta\text{Physician Group } j \text{ Profits from Other Insurers}} \right] + \varepsilon_{ij} \qquad (1)
\end{aligned}$$

[11] 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 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:

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

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 1 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 1 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:

$$\begin{aligned} p^{\star}_{ij} &= a + |P_j|\,\tau_j \xi + \sum_{h \in \mathcal{G}_i \setminus ij} \tau_j p^{\star}_{ih} \frac{D_{ih}}{D_{ij}} \left(1 + \frac{|P_h|}{|\mathcal{G}_i| - |P_j|}\right) + \tau_i c_j(|P_j|) \\ &+ \sum_{n \in \mathcal{G}_j \setminus ij} \tau_i \left(p^{\star}_{nj} - c_j\right) \frac{D_{nj}}{D_{ij}} \left(\frac{|P_j|}{|\mathcal{G}_i| - |P_j|} - \frac{|P_j|}{|\mathcal{G}_i|}\right) + \epsilon_{ij} \end{aligned} \tag{2}$$

This gives the equilibrium negotiated price, plugging the WTP values from the utility function into Equation 1. 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.

3.1 Empirical Implementation

In our empirical setting we cannot estimate Equation 2 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:

$$p^{\star}_{ij} \equiv a + \beta_1 \times \text{Network Value}_j(|P_j|) + \tau_i \times \text{Average Cost}_j(|P_j|) + \epsilon_{ij} \tag{3}$$

where Network $\text{Value}_j(|P_j|)$ is defined by the sum of the first, second, third, and fifth terms in Equation 2, 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 2.

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 3 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:

$$\overline{p^{\star}_{mpct}} = \alpha + \beta_2 ES_{mct} + \beta_3 FS_{mct} + \eta_m + \pi_p + \gamma_c + \nu_{d(c)t} + \varepsilon_{mpct} \tag{4}$$

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.

3.2 Firm Conduct and Measuring Market Structure

In addition to estimating Equation 4 using practice sizes, we also estimate analogues of the model with a variety of alternative concentration measures, such as HHI, the negative log HHI transformation used by Cooper et al. (2012), and the 4-firm concentration ratio. These models fit more directly into the literature relying on structure-conduct-performance (SCP) models. Although SCP models are common in the health economics literature and can be useful for establishing overall patterns in the relationships between prices and market structure, they are generally regarded as having several well-known problems (See Gaynor et al. (2015)). First, these models impose strong implicit assumptions about firm conduct that may not hold in all empirical settings. Second, market structure in SCP models is usually correlated with a variety of unobserved factors, creating multiple forms of potential endogeneity that may be difficult to overcome. We discuss each of these limitations in turn.

Without estimating a structural model of firm conduct simultaneously with performance, the choice of market structure measures in SCP models imposes potentially strong implicit assumptions about the nature of firm conduct. The theoretical model described above demonstrates the conceptual relationship between practice sizes and negotiated prices under the assumption of Nash-Bertrand bargaining. However, when HHI is used in the pricing model, the estimated coefficient is equivalent to the structural elasticity of demand only under the assumptions of 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 firm-level prices or claims data, we do not attempt to estimate firm conduct directly. Instead we take the approach that, using a variety of market structure measures (5 different 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, which we discuss in Section 3.3.

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, among physicians there is frequently no clear analogue 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.[12] In Equation 4 we condition on physician specialty, on specific medical procedures, and on geography, making the services even 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 have nothing concrete to add to the dearth of evidence on this question.

There is some empirical evidence that the assumption of Cournot competition is reasonable in the case of physician practices. 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.

To be clear, despite this defense of the use of HHI as a potentially reasonable measure of market structure, our overall empirical strategy is to demonstrate that the qualitative patterns of estimates are sensitive neither to measures of market structure nor to their underlying assumptions about conduct.

3.3 Endogeneity of Practice Sizes

A second class of concerns described by Gaynor et al. (2015) about SCP models 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 according to Equation 1, ε_{ij} 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 $\widehat{\beta}_2$.

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

[12] 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.

differences, and this error component is positively correlated with market share, possibly causing an upward bias in $\widehat{\beta}_2$.

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 leverage. Our empirical findings suggest that OLS estimates of β_2 and β_3 are attenuated towards zero. Our results generally support the conclusion that endogeneity of market structure in Equation 4 causes substantial bias. A primary goal of our study is to develop new instrumental variables to overcome these biases in a variety of markets, even outside of healthcare, as NCA laws affect firms in many industries.

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, covers 1996-2007.

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. Under Section 1833(q) of the Social Security Act, all physicians must have a unique identifying number to either order services on behalf of a Medicare patient, or to refer a Medicare patient to another physician for services. Since this requirement covers nearly every physician in the US, by 1992 virtually every physician was included in the MPIER directory, and the requirement was strengthened in 1996 under HIPPA, which mandated every physician to receive an identifying number regardless of their association with Medicare. The coding system used in MPIER was in place through 2007.

Between 1992 and 2007 the MPIER provides the street address of physicians' practice affiliations. Physicians can have multiple practice affiliations at the same time, and each location at which a physician treats patients is recorded. The data include the 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. Using the `soundex` fuzzy matching algorithm[13] we construct a longitudinal database of the approximate universe of physician establishments by matching physicians to establishment locations, allowing the locations to have slight differences that may be due to typographical errors in street addresses, but requiring establishments to have the exact same street number and office number.

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

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

as a single entity with insurers. Second, we cannot observe revenues or allocations of time for physicians that work in multiple establishments. To calculate HHIs and other market 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 limitations, this dataset is, to the best of our knowledge, the first longitudinal complete census of all physicians in the US that has been used to study the relationship between practice sizes and negotiated prices.

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 locations with firm linkages via IRS Employer Identification Numbers. Physician practices are identified by NAICS industry code 621111, described as 'Offices of Physicians (Except Mental Health Specialists)' although we do not know exactly how many of the workers at the firm are physicians, and we do not observe the medical specialties of the firms. While the LBD solves the problem of observing firm-level information, it has limitations; for physician markets, being able to calculate concentration measures by medical specialty may be quite important.

We also use the LBD to construct longitudinal measures of health insurance market concentration using data on sales from firms in NAICS code 524114, 'Direct Health and Medical Insurance Carriers'. We control for insurer HHIs in our main specifications.

4.3 MarketScan Negotiated Prices Data

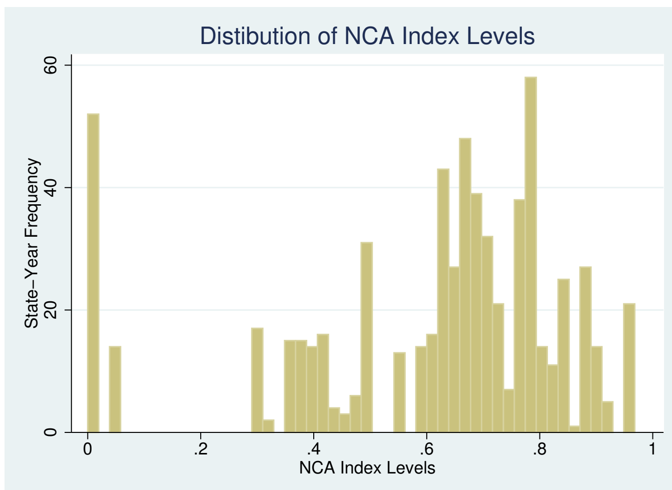
Data on prices negotiated between physicians and private commercial insurers come from the Truven Health Analytics Marketscan database. The database includes the medical claims for all active employees and their dependents from a sample of large firms. We use data between 1996-2007 on average negotiated 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).

The data in our sample contain about 10 million average negotiated prices, based on prices from about 550 million procedure claims. The sample contains only prices for ambulatory services that are not hospital-based; none of our analyses include hospital prices. The prices cover every state-year and nearly every county-year in the US between 1996-2007. The negotiated prices are between about 100 private insurance companies and all of the physicians that any enrollee in the sample visited. The full MarketScan database includes a sample of over 138 million unique enrollees since 1995, and our data include information from all of these enrollees that visited a physician in one of the above medical facility types.

4.4 NCA Law Data

We develop new instrumental variables by 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 (1991-2011). 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) quantified laws in 1991 and 2009. Using the same coding 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).[14] 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)).

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, where 1 corresponds to the state-year policy in which NCAs are easiest

[14] 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.

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.

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.

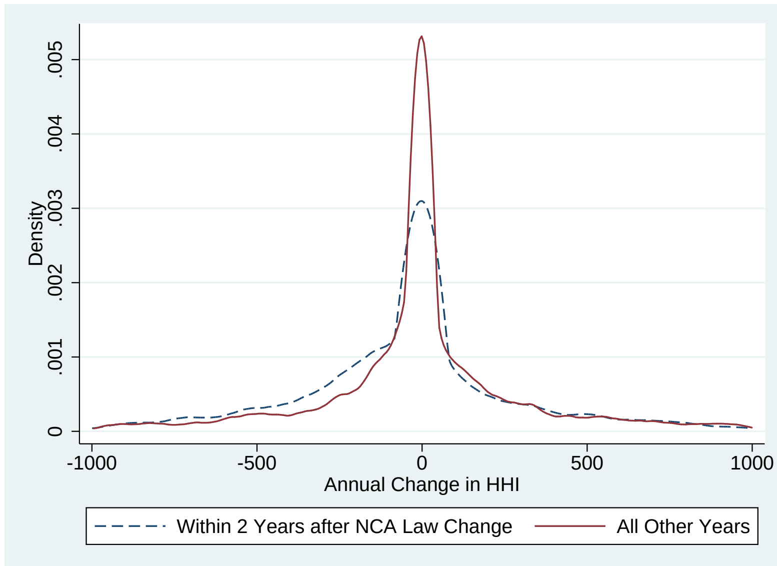
5 IV Description, Mechanism and Validity

In this section we discuss the validity of the instrumental variables, including evidence on the mechanisms through which the instruments affect market structure, tracing the pathway of effects from job separation rates, through changes in establishment birth rates, death rates, and physician practice sizes, and ultimately to HHI.

5.1 Event Studies: IV Effects on Concentration and Prices

The first piece of evidence on the effects of the instruments is shown in Figure 2, which depicts the unconditional kernel density functions of annual changes in establishment HHIs within markets. Each observation underlying these distributions is a market-year-specialty combination. The solid line shows the distribution of changes in HHIs from one year to the next when there have been no recent changes to NCA laws. This distribution is centered around zero and has a relatively small variance. The dashed line shows the same distribution in the two years following any change to NCA laws. In years just after a law change, the density function is visibly and statistically significantly altered (Kolmogorov-Smirnov p-value<0.001), with less mass near zero and more mass in the region of negative HHI changes.

Figure 2: Distribution of Annual HHI Changes

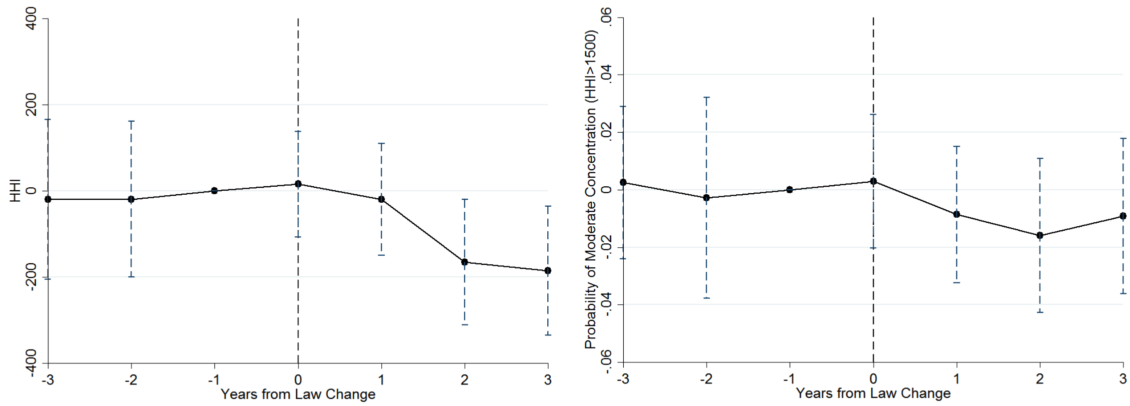


Notes: Distributions are kernel density graphs of the change in annual HHI by CBSA-specialty for specialists. Distributions are truncated at $+/-$ 1000 for display. The p-value of Kolmogorov-Smirnov test of the equality of the full distributions is <0.001.

While the effects of law changes are clearly apparent in the unconditional comparison of HHI changes, our formal analyses of course control for geographic, intertemporal, specialty, and procedure variation. Figure 3 shows controlled event study plots that are more closely comparable to our formal analyses. Each plot is constructed by regressing county-by-specialty establishment-based HHIs on a set of 7 dummy variables indicating each year within 3 years around a law change. Since the law changes occur at different times, the plots include only treatment states that had exactly 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. These restrictions are necessary for cleanly graphing the variation used in our regressions in an event study format, but they limit the treatment set to only 7 events, which reduces the precision of estimates. Figure 3a depicts coefficients from a regression of HHIs on event year dummies, county effects, census division by year effects, and specialty effects, comparable to the specification of our formal regressions. Events that decrease enforceability are scaled by -1, such that the graph can be interpreted as corresponding to an increase in enforceability. The figure suggests that an average law change that increases enforceability tends to decrease HHIs by about 165 points within 2 years after the law change, with very little evidence of a differential pre-trend in treatment states.

Estimates in Figure 3b are similar, but the dependent variable is a binary indicator that equals 1 if HHI is above 1500, the Department of Justice threshold for a 'moderately' concentrated market (DOJ Horizontal Merger Guidelines, August 2010). Consistent with 3a, the figure suggests increasing NCA enforceability leads within two years to a decrease of about 1.6% in the probability that the HHI exceeds the threshold for a moderately concentrated market. To be clear, our measure

Figure 3: Event Study Plots: Concentration Before and After Law Changes



(a) Change in HHI

(b) Probability of 'Moderate' or Greater Concentration (HHI>1500)

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. Year 0 is the calendar year during which the law change occurred, and the dependent variable is normalized to zero in year -1.

of establishment-level employment concentration is not directly comparable to the measure upon which the DOJ threshold is based, which is why we largely avoid making comparisons about HHI levels or using discrete thresholds in our analyses; this figure is only intended to be suggestive that changes in concentration occur both overall on average and at low to moderate concentration levels. The conclusion from the event studies is that NCA laws, taken together, appear to be negatively correlated with market concentration; we return to this point in discussing corroborating evidence from the first-stage regressions.

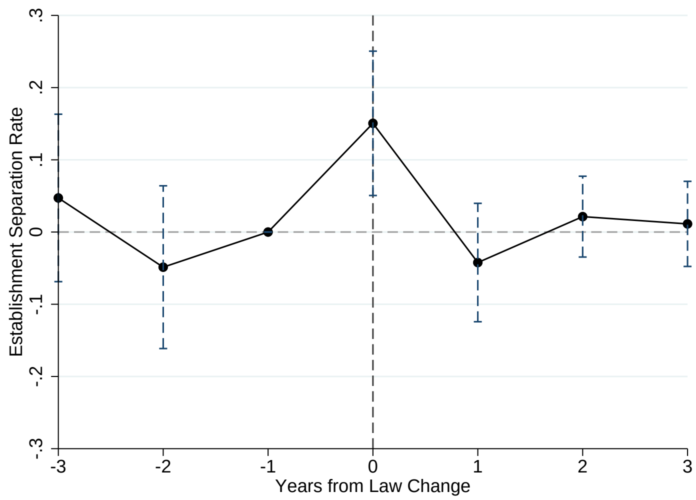
5.2 Mechanism

To understand the mechanisms that lead NCA law changes to affect market concentration, we estimate the effect of changes in NCA enforceability on physician-practice separation rates, establishment sizes, and the rates of new establishment births and deaths.

Figure 4 shows an event study plot with the same design and controls as in Figure 3, except the dependent variable is the average physician-practice separation rate before and after a law change. The event year dummies are scaled so that the graph can be interpreted as an increase in the separation rate when NCA enforceability declines. The coefficient estimates suggest that an average law change that decreases NCA enforceability is associated with a 15 percentage point jump in the rate of job separations in the year of the law change. There is again no clear anticipatory trend prior to the law change.

Figure 4 also shows that the average separation rate returns to the pre-event range. One might wonder why the spike in separation rates is temporary rather than persistent. This pattern is con-

Figure 4: 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. Year 0 is the calendar year during which the law change occurred, and the dependent variable is normalized to zero in year -1.

sistent with the presence of an accumulated stock of physicians who would like to switch practices but are prevented from doing so by an NCA. When the enforceability of the NCA restriction declines, it becomes easier or less costly to move, and a large stock of physicians moves simultaneously. Once the moves are completed there is less pent-up desire to switch practices, and separation rates subsequently decline.

The pattern in Figure 4 bolsters the evidence that NCA laws constrain physicians' choices over practices, suggesting that there are organizational effects that could lead to changes in market concentration. Still, it is not obvious that even an exogenous event causing separations should change establishment sizes or HHIs. Separating physicians could start new small practices, reducing the average practice size, or join larger established practices, increasing establishment sizes. Alternatively, if separations are driven by idiosyncratic preferences, a spike in separations could simply lead to some physicians exiting a practice and other physicians entering it, with no net effect on concentration. The large spike in separations corresponding to the timing of law changes is only suggestive of an underlying mechanism that has the potential to cause the distribution of practice sizes to change.

Table 2 presents fixed effects estimates of the impact of each legal index on the rate of physician establishment births and deaths following changes in NCA laws. In each model the dependent variable is either the number of establishments births or the number of establishment deaths, and the independent variables are one-year lags of each legal index, county-specialty effects, and census-

Table 2: Fixed Effects Models of Establishment Births and Deaths

Dependent Variable:	Births		Deaths	
	By Component	Combined	By Component	Combined
	(1)	(2)	(3)	(4)
Statutory Index$_{t-1}$	–1.281*	–0.612*	–1.348*	–0.734*
	(0.091)	(0.092)	(0.121)	(0.124)
Protectible Interest Index$_{t-1}$	0.583*	1.259*	0.609*	1.203*
	(0.066)	(0.158)	(0.089)	(0.177)
Burden of Proof Index$_{t-1}$	–0.633*	–3.684*	–0.532*	–3.659*
	(0.117)	(0.270)	(0.136)	(0.329)
Consideration Index Inception$_{t-1}$	0.024	3.389*	–0.354*	2.039*
	(0.088)	(0.299)	(0.090)	(0.265)
Consideration Index Post-Inception$_{t-1}$	–0.293*	–0.847*	0.081*	–0.458*
	(0.050)	(0.093)	(0.038)	(0.074)
Blue Pencil Index$_{t-1}$	0.235*	0.288*	–0.197*	–0.307*
	(0.041)	(0.060)	(0.048)	(0.065)
Employer Termination Index$_{t-1}$	–4.015*	–4.673*	–4.428*	–4.533*
	(0.513)	(0.630)	(0.682)	(0.780)
N		599,975		599,975
R-Sq		0.44		0.34

Notes: Columns 1 and 3 report estimates from separate regressions on each law component, and columns 2 and 4 report estimates from regressions including all 7 components. Dependent variables are the number of establishment births (columns 1 and 2) and deaths (columns 3 and 4) from the MPIER data. All specifications control for the aggregate supply of physicians and include 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.

division-year effects. Column 1 presents estimates from 7 separate regressions, each including one legal index at a time. The coefficients suggest that six of the seven instruments have statistically significant effects on the number of new practices born, ranging from a reduction in the birth rate of 4.0 practices per county-specialty-year per one-unit change in the Employer Termination Index, to a 0.6 practice increase per unit increase in the Protectible Interest Index. Since a one unit change in the legal indices is equivalent to switching between the two most extreme observed legal policies, another way of expressing the effects is to scale by the standard deviation of each index, which is given in Appendix Table A3. For example, a one standard deviation increase in the Employer Termination Index is associated with 1.2 fewer practices born in a county. Four of the indices are strongly negatively associated with practice births, suggesting that as increases in enforceability decrease HHIs, this effect is not primarily driven by the creation of new smaller practices. Column 2 shows estimates from a model that includes all 7 indices at once. The signs of all 7 coefficients remain the same as in column 1, and all 7 coefficients are statistically significant.

Columns 3 and 4 show similar estimates from regressions with practice deaths as the dependent variable. Again, all seven law indices have significant effects on practice deaths, and the similar coefficient signs across columns suggests that practice births may tend on average to accompany practice deaths.

Finally, Table 3 shows that these changes in separation rates and establishment births and

Table 3: Fixed Effects Models of Establishment Sizes

Dependent Variable: Log FTE Physicians per Establishment		
	By Component (1)	Combined (2)
Statistory Index$_{t-1}$	–0.169*	–0.140*
	(0.038)	(0.048)
Protectible Interest Index$_{t-1}$	–0.026	–0.178*
	(0.044)	(0.070)
Burden of Proof Index$_{t-1}$	–0.048	–0.262
	(0.042)	(0.146)
Consideration Index Inception$_{t-1}$	–0.121*	0.081
	(0.035)	(0.162)
Consideration Index Post-Inception$_{t-1}$	0.044	0.099*
	(0.031)	(0.032)
Blue Pencil Index$_{t-1}$	–0.151*	–0.163*
	(0.027)	(0.030)
Employer Termination Index$_{t-1}$	–0.159	–0.103
	(0.110)	(0.129)
N	379,370	379,370
R-Sq		0.23

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-year. * indicates significance at the 0.05 level.

deaths also lead to changes in the average sizes of establishments. The dependent variable 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 at a given point in time. 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 different specialties, we do not condition on specialty in these specifications. Column 1 shows that 6 of the 7 indices are negatively correlated with establishment sizes when included separately, consistent with the patterns from the event studies in Figure 3, and three of these 6 indices have significant coefficients. The significant coefficients range 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, which differs somewhat because a single judicial decision can cause correlated changes in multiple indices at the same time. Nonetheless, the evidence is generally consistent with the negative relationship between NCA enforceability and practice sizes.

This combined evidence connects the effects of the instruments from the individual employment level and physician-practice separation rates, to practice-level effects on establishment sizes, births, and deaths, documenting the underlying steps that lead to changes in HHIs. Consistent with the patterns from the event studies, changes in job separation rates lead to negative correlations with new practice creation, average establishment sizes, and HHIs.

5.3 IV Assumptions

At the physician level, a change in law that alters NCA enforceability can have two effects on practices. First, changing the ease with which an NCA can be enforced can alter the fraction of physicians with NCAs in their contracts, changing the probability of treatment. And second, allowing stricter NCAs to be enforced can impact the effect of treatment on the the subset of physicians that have signed NCAs. In that sense the treatment that we use, changes in NCA laws, measures a combined impact of the law change on selection into using an NCA and the effect of the law change on those that use NCAs.

Causal inference of a local average treatment effect (LATE) in IV models requires the existence of instruments with sufficient power in predicting the endogenous regressor. In addition to the discussion of mechanisms above, we show in Section 7.1 that our instruments exceed typical power thresholds.

The exclusion restriction necessary for the validity of the IVs holds as long as NCA law changes affect physician service prices only through physician market concentration. In other words, changes in NCA laws must not be correlated with the error term in the second stage equation. In our structural equation, described below, negotiated prices depend on market concentration and fixed specialty effects, county effects, medical facility type effects, procedure effects, and census-division-by-year effects. By conditioning on this set of covariates, law changes can only be potentially 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 exclusion restrictions are not formally testable, Lavetti et al. (2016) provide direct evidence that is useful for evaluating the plausibility of this condition. 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 at the practice level, they find that the use of NCAs has precisely no effect on negotiated prices conditional on fixed market effects and practice size. They find that, within a given geographic market, the standard deviation in negotiated prices across practices for a given procedure is about 39% of the mean price, but the average price difference associated with NCA use is only 2% of the mean negotiated price 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 it occurs either across markets or through practice size and concentration measures, which is consistent with the requirements of the exclusion restriction.

A second related concern with the exclusion restriction is that there could be a correlation between physician quality and the use of NCAs. For example, it is conceptually possible that there is selection on physician quality into practices that require NCAs. The survey data used in Lavetti et al. (2016) are again useful for demonstrating that there is no evidence of quality differences associated with the use of NCAs. This conclusion comes from three sources of information. First, to the extent that physician quality is correlated with prices, a quality difference would be reflected in a price difference between NCA users and non NCA users in the same market, but such a price difference does not exist. Second, there is no difference in the amount of prior experience physicians have when entering practices that use NCAs versus those that don't. Physician experience is strongly correlated with measures of patient satisfaction and perceived quality (Choudhry et al., 2005.) Finally, the survey data contain rich information about quality from a section of vignette-based questions that were designed by clinical experts to directly elicit knowledge about clinical best practices, guidelines, diagnostic skill, and appropriate treatment recommendations. The study finds no differences associated with the use of NCAs in either the distributions of responses to questions or in aggregate measures of compliance with guidelines.

A closer examination of the data also supports the exogeneity of judicial changes on NCA laws, which is required to satisfy the assumption of random assignment. In addition to showing an absence of pre-trends in the event studies, which supports the notion that judicial decisions were not made in response to trends in physician concentration or prices, it is also informative to analyze the law changes directly. Since judicial decisions are accompanied by opinions written by judges that describe the rationales that led them to their decisions, we can identify the judicial decisions used in our data that were related to physicians and verify that our findings are not sensitive to excluding these events. We further examine law changes to verify that they were not systematically related to other state-level political and economic factors that could also affect prices. This evidence is discussed in Section 7.4.

The final IV assumption is monotonicity. Note that the monotonicity condition in the case of our instruments is not that all seven instruments should move the HHI in the same direction. Rather, the condition requires that each law dimension moves HHIs in all states in the same direction. In split-sample analyses of high and low enforcement states, or states with positive or negative law changes, we find suggestive evidence that is generally consistent with this assumption. 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 the average is informative.

6 Empirical Model

We use two-stage least squares to estimate the effects of changes in state NCA laws on physician market concentration. Since physician practice sizes could be influenced by many factors, including insurer market concentration, consumer demand, and the dynamics of medical markets, we estimate fixed effects specifications that control for as much of this unobserved heterogeneity as possible. To differentiate the effects of increases in HHI driven by larger firms as opposed to larger establishments, our main estimates from the Census LBD include both endogenous regressors in the same specification. This gives two first-stage equations:

$$\begin{aligned} EC_{mc(t-1)} &= \alpha_1 + \beta_1 NCA'_{c(t-1,t-2)} + \beta_2 Ins\ HHI_{c(t-1)} + \eta_m + \pi_f + \theta_p + \gamma_c + \nu_{dt} + \epsilon_{mc(t-1)} \\ FC_{c(t-1)} &= \alpha_2 + \beta_3 NCA'_{c(t-1,t-2)} + \beta_4 Ins\ HHI_{c(t-1)} + \eta_m + \pi_f + \theta_p + \gamma_c + \nu_{dt} + \epsilon_{mc(t-1)} \end{aligned}$$

and the second-stage equation is:

$$\begin{aligned} P_{mfpct} &= \alpha_3 + \beta_5 \widehat{EC}_{mc(t-1)} + \beta_6 \widehat{FC}_{c(t-1)} + \beta_7 Ins\ HHI_{c(t-1)} + \eta_m + \pi_f \\ &+ \theta_p + \gamma_c + \nu_{dt} + \varepsilon_{mfpct} \end{aligned} \tag{5}$$

where m denotes medical specialty, c county, f facility type, p procedure code, d census division, and t year. NCA'_{ct} is a vector of the seven law instruments, measured at the state-year level. EC_{mct} is the establishment-based measure of market concentration, in contrast to FC_{ct}, the firm-based concentration measure. $Ins\ HHI_{c(t-1)}$ is the HHI of health insurance firms in the state, calculated using the Census LBD. η_m, π_f, θ_p, γ_c, and ν_{dt} are fixed effects for specialties, facility types, procedure codes, county, and census division-by-year effects, respectively. Our main specifications use HHIs as concentration measures, though we also show that our findings are robust to a range of alternative concentration measures, including average practice size, the negative log HHI transformation, and the four and eight-firm concentration ratios.

By including census-division by year effects, the estimates identify the extent to which concentration and prices move differentially in markets within a state that experiences a change in NCA laws relative to markets in the other, on average, 4.6 neighboring states in the same census division. This allows each census division to have any arbitrary unobserved idiosyncratic variation over time in both concentration and prices, which we use in lieu of imposing functional form restrictions on time trends.

Since negotiations between physicians and insurers tend to occur annually (or less frequently,) we use a lagged specification that allows average transaction prices observed in year t to be affected by concentration in year $t-1$. This lagged specification is also used in Dafny et al. (2012), Dunn and Shapiro (2014), and Baker et al. (2014). Moreover, since there may be a lag in physicians responding to NCA law changes, the first stage assumes that concentration effects occur either in the contemporaneous or lagged year. Since the dependent variable in the first stage is already lagged, this implies the instruments include first and second lags of the legal indices.

Although our main results are based on the above specification, we also estimate a variety of robustness models using only MPIER data, since Census Bureau confidentiality restrictions make it prohibitive to disclose results from a large number of alternative specifications. In these specifications there is a single first-stage equation corresponding to EC_{mct}, and the models exclude $FC_{c(t-1)}$ and $Ins\ HHI_{c(t-1)}$. Using the MPIER we assess sensitivity to market definitions, different alternative assumptions about the treatment of multi-specialty practices in calculating HHIs, with alternative measures of market concentration and firm sizes, dropping outlier law changes, using only the subset of instruments with negative first stage coefficients, and controlling for insurance market HHI. Rather than focusing entirely on counties as market definitions, we also estimate the model using Primary Care Service Areas (PCSA) and Hospital Service Areas (HSA). PCSA and HSA definitions come from the Dartmouth Atlas of Healthcare, and are calculated by analyzing patients' travel patterns to providers to primary care physicians and hospitals, respectively. PCSAs are on average smaller than counties (there are 6,542 defined PCSAs, or about 2.1 PCSAs per county) and HSAs are roughly similar in size to counties on average, but are defined based on where patients travel for hospital services. Since patients tend to travel short distances for primary care and longer distances for non-emergency hospital care, we view these two measures as plausible lower and upper bounds on the relevant market for physicians, on average.

7 Results

7.1 First-Stage Effects of NCA Laws on HHI

Regression results corroborate the evidence from Section 5 that increases in NCA enforceability have strong, and generally negative, effects on physician market concentration. Table 4 presents estimates from the first-stage models based on employment. The first column shows estimates from seven separate regressions of the establishment-level HHI on each of the seven legal indices. Five of the seven legal indices are statistically significant, and six of the seven 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 452 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 HHI by about 122 points.

Column 2 presents estimates from a similar specification that includes all 7 seven instruments. The Angrist-Pischke excluded instrument F-statistic is 87, and four of the instruments are statistically significant at the 0.01 level in this model. 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 full table of first-stage results showing both first and second lags of all 7 instruments is shown in the appendix (Table A4). When the second lags of each index are used as instruments, the F-statistic is similar (110.45), and when both first and second lags are used the F-statistic is 460, suggesting that any of these choices of lag specifications has sufficient power. In all three specifications, the fixed effects and excluded instruments explain about 75% of the variation in county-specialty-year HHIs.

The main results using Census data are presented in columns 3 and 4, which correspond to the two jointly-estimated first-stage equations from Section 6. Column 3 shows estimates from the establishment HHI first stage, and column 4 from the firm HHI 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 substantially weakens the first-stage power. However, the F-statistics still suggest

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

Dependent Variable:	Establishment HHI_{t-1} (1)	(2)	Estab. HHI_{t-1} (3)	Firm HHI_{t-1} (4)
Statutory $Index_{t-1}$	–2.21	0.42	0.55	–5.72*
	(1.89)	(1.38)	(1.54)	(2.11)
Protectible Interest $Index_{t-1}$	–2.49	12.16*	14.72*	3.17
	(2.63)	(3.51)	(3.52)	(3.13)
Consideration Index $Inception_{t-1}$	–5.63*	21.71	17.58*	13.76*
	(0.83)	(38.57)	(6.89)	(6.62)
Consideration Index $Post\text{-}Inception_{t-1}$	–3.08*	–2.46*	–2.38*	1.65*
	(0.41)	(0.34)	(0.59)	(0.48)
Burden of Proof $Index_{t-1}$	–4.52*	–20.70	–16.47*	–11.50*
	(0.66)	(30.66)	(6.28)	(6.09)
Blue Pencil $Index_{t-1}$	13.03*	12.56*	–0.21	3.89
	(3.92)	(3.90)	(3.23)	(2.74)
Employer Termination $Index_{t-1}$	–10.81*	–19.15*	–24.80*	–8.73*
	(1.50)	(4.51)	(3.91)	(3.60)
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	3,026,780	3,026,780	6,509,400	
N Clusters	121	121	319	
R-Sq		0.75	0.76	
F-Statistic		86.85	12.81	

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. Angrist-Pischke F-Statistic reported in column 2, Kleinbergen-Paap F-statistic reported in columns 3 and 4. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

that the instruments are not overly weak. Since these models have two endogenous regressors, we report the jointly-estimated Kleinbergen-Paap F-statistic (12.81), which is comparable to the Cragg-Donald F-statistic (297.99) suggested by Stock and Yogo (1997), but is robust to non-independent errors. To corroborate that this reduction in power is caused by unobserved specialty, we also re-estimate estimate analogous models in the MPIER data without conditioning on medical specialty, and find the instrument strength declines substantially (Appendix Table A6), although the F-statistics are still somewhat higher in the MPIER model.

Regarding the parameter estimates themselves, comparing columns 2 and 3 reveals that the additional controls for firm concentration and insurer HHI from the Census results have relatively minor impacts on the first stage coefficients. The only clear exception is the coefficient on the Blue Pencil Index, which was the only positive coefficient in the just-identified first stage results but is insignificant and negative once the additional Census controls are introduced. Finally, it is notable that the legal indices have different effects on the establishment and firm HHI measures, as can be seen by comparing column 3 to column 4. For example, an increase in the Statutory Index has a negative effect on firm HHIs of –5.7 but no effect on establishment HHIs. In contrast, the Protectible Interest Index has a significant positive effect on establishment HHIs of about 17.6 but very modest effects on firm HHIs. 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.

Table 5: 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
	(1.48)	(2.33)
Protectible Interest $Index_{t-1}$	14.11*	7.23*
	(3.36)	(3.56)
Consideration Index $Inception_{t-1}$	17.26*	22.56
	(6.94)	(13.17)
Consideration Index $Post\text{-}Inception_{t-1}$	–2.18*	2.79*
	(0.59)	(1.01)
Burden of Proof $Index_{t-1}$	–16.15*	–19.49
	(6.33)	(12.31)
Blue Pencil $Index_{t-1}$	–0.15	0.53
	(3.13)	(3.49)
Employer Termination $Index_{t-1}$	–24.06*	–10.28*
	(3.77)	(4.31)
Insurer HHI_{t-1}	0.00	–0.04
	(0.01)	(0.02)
MPIER Data Used	Yes	Yes
Census Data Used	Yes	Yes
N	6,329,900	
N Clusters	319	
R-Sq	0.83	
F-Statistic	13.56	

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-year. Kleinbergen-Paap F-statistic reported. * indicates significance at the 0.05 level.

Table 5 presents first-stage estimates using HHIs based on sales data from the Census LBD, Economic Census, and SSEL, instead of on employment. There are two main conclusions from these results. First, all seven of the coefficients in column 2 have the same sign as the coefficients in column 4 of Table 4, suggesting that the instruments have similar effects on both HHI measures. This consistency is reassuring, since there is little external evidence on the comparability of sales-based and employment-based HHI measures generally. Second, the instruments are slightly more powerful in the sales-based model, and the F-statistics remain above conventional weak-instruments thresholds.

7.2 The Effect of HHI on Negotiated Prices

Our main estimates are reported in Table 6. The top panel of the table presents results using the sales-based firm HHI measure. Column 1 uses first lags of the seven law indices as instruments, corresponding to the first stage estimates in Table 5. The coefficient on firm HHI of 0.02 implies that a 100 point increase in firm HHI, holding fixed both the establishment HHI and insurer HHI, causes a 2% increase in negotiated prices on average. This result is consistent with multi-establishment growth improving bargaining power relative to insurers. In contrast, the coefficient on the establishment HHI, –0.014, implies that holding firm HHI 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 power of physicians, the increase in their value to insurance networks, and the effect that a larger group has on the cost to the insurer of disagreement. 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 HHI is modest, 0.0007, although to be clear since insurers do not tend to use NCAs the law change events do not affect this variable, 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 HHI 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.

Column 2 of the table reports estimates using both first and second lags of each law index and yields qualitatively similar patterns. The coefficient on firm HHI declines to 0.01 but is still statistically significant, and the coefficient on establishment HHI remains similar, –0.013. To highlight the importance of addressing endogeneity in physician concentration, 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 bottom panel of Table 6 presents corroborating evidence using employment-based measures of the HHI. The estimates are again statistically significant and imply that a 100 point increase

Table 6: Main Estimates: Effect of Market Concentration on Negotiated Prices

	Dependent Variable: ln($Price$)		
	IV First Lags	IV Both Lags	OLS
	(1)	(2)	(3)
Physician Firm HHI, Sales-Based	0.020*	0.010*	0.0001*
	(0.009)	(0.004)	(0.0000)
Physician Establishment HHI	–0.014*	–0.013*	0.0001*
	(0.006)	(0.005)	(0.0000)
Insurer HHI	0.0007	0.0003	–0.0001
	(0.0006)	(0.0004)	(0.0003)
N	6,329,900	6,329,900	6,329,900
N Clusters	319	319	319
F-Stat (Cragg-Donald)	270.27	143.79	
F-Stat (Kleinbergen-Paap)	13.56	10.32	
Physician Firm HHI, Employment-Based	0.016*	0.010*	0.0001*
	(0.007)	(0.005)	(0.0000)
Physician Establishment HHI	–0.014*	–0.017*	0.0001*
	(0.005)	(0.005)	(0.0000)
Insurer HHI	0.0000	0.0000	0.0000
	(0.0002)	(0.0002)	(0.0003)
N	6,509,400	6,509,400	6,509,400
N Clusters	319	319	319
F-Stat (Cragg-Donald)	297.99	174.35	
F-Stat (Kleinbergen-Paap)	12.81	10.44	

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-year. * indicates significance at the 0.05 level.

in firm HHI, conditional on establishment and insurer HHI, increase negotiated prices by about 1% to 1.6%, while the same size increase in establishment HHI decreases prices by 1.4% to 1.7%. These estimates are consistent with the evidence from the first stage models that the sales-based and employment-based HHI measures yield similar results.

By comparison, Table 7 presents IV estimates using only the MPIER data, where firm HHI is not observable. The results suggest that a 100 point increase in the establishment HHI leads to a 2.4% to 2.8% reduction in average negotiated prices, somewhat larger than in the estimates with Census data that control for firm and insurer HHI. The estimated effect is on the upper end of this range when first lags are used as instruments and on the lower end when second lags are used.

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

Table 7: OLS and IV Second Stage: Effect of Establishment-Based Market Concentration on Prices

	Dependent Variable: $\ln(Price)_t$			
	IV	IV	IV	OLS
	(1)	(2)	(3)	(4)
HHI_{t-1}	–0.0283*	–0.0235*	–0.0251*	0.0002*
	(0.0056)	(0.0045)	(0.0047)	(0.0000)
Instruments	First Lags	Second Lags	Both Lags	
N	3,026,780	3,026,780	3,026,780	3,026,780
N Clusters	121	121	121	121
R-Sq	0.97	0.98	0.98	0.82
1st Stage F-Stat	86.85	110.45	460.22	

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-year. Angrist-Pischke F-Statistic reported. * indicates significance at the 0.05 level.

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 A6, suggest that the effect of establishment HHI is between –0.011 and –0.015 in all specifications, very similar to Census estimates when the data structure is made more comparable. We also estimate our main MPIER HHI specifications including the Census insurer HHI control but no firm HHI, 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 Census estimates.

Returning to the discussion of the exclusion restriction from Section 5.3, one additional form 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 8. Column 1 of the table presents second-stage estimates from 7 separate just-identified IV regressions using only the first-lags of each instrument, one at a time, and column 2 presents estimates using first and second lags of each index. All 14 models yield negative coefficients on the establishment HHI, and 10 of the 14 estimates 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, the differences in the legal nature of the instruments would presumably cause heterogeneity by instrument in the second-stage estimates. For example, whereas the Consideration Index affects the way employment contracts are written by affecting whether compensation for NCAs must be explicit, it is far less obvious that law dimensions such as the Burden of Proof Index or the Blue Pencil Index could potentially impact practice cost functions. Both of these dimensions relate to the specific procedures used during litigation related to NCA contracts and are only relevant when an employment spell is terminated and an NCA clause is

Table 8: IV Results Estimated Separately by Law Component

Dependent Variable:	$\ln(Price)$	
Instruments:	First Lags	Both Lags
Statutory Index	–0.047	–0.029*
	(0.061)	(0.015)
	[1.37]	[3.58]
Protectible Interest Index	–0.003	–0.019
	(0.051)	(0.011)
	[0.90]	[3.65]
Consideration Index Inception	–0.043*	–0.032*
	(0.011)	(0.007)
	[45.68]	[246.67]
Consideration Index Post-Inception	–0.020*	–0.021*
	(0.009)	(0.008)
	[55.70]	[33.66]
Burden of Proof Index	–0.043*	–0.031*
	(0.011)	(0.006)
	[46.98]	[260.14]
Blue Pencil Index	–0.007	–0.008*
	(0.005)	(0.003)
	[11.05]	[24.31]
Employer Termination Index	–0.024*	–0.024*
	(0.006)	(0.006)
	[52.04]	[26.08]

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-year. First-stage Angrist-Pischke F-statistics are shown in brackets. * indicates significance at the 0.05 level.

litigated. In order for the Burden of Proof index to violate the exclusion restriction, it would have to be the case that for an employment contract that was previously agreed to by both the worker and firm, after being potentially violated and litigated, the cost to the firm of producing evidence for the litigation affected prices negotiated with insurers. Similarly, with respect to the Blue Pencil Index, it would have to be true that negotiated prices would be affected by a change in the ability of a judge to ex post adjust the terms of a contract that was operable during the time of employment. The consistency of estimates over a range of instruments, each of which has unique and distinct legal mechanisms for affecting organizational incentives, makes it less likely that a potential violation of the exclusion restriction for any one legal measure could be driving 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 allow efficiency gains via economies of scale that dominate leverage effects from size in negotiation, causing negotiated prices to fall. These economies of scale can arise, for example, when physician practices share equipment, information systems, laboratory facilities, nurses, and technical and administrative staff over a larger number of physicians and patients. The contrasting firm-level estimates, however, suggest that consolidation of multi-establishment firms increases the combined impact of bargaining power and the value of a larger physician practice to an insurer network by more than any efficiency gains within the practice, leading to higher negotiated prices. This result is consistent with the notion that most of the efficiency gains from larger physician firms come from increases in practice size *at a given location*; meanwhile, consolidation across locations has smaller efficiency gains but still affects bargaining leverage in negotiation, causing a net positive effect on prices.

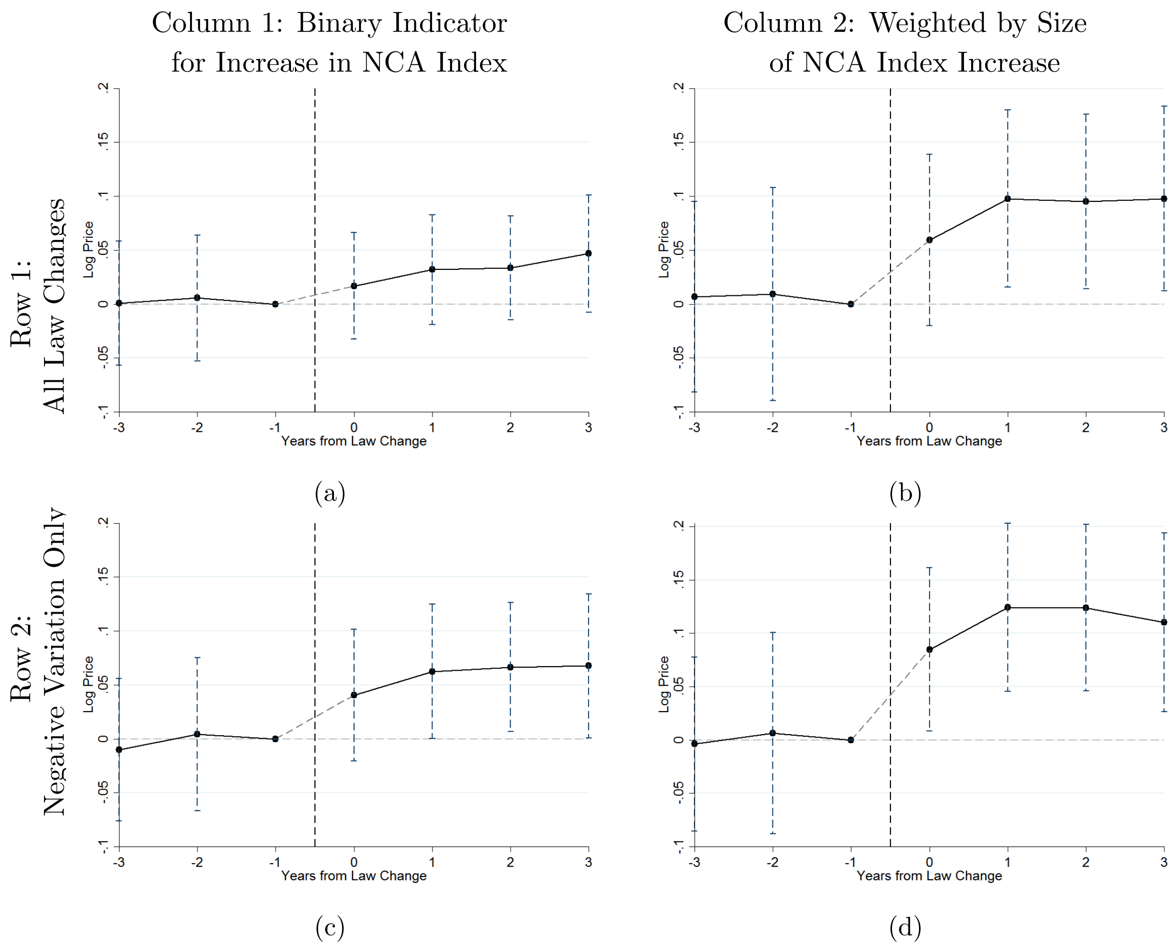
7.3 Reduced-Form Price Effects

Figure 5 presents event studies of the reduced-form effects of NCA law changes on negotiated prices. Each of the four figures suggests the same two conclusions: increasing NCA enforceability leads to higher prices, and states that experienced law changes show little evidence of differential price trends prior to the law changes. In addition, 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 the effects occur fairly quickly. The figures are constructed similarly to the previous event studies in Figure 3, comparing treatment states with exactly one law change within the event window to control states in the same census division with no law changes. The plotted coefficients are from regressions of procedure-level log prices on event year indicators and the same control variables used in the regressions: county effects, census division by year effects, specialty effects, facility type effects, and procedure code effects.

The combination of these reduced-form event studies and our main results provides policy-relevant information. In our main regression results the net direction of aggregate price effects is ambiguous—it depends on whether law changes tend to cause more within-establishment or across-establishment growth. This ambiguity is difficult to reconcile directly since the LBD does not include specialty information, preventing an accurate decomposition of the components of variation in concentration. The reduced-form effects are consistent with the predominant impacts of NCA law changes occurring through within-establishment growth.

Figure 5b scales the event year indicators by the size of the law change, so that the estimates can be interpreted as the impact of a 0.1 unit increase in the average NCA enforceability index. The figure shows that the magnitudes of the price effects are larger when the size of the law changes are larger, and a 0.1 unit increase in NCA enforceability leads to about 9.8% higher prices on average within 2 years. Given the size of the market for physician services, about $635 billion in 2015, these estimates suggest that the impact of NCA enforcement policies on physician prices are of first-order importance.

Figure 5: 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.

Figures 5c and 5d are estimated using only variation that decreases NCA enforceability, although the coefficients are scaled by -1 where appropriate to conform with the interpretation of the price effect of increasing enforceability. These figures show that decreases in enforceability have (negative) price effects that are slightly larger in magnitude, though the results are generally consistent with symmetric effects.

Regression versions of the reduced-form results using the full sample, instead of the limited event study sample that only includes the 7 treatment states with exactly 1 law change during the event window and the corresponding control states with zero law changes, are shown in Table 9. The results in the full sample have the same sign as the event study estimates and suggest prices increase by 3.7% after 2 years per 0.1 unit increase in the weighted average NCA index, compared

Table 9: Reduced-Form Price Effects, by NCA Index

Dependent Variable:	ln($Price$)	
	NCA$_{(t-1)}$	NCA$_{(t-2)}$
NCA Index (Weighted Average)	0.491*	0.370*
	(0.094)	(0.095)
Statutory Index	0.103	0.118*
	(0.062)	(0.038)
Protectible Interest Index	–0.007	–0.094
	(0.121)	(0.105)
Consideration Index Inception	0.242*	0.179*
	(0.045)	(0.049)
Consideration Index Post-Inception	0.062*	0.059*
	(0.030)	(0.025)
Burden of Proof Index	0.193*	0.131*
	(0.036)	(0.039)
Blue Pencil Index	–0.093	–0.147*
	(0.078)	(0.061)
Employer Termination Index	0.260*	0.177*
	(0.082)	(0.075)
N (Each Model)	3,026,780	3,026,780

Notes: Each coefficient comes from a separate regression of log prices on either the first lag (column 1) or second lag (column 2) 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-year. * indicates significance at the 0.05 level.

to 9.8% in the event study sample. Rescaling the 3.7% estimate by the standard deviation of the NCA index (0.259) suggests that a one standard deviation increase in NCA enforceability increases average prices by 9.6%. Six of the seven instruments have significant reduced-form effects on prices, and five have positive coefficients. The only instrument with a negative and significant sign, the Blue Pencil Index, also happens to be the only instrument with a positive and significant sign in the first stage regressions, which is why all 7 second-stage IV estimates have the same sign in the just-identified models.

7.4 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 data measurement concerns.

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 estimates from models using HHI as estimates of the elasticity of demand requires the potentially undesirable assumptions that goods are homogeneous and firms engage in Cournot competition (see Section 3.2). 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 10: Alternative Measures of Market Concentration (Establishment-Based)

	Dependent Variable: $\ln(Price)_t$		
	IV	IV	OLS
Instruments:	First Lags	Both Lags	
Negative Log $\text{HHI}_{(t-1)}$	0.190*	0.283*	0.004*
	(0.084)	(0.092)	(0.001)
1st Stage F-Stat	[80.33]	[2043.69]	
Mean Establishment $\text{Size}_{(t-1)}$	–0.043*	–0.034*	0.0003*
	(0.014)	(0.013)	(0.0000)
1st Stage F-Stat	[245.13]	[652.49]	
4-Firm Market $\text{Share}_{(t-1)}$	–0.021*	–0.022*	–0.0001
	(0.007)	(0.006)	(0.0001)
1st Stage F-Stat	[21.96]	[11.81]	
8-Firm Market $\text{Share}_{(t-1)}$	–0.030*	–0.029*	–0.0001
	(0.011)	(0.010)	(0.0001)
1st Stage F-Stat	[13.96]	[4.95]	

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. Angrist-Pischke F-Statistics reported in brackets. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

Table 10 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 bias relative to the OLS specification goes in the same direction. When average establishment size is used we find that increasing the average number of physicians in a practice by one reduces negotiated prices by about 3.4% to 4.3%. Similarly, in markets that become more concentrated in terms of the market shares of the 4 largest or 8 largest establishments, negotiated prices fall significantly. Across the variety of market structure measures and instrument specifications, we conclude that there is a statistically significant negative relationship between market concentration and negotiated prices in all fifteen models tested.

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. Market definition is often a crucial assumption in evaluating policies aimed at ensuring sufficient *levels* of competition, but since we rely on changes in concentration within markets, our estimates do not appear to be very sensitive to the assumption of market definition. The magnitudes of our estimates are very stable when using either smaller or larger market definition assumptions. Table 11 presents estimates of the main specification using counties, hospital service areas (HSAs), and primary care service areas (PCSAs) as potential market definitions. 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 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 11: Sensitivity of MPIER IV Estimates to Market Definition

	Dependent Variable: $\ln(Price)$		
	County	HSA	PCSA
	Full Sample		
$\text{HHI}_{(t-1)}$	–0.025*	–0.023*	–0.029*
	(0.005)	(0.008)	(0.008)
1st Stage F-Stat	[460.2]	[2119.6]	[270.4]
	Primary Care		
$\text{HHI}_{(t-1)}$	–0.017*	–0.018*	–0.024*
	(0.005)	(0.005)	(0.004)
1st Stage F-Stat	[542.3]	[63.1]	[122.2]

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 and twice lagged law components, as in column (3) of Table 7. Standard errors, in parentheses, are clustered by state-year. First-stage Angrist-Pischke F-statistics are reported in square parentheses. * indicates significance at the 0.05 level.

The estimates are very similar for all three market definitions, ranging from -2.3% in HSAs to -2.9% in PCSAs. This conclusion also holds within every physician specialty group, with significant negative effects for every combination of market definition and specialty, and a general pattern that effects are slightly larger in magnitude in smaller PCSA markets. Overall, we conclude that market definition assumptions do not substantively alter our conclusions.

Sensitivity to Large NCA Law Changes: Figure A1 shows that a small number of law changes are of much larger magnitude than the average change. Appendix Table A7 presents estimates in which we drop outlier states with very large NCA law changes. The estimates are very similar, remain statistically significantly different from zero, and are not significantly different from each other. The first-stage power increases slightly in all three specifications.

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 A8 we consider 4 different possible sets of assumptions about the treatment of multispecialty practices in measuring concentration. The estimates are similar under every alternative assumption tested.

Heterogeneity by Specialty Type: As discussed in Section 2, there may be heterogeneity in the usage and value of NCA contracts for physicians with different specialties. Since the benefits of NCAs in protecting the value of patient stocks is likely to be largest for physicians with many repeated interactions with the same patients, hospital-based practices that employ specialists like surgeons may derive less value from NCAs. This notion is consistent with the findings of Lavetti,

Table 12: Effect of Concentration on Prices by Medical Specialty

Dependent Variable:	$\ln(Price)_t$	
Instruments	First Lags	Both Lags
	(1)	(2)
	All Physicians	
HHI_{t-1}	–0.028*	–0.025*
	(0.006)	(0.005)
N	3,026,780	3,026,780
1st Stage F-Stat	86.9	460.2
	Primary Care Physicians	
HHI_{t-1}	–0.024*	–0.017*
	(0.005)	(0.005)
N	473,033	473,033
1st Stage F-Stat	47.2	542.3
	Non-Surgical Specialists	
HHI_{t-1}	–0.013*	–0.013*
	(0.003)	(0.003)
N	300,990	300,990
1st Stage F-Stat	39.2	98.3
	Surgical Specialists	
HHI_{t-1}	–0.004	–0.005
	(0.007)	(0.005)
N	272,913	272,913
1st Stage F-Stat	8.3	17.2

Notes: All specifications are identical to those in Table 7. 'Primary Care Physicians' 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. Angrist-Pischke F-statistics reported. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

Simon, and White (2016) that hospital-based physicians are significantly less likely to have signed NCA contracts. In Table A12 we present results from subsamples of the data that are split by groups of medical specialties. The results suggest broad and consistent effects across both primary care and specialist physicians that are less likely to be hospital-based. We estimate that a 100 point increase in HHI reduces prices by 1.7% to 2.4% for primary care physicians and by 1.3% for non-surgical specialists. In contrast, for surgical specialists we find no significant price effects and the instruments are much weaker, consistent with these physicians being less likely to use NCAs.

Interactions between Physician and Insurer Concentration: Our main results in Table 6 include controls for insurer HHI, which we find have little effect on our estimates. This result is surprising given the previous literature, such as Dafny et al. (2012), which shows that insurer concentration is an important determinant of market outcomes. One important limitation to our findings is that the effect of insurer concentration on prices is identified only by year-to-year changes in insurer concentration, which may be both small in magnitude and endogenous, and we do not have an instrument for insurer concentration. To further explore this result, we re-estimate the MPIER model specification including interactions between physician establishment HHI and categories of insurer HHI using 2007 data on insurers from the American Medical Association. Appendix Table A11 corroborates our main results, showing that the effect of establishment concentration on prices has very little sensitivity to insurer HHI—the coefficient estimates remain between –0.023 and –0.025 in markets in which the insurer concentration level is below 2500, between 2500-5000, or above 5000. One potential explanation that could reconcile this finding with the literature is if the price effects of insurer concentration tend to be driven largely by the first two terms of Equation 1, while the coefficient we estimate is driven primarily by the cost term. If insurer HHI is not strongly correlated with τ_i, the coefficient on the cost term, then it is conceptually possible for the two sets of findings to be consistent, given that our specifications include county effects and census division by year effects, which absorb much of the impact of insurer concentration on price levels.

Heterogeneity in Urban and Rural Markets: One may expect NCA laws to affect practice organization and prices differently in urban versus rural areas, since urban markets tend to have lower levels of baseline concentration. Policy-makers, who have been increasingly concerned about inadequate supply of physicians in rural markets, are likely to also want to know the extent to which consolidation is occurring and affecting prices in these areas. On the other hand, the consequences of non-compete laws may be entirely an urban phenomenon.

Table 13 presents estimates splitting the sample by metro and non-metro counties. In metro counties the instruments are strongest, and we find that a 100 point increases in establishment HHI causes a 3.1% to 3.4% decline in negotiated prices. In non-metro counties the effect is somewhat smaller, 1.1% to 1.3%, but still statistically significant despite somewhat weaker instruments. This pattern is potentially consistent with greater economies of scale lead in metro markets, where input factor prices such an nursing and staff labor, rent, and equipment costs tend to be higher. The weaker first-stage in non-metro markets may also be potentially explained by the lower rate use of NCA contracts among physicians in rural markets documented by Lavetti, Simon, and White (2016). In most rural markets in the US the supply of physicians per capita is much lower, suggesting that physician groups may derive less value from using NCAs to protect patient stocks.

There is a large literature studying the distinction between urban and rural markets in under-

Table 13: Effect of Concentration on Prices in Urban and Rural Counties

	Dependent Variable: $\ln(Price)_t$					
	All Counties		Metro Counties		Non-Metro Counties	
Instruments	First Lags	Both Lags	First Lags	Both Lags	First Lags	Both Lags
	(1)	(2)	(3)	(4)	(5)	(6)
HHI_{t-1}	–0.028*	–0.025*	–0.034*	–0.031*	–0.013*	–0.011*
	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
N	3,026,780	3,026,780	2,077,627	2,077,627	949,153	949,153
1st Stage F-Stat	86.9	460.2	54.7	364.3	15.4	15.1

Notes: All specifications are identical to those in Table 7. Angrist-Pischke F-statistics reported. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

standing the relationship between concentration and prices. Whereas a longstanding approach was to use patient flows to define relevant markets, Capps, Dranove, and Satterthwaite (2003) showed that this approach can be misleading particularly in metro areas, where the willingness of some patients to travel long distances to access lower prices may not indicate that all patients are similarly willing to travel, potentially understating local market power. This intuition may suggest that one would expect to see more positive price effects of a given change in concentration in metro areas. However, it is worth noting that the geographic distribution of willingness to pay affects prices in the bargaining framework through terms that are distinct from the cost function term, which we find dominates these other bargaining terms. Nonetheless, any attenuation in the positive network value components of our estimate in metro areas could potentially cause the combined coefficient to be more negative, consistent with the pattern in Table 13, and we cannot distinguish this explanation from the possibility that economies of scale are larger in cities. Reassuringly, our estimates are similar when using market definitions based on patient flows (PCSA and HSA) or based on counties.

Balanced Panel: 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 12 years of our panel. The IV estimates, shown in Appendix Table A13, are similar but slightly larger in the balanced panel, –0.032 to –0.036, which is partially caused by the balanced panel containing a higher proportion of metro counties.

MPIER Fuzzy Matching Algorithm and Measurement Error: There are a few types of assumptions necessary to construct HHIs from the raw MPIER data. First, some addresses are missing, so we test the sensitivity of estimates to the treatment of missing addresses. To bound the effects, we estimate the main specification under the assumption that all missing addresses are separate solo practices, and again under the assumption that all missing addresses belong to a single practice. Appendix Table A8 presents estimates under each of these assumptions, interacted with the assumption about treatment of multi-specialty practices. In all eight specifications the estimates are statistically significant and negative and have large F-statistics of at least 460. The second stage estimates range from –0.014 to –0.025, and all 8 are statistically significant at the 0.05 level.

Second, 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, to allow 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 A14 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 are not at all sensitive to this tolerance parameter, ranging from –0.025 to –0.026 (SE 0.005) in all nine specifications.

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 as well as state residents' subjective views from the Generalized Social Survey (GSS) on a variety of political, economic, and cultural topics and correlate them with the law changes. Appendix Table A16 shows that log payroll per worker, unemployment rates, and population are all uncorrelated with the law changes (columns 1-3). Politically, the share of votes to Republican candidates in presidential and congressional elections is also uncorrelated with the law changes (column 4).

Appendix Table A17 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.

8 Discussion

This paper makes three main contributions towards understanding competition in the market for physician services 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 a major advantage in that it includes total sales from all payers, in contrast to previous studies that have relied on either a single private payer or Medicare to infer approximate market shares.

Second, we construct new instrumental variables from state judicial decisions that cause shocks to physician market concentration. We use these instruments as a new source of identification to estimate the causal effect of physician market concentration on negotiated prices. The instruments alleviate a variety of concerns about endogeneity associated with unobserved factors that could be correlated with both prices and market structure, such as cost heterogeneity or latent quality variation.

Third, we draw attention to a key issue in the measurement of market structure by distinguishing between establishments and firms, which may control multiple practices. Our results suggest that this distinction is crucial for empirically understanding the trade-offs between economies of scale in physician practices and the effect of larger practices on negotiation leverage with insurers. We find that when *establishments* grow larger, economies of scale dominate other bargaining effects, leading to a net reduction in prices of about 1.3% to 1.7% 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% to 2%, suggesting that any associated economies of scale are outweighed by the effects of firm consolidation on bargaining leverage. 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.

As a matter of interpretation, one question that we cannot fully address in our analyses 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. Similarly, if multi-establishment consolidation causes price increases by affecting the bargaining weights of physicians relative to insurers, this may be of less concern to antitrust regulators than if the same price increases 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.

Our findings highlight the important role that states play in affecting healthcare 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, abstracting from general equilibrium effects, our estimates suggest that if NCA enforceability decreased nationally by 0.1 units of the Bishara Index, total physician spending would fall by about 3.7%, over $20 billion annually based on 2015 spending levels.[15] Yet 39 states have never legislatively chosen an NCA policy and instead leave the decision to the judicial branch, in which common law traditions shape current policies. Our findings suggest there may be substantial value to states conducting comprehensive assessments of their NCA policies and actively legislating, drawing on the expanding research on the impacts of NCAs.

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Appendix: For Online Publication

Table A1: NCA Law Change Frequencies by Census Division

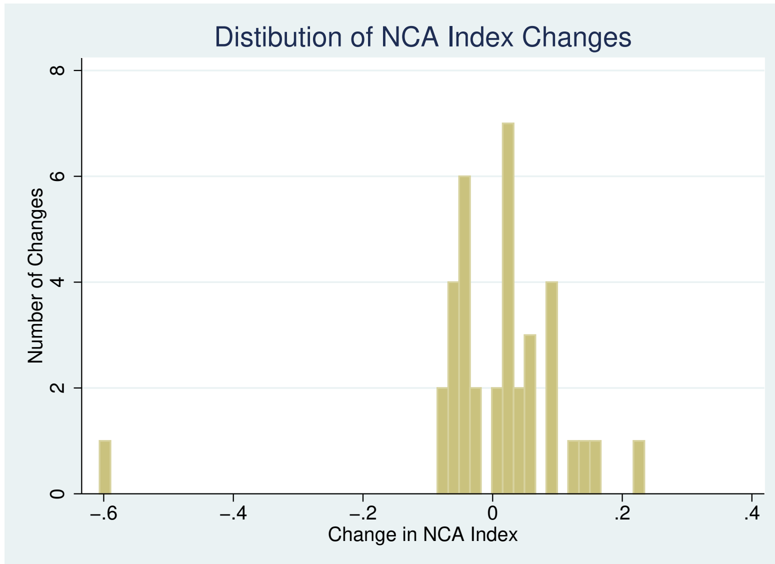
	East North Central	East South Central	Middle Atlantic	Mountain	New England	Pacific	South Atlantic	West North Central	West South Central	Total
					Positive Changes					
Statutory Index	0	1	0	0	0	0	0	1	0	2
Protectible Interest Index	0	1	1	2	1	1	1	2	2	11
Burden of Proof Index	1	1	1	2	1	0	1	0	0	7
Consideration 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	0.60	0.24	605
Burden of Proof Index	0.57	0.27	602
Consideration Index Inception	0.84	0.30	563
Consideration Index Post-Inception	0.70	0.33	526
Blue Pencil Index	0.53	0.34	538
Employer Termination Index	0.62	0.30	408

Notes: Statistics in the table represent data from 1996-2007 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.

Table A4: IV First Stage: Effect of NCA Laws on Establishment-Based Market Concentration

	Dependent Variable: HHI_{t-1}		
	(1)	(2)	(3)
Statutory Index_{t-1}	0.42		1.80
	(1.38)		(1.99)
Protectible Interest Index_{t-1}	12.16*		8.05*
	(3.51)		(3.95)
Consideration Index Inception_{t-1}	21.71		15.22
	(38.57)		(41.59)
Consideration Index $\text{Post-Inception}_{t-1}$	–2.46*		–1.27
	(0.34)		(0.64)
Burden of Proof Index_{t-1}	–20.70		–13.90
	(30.66)		(33.39)
Blue Pencil Index_{t-1}	12.56*		0.99
	(3.90)		(2.29)
Employer Termination Index_{t-1}	–19.15*		–11.17*
	(4.51)		(4.41)
Statutory Index_{t-2}		–2.15	–1.85
		(1.14)	(1.77)
Protectible Interest Index_{t-2}		6.55*	2.92*
		(1.48)	(1.13)
Consideration Index Inception_{t-2}		–3.98*	0.36
		(1.39)	(1.15)
Consideration Index $\text{Post-Inception}_{t-2}$		–2.54*	–1.55*
		(0.36)	(0.61)
Burden of Proof Index_{t-2}		–1.99	–3.70*
		(1.34)	(0.82)
Blue Pencil Index_{t-2}		16.77*	16.18*
		(2.75)	(2.29)
Employer Termination Index_{t-2}		–6.71*	–1.62
		(1.77)	(1.25)
N	3,026,780	3,026,780	3,026,780
N Clusters	121	121	121
R-Sq	0.75	0.75	0.75
AP F-Stat	86.85	110.45	460.22

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-year. * indicates significance at the 0.05 level.

Table A5: Sensitivity to IV Estimator

Instruments:	Dependent Variable: $\ln(Price)_t$ First Lags	Second Lags	Both Lags
	2SLS (Baseline)		
HHI_{t-1}	–0.028*	–0.024*	–0.025*
	(0.006)	(0.005)	(0.005)
	LIML		
HHI_{t-1}	–0.035*	–0.030*	–0.036*
	(0.007)	(0.006)	(0.007)
	2-Step GMM		
HHI_{t-1}	–0.023*	–0.016*	–0.020*
	(0.004)	(0.004)	(0.003)
N	3,026,780	3,026,780	3,026,780
N Clusters	121	121	121
1st Stage AP F-Stat	86.85	110.45	460.22

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-year. * indicates significance at the 0.05 level.

Table A6: IV Second Stage Estimates: MPIER HHIs, Markets defined by county only

	Dependent Variable: ln($Price$)			
	IV	IV	IV	OLS
Instruments:	First Lags	Second Lags	Both Lags	
$\text{HHI}_{(t-1)}$	–0.011*	–0.015*	–0.011*	0.000
	(0.050)	(0.005)	(0.005)	(0.000)
N	3,243,820	3,243,820	3,243,820	3,243,820
N Clusters	121	121	121	121
1st Stage AP F-Stat	29.03	53.70	168.33	

Notes:All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, 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-year. * indicates significance at the 0.05 level.

Table A7: IV Estimates Dropping Largest NCA Law Changes

	Dependent Variable: $\ln(Price)_t$		
	(1)	(2)	(3)
HHI_{t-1}	–0.0263*	–0.0212*	–0.0218*
	(0.0052)	(0.0041)	(0.0039)
Instruments	First Lags	Second Lags	Both Lags
N	2,853,469	2,853,469	2,853,469
N Clusters	111	111	111
1st Stage AP F-Stat	92.47	119.33	470.95

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. The sample excludes state-years with the largest law changes, which account for 6.3% of the main sample. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

Table A8: IV Second Stage Estimates for Alternative MPIER HHI Measures

	Dependent Variable: ln($Price$)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$HHI_{(t-1)}$	–0.025*	–0.016*	–0.014*	–0.014*	–0.022*	–0.022*	–0.020*	–0.020*
	(0.005)	(0.003)	(0.004)	(0.003)	(0.007)	(0.007)	(0.008)	(0.008)
N	3,026,780	3,026,780	2,936,694	2,936,694	3,026,780	3,026,780	2,936,694	2,936,694
N Clusters	121	121	121	121	121	121	121	121
R-Sq	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98
1st Stage AP F-Stat	460.22	704.58	1735.79	868.56	710.56	816.02	498.02	598.01

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 lagged and twice lagged law components (corresponding to column 3 in Table 7). All standard errors clustered by state-year. * indicates significance at the 0.05 level.

Table A9: IV Estimates Using Only Instruments with Negative First Stage

	Dependent Variable: $\ln(Price)_t$		
	(1)	(2)	(3)
HHI_{t-1}	–0.012*	–0.021*	–0.028*
	(0.005)	(0.005)	(0.005)
Instruments	First Lags	Second Lags	Both Lags
N	3,263,781	3,034,073	3,026,780
N Clusters	136	123	121
1st Stage AP F-Stat	109.18	126.30	515.27

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 8. 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-year. * indicates significance at the 0.05 level.

Table A10: Positive and Negative Law Changes

	Dependent Variable: ln($Price$)		
	(1)	(2)	(3)
	Positive Law Changes		
HHI_{t-1}	–0.026*	–0.025*	–0.028*
	(0.008)	(0.006)	(0.005)
N	1,355,532	1,269,528	1,269,528
N Clusters	66	60	60
1st Stage AP F-Stat	88.07	108.36	700.24

	Dependent Variable: ln($Price$)		
	(1)	(2)	(3)
	Negative Law Changes		
HHI_{t-1}	–0.010	–0.027*	–0.026*
	(0.007)	(0.007)	(0.007)
N	2,798,107	2,623,004	2,623,004
N Clusters	99	90	90
1st Stage AP F-Stat	27.74	54.66	37.55

Notes: All specifications are the same as in Table 6. Top panel includes states with positive changes in NCA enforceability and control states, bottom panel includes states with negative changes in NCA enforceability and control states. All standard errors clustered by state-year. * indicates significance at the .05 level.

Table A11: Interactions between Physician and Insurer Concentration

	Dependent Variable: $\ln(Price)_t$		
	(1)	(2)	(3)
$Phys\ HHI_{t-1}$	–0.024*	–0.025*	–0.025*
	(0.004)	(0.005)	(0.006)
$Phys\ HHI_{t-1} \times I(Ins\ HHI > 2500)$	–0.001		
	(0.002)		
$Phys\ HHI_{t-1} \times I(Ins\ HHI > 4000)$		0.002	
		(0.002)	
$Phys\ HHI_{t-1} \times I(Ins\ HHI < 2500)$			0.002
			(0.002)
$Phys\ HHI_{t-1} \times I(Ins\ HHI > 5000)$			0.000
			(0.002)
N	3,026,780	3,026,780	3,026,780
N Clusters	121	121	121
1st Stage F-Stat	297.13	338.98	245.75

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 in the typical 10,000 point scale. All standard errors are clustered by state-year. Kleinbergen-Paap F-Statistics reported. * indicates significance at the 0.05 level.

Table A12: Effect of Concentration on Prices, by Medical Specialty and Urban Status

	Dependent Variable: $\ln(Price)_t$					
	All Counties		Metro Counties		Non-Metro Counties	
Instruments	First Lags	First and Second Lags	First Lags	First and Second Lags	First Lags	First and Second Lags
	(1)	(2)	(3)	(4)	(5)	(6)
	All Physicians					
HHI_{t-1}	–0.028*	–0.025*	–0.034*	–0.031*	–0.013*	–0.011*
	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
N	3,026,780	3,026,780	2,077,627	2,077,627	949,153	949,153
1st Stage AP F-Stat	86.9	460.2	54.7	364.3	15.4	15.1
	Primary Care Physicians					
HHI_{t-1}	–0.024*	–0.017*	–0.036*	–0.026*	–0.007	–0.006
	(0.005)	(0.005)	(0.008)	(0.006)	(0.004)	(0.003)
N	473,033	473,033	306,449	306,449	166,584	166,584
1st Stage AP F-Stat	47.2	542.3	22.8	458.0	14.9	12.2
	Non-Surgical Specialists					
HHI_{t-1}	–0.013*	–0.013*	–0.026*	–0.023*	0.001	0.000
	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)
N	300,990	300,990	234,402	234,402	66,588	66,588
1st Stage AP F-Stat	39.2	98.3	84.4	277.8	31.2	21.8
	Surgical Specialists					
HHI_{t-1}	–0.004	–0.005	–0.009	–0.009	–0.000	–0.003
	(0.007)	(0.005)	(0.006)	(0.005)	(0.008)	(0.003)
N	272,913	272,913	191,790	191,790	81,123	81,123
1st Stage AP F-Stat	8.3	17.2	29.21	55.0	1.1	2.4

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 those in columns (1) and (3) of Table 7 for all counties, metro counties, and non-metro counties. The first two columns of the first panel reproduce the second stage results for all physicians in Table 7. 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-year. * indicates significance at the 0.05 level.

Table A13: IV Estimates on Balanced Panel

	Dependent Variable: ln($Price$) (1)	(2)	(3)
$\text{HHI}_{(t-1)}$	–0.036*	–0.034*	–0.032*
	(0.006)	(0.005)	(0.005)
N	2,032,976	2,032,976	2,032,976
N Clusters	121	121	121
1st Stage AP F-Stat	40.33	83.65	730.41

Notes: All specifications are the same as in Table 7, 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-year. * indicates significance at the .05 level.

Table A14: Sensitivity of MPIER Second Stage IV Estimates to Fuzzy Matching Algorithm Parameter

Normalized Levenshtein Distance Threshold	IV Estimate	First Stage AP F-Stat.
0.01	–0.026* (0.005)	516.27
0.05	–0.025* (0.005)	535.96
0.10	–0.025* (0.005)	489.11
0.15	–0.026* (0.005)	478.49
0.20	–0.025* (0.005)	460.22
0.25	–0.027* (0.005)	530.28
0.30	–0.026* (0.005)	613.38
0.35	–0.026* (0.005)	607.27
0.40	–0.026* (0.005)	587.15

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 over-estimating 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-year. * indicates significance at the 0.05 level.

Table A15: Fixed Effects Models of Aggregate Physician Supply

Dependent Variable:	Log Physicians per 100,000 Population	
Bishara Score	-0.027	
	(0.041)	
Lagged Bishara Score	–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.	0.88	0.88

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 A16: 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 A17: 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 (1)	To Help Pay for Medical Care (2)	Urban Issues (3)	Welfare (4)	Nation's Health (5)	A Republican (6)	Satisfied With His Finacial Situation (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.