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

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Abstract

We study the relationship between concentration in the market for physician services and prices negotiated between physician practices and private insurance companies. We develop a new set of instrumental variables for changes in concentration: 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 privately negotiated prices with insurance companies, we show that negotiated prices fall when physician establishments become larger due to changes in NCA laws. Our results imply that a 100 point increase in the establishment-based Herfindahl Index (HHI) causes a 2.4 to 2.8 percent decline in negotiated prices, suggesting that insurers extract some efficiency gains from larger establishments. In contrast, when physically distinct establishments negotiate jointly as a firm the opposite is true—a 100 point increase in the firm-based HHI increases negotiated prices by 1.2 to 2.0 percent. The price effects are largest in metropolitan markets and for non-surgical physician specialties.

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1 Introduction

Spending on physician services accounts for about 20% of all U.S. medical spending and has been rising even faster than total medical spending.[1] Meanwhile, anecdotal evidence has suggested that physician practices have consolidated 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]

This paper provides new comprehensive evidence on the concentration of physician markets and addresses two major challenges to understanding the causal relationship between physician market concentration and negotiated prices. The first challenge is that longitudinal data on physician practice sizes linked to prices are very difficult to obtain. We employ two complementary data sets on 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). While these data lack information on medical specialties, they allow us 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.

The second empirical challenge is that physician market structure may be correlated with a variety of unobserved factors including variation in quality, costs, and demand factors, potentially causing endogeneity in pricing models (Gaynor et al. (2015)). To overcome this problem, we construct new instrumental variables using judicial decisions that cause changes to state laws governing the enforceability of non-compete agreements (NCAs), which restrict an employee's ability to leave a firm and compete against it. As documented by Bishara (2011), NCA laws vary along seven quantifiable dimensions across states and over time due to judicial decisions. We construct a panel of law changes for each of the legal dimensions for every state between 1991-2009 using the coding methodology developed by Bishara (2011).[3] Lavetti et al. (2016) provide evidence from survey data that the use of

[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.

[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)

NCAs in physician employment contracts is very common, with about 45% of primary care physicians in group practices bound by NCAs. By altering the ability of workers to exit firms and compete within a local market, these law changes create shocks to the organizational incentives of physicians that have substantial effects on physician practice sizes.

We provide a variety of evidence on the mechanisms through which NCA law changes affect negotiated prices. First, we show that law changes cause significant abrupt effects on the rate of physician-establishment job separations, which in turn leads to changes in the distribution of practice sizes, the rate of births of new physician practices, the rate of death of existing practices, and ultimately on the Herfindahl-Hirschman Index (HHI) of market concentration.

We use these law changes to estimate IV regressions of negotiated prices on market concentration, including fixed effects for geographic market, census division by year, medical specialty, service procedure code, and medical facility type. 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 2.4% to 2.8% on average. In contrast, IV estimates of the effect of firm-level concentration measures using tax IDs from the LBD show that larger firms tend to increase negotiated prices. The estimates imply that a 100 point increase in the *firm*-based HHI raises prices by 1.2% to 2.0%.[4] In both models, comparable 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 tradeoff between the efficiency gains of larger establishments and 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 large economies of scale could be due, for example, to larger physician practices sharing nursing, laboratory, technological, and administrative resources. However, estimates from the LBD suggest that when physician firms grow larger the net effect on prices is positive, implying that any economies of scale from mergers of physically-distinct practices have smaller price effects than the associated bargaining leverage effect. The estimate provides a lower bound of the effect of physician firm size on the ability to negotiate higher prices.

In addition to estimating average effects across all markets, we examine heterogeneity across metro and non-metro markets and by physician specialty. We find identical patterns in both metro and non-

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.

[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) also use Marketscan price data (2003-2010) but estimate market structure based on Medicare beneficiaries.

metro counties, although the effects are largest in urban areas. We also find that the effects hold broadly across many physician specialties, with the exception of surgical specialists, who tend to treat patients in hospital settings, where NCAs are less common.

There are two potential concerns regarding the exclusion restriction of our instruments. First, practices using NCAs may have different cost functions, directly altering negotiated prices Second, there could be selection on physician quality into practice 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 with their negotiated services prices and a variety of quality measures. First, there is no statistically significant or meaningful difference in the prices negotiated between insurers and physician practices that use NCAs relative to practices that do not (the decision to impose NCAs is made at the firm level, not the physician level).[6] Second, there is no evidence of quality differences associated with the use of NCAs. In addition to a lack of difference in negotiated prices suggesting 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, 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.[7]

In addition to the IV conditions, there are also several general concerns about using a structure-conduct-performance (SCP) approach to estimate causal effects of market structure on prices (Gaynor et al., (2015)). One concern is that estimates can be sensitive to assumptions about market definition, which we address by showing that results are consistent across a wide 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 work aiming to identify the effects of provider consolidation in medical care markets has generally taken a structural approach to modeling the bargaining game between hospitals and insurers.[8] 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 and physicians. For example, the approach developed by Ho and Lee (2016) uses data on costs for each firm in a market, which may be available for hospitals but does not exist to our knowledge for physician practices. In addition, this approach requires claims data to estimate demand and willingness to pay in a manner that allows for unobserved quality heterogeneity. Since we do not have firm-level prices or data on individual patient choices and insurance networks, we cannot

[6] 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.

[7] See Choudhry et al. 2005.

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

implement these approaches.

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. We show that the estimates are very similar across all of these models, suggesting that, in our setting, assumptions about firm conduct and market definition are not as important as the endogeneity of market structure measures. Of course, the parameters that we are able to identify through this approach are combinations of the more primitive structural parameters. Still, these parameters provide meaningful and intuitive answers to important policy questions. To provide a closer link to the structural literature, we derive a theoretical linkage between our estimation approach and one particular structural model that assumes Nash-Bertrand conduct, adapted from the Ho and Lee (2016) bargaining model, and we demonstrate the connection between our estimates and more primitive structural parameters. We adapt the model slightly to fit our empirical setting by modeling consumer willingness to pay for insurance networks as being driven by physician access.

According to the model, a decline in negotiated prices as practice sizes increase, as we find in the establishment-level estimates, can be explained by a declining average cost function. The model and estimates together imply that to the extent larger practices can use the threat of exiting a insurance network to negotiate higher prices, this network value effect[9] is less responsive to practice size than the cost function is, causing the combined effect to be negative. In contrast, when practices merge but retain physically distinct establishments they may not achieve the same reduction in average costs, since each establishment still incurs fixed costs, but the firm still gains network value. This intuition provides a framework for interpreting the estimates, demonstrating why price effects may be positive at the firm-level but negative at the establishment level. The combination of the establishment-level and firm-level results provides a lower bound estimate of the rate at which average costs fall with practice sizes, and a lower bound estimate of the effect of practice size on network value.

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.[10] Many studies, including Pauly (1993) and Anderson et al. (2003) have shown that this difference in spending is primarily due to differences in prices, not differences in quantities. This finding has driven researchers in the U.S. 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 specialties or geographic markets), or does not address the potential endogeneity of variation in market structure. Our results may also be useful in assessing the potential impacts of the Affordable Care Act on competition in physician markets, particularly given the expansion of accountable care organizations.

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

[9]We define network value more precisely as a combination of structural parameters that affect the relative costs of disagreement in a negotiation between a practice and insurer.

[10]See OECD Health Statistics 2014

sources that we use, and Section 5 elaborates on the instrumental variables that we develop, including evidence on the mechanisms, instrument validity, a discussion of the IV assumptions, and evidence on the exclusion restriction. Section 6 describes our main empirical model. Section 7 describes results, beginning with the first-stage and instrument strength, the estimated effects on prices, and a discussion of several robustness tests. Section 8 concludes.

2 Background: Non-Compete Laws and Physicians

2.1 NCA Laws and Changes

Non-compete agreements (NCAs) 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 these contracts who leave their firms must either leave the geographic market, wait until the NCA has expired, or take a job outside of medicine.[11] 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 determining the enforceability of NCAs. First, does the firm have a legitimate business interest that is capable of being protected by an NCA? Second, does the NCA cause an undue burden on the worker? And third, is the NCA contrary to the public interest? Changes in the interpretation and relative importance of these questions have caused judicial decisions to frequently break from precedent, effectively changing NCA laws in the state.

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 NCAs thereafter applied only to employees attempting to establish a new business, but cannot prevent a worker from joining a competing firm that already exists. This sudden change allowed all workers in the state to escape the restrictions of NCAs that they had already signed and move to other firms.

To take advantage of the rich variation in the relevant legal environments, we quantify variation in

[11] 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.

NCA laws across states and 64 law change events using the methodology developed by Bishara (2011). These data are described in detail in Section 4.4.

2.2 Physician Markets and the Use of NCAs

In general, NCAs are useful contracting tools to mitigate investment holdup problems in firms whose critical assets can be taken with employees when they leave. This problem tends to arise in innovative firms that invest intellectual capital, and in service firms that invest in client relationships. Owners deciding whether to invest in intangible capital, including human capital and relationships with clients, face the risk that employees will leave at any moment, reducing the firm's return on the investment. NCAs can thus increase the owner's incentive to invest by limiting the employee's mobility.

Physician practices in particular, because of their reliance on human capital and client relationships, can benefit from the use of NCAs. Information asymmetries between physicians and patients make search costs for physicians high and generate loyalty towards known physicians. The loyalty of patients is arguably the most valuable asset of most physician practices—the patient base is generally 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 hiring a new physician to join the practice and steering patients towards them could lead to the loss of both if the physician subsequently departs to practice at another local firm and the patients follow the physician. NCAs can prevent this type of loss and thus encourage investment in physician employees and their client relationships.

The directions of the impacts of the components of NCA laws on physician market structure are mixed. Of the seven dimensions of NCA laws, an increase in enforceability along five dimensions tends to reduce physician market HHIs, while increasing enforceability on the other two dimensions increases HHIs. For example, one dimension of the law, which we call the 'Employer Termination Index,' measures the extent to which state law allows a firm to fire a worker and still enforce the NCA against them. In some states this would be legal, while in other state 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, which we call the 'Blue Pencil Index,' measures variation in the extent to which contracts that are written to be overly restrictive to workers can still be enforced by allowing judges to modify contracts ex-post. Increasing this aspect of NCA law causes HHIs to increase, potentially because physicians are simply less likely to leave group practices. 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 anticipatory responses, abrupt increases in job separation rates in the year following a law change, and no apparent correlations between law changes and state-level economic or political measures.

An important fact to note is that physicians do, in fact, frequently and systematically use NCAs. This was documented by Lavetti et al. (2016), who show that about 45% of primary care physicians in group practices are bound by NCAs, and study the incentives behind the use of NCAs in highskilled service firms like physician practices. They find that in states where NCAs are easier to enforce, physician practices are much more likely to use NCAs. In a five state sample, use ranges from about 30% of employed physicians in California, a low enforceability state, to 66% in Pennsylvania.

3 Theoretical 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 use the theory to guide our empirical research design. 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 prices p_{ij}, which are private knowledge of the negotiating parties. Prices p_{ij} are negotiated on a capitated basis.[12] Simultaneously with bargaining, insurers set profit-maximizing uniform premiums ϕ_i that they will charge all consumers. 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 wait the required amount of time necessary to visit a physician.

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

The profit function of insurer i is:

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

where D_i represents the number of enrollees in insurance plan i, which depends on wait times $w_i(\phi_i, \mathcal{G})$

[12]In reality many contracts with physicians are capitated, but for non-capitated contracts one could consider a capitated payment to be similar to an average price for an expected bundle of services.

in network i, and D_{ij} is the number of enrollees in plan i who visit physician group j.[13] 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:

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

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 between physicians and insurers simplifies to:

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

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

[13] 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.

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. That is, the change in utility that the consumer gets from 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 $\frac{\partial p^\star_{ih}(D_{ih} - D_{ih-j})}{\partial |P_j|} < 0$, 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 generate an empirical analog of the FOC, suppose in disagreement the potential consumers of group j are distributed proportionally among the remaining physician groups in the network. Then:

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

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, $|\mathcal{G}_i|$ given 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 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 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 no not observe the negotiated price for each practice; we only know the average price across all practices in a market.

The empirical analog of the structural model that we consider is thus:

$$\overline{p^{\star}_{ijt}} = \alpha + \beta_2 \,|P_{jt}| + \eta_m + \pi_p + \gamma_c + \nu_{dt} + \varepsilon_{ijt} \tag{4}$$

where β_2 gives the effect of a change in practice size on average negotiated prices, which occurs through a combination of changes to network value and average cost, according to the model. Equation 4 combines information from many markets, but accounts 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, ν_{dt}, which of course nest year effects while also allowing prices to change arbitrarily over time across census divisions.

Given the limitations of the empirical model relative to structural analog, is β_2 still 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 probably not, since we cannot separate the effects of changes in average costs from the effects of network value. However, it turns out to be informative in our setting when we consider the distinction between establishments and firms. To see why, consider a hypothetical merger between two 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. If the merger were to cause a change in negotiated prices such that $\beta_2 < 0$, it would imply that declining average costs dominate the impact of any change in network value on negotiated prices.

In our empirical analyses we estimate an aggregated version of this model using establishment-based measures of practice sizes and find that $\beta_2 < 0$. This finding suggests insurers extract the efficiency gains from larger establishments in the form of lower prices, and these efficiency gains outweigh any increase in network value. However, we then estimate the same model using physician firm sizes, linking multi-establishment practices together according to their IRS tax IDs. In this model we find $\beta_2 > 0$, the opposite sign. Knowing that average cost functions are decreasing in practice size from the establishment-level analyses, this finding implies growth that occurs through multi-establishment consolidation yields efficiency gains that are smaller than the effects on network value, causing negotiated prices to increase. The combined set of results reveals two lower bounds of interest. The establishment-based model implies a lower bound of the rate at which average costs fall with practice sizes, and the firm-based model implies a lower bound on the effect of firm sizes on network value.

This model aims to facilitate the interpretation of this change in signs in the empirical estimates, and to clarify the meaning of β_2 relative to the more fundamental structural parameters that it represents.

3.2 Firm Conduct and Measuring Market Structure

In addition to estimating Equation 4 using practice sizes, we also estimate analogs 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 some reasons to be less concerned with 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)). In the case of physicians, especially primary care physicians, there is often no clear analog 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.[14] 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.

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 about SCP models described by Gaynor et al. (2015) 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 could all be correlated with market structure, causing bias. Moreover, these bias components could oppose each other, creating ambiguity about the net direction of the 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,

[14] 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.

the market share of high cost practices will be lower. The negative correlation between latent average cost and market share will 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 differences, and this error component is positively correlated with market share, causing an upward bias in $\widehat{\beta}_2$.

As a result, the net bias is ambiguous. Moreover, 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 from establishment-based models are upward biased relative to IV estimates. In contrast, when physician groups merge together but retain physically distinct establishments there are fewer opportunities for sharing fixed costs, suggesting that the growth may be more strongly motivated by bargaining with insurers. We find that OLS estimates of β_2 from models identified by firm-level variation are downward biased relative to IV estimates.

Our results generally support the conclusion that endogeneity of market structure in Equation 4 causes substantial bias. A primary goals of our study is to develop new instrumental variables to overcome these biases in a variety of markets, even outside of healthcare.

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, at which point it was replaced by a new system.

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[15] 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 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 using 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 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 Specialtists)' 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 along with physician 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 every active employee 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, by year, by physician specialty, by Current Procedural Terminology (CPT) code, and by medical facility type (for example, physician office, hospital outpatient facility, hospital inpatient facility, urgent care facility, end-stage renal disease facility).

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

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 Medstat 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 a new instrumental variable 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 was 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 A1.

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).[16] During the 1996-2007 period that we study, there were 64 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 to enforce. Figure 1 shows the frequencies of these NCA index values in all state-year pairs in our sample, and Figure 2 shows the distribution of changes in index values within states over time.

5 IV Description, Mechanism and Validity

In this section we discuss the validity of the new instrumental variables that we construct, 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.

[16]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.

5.1 IV Description and Effects on Market Concentration

In our empirical specifications we estimate the relationship between negotiated prices and several different measures of market structure, including the average practice size, the HHI, the negative log HHI transformation used by Cooper et al. (2012), and the four-firm and eight-firm concentration ratios.

The first piece of unconditional evidence on the effects of the instruments is shown in Figure 3, which depicts the kernel density functions of annual changes in HHIs within markets. Each observation underlying these distributions is a CBSA-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 a given state. In years just after a law change, the density function is visibly and statistically significantly changed (Kolmogorov-Smirnov p-value<0.001), with less mass near zero, and more mass in the region of negative HHI changes. While the effects of law changes are clearly apparent in the unconditional comparison of HHI changes, our analyses condition, of course, on unobserved geographic and intertemporal variation as well as on differences across specialties.

Of the seven NCA law components we use as instrumental variables, two of them (Protectible Interest Index and Blue Pencil Index) tend to be positively correlated with HHIs and the remaining five negatively correlated.[17] The positively correlated indices measure how broadly courts have defined firms' protectible interests and whether courts are allowed to edit NCA contracts ex post (with a 'Blue Pencil') to make them enforceable in the event that they were written too broadly (as opposed to invalidating the contract altogether). In strongly restrictive states each of these two components could act both to deter workers from leaving a firm and to encourage employers to invest in hiring more employees because their investment is less likely to be lost. Both of these effects could plausibly lead firms to grow larger over time, all else equal.

The negatively correlated law components are the Statutory Index, Burden of Proof Index, Consideration Indices (Pre and Post Inception of Employment), and Employer Termination Index. These components measure, respectively, whether the state has a strong statute that favors NCA enforceability; whether plaintiffs in litigation have a weak burden of proof; whether the contract must be explicit about what compensation ('consideration,' in legal terminology) is being made to the worker in exchange for accepting an NCA; whether being offered a job, or not being fired, is considered sufficient compensation; and whether an NCA can still be enforced against a worker if a firm chooses to fire the worker. A strengthening of any of these components could plausibly lead to more separations between workers and firms. For example, if a firm imposes an NCA after a job has begun in a state where no additional compensation is required, the worker may be more likely to quit. The ability to enforce an NCA if a firm fires a worker may decrease the cost of firings, making them more likely to occur.

Since the law changes occur at different times and in different locations, we include controls to account for any potential correlation between law changes and other factors that change over time.

[17] This is an approximate partition because for some legal indices the sign of the correlation changes in the first and second lags.

Figures 4 and 5 display the average residuals from regressions of HHI on year fixed effects, state effects, and census-division-by-year effects. The three functions shown in each graph are group averages of the specialty-level HHIs, where groups are defined as primary care physicians, surgical specialists, and non-surgical specialists. We define groups this way following evidence from Lavetti et al. (2016), which concludes that physician practices use NCAs to protect the value of practice's investments in relationships with patients, and shows that NCAs are used more frequently in settings where physicians have repeated interactions with the same patients. Repeated interactions are less likely to occur in hospital settings, especially for surgical procedures, so we use surgical specialists as a group that is less likely to be bound by NCA contracts, and we distinguish between primary care physicians and non-surgical specialists who may face substantially different competitive forces.

Figure 4 shows changes in HHIs in an eight year window around a reduction in the two positively correlated laws, which we combine to form NCA Component Group Index 1. The first graph shows unconditional raw HHIs, which appear flat until the law change and then trend slightly downward after the change. The second graph (upper right) removes year effects to adjust for the differences in timing of the law changes, and shows a stronger downward break after the decline in Index 1. This downward break remains and strengthens when state effects (lower left) and census division by year effects (lower right) are also removed. In this final figure HHIs are relatively flat prior to the law changes, but then abruptly decrease by about 50 to 100 HHI points in each of the two years following the law change, remaining at the lower levels in the third and fourth years.

Figure 5 graphs HHIs before and after increases in NCA Component Group Index 2, which is similarly defined as an aggregate index of the five law components that are negatively correlated with HHIs. The unconditional data in the first graph of Figure 5 show distinct but small declines in HHIs for all three groups of physicians a year after the law change. These breaks become substantially larger as more fixed effects are removed and appear strongest in the fourth graph, after controlling for year, state, and census-division by year effects. These law changes appear to take slightly longer to have effects, but cause a sudden reduction of about 100 to 200 HHI points within two years. One reason why the effects in Figure 5 may appear delayed by one year is because we do not observe the precise date of the law change, only the year during which the change occurred.[18] It also seems plausible that physicians and practices may need time to re-optimize given the new legal circumstances, and may not adjust immediately to all types of law changes.

5.2 Mechanism

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

Figure 6 shows the unconditional average physician-practice separation rates three years before and

[18] A change in market concentration that occurs very quickly will be incorporated into observed HHIs in year 0 and appear as a change between year -1 and year 0. For law changes that occur late in a given calendar year, a very quick effect may not be observed until year 1 and appear as a change between year 0 and year 1. An effect that takes longer to occur may not be observed until year 2, and appear as a change between year 1 and year 2.

after a decline in each of the legal indices. Average separation rates are calculated across all county-specialty-year combinations for which the relevant legal change occurred. Since many physicians have multiple locations at which they treat patients, a separation occurs whenever a physician stops treating patients at any particular location, so the average separation rates shown in the graphs are higher than typical physician-firm separation rates. The first subfigure, for example, shows that in the three years prior to decreases in the Statutory Index, average separation rates ranged between about 10-20% per year. In the first year following any decline in the Statutory Index, the average separation rate jumps abruptly to about 33% before falling back to pre-change levels within three years. Remarkably, for all 6 of the instruments for which we ever observe a reduction, there is a substantial jump in separation rates that occurs precisely one year after the law change. In all six cases the separation rate jumps by over 100% in the year following the law change relative to year -1, the full year prior to the law change.

Figure 6 also shows that within 3 years after each law change, average separation rates always return to the pre-change range. One may wonder why the spike in separation rates is temporary rather than persistent. This pattern is consistent 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 move simultaneously. Once the moves are completed there is less pent-up desire to switch practices, and separation rates subsequently decline. By similar logic, increases in enforceability (not shown) have slower effects on separation rates since the stock of physicians that would like to move prior to the law change remains even more constrained after the law change.

The strikingly large and consistent patterns in Figure 6 provide some additional useful information. First, they support the evidence that NCA laws constrain physicians' choices over practices, suggesting that there are organizational effects that could lead to changes in market concentration. Second, the graphs suggest that there is no clear anticipatory effect of the law change, which makes sense because it may be difficult to predict when judicial decisions will change previous precedent. Third, the abrupt change in separation rates suggests that re-organization of practices occurs fairly quickly, within about one year. Accordingly, our main empirical specifications focus on the effects of legal changes on practice sizes and market concentration in the year of the law change and the subsequent year.

Further evidence that there is an intuitive mechanism through which law changes affect concentration can be seen in Table 1. The table includes fixed effects estimates of the impact of each legal index on the number of new physician practice births and deaths in each county-specialty-year combination. In each model the dependent variable is either the number of new establishments born (births) or the number of establishments that fully dissolve (deaths). The independent variables are one-year lags of each legal index as well as fixed county-specialty effects and census-division-year effects.

Column 1 shows that all seven instruments have statistically significant effects on the number of new practices born, ranging from a reduction in the birth rate of 4.7 practices per county-specialty-year per one-unit change in the Employer Termination Index, to a 3.4 practice increase per unit increase in the Consideration 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 by scaling by the standard deviation of each index, which is given in Appendix Table A2. For example, a one standard deviation increase in the Employer Termination Index is associated with 1.4 fewer practices born. Column 2 shows that the estimated number of establishment deaths is very similar in magnitude and direction to the estimated effect on establishment births. This suggests that when physicians separate during an establishment death, some separating physicians start new practices. Columns 3 through 6 show that the effects on births and deaths are larger in metropolitan counties than in rural non-metro counties, but are still statistically and economically significant in the latter.

Finally, Table 2 shows that these changes in separation rates and establishment births and deaths also lead to changes in the average sizes of establishments. The table shows estimates from fixed effects specifications in which 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 are 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. The estimates suggest that three of the seven legal indices have statistically significant negative effects on average establishment sizes, and two have significant positive effects. The effects range from a reduction in establishment sizes of about 18% to an increase in 10% per unit change in each index, or about -5% to 3% per standard deviation change in each index.

This combined evidence connects the effects of the instruments from the individual employment level through physician-practice separation rates, to practice-level effects on establishment sizes, births, and deaths, documenting the underlying steps that lead to changes in HHI. One further question is why some legal changes lead to increases in establishment sizes, while others lead to declines. The answer to this question is not entirely transparent, although we provide some intuitive but speculative arguments above related to the legal consequences of each index. However, suppose we were agnostic about why each index causes changes in HHIs, and view of the role of the instruments simply as judicial decisions that cause exogenous shocks to the employment contracts of physicians, leading to reorganization of practices that has some random chance of affecting concentration. Even this interpretation would not be problematic for the research design. For example, consider a hypothetical experiment in which physician establishments are randomly destroyed and then recreated, and the size of each newly created establishment is determined by a random draw from the pre-experiment distribution of establishment sizes. This experiment could be used to estimate the causal effects of practice sizes or market concentration on negotiated prices, even if the population distribution of practice sizes does not change at all. One potential view of the instruments is that they serve as such a shock, with potentially ambiguous effects on the sizes of newly-formed establishments. We make use of the variation in market concentration that occurs as a result of the reorganization process.

5.3 IV Assumptions

At the physician level, a change in laws 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.

There are multiple potential objects of interest to policymakers. To a judge who is interested in determining whether NCAs tend to cause an undue burden on workers, or whether firms have a legitimate business interest in using NCAs, observing treatment directly and estimating local average treatment effects could be the most informative way to evaluate the effects of NCAs. However, it is also of interest to know how changing state laws that govern NCA enforceability will affect aggregate outcomes. In evaluating these effects, the object of greater interest may be the combined impact of the law change on selection into treatment and the effect of treatment on treated. This is the measure that we identify in our first-stage models, described in Section 6.

The first assumption required for causal inference in IV models is the Stable Unit Treatment Value Assumption (SUTVA) of Rubin (1974),[19] which requires, using the above notation, that:

$$\text{If } NCA_i = NCA_i', \text{ then } C_i(NCA_i) = C_i(NCA_i')$$

and

$$\text{If } NCA_i = NCA_i' \text{ and } C_i = C_i', \text{ then } P_i(NCA_i, C_i) = P_i(NCA_i', C_i')$$

This assumption says that potential prices in county i are unrelated to NCA policies in other counties, conditional on the included fixed effects. The assumption holds as long as we have properly defined geographic markets, across which agents should not constrain or impact each other. We test a variety of market definitions to assess whether this assumption seems plausible.

The second assumption required is unconfounded assignment,

$$\Pr(NCA = r \mid X) = \Pr(NCA = r' \mid X)$$

This assumption requires that changes in laws are a good as random, conditional on covariates. The assumption is satisfied as long as judicial decisions that cause law changes are not correlated with physician market concentrations or on prices negotiated between physicians and insurers. We validate that this assumption is plausible by analyzing the law changes themselves. Since judicial decisions are accompanied by opinions written by judges that describe the rationales that led them to their decisions, we verify that none of the decisions used in our data was made considering factors related to physician market concentration or prices. 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.

5.4 Exclusion Restriction

The final requirement for instrument validity is the exclusion restriction, which holds as long as NCA law changes affect physician service prices only through physician market concentration. In other words, changes in NCA laws are not correlated with the error term in the second stage equation.

[19]See also Angrist et al. (1996).

In our structural equation, described below, negotiated prices depend on market concentration and unobserved 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 not statistically significant. In addition, the price difference between NCA users and non-users is no different in higher versus lower NCA enforcement states. This evidence suggests that, despite substantial variation in negotiated prices for identical procedures across practices within markets, none of this variation is significantly correlated with the use or enforceability of NCA contracts. 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.[20] Finally, the survey data contain a rich amount of information about quality from a section containing vignette-based questions that were designed by clinical experts to directly elicit knowledge about clinical best practices, guidelines, diagnostic skill, and appropriate treatment recommendations. There are no systematic differences associated with the use of NCAs either in the distributions of responses to questions or in aggregate measures of compliance with guidelines.

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

[20]See Choudhry et al. (2005).

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. The first and second stages are:

$$C_{mc(t-1)} = \alpha_1 + NCA'_{c(t-1,t-2)}\beta_1 + \eta_m + \pi_f + \theta_p + \gamma_c + \nu_{dt} + \epsilon_{mc(t-1)} \quad (5)$$

$$P_{mfpct} = \alpha_2 + \beta_2 \widehat{C}_{mc(t-1)} + \eta_m + \pi_f + \theta_p + \gamma_c + \nu_{dt} + \varepsilon_{mfpct} \quad (6)$$

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 quantified NCA law dimensions, which are measured at the state-year level. C_{mct} is a measure of market concentration. In most of our analyses we measure concentration as HHIs, but we also estimate the model alternative measures including average practice size (the number physicians per practice), the negative log HHI transformation, and the four and eight-firm concentration ratios. The fixed effects specification controls for specialty effects, facility type effects, procedure code effects, county effects, and census-division by year effects. In all of the models presented, standard errors are clustered by state-year.

By including census-division by year effects we estimate 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 census divisions to have 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.

For robustness, we test similar models with different market definitions, with HHI measures calculated in different ways from multiple data sources, with alternative measures of market concentration and firm sizes, and controlling for insurance market HHI as well. 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. There are 6,542 defined PCSAs (or about 2.1 PCSAs per county on average) and 3,436 HSAs. In specifications that use PCSAs we measure county-level average prices in the second stage, since that the finest geographic level at which our data on negotiated prices exist, but PCSA-level concentration in the first stage. We estimate the model using HHIs based on employment counts from the MPIER, and based on sales, payroll, and employment counts from the LBD.

7 Results

7.1 First-Stage Effects of NCA Laws on Establishment HHI

Regression results corroborate the evidence from Section 5 that NCA law changes have strong effects on physician market concentration. Estimates from the first-stage model (Equation 5) are presented in Table 3. The three specifications presented in the table show estimates from fixed effects regressions of lagged establishment-level HHI (from the MPIER data) on lagged NCA law components (col. 1), twice lagged law components (col. 2), and on both the first and second lags (col. 3). Each of the specifications defines markets by county-specialty-years.

The first specification has an Angrist-Pischke excluded instrument F-statistic of about 87, and four of the instruments are statistically significant at the 0.01 level. The HHI measure is scaled to range between 0 and 100, so that a one unit change corresponds to a 100 point change on the typical 10,000 point scale. We find, for example, that a change in the Protectible Interest Index that moves NCA laws from the least enforceable observed policy to the most enforceable policy (0 to 1, as scaled) increases HHIs by about 1216 points. Of course, the observed changes in NCA laws are far less extreme in magnitude. A one standard deviation change in the Protectible Interest Index (0.24), for example, increases HHIs by about 292 points.

The specification using second lags also suggests strong first-stage effects, with an F-statistic of 110, and five instruments significant at the 0.01 level. Two of the law components tend to increase HHIs, while the remaining three decrease HHIs, and the directions of all significant effects are the same in both specifications. Including all of the first and second lags in the model further improves the strength of the combined set of instruments, increasing the F-statistic to 460. In all three models, the fixed effects and excluded instruments explain about 75% of the variation in county-specialty-year HHIs.

We also investigate heterogeneity in the estimates according to whether the market is metropolitan or non-metro, and by different groups of physician specialties. In general, we conclude that the instruments are strongest in metropolitan markets, and among primary care and non-surgical specialties. For example, Appendix Table A9 shows that the F-stat is 364 in metro counties, but only 15 in non-metro counties. Still, five of the instruments are statistically significant in the non-metro sample. Appendix Tables A10, A11, and A12 present first-stage estimates by combinations of physician specialties and metro/non-metro counties. The first stage is very strong for primary care physicians (F-stats of 542 overall, 458 in metro counties) and non-surgical specialists (98 overall, 278 in metro counties). However, the instruments are somewhat weaker for surgical specialists (17 overall and 55 in metro counties), as expected based on evidence from Lavetti et al. (2016) showing that hospital-based physicians and physicians that do not tend to have repeated interactions with patients are less likely to use NCAs. Still, the instruments pass conventional F-stat thresholds. For example, with one endogenous regressor and 3 to 14 instruments the Stock and Yogo (1997) critical value thresholds for 10% relative bias under 2SLS range from about 9 to 12.

7.2 First-Stage Effects of NCA Laws on Firm HHI

Using Census data, we also estimate the first-stage effects of NCA law changes on firm-level HHIs using employment shares and firm sales. The main limitation of these data are that specialties are not observed, which substantially weakens the first-stage power. Using employment-based HHIs, the F-statistics range from about 6 to 10. 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 A16), although the F-statistics are still higher in the MPIER model.

The first stage results for employment-based and sales-based concentration measures are presented in Appendix Tables A13 and A14, respectively. Although the F-statistics are low, five of the instruments have statistically significant effects on HHIs. Due to the potential bias associated with weak instruments, we estimate all Census-based models using limited information maximum-likelihood (LIML), which is approximately median unbiased with weak instruments.[21] We find that the LIML estimates are very similar to the 2SLS estimates in the second stage (0.019 versus 0.020, see Table 6), suggesting that any potential bias from weak instruments is small despite the missing specialty data.

7.3 The Effect of HHI on Negotiated Prices

The second stage estimates of the effects on negotiated prices are reported in Table 4. Columns 1-3 in the table correspond to the same column numbers in Table 3, where each column uses a different set of instruments. Column 4 shows that the OLS estimate, 0.0002, is very close to zero (although statistically significant), similar to the findings of Dunn and Shapiro (2014) and Baker et al. (2014), whose estimates use, respectively, cross-sectional variation in HHIs, and panel variation but without instrumenting for changes in concentration.

However, the second stage IV estimates suggest that there is substantial endogeneity bias present in the OLS model. We find that a 100 point increase in the HHI causes a 2.4% to 2.8% reduction in average negotiated prices. 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. Our preferred specification, using both first and second lags of each instrument, is presented in column 3, and the estimates imply a 2.5% price reduction per 100 point increase in establishment HHI. For example, for a typical outpatient office visit with an established patient (CPT code 99213), the model predicts that a 100 point increase in HHI would reduce average prices from \$69.60 to \$67.90.[22] The Hansen J-statistic is 18, with a corresponding p-value of 0.16, so we fail to reject the overidentification restriction and the null hypothesis that the instruments are uncorrelated with the error term. We also estimate just-identified versions of the model using each instrument separately, and find similar results for every instrument (See Section 7.4).

The signs of these estimates suggest that the efficiency gains of larger group practices outweigh any effects of larger group sizes 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.

[21] See Angrist and Pischke (2009).

[22] Authors calculation based on 2009 Marketscan data.

In all three IV models, the unexplained variation in prices is about 2% of the total sum of squares.

The results are also consistent with reduced-form estimates of the effects of NCA law changes on prices, presented in Appendix Table A3. In the first lags specification, two of the instruments are statistically significant, and three are significant in the second lags specification. The reduced-form estimates suggest that a one standard deviation increase in the lagged Protectible Interest Index reduces prices by 15 log points, while a one standard deviation increase in the lagged Employer Termination Index increases prices by 18 log points. Second stage IV estimates suggest that price effects are negatively correlated with HHI, and consistent with this result the first stage estimate of the effect of the lagged Protectible Interest Index is significant and positive, while the coefficient on lagged Employer Termination Index is significant and negative. This same pattern of opposing signs in the reduced form and first stage estimates holds for every pair of coefficients that is significant in both models, for both of the lag specifications.

As discussed in Section 7.1, there appears to be substantial heterogeneity in the effects of NCA laws on physicians with different specialties. In Table 5 we present results from subsamples of the data that condition on combinations of metro status and medical specialty. The second stage estimates are statistically significant in both metro and non-metro markets. Consistent with the evidence from the first-stage, we find that the effects are largest in metro counties, where a 100 point increases in HHI causes a 3.1% to 3.4% decline in negotiated prices. In non-metro counties the effect is a decline of 1.1% to 1.3%.

Consistent with evidence from the first-stage models, the second-stage estimates are strongest for primary care physicians and non-surgical specialists. We find that a 100 point increase in HHI reduces prices for primary care physicians by 1.7% to 2.4% overall, but this is largely driven by 2.6% to 3.6% effects in metro markets, while the effects are statistically insignificant in non-metro counties. The same pattern is observed for non-surgical specialties, with a 1.3% price reduction overall, 2.3% to 2.6% price reductions in metro counties, and no effect in non-metro counties. For surgical specialists, however, we find no significant price effects in any model.

Estimates of the effect of firm-level concentration on prices, from the LBD, add some subtlety to the story. Table 6 shows *positive* and significant effects of firm-level HHI on prices using both sales-based and employment-based measures of concentration. The estimates suggest that a 100 point increase in firm-level HHI leads to a 1.9% to 2.0% increase in prices using the sales-based measure (upper panel), or a 1.1% to 1.2% increase in prices using the employment-based measure (lower panel). LIML estimates are nearly identical to the 2SLS estimates, suggesting that there is little bias from weak instruments. OLS estimates are again slightly positive, but very close to zero and economically insignificant. Consistent with the MPIER estimates, effects are larger in metro counties than in non-metro counties. Note that these specifications also control for insurer HHI in the state, but this control variable is not statistically significant and its inclusion in the model does not meaningfully affect the instrument strength or any other parameters of interest. We also estimate our main MPIER HHI specifications including the Census Insurer HHI control, and we find that it does not substantively alter those estimates either.[23]

Taken together, our results from MPIER and Census data suggest that the effects of consolidation

[23]See Appendix Table A15, Table 4, and Section 7.4 for additional discussion of this point.

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 suggests that most of the efficiency gains from larger physician firms comes from increases in practice size *at a given location*, whereas consolidation across locations has smaller efficiency gains, while still affecting bargaining leverage in negotiation, causing a net positive effect on prices.

7.4 Robustness

We conduct many supplemental analyses to ensure that our results are robust to a wide variety of model assumptions and potential data measurement concerns.

Market Structure Measure: The main measure of market concentration we use in our analysis is the Herfindahl-Hirschman Index (HHI), which is the most commonly used measure of market concentration in the literature (See Gaynor et al. 2015). Of course, as discussed in Section 3.2, interpreting estimates from models that measure market structure using HHI as estimates of the elasticity of demand requires the potentially undesirable assumptions that goods are homogeneous and firms engage in Cournot competition. Rather than estimating firm conduct directly, which would be difficult to do without data on patient-level choices, we test the sensitivity of our estimates to these assumptions by replacing the measure of market structure with a variety of alternative measures. We re-estimate the main specifications using the negative log HHI transformation, average establishment size, 4-firm market share, and 8-firm market share. Table 7 shows that the qualitative conclusions are identical for all of these choices of market structure. In the negative log HHI specification, the sign is positive (which is consistent since the measure is negated), and the 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 8 presents estimates of the main specification for all physicians and separately by specialty group 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 choices between hospitals to define markets. We chose HSAs as a plausible upper bound on the size of markets, since patients tend to travel further on average to hospitals than they do for ambulatory physician visits. PCSAs are also similarly defined by the Dartmouth Atlas 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.

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 alter our substantive conclusions.

Sensitivity to Large NCA Law Changes: Figure 2 shows that there is a small number of law changes that are much larger in magnitude than the typical variation observed in the data. Appendix Table A5 presents estimates from the main establishment-level specification 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, to an insurer concerned about the negative consequences of failing to reach an agreement with such a practice, they 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. Comparing columns (1) and (5) in Appendix Table A8 shows that the estimates are very similar, but slightly smaller (2.2% compared to 2.5%,) if we instead calculate HHIs including only physicians in a given specialty, ignoring potential effects of multi-specialty bargaining.

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 A7 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, suggesting that to the extent that matching addresses to establishments results in measurement error, this error is not strongly correlated with the instruments.

Just-Identified IV Estimates: Our main specifications use either seven or fourteen instruments, depending on the lag structure. We also estimate just-identified specifications using each instrument individually. Appendix Table A6 presents estimates of the first and second stage models for each of the 21 specifications. The estimate is negative in all 21 models (ranging from –0.007 to –0.047) and statistically significant at the 0.05 level in 15 of the 21 models. We prefer the overidentified specification because there are plausible mechanisms and evidence suggesting that all seven of the indices affect market structure in some way, as discussed in Section 5.2.

Insurance Market Concentration: Our main specifications do not control for insurer market concentration, which could clearly have important effects on negotiated prices. The reason we exclude insurer HHI from the main specification is that our only source of data on insurer sales comes from Census data, and Census disclosure rules prevent us from estimating all of the specifications and subsamples used in our analyses using Census data. Instead, we show that the inclusion of insurer HHI as a control variable has only modest effects on our main estimates. Appendix Table A15 presents estimates from the main specification, overall and for metro and non-metro counties. The estimates decrease slightly but remain statistically significant and are not significantly different from the baseline estimates in any specification. The overall estimate falls from –0.025 to –0.019, and the estimate for metro counties falls from –0.031 to –0.030. Moreover, the estimated coefficient on insurer HHI is very close to zero (0.00007) and not statistically significant in any model.

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. Of course the inclusion of state fixed effects in all of our specifications removes any average differences between states that might affect both 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 A18 presents results from regressions of several objective measures on the laws, with state and year fixed effects. 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 A19 presents regressions of a variety of views, from GSS survey responses, on the law changes. All specifications are linear probability models in which a positive outcome represents the respondent's agreement with the statement presented in each column. 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 our understanding of 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 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, a major advantage of this database is 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. Specifically, the instruments quantify state-level judicial decisions that alter the enforceability of non-compete agreements between physicians and their practices. We show that these law changes cause abrupt shocks to job separation rates and lead to the formation and destruction of practices. The resulting reorganization is reflected in a variety of market structure measures, including HHIs and average practice size.

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 2.5% per 100 unit increase in HHI. However, when physician *firms* grow larger the opposite is true—a 100 point increase in HHI increases prices by about 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 are more likely to benefit consumers through lower prices.

Although we hope these contributions improve the understanding of an important aspect of healthcare markets, there are some limitations to our study. First, our data contain only market-level average negotiated prices, rather than the full distribution of negotiated prices at the practice level. More complete data on prices could be useful in assessing, for example, the extent to which there are spillover effects of consolidation on other practices in a market, or whether using average prices attenuates estimates due to measurement error. Second, without complete data on patient choices and firm costs, we do not estimate a model of firm conduct directly, and cannot identify some fundamental parameters relevant to understanding competition, such as market power parameters. Finally, we have no available measures of quality in physician markets, which could be important for understanding multidimensional competition between firms. Despite these shortcomings, we hope our results help further the understanding of competition in physician markets and help guide future research in this area.

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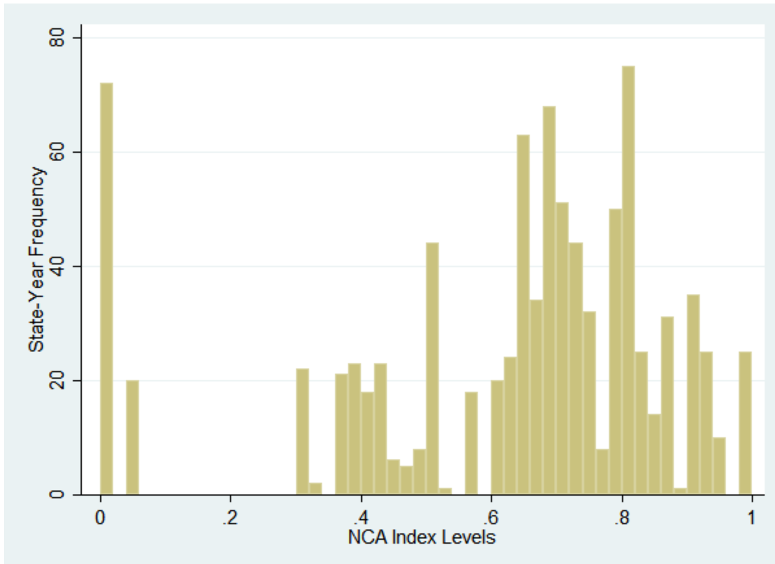
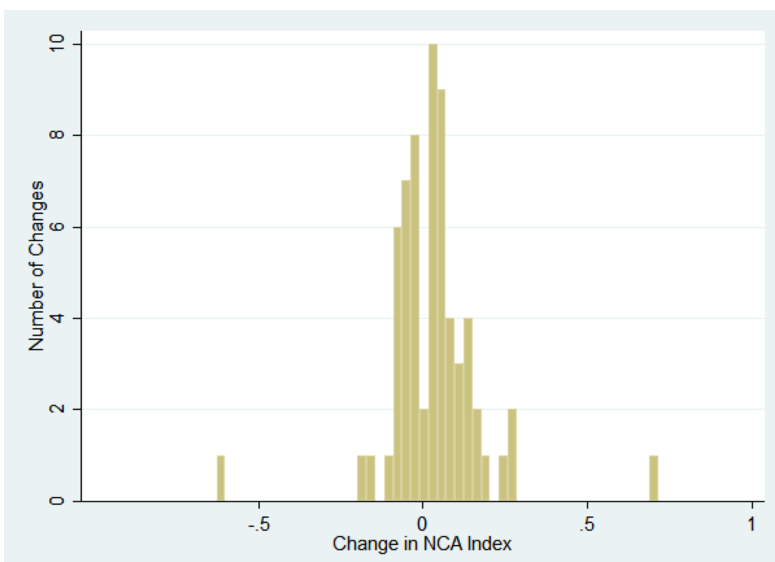
Figure 1: Distribution of NCA Index Levels



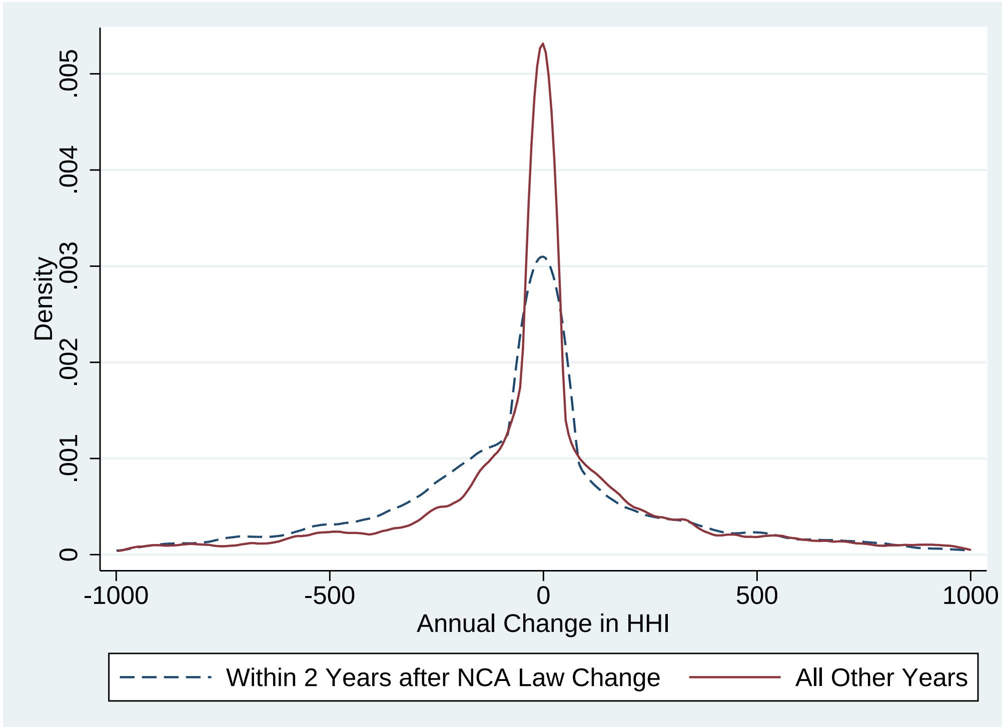
Notes: Data points underlying the histogram are state-year observations of the NCA Index, a weighted sum of the 7 NCA law dimensions. The Index is scaled to range from 0 to 1, where 0 is the least restrictive state-year in the sample and 1 is the most restrictive.

Figure 2: Distribution of NCA Index Changes



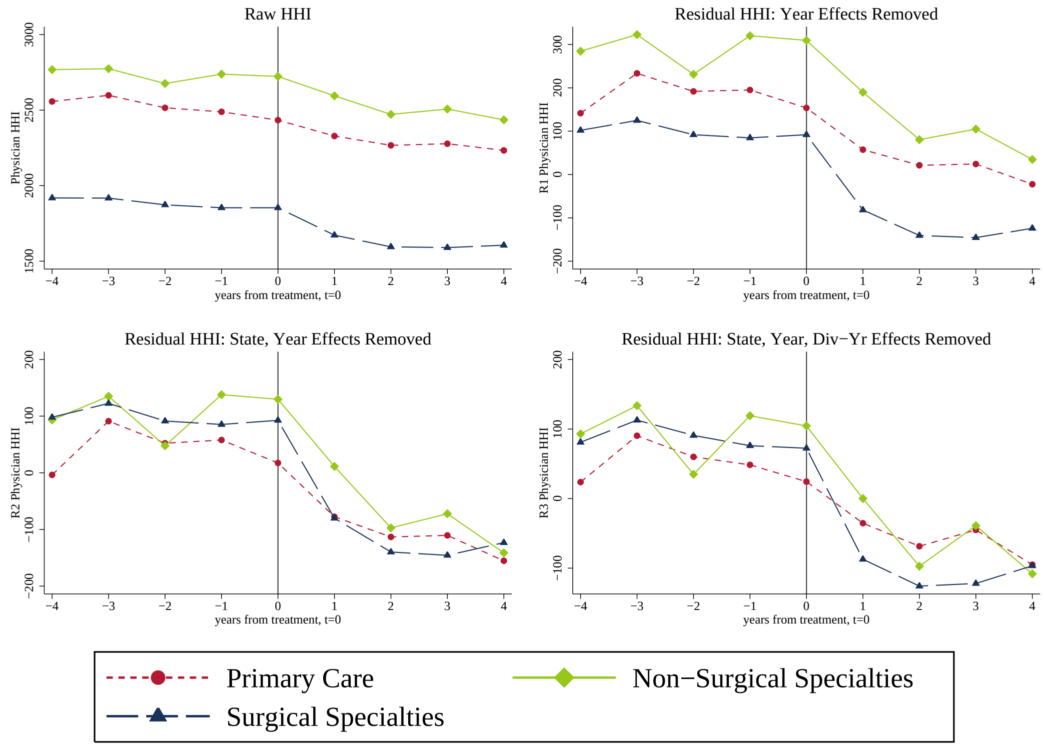
Notes: Data points underlying the histogram are state-year observations of year-to-year changes in the NCA Index, which is a weighted sum of the 7 NCA law dimensions. The Index is scaled to range from 0 to 1, where 0 is the least restrictive state-year in the sample and 1 is the most restrictive. Changes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

Figure 3: Distribution of Annual HHI Changes



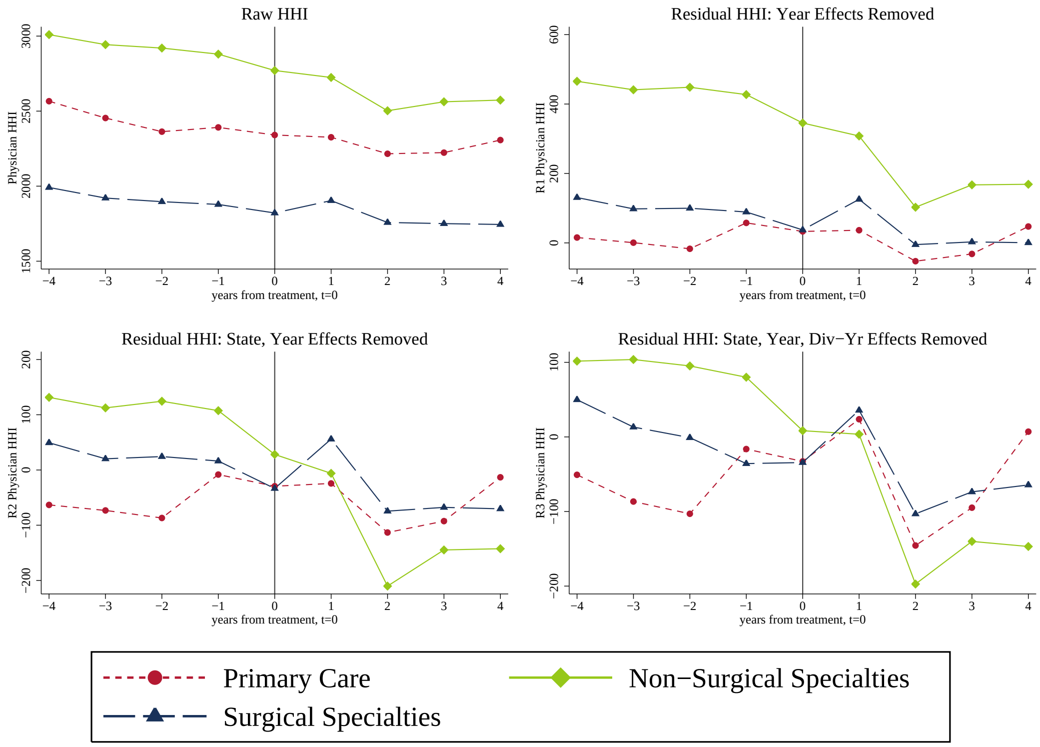
Notes: Distributions are kernel density graphs of the change in annual HHI by CBSA-specialty for specialists. Distributions are truncated at +/− 1000 for display. The p-value of Kolmogorov-Smirnov test of the equality of the full distributions is <0.001.

Figure 4: HHIs Before and After Declines in NCA Index 1



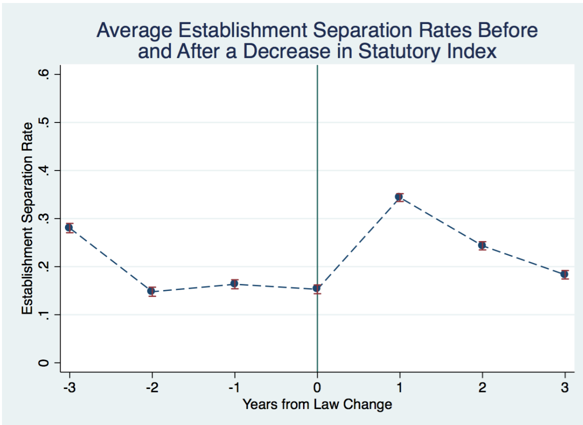
Notes: This figure plots HHIs by medical specialty before and after a decline in NCA Component Index 1, which contains the subset of laws that are positively correlated with HHI. Law changes in all states that experience a decline in this index are synchronized to occur at time 0. The four panels show raw HHIs and HHIs with various fixed effects removed, as noted. Physician HHIs are calculated by county and medical specialty from MPIER data on physician establishment sizes.

Figure 5: HHIs Before and After Increases in NCA Index 2

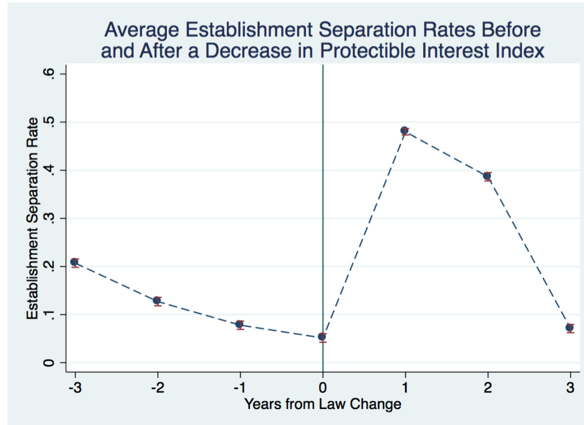


Notes: This figure plots HHIs by medical specialty before and after an increase in NCA Component Index 2, which contains the subset of laws that are negatively correlated with HHI. Law changes in all states that experience an increase in this index are synchronized to occur at time 0. The four panels show raw HHIs and HHIs with various fixed effects removed, as noted. Physician HHIs are calculated by county and medical specialty from MPIER data on physician establishment sizes.

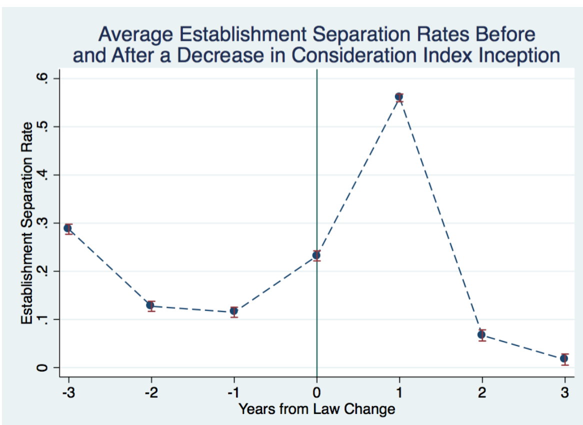
Figure 6: Physician-Establishment Separation Rate Event Studies:
Average Separation Rates Before and After Decrease in Law Indices



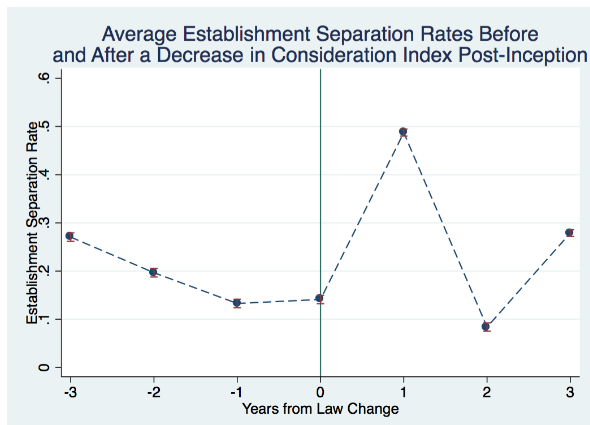
(a) Statutory Index



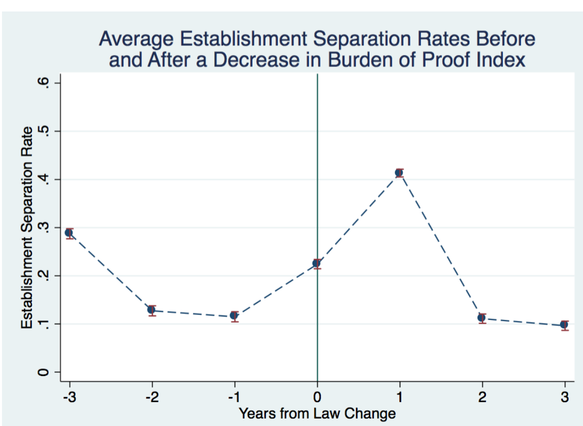
(b) Protectible Interest Index



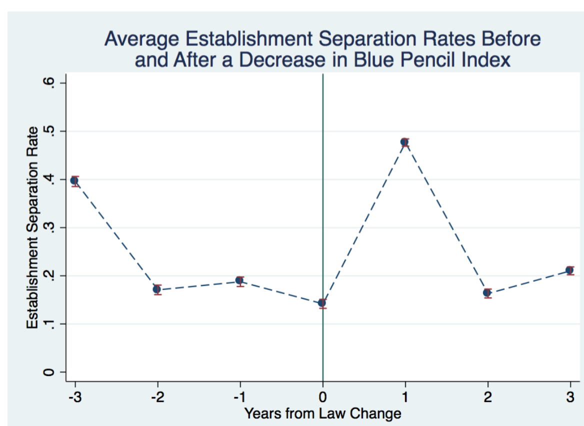
(c) Consideration Index Inception



(d) Consideration Index Post-Inception



(e) Burden of Proof Index



(f) Blue Pencil Index

Notes: Separations of physicians from any establishment the physician is associated are calculated using MPIER data. 95% confidence interval bars are shown around each data point. 'Employer Termination Index' is excluded from the figure because there are no observed decreases in this index during the study period.

Table 1: NCA Laws and Establishment Births and Deaths

Dependent Variable:	$Births_t$	$Deaths_t$	$Births_t$	$Deaths_t$	$Births_t$	$Deaths_t$
	All Counties		Metro Counties		Non-Metro Counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Index$_{t-1}$	–0.606*	–0.730*	–0.994*	–1.415*	0.032	0.247*
	(0.092)	(0.124)	(0.172)	(0.236)	(0.033)	(0.037)
Protectible Interest Index$_{t-1}$	1.264*	1.204*	3.002*	2.477*	0.010	0.153*
	(0.159)	(0.177)	(0.323)	(0.352)	(0.050)	(0.053)
Burden of Proof Index$_{t-1}$	–3.683*	–3.658*	–4.600*	–4.319*	–1.716*	–1.854*
	(0.270)	(0.329)	(0.545)	(0.688)	(0.100)	(0.105)
Consideration Index Inception$_{t-1}$	3.391*	2.042*	4.462*	2.400*	0.497*	–0.372*
	(0.299)	(0.265)	(0.471)	(0.447)	(0.095)	(0.111)
Consideration Index Post-Inception$_{t-1}$	–0.849*	–0.460*	–1.592*	–0.960*	–0.143*	0.039
	(0.093)	(0.074)	(0.189)	(0.156)	(0.032)	(0.027)
Blue Pencil Index$_{t-1}$	0.286*	–0.308*	0.428*	–0.454*	0.282*	–0.065*
	(0.060)	(0.065)	(0.097)	(0.101)	(0.028)	(0.022)
Employer Termination Index$_{t-1}$	–4.679*	–4.539*	–4.017*	–3.694*	0.123	0.741*
	(0.630)	(0.780)	(0.878)	(1.058)	(0.140)	(0.173)
Number of Physicians in County$_t$	0.070*	0.123*	0.066*	0.124*	0.153*	0.037*
	(0.012)	(0.019)	(0.013)	(0.019)	(0.004)	(0.005)
N	599,975	599,975	284,898	284,898	315,077	315,077
R-Sq	0.435	0.340	0.449	0.353	0.320	0.099

Notes: All specifications are OLS regressions of the number of establishment births and deaths (MPIER) on the 7 NCA law indeces, controlling for the aggregate supply of physicians, and including fixed effects for county by medical specialty, and census division by year. Huber-White standard errors reported in parentheses. * indicates significance at the 0.05 level.

Table 2: NCA Laws and Establishment Sizes

Dependent Variable: Log Number of FTE Physicians per Establishment	All Counties (1)	Metro Counties (2)	Non-Metro Counties (3)
Statutory Index$_{t-1}$	–0.140*	–0.247*	–0.011
	(0.048)	(0.048)	(0.060)
Protectible Interest Index$_{t-1}$	–0.178*	–0.234*	–0.215*
	(0.070)	(0.069)	(0.090)
Consideration Index Inception$_{t-1}$	–0.262	–0.094	–0.481*
	(0.146)	(0.195)	(0.119)
Consideration Index Post-Inception$_{t-1}$	0.081	–0.010	0.190
	(0.162)	(0.214)	(0.127)
Burden of Proof Index$_{t-1}$	0.099*	0.136*	0.065
	(0.032)	(0.028)	(0.044)
Blue Pencil Index$_{t-1}$	–0.163*	–0.148*	–0.155*
	(0.030)	(0.049)	(0.056)
Employer Termination Index$_{t-1}$	–0.103	–0.051	–0.316*
	(0.129)	(0.124)	(0.140)
Number of Physicians in County$_t$	0.002*	0.001*	0.029*
	(0.000)	(0.000)	(0.001)
N	379,370	205,899	173,471
R-Sq	0.227	0.241	0.224

Notes: All specifications are OLS regresssions of the log number of FTE physicians per establishment in a county-year on the 7 NCA law indeces, controlling for the aggregate supply of physicians in the county and including 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.

Table 3: 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 $Post\text{-}Inception_{t-1}$	–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 $Post\text{-}Inception_{t-2}$		–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 4: 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)
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 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 5: 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 Tables 3 and 4 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 4. 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 6: OLS and IV Second Stage: Effect of Firm-Based (LBD) Market Concentration on Prices

	Dependent Variable: ln($Price$)				
	OLS	2SLS	LIML	2SLS	2SLS
Sample Counties	All	All	All	Metro	Non-Metro
	(1)	(2)	(3)	(4)	(5)
Firm Sales HHI	0.000*	0.019*	0.020*	0.014	0.009*
	(0.000)	(0.007)	(0.008)	(0.009)	(0.003)
Insurer HHI	–0.0003	0.0003	0.0004	0.0001	0.000008
	(0.0003)	(0.0005)	(0.0006)	(0.0006)	(0.000327)
N	6,899,000	6,899,000	6,899,000	4,449,000	2,450,000
N Clusters	349	349	349	349	325
1st Stage F-Stat		2.494	2.494	2.330	6.605
Firm Employment HHI	0.00018*	0.011*	0.012*	0.015*	–0.002
	(0.00005)	(0.005)	(0.006)	(0.006)	(0.003)
Insurer HHI	–0.0002	–0.0002	–0.0002	–0.0005	–0.0003
	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0004)
N	6,899,000	6,899,000	6,899,000	4,449,000	2,450,000
N Clusters	349	349	349	349	325
1st Stage F-Stat		6.458	6.458	5.807	10.102

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). Price data are from the Truven Health Marketscan database. Physician HHI and insurer HHI 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.

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

	Dependent Variable: $\ln(Price)_t$			
	OLS	IV	IV	IV
Instruments:		Lagged Laws	Twice Lagged Laws	First and Second Lags
Negative Log HHI$_{(t-1)}$	0.004*	0.190*	0.210*	0.283*
	(0.001)	(0.084)	(0.072)	(0.092)
1st Stage AP F-Stat		[80.33]	[88.11]	[2043.69]
Mean Establishment Size$_{(t-1)}$	0.0003*	–0.043*	–0.039*	–0.034*
	(0.0000)	(0.014)	(0.017)	(0.013)
1st Stage AP F-Stat		[245.13]	[38.82]	[652.49]
4-Firm Market Share$_{(t-1)}$	–0.0001	–0.021*	–0.019*	–0.022*
	(0.0001)	(0.007)	(0.005)	(0.006)
1st Stage AP F-Stat		[21.96]	[27.48]	[11.81]
8-Firm Market Share$_{(t-1)}$	–0.0001	–0.030*	–0.025*	–0.029*
	(0.0001)	(0.011)	(0.010)	(0.010)
1st Stage AP F-Stat		[13.96]	[14.64]	[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. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

Table 8: Sensitivity of MPIER IV Estimates to Market Definition

	Dependent Variable: ln($Price$)		
	County	HSA	PCSA
All Specialties HHI$_{(t-1)}$	–0.025*	–0.023*	–0.029*
	(0.005)	(0.008)	(0.008)
1st Stage AP F-Stat	[460.2]	[2119.6]	[270.4]
Primary Care HHI$_{(t-1)}$	–0.017*	–0.018*	–0.024*
	(0.005)	(0.005)	(0.004)
1st Stage AP F-Stat	[542.3]	[63.1]	[122.2]
Non-Surgical Specialists HHI$_{(t-1)}$	–0.013*	–0.016*	–0.029*
	(0.003)	(0.003)	(0.007)
1st Stage AP F-Stat	[98.3]	[77.5]	[98.1]
Surgical Specialists HHI$_{(t-1)}$	–0.005	–0.012*	–0.026*
	(0.005)	(0.004)	(0.005)
1st Stage AP F-Stat	[17.2]	[35.4]	[121.6]

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 4. Standard errors, in parentheses, are clustered by state-year. First-stage AP F-statistics are reported in square parentheses. * indicates significance at the 0.05 level.

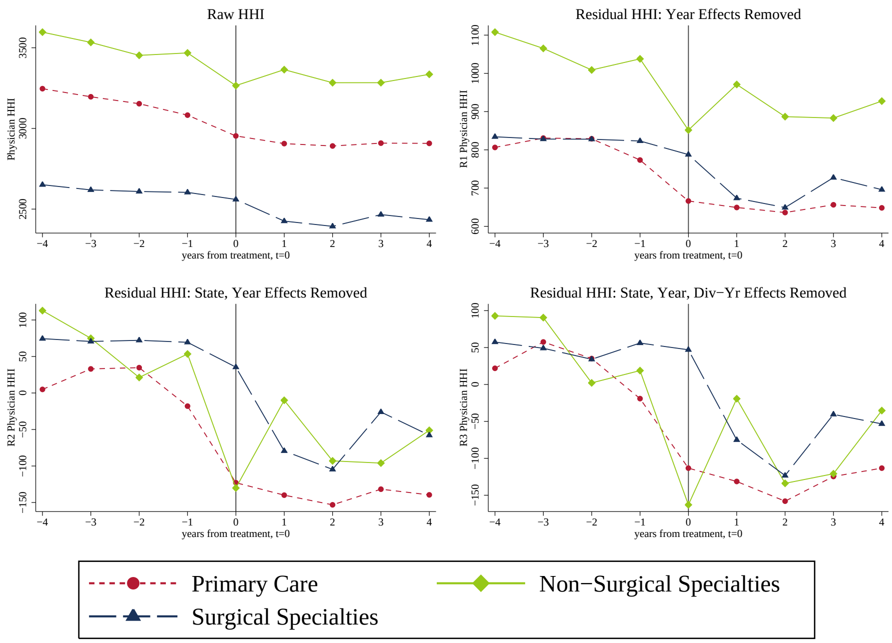
Table 9: NCA Law Changes and Firm Size (LBD)

Dependent Variable:	Number of Employees in Physician Firm		
	All Counties	Metro Counties	Non-Metro Counties
	(1)	(2)	(3)
Statutory Index$_{t-1}$	7.766*	10.825	0.027
	(3.100)	(5.518)	(2.027)
Protectible Interest Index$_{t-1}$	11.083*	25.795*	2.297
	(4.961)	(10.455)	(2.955)
Consideration Index Inception$_{t-1}$	-24.803	-55.347*	-20.955*
	(13.040)	(23.035)	(10.323)
Consideration Index Post-Inception$_{t-1}$	-5.631*	-9.982*	-1.539
	(1.803)	(4.137)	(1.257)
Burden of Proof Index$_{t-1}$	41.029*	77.889*	23.537*
	(14.425)	(23.543)	(10.546)
Blue Pencil Index$_{t-1}$	-2.213	-4.944	2.334
	(2.694)	(3.928)	(1.710)
Employer Termination Index$_{t-1}$	21.018*	42.709*	-2.726
	(5.935)	(12.556)	(3.538)
Number of Physicians in County	0.009*	0.007*	0.161*
	(0.001)	(0.001)	(0.026)
N	25,000	10,000	15,000
N Clusters	328	328	312

Notes: All specifications are OLS regressions of number of employees per physician firm on the 7 NCA law components, controlling for the aggregate supply of physicians in the market and including fixed effects for county and census division by year. Regressions include all states in the U.S., not just those with NCA law changes. Data on physician firm size come from the Census' LBD. Standard errors are clustered by state-year. * indicates significance at the 0.05 level.

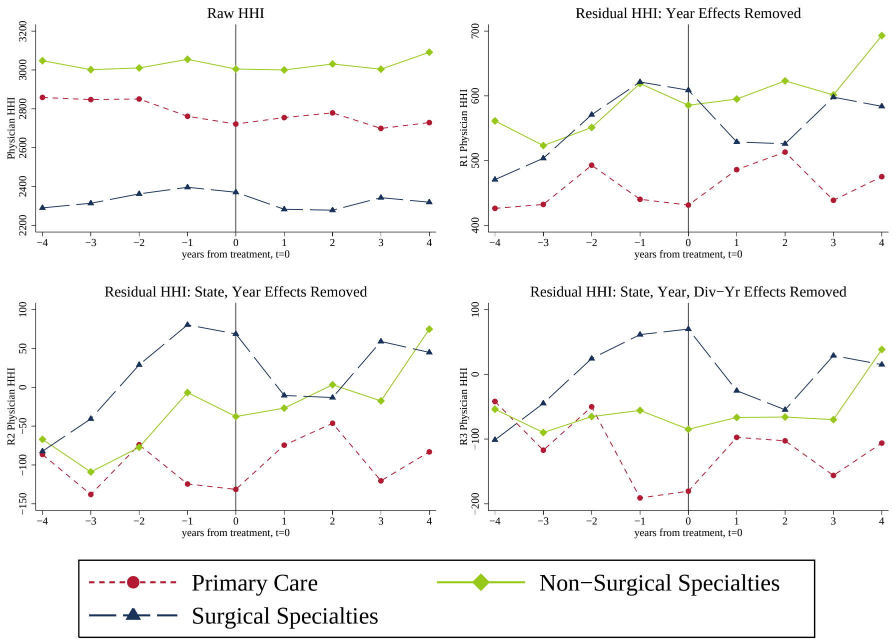
Appendix: For Online Publication

Figure A1: HHIs Before and After Increases in NCA Index 1



Notes: This figure plots HHIs by medical specialty before and after an increase in NCA Component Index 1. Law changes in all states that experience an increase in this index are synchronized to occur at time 0. The four panels show raw HHIs and HHIs with various fixed effects removed, as noted. Physician HHIs are calculated by county and medical specialty from MPIER data on physician establishment sizes.

Figure A2: HHIs Before and After Declines in NCA Index 2



Notes: This figure plots HHIs by medical specialty before and after a decline in NCA Component Index 2. Law changes in all states that experience a decline in this index are synchronized to occur at time 0. The four panels show raw HHIs and HHIs with various fixed effects removed, as noted. Physician HHIs are calculated by county and medical specialty from MPIER data on physician establishment sizes.

Table A1: 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.

Table A2: 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 A3: Reduced Form Estimates: NCA Laws and Prices

	Dependent Variable: ln($Price$)		
	(1)	(2)	(3)
Statutory Index$_{t-1}$	0.008		0.012
	(0.087)		(0.089)
Protectible Interest Index$_{t-1}$	–0.612*		–0.545*
	(0.204)		(0.265)
Consideration Index Inception$_{t-1}$	1.590		1.616
	(2.032)		(1.722)
Consideration Index Post-Inception$_{t-1}$	0.030		0.001
	(0.029)		(0.029)
Burden of Proof Index$_{t-1}$	–1.066		–1.117
	(1.655)		(1.401)
Blue Pencil Index$_{t-1}$	–0.068		0.030
	(0.080)		(0.068)
Employer Termination Index$_{t-1}$	0.616*		0.562
	(0.228)		(0.295)
Statutory Index$_{t-2}$		0.087	0.002
		(0.049)	(0.057)
Protectible Interest Index$_{t-2}$		–0.211	–0.073
		(0.122)	(0.103)
Consideration Index Inception$_{t-2}$		1.553*	1.182*
		(0.138)	(0.148)
Consideration Index Post-Inception$_{t-2}$		0.051*	0.038
		(0.023)	(0.020)
Burden of Proof Index$_{t-2}$		–1.114*	–0.938*
		(0.142)	(0.119)
Blue Pencil Index$_{t-2}$		–0.110	–0.132*
		(0.065)	(0.040)
Employer Termination Index$_{t-2}$		0.156	0.021
		(0.139)	(0.114)
N	3,026,780	3,026,780	3,026,780
N Clusters	121	121	121
R-Sq	0.988	0.988	0.988

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 from 0 to 1, where 1 is the strongest observed measure of the variable in any state and year in the data. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

Table A4: IV Second Stage Sensitivity to Estimator

	Dependent Variable: $\ln(Price)_t$		
Instruments:	First Lags	Second Lags	First and Second 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 A5: Second Stage Estimates Dropping Largest NCA Law Changes

	Dependent Variable: $\ln(Price)_t$			
	IV	IV	IV	OLS
	(1)	(2)	(3)	(4)
HHI_{t-1}	–0.0263*	–0.0212*	–0.0218*	0.0002*
	(0.0052)	(0.0041)	(0.0039)	(0.0000)
N	2,853,469	2,853,469	2,853,469	2,853,469
N Clusters	111	111	111	111
R-Sq	0.98	0.98	0.98	0.82
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 A6: Second-Stage IV Estimates by Law Component

Instruments:	Dependent Variable: ln(*Price*) First Lags	Second Lags	First and Second Lags
Statutory Index	–0.047	–0.032	–0.029*
	(0.061)	(0.020)	(0.015)
	[1.37]	[6.26]	[3.58]
Protectible Interest Index	–0.003	–0.040	–0.019
	(0.051)	(0.029)	(0.011)
	[0.90]	[0.97]	[3.65]
Consideration Index Inception	–0.043*	–0.028*	–0.032*
	(0.011)	(0.007)	(0.007)
	[45.68]	[229.67]	[246.67]
Consideration Index Post-Inception	–0.020*	–0.021*	–0.021*
	(0.009)	(0.009)	(0.008)
	[55.70]	[41.66]	[33.66]
Burden of Proof Index	–0.043*	–0.026*	–0.031*
	(0.011)	(0.007)	(0.006)
	[46.98]	[215.65]	[260.14]
Blue Pencil Index	–0.007	–0.008*	–0.008*
	(0.005)	(0.003)	(0.003)
	[11.05]	[36.07]	[24.31]
Employer Termination Index	–0.024*	–0.022*	–0.024*
	(0.006)	(0.008)	(0.006)
	[52.04]	[21.21]	[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 two columns are all just-identified models. The third 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 F-statistics are shown in brackets. * indicates significance at the 0.05 level.

Table A7: 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 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, as in column (3) of Table 3 (corresponding 2nd stage in Table 4). All standard errors clustered by state-year. * indicates significance at the 0.05 level.

Table A9: First Stage Models, All Specialties

	Dependent Variable: HHI_{t-1}					
	All Counties		Metro Counties		Non-Metro Counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Index$_{t-1}$	0.415	1.800	0.483	1.636	0.875	1.855
	(1.381)	(1.990)	(0.914)	(1.047)	(4.115)	(5.903)
Protectible Interest Index$_{t-1}$	12.159*	8.048*	5.992*	3.513	29.287*	22.463*
	(3.509)	(3.949)	(2.712)	(3.007)	(8.147)	(9.533)
Consideration Index Inception$_{t-1}$	21.706	15.223	13.764	8.943	–215.591	421.905
	(38.567)	(41.591)	(49.841)	(53.563)	(542.991)	(708.209)
Consideration Index Post-Inception$_{t-1}$	–2.461*	–1.268	–2.380*	–1.270*	–2.995*	–1.427
	(0.344)	(0.643)	(0.406)	(0.580)	(0.504)	(0.949)
Burden of Proof Index$_{t-1}$	–20.702	–13.905	–14.414	–9.053	182.757	–357.267
	(30.656)	(33.389)	(39.811)	(42.967)	(483.517)	(613.821)
Blue Pencil Index$_{t-1}$	12.561*	0.986	15.159*	7.579*	5.382	–11.183*
	(3.898)	(2.288)	(2.988)	(1.987)	(7.042)	(4.110)
Employer Termination Index$_{t-1}$	–19.155*	–11.174*	–11.326*	–5.924		
	(4.507)	(4.410)	(3.676)	(3.346)		
Statutory Index$_{t-2}$		–1.851		–1.316		–1.699
		(1.773)		(0.939)		(4.888)
Protectible Interest Index$_{t-2}$		2.920*		1.684*		5.770*
		(1.130)		(0.795)		(2.587)
Consideration Index Inception$_{t-2}$		0.357		–0.147		–569.692
		(1.148)		(1.228)		(613.983)
Consideration Index Post-Inception$_{t-2}$		–1.548*		–1.500*		–1.926*
		(0.614)		(0.588)		(0.854)
Burden of Proof Index$_{t-2}$		–3.696*		–3.043*		470.102
		(0.821)		(0.896)		(471.594)
Blue Pencil Index$_{t-2}$		16.185*		10.762*		22.315*
		(2.294)		(1.826)		(4.830)
Employer Termination Index$_{t-2}$		–1.620		0.665		
		(1.254)		(0.885)		
N	3,026,780	3,026,780	2,077,627	2,077,627	949,153	949,153
N Clusters	121	121	121	121	111	111
R-Sq	0.745	0.745	0.724	0.724	0.813	0.813
AP F-Stat	86.85	460.22	54.72	364.26	15.38	15.08

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 from 0 to 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 A10: First Stage Models, Primary Care Physicians

	Dependent Variable: HHI_{t-1}					
	All Counties		Metro Counties		Non-Metro Counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Index$_{t-1}$	–1.275	–0.495	–5.155*	–2.224	11.490	8.697
	(2.008)	(2.306)	(2.034)	(2.060)	(7.563)	(9.290)
Protectible Interest Index$_{t-1}$	14.868*	15.677*	4.023	3.515	40.115*	37.587*
	(3.322)	(4.500)	(2.910)	(3.450)	(9.348)	(11.821)
Consideration Index Inception$_{t-1}$	22.492	17.068	22.156	18.276	63.539	–99.413
	(71.854)	(76.091)	(45.354)	(48.584)	(241.497)	(432.033)
Consideration Index Post-Inception$_{t-1}$	–2.960*	–3.435*	–1.948*	–1.682*	–4.260*	–5.740*
	(0.558)	(0.557)	(0.533)	(0.410)	(1.099)	(1.046)
Burden of Proof Index$_{t-1}$	–21.263	–15.097	–20.995	–16.450	–58.894	111.883
	(57.384)	(60.986)	(36.261)	(38.982)	(197.283)	(393.585)
Blue Pencil Index$_{t-1}$	16.428*	–6.901	9.650*	–3.130	37.221*	–3.732
	(6.455)	(3.637)	(4.706)	(4.621)	(15.167)	(9.810)
Employer Termination Index$_{t-1}$	–23.678*	–20.222*	–9.376*	–6.173		
	(4.649)	(5.036)	(3.969)	(3.845)		
Statutory Index$_{t-2}$		–0.449		–2.673*		3.304
		(2.017)		(1.343)		(6.641)
Protectible Interest Index$_{t-2}$		–1.713		–1.706		4.881
		(2.240)		(1.186)		(7.161)
Consideration Index Inception$_{t-2}$		7.575*		7.498*		126.048
		(3.163)		(2.135)		(323.119)
Consideration Index Post-Inception$_{t-2}$		0.555		–0.492		2.233*
		(0.386)		(0.512)		(0.849)
Burden of Proof Index$_{t-2}$		–10.283*		–9.408*		–119.336
		(2.267)		(1.615)		(254.437)
Blue Pencil Index$_{t-2}$		33.752*		18.742*		57.502*
		(4.048)		(4.885)		(12.000)
Employer Termination Index$_{t-2}$		3.708		5.078*		
		(2.504)		(1.336)		
N	473,033	473,033	306,449	306,449	166,584	166,584
N Clusters	121	121	121	121	111	111
R-Sq	0.844	0.844	0.787	0.787	0.905	0.905
AP F-Stat	47.17	542.30	22.76	458.01	14.94	12.24

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Sample includes primary care MDs, Internal Medicine, Family Practice, Geriatric Medicine, and Pediatric specialists. 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. 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 A11: First Stage Models, Non-Surgical Specialists

	Dependent Variable: HHI_{t-1}					
	All Counties		Metro Counties		Non-Metro Counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Index$_{t-1}$	5.900*	3.580	5.740*	1.268	13.850	15.727
	(2.816)	(4.492)	(2.696)	(3.042)	(15.842)	(18.874)
Protectible Interest Index$_{t-1}$	4.125	3.901	11.163	13.646	–12.612	–14.591
	(6.921)	(7.439)	(6.279)	(7.091)	(15.230)	(15.868)
Consideration Index Inception$_{t-1}$	–9.220	–17.889	1.723	–9.457	–128.942	101.306
	(69.229)	(66.676)	(59.257)	(65.623)	(601.792)	(504.853)
Consideration Index Post-Inception$_{t-1}$	–9.704*	–6.707*	–6.089*	–4.729*	–16.676*	–12.251*
	(0.777)	(1.894)	(0.501)	(0.517)	(2.004)	(5.316)
Burden of Proof Index$_{t-1}$	2.745	12.448	–6.748	5.455	99.640	–138.911
	(55.105)	(53.947)	(47.420)	(53.015)	(479.350)	(385.828)
Blue Pencil Index$_{t-1}$	–14.945*	–19.174*	6.433	5.288*	–73.125*	–88.007*
	(3.740)	(3.459)	(3.925)	(2.447)	(6.661)	(7.755)
Employer Termination Index$_{t-1}$	–9.999	–2.826	–18.855*	–13.536		
	(8.447)	(8.421)	(8.113)	(7.966)		
Statutory Index$_{t-2}$		3.041		5.290*		–1.076
		(4.215)		(2.359)		(11.909)
Protectible Interest Index$_{t-2}$		3.022		1.966		1.759
		(3.243)		(2.228)		(11.875)
Consideration Index Inception$_{t-2}$		–5.441		–2.060		–419.933
		(5.191)		(4.505)		(393.291)
Consideration Index Post-Inception$_{t-2}$		–4.019		–1.809*		–6.009
		(2.112)		(0.453)		(5.558)
Burden of Proof Index$_{t-2}$		–1.058		–4.984		369.487
		(3.991)		(3.695)		(312.851)
Blue Pencil Index$_{t-2}$		5.864		1.649		20.253*
		(5.502)		(5.941)		(8.861)
Employer Termination Index$_{t-2}$		–4.280		–2.346		
		(3.615)		(2.495)		
N	300,990	300,990	234,402	234,402	66,588	66,588
N Clusters	121	121	121	121	111	111
R-Sq	0.803	0.803	0.765	0.765	0.875	0.875
AP F-Stat	39.23	98.25	84.39	277.84	31.21	21.79

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Sample includes specialists in Proctology, Urology, Dermatology, Cardiovascular Dis/Cardiology, Neurology, Gastroenterology, and Hematology. 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. 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 A12: First Stage Models, Surgical Specialists

	Dependent Variable: HHI_{t-1}					
	All Counties		Metro Counties		Non-Metro Counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Index$_{t-1}$	–0.590	0.182	3.598	4.128	–7.380	–4.541
	(2.818)	(2.929)	(2.630)	(3.728)	(6.399)	(6.759)
Protectible Interest Index$_{t-1}$	–9.360	–15.258	–5.169	–10.877	–6.161	–7.993
	(6.853)	(8.479)	(5.818)	(7.030)	(11.879)	(13.428)
Consideration Index Inception$_{t-1}$	–8.505	–9.411	7.430	3.577	–532.191	–241.680
	(45.493)	(41.100)	(40.781)	(36.908)	(947.748)	(836.785)
Consideration Index Post-Inception$_{t-1}$	–5.914*	–1.454	–6.742*	–4.583*	–4.067	4.489
	(1.692)	(2.404)	(1.090)	(1.415)	(3.461)	(5.203)
Burden of Proof Index$_{t-1}$	4.201	5.244	–9.861	–5.468	453.414	220.514
	(36.404)	(33.076)	(32.543)	(29.584)	(732.722)	(624.435)
Blue Pencil Index$_{t-1}$	–2.501	–22.238*	–7.327	–20.000*	4.313	–26.533*
	(5.262)	(3.733)	(4.384)	(2.846)	(9.309)	(11.333)
Employer Termination Index$_{t-1}$	6.924	12.539	2.598	9.587		
	(7.724)	(9.514)	(6.846)	(7.938)		
Statutory Index$_{t-2}$		–0.551		–1.243		–0.372
		(2.482)		(3.024)		(7.100)
Protectible Interest Index$_{t-2}$		6.437		5.546		1.748
		(5.511)		(3.453)		(11.774)
Consideration Index Inception$_{t-2}$		6.801		6.301*		–448.260
		(3.597)		(1.883)		(671.876)
Consideration Index Post-Inception$_{t-2}$		–5.904*		–2.750		–11.687*
		(2.465)		(1.555)		(5.080)
Burden of Proof Index$_{t-2}$		–6.394*		–8.019*		357.391
		(2.615)		(1.327)		(528.154)
Blue Pencil Index$_{t-2}$		28.377*		17.939*		44.392*
		(3.822)		(3.234)		(13.332)
Employer Termination Index$_{t-2}$		–3.092		–0.408		
		(6.212)		(3.985)		
N	272,913	272,913	191,790	191,790	81,123	81,123
N Clusters	121	121	121	121	111	111
R-Sq	0.778	0.778	0.739	0.739	0.849	0.849
AP F-Stat	8.32	17.23	23.14	55.03	1.08	2.39

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Sample includes specialists in General Surgery, Neurological Surgery, Orthopaedic Surgery, Thoracic Surgery, Anesthesiology, and Radiology. 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. 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 A13: IV First Stage: Effect of NCA Laws on Firm Employment-Based HHI (LBD)

	Dependent Variable: HHI_{t-1}			
	2SLS	LIML	2SLS	2SLS
	All	All	Metro	Non-Metro
	(1)	(2)	(3)	(4)
Statutory $Index_{t-1}$	–3.298	–3.298	–0.485	–8.224*
	(2.268)	(2.268)	(1.901)	(3.577)
Consideration Index $Post\text{-}Inception_{t-1}$	1.524*	1.524*	2.707*	–1.524*
	(0.465)	(0.465)	(0.514)	(0.751)
Blue Pencil $Index_{t-1}$	–3.895	–3.895	–8.115*	–1.549
	(4.118)	(4.118)	(3.864)	(5.523)
Statutory $Index_{t-2}$	–0.591	–0.591	–1.428	1.816
	(1.833)	(1.833)	(1.605)	(2.503)
Protectible Interest $Index_{t-2}$	0.418	0.418	–0.245	0.974
	(1.660)	(1.660)	(1.399)	(2.952)
Blue Pencil $Index_{t-2}$	12.996*	12.996*	12.428*	19.008*
	(4.295)	(4.295)	(3.972)	(5.645)
Employer Termination $Index_{t-2}$	–1.259	–1.259	0.709	–6.756*
	(3.489)	(3.489)	(3.735)	(3.247)
Insurer HHI	0.007	0.007	0.022*	–0.016
	(0.006)	(0.006)	(0.007)	(0.012)
N	6,899,000	6,899,000	4,449,000	2,450,000
N Clusters	349	349	349	325
F-Stat	6.458	6.458	5.807	10.102

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. These are the first-stage results that correspond to the second stage results presented in columns 2-5 of Table 6, panel 2. Firm-level market concentration (HHI) is based on employment from the LBD. Regressions include all states, not just those with NCA law changes (for ease of Census disclosure). 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. Physician and Insurer 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. * indicates significance at the 0.05 level.

Table A14: IV First Stage: Effect of NCA Laws on Firm Sales-Based HHI (LBD and SSEL)

	Dependent Variable: HHI_{t-1}			
	2SLS	LIML	2SLS	2SLS
	All	All	Metro	Non-Metro
	(1)	(2)	(3)	(4)
Statutory $Index_{t-1}$	–0.636	–0.636	–0.051	–3.109
	(3.471)	(3.471)	(3.950)	(5.314)
Consideration Index $Post\text{-}Inception_{t-1}$	2.559*	2.559*	1.899	3.223*
	(1.041)	(1.041)	(1.210)	(1.588)
Blue Pencil $Index_{t-1}$	–6.061	–6.061	–8.380	4.566
	(4.944)	(4.944)	(5.297)	(7.528)
Statutory $Index_{t-2}$	–3.358	–3.358	–1.417	–3.836
	(3.116)	(3.116)	(2.874)	(4.639)
Protectible Interest $Index_{t-2}$	–5.692*	–5.692*	–2.095	–10.474*
	(2.713)	(2.713)	(1.908)	(4.967)
Blue Pencil $Index_{t-2}$	7.724	7.724	0.272	18.962*
	(4.736)	(4.736)	(4.802)	(6.580)
Employer Termination $Index_{t-2}$	5.835*	5.835*	3.658*	10.600
	(2.875)	(2.875)	(1.759)	(8.690)
Insurer HHI	–0.027	–0.027	–0.028	–0.024
	(0.019)	(0.019)	(0.028)	(0.022)
N	6,899,000	6,899,000	4,449,000	2,450,000
N Clusters	349	349	349	325
AP F-Stat	2.494	2.494	2.330	6.605

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. These are the first-stage results that correspond to the second stage results presented in columns 2-5 of Table 6, panel 1. Firm-level market concentration (HHI) is based on sales from the SSEL of the US Census (linked to the LBD). 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. Physician and Insurer 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. * indicates significance at the 0.05 level.

Table A15: OLS and IV Second Stage: Effects of MPIER HHI on Prices with Census Insurer HHI Control

	Dependent Variable: ln($Price$)			
	OLS	2SLS	2SLS	2SLS
Sample Counties	All	All	Metro	Non-Metro
	(1)	(2)	(3)	(4)
HHI_{t-1}	0.0001	–0.0187*	–0.0301*	–0.0053*
	(0.0000)	(0.0058)	(0.0065)	(0.0039)
Insurer HHI	0.00003	0.00007	0.00009	0.00008
	(0.00002)	(0.00003)	(0.00006)	(0.00003)
N	3,227,000	3,227,000	2,208,000	1,019,000
N Clusters	133	133	133	122
1st Stage F-Stat		313.18	125.61	16.93

Notes: All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. Only states with NCA law changes are included in the regressions. Physician HHI is calculated from establishment sizes in MPIER data, provided by CMS. Insurer HHI is a sales-based measure from the Census' LBD and SSEL. Physician and insurer 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 clustered by state-year. * indicates significance at the 0.05 level.

Table A16: IV Second Stage Estimates: MPIER HHIs, Markets defined by county only

	Dependent Variable: ln($Price$)			
Instruments:	IV First Lags	IV Second Lags	IV First and Second Lags	OLS
$\mathrm{HHI}_{(t-1)}$	–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 A17: NCA Laws and Aggregate Physician Supply

Dependent Variable: Log Count of Physicians in County, by Specialty	All Counties (1)	Metro Counties (2)	Non-Metro Counties (3)
Statutory Index	–0.114	–0.194*	–0.010
	(0.061)	(0.072)	(0.067)
Protectible Interest Index	–0.318*	–0.337*	–0.280*
	(0.087)	(0.087)	(0.104)
Consideration Index Inception	0.250*	0.392*	0.143
	(0.115)	(0.136)	(0.121)
Consideration Index Post-Inception	0.115*	0.111*	0.116*
	(0.020)	(0.018)	(0.027)
Burden of Proof Index	–0.384*	–0.490*	–0.303*
	(0.111)	(0.127)	(0.116)
Blue Pencil Index	–0.046	0.046	–0.061
	(0.102)	(0.201)	(0.060)
Employer Termination Index	–0.143	–0.098	–0.282
	(0.194)	(0.173)	(0.226)
Log Population	0.470*	0.468*	0.367*
	(0.036)	(0.050)	(0.047)
Log Per Capita Income	0.137*	0.055	0.122*
	(0.033)	(0.045)	(0.039)
N	593,244	304,456	288,788

Notes: Physician counts by county are calculated from MPIER data. All specifications include fixed effects for county, census division by year, procedure code (CPT), physician specialty, and facility type. All standard errors are clustered by state-year. * indicates significance at the 0.05 level.

Table A18: 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 A19: Correlation of Law Changes with Political and Economic Views in the GSS

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

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