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Tort Reform and Physician Labor Supply

A Review of the Evidence

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Tort Reform and Physician Labor Supply: A Review of the Evidence

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ABSTRACT

There is a large empirical literature examining the relationship between medical liability reform and the supply of physician services. Despite the general consensus that malpractice reform leads to an increase in physician supply, usually targeted amongst a subset of physicians, debates rage at the state level over the effectiveness of any given reform. This paper reviews the evidence on the relationship between tort reform and physician supply and assess the implications for any given state. Although our difference in difference methodology prevents drawing conclusions about the impact of reforms on overall physician supply, we find that noneconomic damage caps increase the supply of physicians in high risk specialties. However, these effects, even for the high risk specialties, vary significantly across states. It is unclear whether these differences represent heterogeneous treatment effects across states, or simply random error in the estimates. New approaches are needed to estimating state-specific effects of tort reform to have the most impact on local policy debates.

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I. Introduction

There is perhaps no other area of tort law that has aroused as much political interest and action as medical malpractice. State policymakers regularly debate and frequently change the rules under which those who feel they have been injured by a physician's negligent treatment can seek redress for their claim. These efforts to reform the medical malpractice liability system, commonly referred to as "tort reform," are constantly at the center of controversy. This is in no small part because two politically important groups, physicians and trial attorneys, are on opposite sides of the issue. The public and their representatives are pulled by the competing claims that changes to the rules governing the medical malpractice system harm injured patients who lose their "access to justice" versus doctors and their representatives who argue that such changes are necessary to insure "access to health care."

Historically, major reform efforts have come in periodic waves following instability in the medical malpractice insurance market (Studdert, Mello and Brennan, 2004). The last such reform wave came in the early 2000s. Events came to a head from 2002-2005 in a number of states. In West Virginia nearly 40 surgeons walked off the job to protest the cost of malpractice insurance and the AMA declared 19 states to be in "full blown medical liability crisis."[1] (Anderson, 2005) In response many states adopted reforms in the early 2000s, including 8 states that adopted caps on noneconomic (or "pain and suffering") damages.[2]

In more recent years, these reforms have since come under attack in several states. There have been efforts attempting to roll back changes enacted earlier in the decade and even years before, often through constitutional challenges as opposed to legislative action. An argument that

[1] In the early 2000s AMA President Donald J. Palmisano repeatedly made the link between physician labor supply and tort reform. For example, "In the 19 crisis states, physicians are taking early retirement, or abandoning high-risk services, because they cannot afford or find liability insurance." (http://www.prnewswire.com/news-releases/ama-supports-presidents-call-for-liability-reform-59028132.html)

[2] The states which enacted noneconomic damage caps are Florida (2003), Georgia (2005), Mississippi (2003), Nevada (2003), Ohio (2003), Oklahoma (2004), Texas (2004), and West Virginia (2003). Illinois enacted a noneconomic damage cap in 2006 although the cap did not take effect.

is frequently made is that the reforms have reduced litigation but have not delivered on their promised benefits. Specifically, it is common for opponents of the reforms to argue that they have not controlled cost nor have they had any effect on access to physician services. As these reforms are adopted and implemented at the state level, those on both sides of the debate tend to examine the evidence in terms of a specific state. For example, Texas Governor Rick Perry argued that "… 21,000 more physicians [are] practicing medicine in Texas because they know they can do what they love and not be sued."[3] Statements arguing that there was no change in Texas are frequently made by opponents.[4]

In contrast to this state-specific focus, most of the existing academic literature examines the experience of all states adopting reforms relative to those that do not (or that do, but at different times). One of the most debated, and most studied, of the potential effects of tort reforms is whether they increase the number of physicians practicing in states who adopt. The findings are generally consistent with an increase in the number of physicians in states that adopt reforms for high risk specialties. We summarize the key findings from the literature and the approach used in Table 1. Kessler, Sage and Becker (2005) found an overall effect of a 2.4% increase in physician supply associated with what they label "direct" tort reforms. Encinosa and Helliger (2005) found a 2.2% increase in the number of doctors practicing in the state adopting non-economic caps, although the effects largely occurred three or more years after enactment. Klick and Stratmann (2007) found a 3.9 to 6.6% increase in physician supply in high-risk specialties when measured against a low risk control group, compared to a .8 to 2.9% increase for the same group without the control group. Matsa (2007) found a 4 to 7% increase in the supply of physicians to rural counties, but a negative effect overall. Baicker and Chandra (2005)

[3] This quote was made Wednesday, August 17th, 2011 in a speech at "Politics and Eggs" in Bedford, N.H.
[4] For example the American Association for Justice "The most frequently echoed myth concerning medical negligence is the notion that doctors are fleeing states and retiring early, creating physician shortages."

found a negative elasticity of labor supply with respect to increases in premiums and payments but only for OB-GYN, internal medicine and surgical specialties and physicians over 55 although the results are inconsistent and not particularly robust.

Despite the breadth of the literature, there are two key limitations of these articles from the standpoint of policy analysis. First, while the effects all point to an increase in the number of physicians in an area in response to the adoption of liability reform, the effects are weak in aggregate and tend to suggest that they are centered in a subset of physicians (e.g., physicians in high risk specialties, older physicians or rural areas). Moreover, the articles typically do not address the experience of specific states. This is unsatisfying to many state policymakers and stakeholders, who are primarily concerned with what is happening in "my state." Moreover, most of the existing multi-state studies use data series ending in the early 2000s, immediately before the latest set of states adopted reforms. Thus, the academic literature has not provided conclusive evidence on the impact of tort reform on the supply of physician services sufficiently to settle debates such as the recent exchange over the effect of reforms in Texas.

In this paper, we re-examine the evidence on the relationship between malpractice liability reform and physician supply. Our goals are threefold: we examine whether the standard difference-in-differences approach provides a useful means for estimating the impact of a reform in a particular state, we study whether the effect of reform appears overall or whether it is focused in particular specialties, and we assess whether the relationship between the adoption of reform and physician supply is consistent with previous work when we expand the sample to include the most recent reforms. We place particular emphasis on Klick and Stratman (2007), whose methodology we follow closely in this paper. Moreover we focus on the effects of one particular reform, noneconomic damage caps, because this is the most common reform among the recent cohort of legal changes and is the most hotly debated.[5]

In their paper, Klick and Stratman (2007) emphasize that any examination of the physician labor market must account for two potentially confounding factors. The first is that there are strong underlying trends in state physician labor markets, and these trends are likely to be highly state-specific, given that medical licenses are governed at the state level. This necessitates a control group to identify the causal effect of the policies. Given the particulars of the market for physicians compared to the rest of the labor market, Klick and Stratman (2007) argue that the best control group for physicians is other physicians who are less likely to be attracted by the reforms. In particular, they argue that comparing physicians with a low liability risk—who therefore should care less about the presence of reforms—to physicians in specialties with a higher risk allows for the identification of the *differential* effect of the policy on physicians in higher risk specialties. While this approach does not necessarily provide much information about the impact of reform on the overall market, under the right assumptions it does identify the effect for those higher risk specialties.

In our analysis, we examine the effect of damage caps on the number of total physicians in a state and find little evidence of an increase in the number of physicians. If anything, we find that the number of physicians is negatively associated with caps, although this appears to be most likely due to preexisting trends in the states which passed tort reform and not caused by the reforms themselves. However, when we compare the differential impact of reforms on high- versus low-risk specialties in states that adopt to those that do not using a "difference-in-difference-in-differences" strategy, we find that the adoption of noneconomic damage caps leads

[5] We estimate the impact of other reforms on physician supply in our regressions, but do not discuss them in this manuscript. Full results are available from the authors on request.

to a 1.5% to 6.6% increase in the number of physicians in high risk specialties (with the variation coming in how we define high risk specialties). These findings are consistent with Klick and Stratman's analysis from an earlier time period (1980-2002).

To put this in some perspective consider there are 54 doctors per 100,000 people practicing in Klick Stratman's high risk specialties. Thus our estimate of 6.6% would result in about 3.6 per 100,000 more physicians practicing in one of the Klick and Stratman high risk specialties per year.[6] It is notoriously difficult to translate the number of doctors into patient encounters, but according to the 2003-2004 National Ambulatory Medical Care Survey doctors in these specialties had an average of 96 consultations per week (Hing and Burt, 2007).[7] This would translate into just over 344 additional total consultations per week.

In addition to the specialty-specific analysis, we explore the extent to which these estimates are consistent across states. For example, how closely did the change in the number of obstetricians per capita in West Virginia after the state adopted a noneconomic damage cap in 2004 mirror the national estimates of the relationship between tort reform and the supply of obstetricians. Our results suggest that the estimated impact of noneconomic damage caps, even on the high-risk specialties, varies widely across states. That is, the estimated effect of noneconomic caps on the supply of physicians in high-risk specialties appears to vary substantially according to the specific state that adopts. Unfortunately, it is difficult to determine *ex ante* whether this is because states actually had truly different experiences from reform, or if these differences simply reflect chance or some other, observed confounder. Given this, the state-specific estimates do not appear particularly informative. The national estimates still

[6] That is for emergency medicine (8.35*0.066=0.551) + internal medicine (2.08) + neurological surgery (0.115) + thoracic surgery (0.007) and OBGYN (0.748).
[7] See Table 9 from Hing and Burt (2007), which includes office visits, hospital visits and telephone and internet consultations. Note that we assumed that emergency medicine and thoracic surgery had 88.4 consolations per week, which is almost surely too low for both specialties.

provide our best estimate of the impact any given state should expect to experience after adopting a reform. Nevertheless, a retrospective identification of the "true" effect of a given reform in a given state remains an elusive goal. Until we better understand the heterogeneity in treatment effects across states, these policy debates are likely to continue.

The next section of this paper describes in more detail the different approaches that have been used in the literature on tort reform and physician supply. We then describe the data and methods that we use to produce our estimates. Section IV describes our results, both nationally and across states. We conclude with a discussion of the policy implications of our findings, and offer some suggestions for future research.

II. Policy Evaluation and the Existing Literature

There is a vast literature studying the impact of the threat of medical malpractice liability on different aspects of the health care system. With a few exceptions, the vast majority of these examine the impact of state legislative reforms on outcomes such as health care costs, health outcomes and the supply of physicians to the state.[8] There is a large literature in economics that discusses the conditions under which we can obtain consistent estimates of the impact of a policy on some outcome of interest.[9] In this section we describe how this literature relates to the estimation of the impact of noneconomic damage caps on physician labor supply.

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[8] For examples of studies of the effect of tort reform on outcomes other than physician supply, see Kessler and McClellan (1996), Kessler and McClellan (2001a,b), Currie and MacLeod (2004), Avraham et al. (2010) and others. Some exceptions to this approach include recent work by Frakes, who studies the impact of changes in national standards on similar outcomes (see Frakes, 2012; 2013). Lakdawalla and Seabury (2012) study the impact of malpractice liability on health care costs, but use variation in local jury verdicts as opposed to reform to measure liability risk. And Seig (2000) parameterized an extensive form game to examine the potential impact of malpractice reform on many different aspects of physician behavior.

[9] The treatment effects literature is enormous and we make no attempt to fully summarize it here. Some particularly influential papers include Rubin (1974), Rosenbaum and Rubin (1983), Heckman and Robb (1984), Heckman (1992, 1997), Imbens and Angrist (1994), and many others. For a useful textbook treatment see Woolridge (2002).

Use P_{st} to denote the number of physicians per capita in state s in time t and suppose that C_{st} is equal to 1 if state s has a noneconomic damage cap in place in time t and 0 otherwise. The question of most interest to policymakers is the effect of adopting a reform in their state. If we use θ to denote the treatment effect, then we can define the effect of interest as:

$$(1) \qquad \theta_{st} = P_{st}(C_{st} = 1) - P_{st}(C_{st} = 0)$$

Of course, this is impossible to observe in practice because it relies on a counterfactual; we cannot observe a state with and without a policy at the same point in time.

Instead of observing the treatment effect in Equation 1, we estimate a treatment effect $\hat{\theta}_{st}$ based on the expected value of the outcome with and without a policy. That is, we estimate:

$$(2) \qquad \hat{\theta}_{st} = E[P_{st}|C_{st} = 1] - E[P_{st}|C_{st} = 0]$$

For notational simplicity, assume that there are two time periods: before adoption ($t = 0$) and after adoption ($t = 1$). Additionally, we assume there are two states, the adopting state a and the non-adopting state n. In what follows, we review the different approaches used in the literature to estimate $\hat{\theta}_{st}$ in the context of tort reform and physician labor supply, and highlight some of the assumptions and limitations associated with each.

2.1 The Time Series Approach

The time series approach compares outcomes in the adopting state before and after adoption. Using our notation, the time series estimator $\hat{\theta}^{ts}$ is defined simply as:

$$(3) \qquad \hat{\theta}^{ts} = P_{a1} - P_{a0}$$

Despite the obvious problems with this approach, namely the lack of ability to control for other, confounding sources of variation, it is quite attractive in policy debates. An interested party need only find evidence of a change in the time series around the time period of reform to argue that a policy worked. In principle, if the impact of a policy is dramatic enough and coincides closely with its effective date, and if there are no other conflicting trends, it is possible to estimate the effect of the policy with the time series approach. To state this more formally, let $P_{a1(C=0)}$ denote the supply of physicians that would have been observed in state a had state a chosen not to adopt. The time series approach provides a consistent estimate of the treatment effect if and only if $E[P_{a0}] = E\left[P_{a1(C=0)}\right]$. That is, the time series approach is only valid if the outcomes of interest are otherwise stable. In practice, it is highly unlikely that the effects of tort reform on physician supply would be so immediate or large enough to stand out so dramatically to distinguish themselves from other trends in physician supply.

Beyond the potential for confounding variation, a key problem with the time series approach is that the number of physicians is actually quite stable. The top panel of Figure 1 shows the average number of physicians per 100,000 individuals in states that enact non-economic damage caps at some point and in those that do not. The lower panel of Figure 1 shows the number of states with non-economic damage caps. Two things stand out from this figure; the trends in the reform and non-reform states appear quite similar, and the reform states show no immediate or dramatic change in response to the increasing number of caps. Should we conclude from this figure that noneconomic damage caps have no effect on physician labor supply?

Clearly the answer is no, for the obvious reason that the state-level doctor counts do not vary significantly. Doctors face high barriers to entry due to state licensing requirements and medical school, so it is rare to see significant jumps in the number of physicians in any given year. This means that it is unsurprising that doctors do not appear and disappear in the series easily, and hence any impact of tort reform is expected to be gradual. In such a scenario, it is difficult to determine the effect of the policies (if any) simply by examining the trend.

This problem is not, of course, unique to studies of physician labor supply. Donohue and Wolfers (2005) examine the academic literature pertaining to the death penalty debate and point out that time series evidence in this case provided very misleading evidence for a deterrence effect. They demonstrate that Canada, which did not have death penalty during much of the sample period, exhibited similar trends to those found in the US surrounding the Furman decision. Their conclusion is that "As economists have come to understand how difficult it is to control convincingly for all relevant factors, many have lost faith in the ability of pure time-series analysis to isolate causal relationships."

A final, and related, problem with the time series approach in this context comes from the fact that the number of physicians in a state is typically standardized according to the state population. However, estimates of the state population actually vary much more than do physician counts, because doctors tend to be less mobile than the general population. This means that much of the year-to-year variation in the number of physicians per capita is actually driven in part by variation in the state population estimates. This isn't necessarily problematic, as it represents measurement error in the dependent variable and will not corrupt our estimates unless the error is correlated with the adoption of noneconomic damage caps. However, it provides additional noise which can make it difficult to identify real effects of a policy change through a simple time series.

2.2 Difference-in-Differences Estimation

One way to deal with the issues surrounding the time series approach is to measure treatment effects relative to a control group made up of non-reforming states. The difference between the two groups of states, which differs in each year, is the estimated impact of reform. The difference-in-differences estimator $\hat{\theta}^{dd}$ is given by:

$$(4) \qquad \hat{\theta}^{dd} = [P_{a1} - P_{n1}] - [P_{a0} - P_{n0}]$$

The idea behind this approach is that, as long as the expected difference between the treatment and control groups is fixed over time in the absence of treatment, subtracting the outcomes of the control group allows the analyst to net out unobservable effects that are correlated with reforms. This approach has been used by several prior studies in this literature (see Encinosa and Helliger (2005) and Kessler et al. (2005)).

The most obvious advantage of the difference-in-differences estimator over the time series estimator is that it eliminates bias from confounding trends that are common across states. However, an important limitation to this approach is that it assumes that the differences in outcomes between the states—absent treatment—are fixed over time. Using the earlier notation, then the difference-in-differences only provides a consistent estimate of the treatment effect if $\left[P_{a1(c=0)} - P_{n1}\right] = [P_{a0} - P_{n0}]$. If this assumption if violated then the estimate $\hat{\theta}^{dd}$ is biased because it attributes the difference that would have occurred even in the absence of treatment to the effect of the policy.[10] Whether the bias is positive or negative depends on the difference in trends between adopting and non-adopting states.

More generally, labor markets are attractive to physicians for a multitude of reasons: income potential, culture amenities, weather. Liability is only one factor in these decisions and the marginal factor for only a subset of doctors. But if a state's propensity to adopt liability reform is correlated with any of these confounding factors, it will compromise the estimated treatment effect.

One possible solution to this problem would be to control for factors impacting the broader labor market. This is the standard approach in the literature, which estimates $\hat{\theta}^{dd}$

[10] If θ^* is the true policy effect, then $\hat{\theta}^{dd} = \theta^* + \left\{\left[P_{a1(C=0)} - P_{n1}\right] - [P_{a0} - P_{n0}]\right\}$

conditional on a large set of covariates X. However, when the outcome variable has strong state-specific trends, as in the case with physician labor supply, it is difficult to know exactly what to control for or how to best control for it. Another solution is to identify a second control group. If there are differential trends across adopting and non-adopting states, the ideal scenario would be to have a control group within the two states that are subject to the same trends in the physician labor market, but are not directly affected by the reform. This is the "difference-in-difference-in-differences" approach, which is useful for estimating the causal effect of a policy in the presence of confounding trends.[11]

2.3 Difference-in-Difference-in-Differences Estimation

Klick and Stratman (2007) suggest that an appropriate control group for physicians in a state is other physicians. Specifically, they use categories of physicians for whom the threat of liability is infrequent, suggesting that tort reform should be at best a marginal factor in deciding to enter or exit a state. They then estimate the impact of the reform on the supply of physicians for whom reform should be more attractive relative to physicians in these low-risk specialties.

Several studies, including Klick and Stratman (2007) as well as ours, have used different groups of physicians in which control group is defined as the specialties that are less likely to be sued in a database of closed claims, such as the Florida medical malpractice data. The treatment group is defined as the specialties that are more likely to be sued, such as surgeons and obstetricians. In theory this provides an accurate estimate of the impact of reform on the high risk specialties. It removes both the impacts on national physicians' labor market by using non-reform states as a control and it eliminates any state-level factors that are common across specialties – even those that vary over time – by treating the lower risk doctors as a control.

[11] See Gruber (1994) for a well-known example.

Modifying our notation slightly, let P_{qst} represent the number of physicians in specialty q in states s in year t, and let $q = l$ for low-risk specialties and $q = r$ for physicians in high-risk specialties. With this, we can define the difference-in-difference-in-differences estimator $\hat{\theta}^{ddd}$ as:

$$(5) \qquad \hat{\theta}^{ddd} = \{[P_{ra1} - P_{rn1}] - [P_{la1} - P_{ln1}]\} - \{[P_{ra0} - P_{rn0}] - [P_{la0} - P_{ln0}]\}$$

This model estimates the differential effect of the reform on the number of physicians in high-risk specialties compared to the number of physicians in low risk specialties.

The question is, how do we interpret $\hat{\theta}^{ddd}$? Does this inform us about the effect of noneconomic damage caps in the general state population? Consider the following two assumptions:

$$(\text{A1}) \qquad [P_{la1(C=0)} - P_{ln1}] - [P_{la0} - P_{ln0}] = [P_{ha1(C=0)} - P_{hn1}] - [P_{ha0} - P_{hn0}]$$

$$(\text{A2}) \qquad [P_{la1} - P_{ln1}] - [P_{la0} - P_{ln0}] = 0$$

Here $P_{ja1(C=0)}$ is the supply of physicians in specialty j in state a in the post-treatment period in the absence of treatment. Assumption A1 justifies the use of the difference-in-difference-in-differences approach; it essentially states that the trend differences across the treatment and non-treatment states are identical in the high-risk and low-risk specialties. If this assumption does not hold, then this approach would suffer a bias analogous to that we discussed in the simple difference-in-differences approach.

The second assumption A2 states that the effect of the treatment is zero on physicians in the low risk specialties. Under this assumption, the parameter $\hat{\theta}^{ddd}$ estimates the causal impact of the reform on the total number of physicians in the state. However, this assumption is quite strict. For example, if physicians in low-risk specialties still fear the threat of liability and are attracted by reform, then $\hat{\theta}^{ddd}$ will understate the total effect of the policy. This is probably likely, given that there is significant heterogeneity in malpractice liability risk across very fine distinctions of physicians (Jena et al., 2011). In this case, the $\hat{\theta}^{ddd}$ still provide a consistent estimate of the *differential* effect of tort reform on physicians in high-risk specialties. What is lost is the ability to say anything about the impact of the reform on the overall number of physicians in the state, at least to the extent that physicians in low-risk specialties are attracted by tort reforms.

2.4 Identifying the Impact of a Reform in a Particular State

All of the approaches outlined above estimate a single treatment effect representing the impact of the treated group relative to the control group(s). However, just as we might expect different effects of the reforms across different types of physicians, we might also expect different effects in different states. Such differences could arise due to the underlying level of litigation risk—for example, Helland and Showalter (2009) find large differences in liability risk across states—or the nature of the reforms (i.e. a \$250,000 cap on noneconomic damages will be more binding than a \$500,000 cap). Other sources of heterogeneity include the penetration of managed care organizations (MCOs), differences in hospital or insurance concentration, state Medicaid policy, or even simple random error.

Some of these factors can be controlled for as covariates with the appropriate data; for example, estimates of liability risk could be included directly in the regression. But some level of heterogeneity is probably unavoidable. Thus if we are interested in the impact of treatment on any particular specialty in any particular state, we have three choices:

1. Use time series analysis and assume that reform is responsible for all of the change in the number of doctors in that state; or
2. Use the average impact of tort reform among the reform states to estimate the impact in the state; or
3. Try to estimate the actual state-specific impact of reform.

Each of these approaches has potential problems. The problems with the time series approach are obvious, as a single state's time series with no control group is probably no more informative than the time series of a group of adopting states.

The second approach is valid under the assumption of homogeneity. That is, suppose θ_i^* is the true effect that the policy would have if it were adopted in state i, then as long as the expected effect is common across states, so that $E(\theta_i^*) = E\left(\theta_j^*\right)$ for all $i \neq j$, then a consistent estimate of $\hat{\theta}^{dd}$ or $\hat{\theta}^{ddd}$ would be generally applicable across all states. However, the validity of the homogeneity assumption is challenged by Heckman's concept of "essential heterogeneity." The basic idea of essential heterogeneity is that the decision by states to adopt any given policy (in this case tort reform) is made based on the individual state's predicted gains from take up (see Heckman, Urzua and Vytlaci, 2006). In this context, we might be concerned that the first states to adopt noneconomic damage caps may have done so because they expected greater gains than states adopting later. In this case, difference-in-difference estimates will not generally provide unbiased estimates of the treatment effect.

Even if the probability of adoption is independent of the potential effect of the policy, the average effect may provide limited information to a particular state about the effectiveness of its policy if there is substantial treatment heterogeneity. One strategy is to attempt to estimate the state-specific effects of a policy. However, as we show later on, such estimates are not always easily interpreted or informative. In the absence of a useful estimate of state-specific estimates, the most useful approach from a practical standpoint is probably to assume homogeneity and rely on the average treatment effects estimates.

III. Empirical Framework

The goal of our empirical work in this paper is threefold. We examine whether the standard difference-in-differences approach provides a useful means for estimating the impact of a reform in a particular state, we study whether the effect of reform appears overall or whether it is focused in particular specialties, and we assess whether the relationship between the adoption of reform and physician supply is consistent with previous work when we expand the sample to include the most recent reforms. Here we describe the data and empirical specification we use to accomplish these goals.

3.1 Data Sources

Our doctor counts data come from the Area Health Resource File (AHRF). The AHRF doctor counts are collected from the American Medical Association (AMA) Physician Masterfile. The AMA Masterfile includes data on the education, training and licensing of more than 1.4 million physicians dating back to 1906, and the AHRF includes aggregate counts of physicians by county or state. The physician counts included in the AHRF data are broken down by detailed specialty from 1995-2010, although the information is missing for 2009.[12] The state population data came from inter-census estimates of annual state population produced by the US Census Bureau. The estimates of physician liability risk come from Jena et al. (2011) and Klick and Stratmann (2007).

The data on state level tort reform measures comes from Ronen Avraham's Database of State Tort Law Reforms (DSTLR 4th) which provides the effective date of the ten most prevalent kinds of tort reform measures for all 50 states and the District of Columbia during our sample period. Several of the studies mentioned above use the direct and indirect classification

[12] It is important to note that the ARF categories are not mutually exclusive. So, for example, internal medicine physicians might also appear in another specialty, such as general or family practice.

introduced by Kessler and McClellan (1996). According to this definition, direct reforms are those that reduce payments whereas indirect reforms make recovery more difficult. We focus specifically on the adoption of noneconomic damages caps, as several states have newly adopted caps since previous studies were done (most notably, the $250,000 cap introduced in Texas). We include the other reforms in the regression model as other covariates. We note that including the other reforms potentially reduces the power of our test, since the reforms are typically adopted in clusters as states often adopt a package of reforms (Avraham, 2010).

The descriptive statistics of the sample are provided in Table 2. The table reports the mean, standard deviation, minimum and maximum of the number of physicians, overall and by specialty, and the presence of tort reform at the state-year level. There are about 240 physicians per 100,000 in the population over this period, and about 215 per 100,000 that engaged in patient care (others could be in research, administrative positions, etc.). In our estimates we focus on physicians engaged in patient care since they are most likely to be involved in a malpractice claim, and hence should care more about the presence of tort reform. By far the most common specialties involve primary care, such as family or general practice or internal medicine; these three specialties account for over 42% of physicians engaged in patient care.

About 39% of states have noneconomic damage caps in the year in our sample. This is less common than many of the other reforms, such as punitive damage caps or joint and several liability reforms. Nevertheless, we focus on noneconomic damage caps because of their prominence in the policy debate.

3.2 Empirical specification

We estimate the impact of noneconomic damage caps on the log number of physicians of in a specialty per 100,000 of the population using multivariate regression. We follow the existing literature in interpreting changes in the number of physicians per capita in response to the passage of damage caps as changes in the supply of physicians.[13] Our empirical specification is modeled off of Klick and Stratman's, which includes individual specialty fixed effects but estimates the differential effect of reform on groups of high-risk specialties. We use two sources of information on specialty-specific malpractice risk to create these groups. The first is the ranking of specialties used by Klick and Stratman, which was derived from an examination of the Florida closed claim malpractice data. Specifically, they ranked specialties according to risk by identifying the 5 and 10 specialties mostly likely to be sued (neurological surgery, thoracic surgery, obstetrics and gynecology, general practice, emergency, plastic surgery, radiology, anesthesiology, general surgery, and cardiovascular disease).

In addition to the top 10 specialties, Klick and Stratman also identified the bottom 10 in terms of risk. However, a problem with applying this ranking to our data is that the data source we use, the AHRF, differs in how it classifies specialties from the AMA version used by Klick and Stratman. While the high-risk specialties overlap, the low-risk specialties do not. Moreover, the low-risk classification presents a potential problem with Klick and Stratman's estimate. That is, many of the 10 lowest risk specialties are relatively rare, in the sense that the physician counts are extremely low, and the classification is somewhat obscure. This could be problematic if there is not enough variation in the low-risk group. Additionally, if the lowest risk specialties are sufficiently obscure, they may not be subject to the same general trends in a state's health care market, so they would not serve as an appropriate control group.[14]

[13] Of course, this assumes that noneconomic damage caps have no impact on the demand for physician services. See Dranove and Ramanarayanan (2012) for a study of the relationship between malpractice risk and the demand for physicians.
[14] In practice, as our results demonstrate, the Klick and Stratman designation of low risk specialties does not cause problems since it appears that outside of the top portion of the risk distribution, however defined, lower risk specialties appear to behavior similarly.

Our solution is to use all non-high-risk specialties as the control group, even though this means that some of them are likely impacted by the presence of a noneconomic damage cap, just not as much as the physicians in the high risk categories.[15] This is a violation of assumption A2 above, which will cause us to understate the impact of the reforms on physician supply. Thus, our estimates should be interpreted as conservative estimates of the average effect of noneconomic damage caps on physician labor supply.

The second source of data on risk is based on the ranking provided by Jena et al. (2011). Their ranking was based on a multistate database of closed malpractice claims from a large, national malpractice insurer.[16] Jena et al. rank over 20 physician specialties according to the frequency of lawsuits in a year. Although the classifications of specialty according to risk overlap, there are some variations. We use both measures to verify the robustness of the findings to the choice of high risk specialty. Additionally, we employ a specification where we directly interact the frequency estimate of the specialty with the passage of a cap, to determine how variations in frequency impact the relationship between tort reform and physician supply.

The actual estimating equation we use is:

$$\ln(doctors_{ist}) = \alpha + \beta Noneconomic\ Cap_{st} * high\ risk$$
$$+\beta Noneconomic\ Cap_{st} + \gamma Reform_{st} + \tau_i + \lambda_t + \varphi_s + \varepsilon_{ist}$$

where i is the specialty, s is state and t is year, $doctors$ represents the number of physicians per 100,000 of the population, $Noneconomic\ Cap$ is an indicator for a state having a noneconomic damage cap in place in a state in a year, and $Reform$ represents a vector of indicators for the other types of malpractice reform. Like Klick and Stratman, we include specialty fixed effects

[15] Even if the impact is not direct there are several reasons the no spill over assumption could be false. For example if insurers pool high risk and low risk specialties, i.e. low risk specialties subsidize high risk, then low risk specialties will face lower premiums and hence reforms would impact their location decisions.
[16] See also Seabury et al. (2011), Jena et al. (2012), Seabury et al. (2013) and Jena et al. (2013) for more detail on these data.

τ_i, year fixed effects λ_t, and state fixed effects φ. However, we do not include an interaction between specialty and year as they did. The results we present here are robust to the inclusion of the interactions, so we focus on the less saturated model. As noted above, our sample runs from 1995 to 2010 with information on doctors counts by specialty being omitted in the 2009 data. Following Klick and Stratman, we include all 50 states but exclude DC in our estimation sample. The term ε_{ist} represents the robust standard error.

IV. Results

Our central empirical results are presented in Table 3. The first column provides the simple difference-in-differences estimate of the effect of reform. Strikingly, the base estimate of the relationship between noneconomic damage caps and the supply of physicians is negative. There are two possible explanations for the negative impact of reforms overall. The first is that reforms drive doctors out of the state in a causal manner, but this seems highly implausible. At a minimum, noneconomic damage caps could be ineffective at reducing physician risk, or the savings are not passed on to doctors in the form of reduced premiums, but it is highly unlikely that the existence of an ineffective law would actually drive off doctors. More likely is that, at least in our sample period, the reforms were adopted by states with lower rates of growth in the number of physicians, and that the liability environment was only one factor in that lower growth rate.

In Table 3 we again estimate the direct effect of non-economic damage caps. We include lags and leads of between 1-3 years. Column 1 repeats column 1 of Table 2 for reference. In Column 2 we estimate the model with indicators for 1-3 years before and after adoption. Although none of the effects are significant the coefficients for 1-3 years prior are negative (and continue negative after adoption. This is also true using shorter lags or leads 1-3 post adoption.

This suggests that states adopting reforms are experiencing reductions in the number of physicians prior to adoption. This finding motivates the difference-in-difference-in-differences approach favored by Klick and Stratman.

Columns 2-6 report the results from a comparison of high risk versus low risk specialties. We find that, for all of our risk measures, the supply of physicians in the high risk specialties increased as a result of reforms. As we would expect, the impact is larger when we compare the 5 highest risk specialties to other physicians. For example, using the top 5 from Klick and Stratman, we find that reform results in a 6.6% increase in the number of doctors in the high risk specialties. When we compare the 10 highest risk specialties to other physicians, the estimate falls to 3.4%.

Using the Jena et al. classification of risk, we find smaller impacts with the number of physicians in the top 5 specialties increasing by 2.5% relative to other specialties. This number falls to 1.5% if we the top 10 specialties to other physicians. Finally, when we include the Jena et al. estimates of malpractice frequency directly interacted with the cap, we find that an increase in risk in capped states increases the number of physicians in that specialty. Interpreting this coefficient, a specialty with a 10% increase in the risk of facing a malpractice claim in a year would respond to tort reform with approximately a 5.5% increase in supply compared to a specialty with a 0% chance of a claim.

To better illustrate the relationship between specialty risk and the supply response of physicians, Figure 2 shows the estimated impact of reform on the supply of physicians ranked by the annual frequency of malpractice claims in the year. While there is wide variation in the estimated effect of reforms, the effect is positive in most cases and there is a general upwards slope, meaning that, on average, the supply of physicians in specialties with higher risk is more responsive to noneconomic damage caps.[17]

While these results highlight the relationship between reform and the response of high risk specialties, we are left with the question as to whether it is possible to evaluate the results of an individual state's reform. In principle, we can study differences in the effects of reform across states analogously to specialty: by interacting the noneconomic damage cap indicator with an indicator for state as well as for specialty. Thus, for each of the states that enacted a non-economic damage cap in our sample period, we estimate a post-reform specialty-and-state-specific effect. Since our data are at the state-specialty-year level, we are exploiting the maximum level of interactions allowed by our data. We estimate the model using the Klick and Stratment and Jena et al. top 10 high-risk specialties, although the results are similar for all of the high risk measures.

Figure 3 shows the distribution of the estimated state-specialty effects for the Klick and Stratman (top panel) and Jena et al. (lower panel) risk measures. The vertical line indicates the average effect across all specialties and states, while the histogram represents the distribution of effects around the mean. We have also imposed the normal distribution for comparison.

In general, the distributions are approximately normal and centered on the estimated effect. However, in both cases the right tail of the distribution suggests several state-specialties with very high estimated effects. The point driven home by Figure 3 is that there is wide variation in the estimated effect of reform across states. It is difficult to interpret this variation, and examining the impact of reform on any particular state does not yield a consistent story. If a researcher were to examine the effect of noneconomic damage caps on the supply of physicians to a particular state and high-risk specialty, independent of the information contained in the

[17] The results are robust to the exclusion of any individual specialty.

experience of other states, that researcher could find impacts ranging from a 32% drop (the left tail of the distribution) to an 88% increase (the right tail). Unfortunately, it is unclear the extent to which this variation is driven by legitimate heterogeneity of treatment effects, measurement error, or both.[18]

V. Conclusions

Given the intensity of the policy debates over medical malpractice liability over the past 40 years, it is not surprising that a large academic literature has arisen studying the issue. In this paper, we review the evidence on a particular aspect of this literature that has been the subject of much attention—how do reforms to the malpractice liability system affect the supply of physicians willing to practice in an area. Past studies have varied considerably in their definition of the control groups: adopting versus non-adopting states, specialties with high versus low litigation risk, urban versus rural, etc. Despite this variation in studies, estimates of the effect of the reforms have been remarkably consistent, in the sense that they tend to find a positive impact of reforms on physician labor supply that is concentrated in certain subpopulations (e.g., high risk specialties).

In this paper update these estimates using more recent data and a slightly different specification. For the most part, our results are consistent with previous findings. We find that noneconomic damage caps are associated with an increase (from 1% to 7%) in the supply of physicians in high-risk specialties in a state. However, we do find that the baseline effect of noneconomic damage caps points to a negative association between caps and physician supply. We think that this reflects a tendency that states who adopt have different underlying trends in

[18] One reason for this heterogeneity is the different insurance markets in the states. Prior research, and our own, has estimated liability risk using the likelihood or cost of litigation by specialty. Yet if insurance is successful at spreading this risk across specialties the impact would be mitigated. It seems likely that states vary in both their underlying liability risk (see Helland and Showalter, 2009) and in their insurance markets ability to spread that risk across physicians.

the market for physician services, specifically that they have slower growth in physicians per capita. A simple explanation for this is that the adopting states are motivated to adopt because they have relatively poor access to physicians, and adopt tort reform as an attempt to offset that. In any event, this motivates the comparison between high-risk and low-risk specialties. While this evidence clearly indicates an increase in physicians in high risk specialties after the adoption of noneconomic damage caps, the implications for the aggregate market for physicians are ambiguous.

This ambiguity may be one reason why the national estimates have not always gained traction in state-level debates, where the discussion usually focuses on the impact of a particular state's policy. In theory it is possible to estimate state-specific effects for adopting states, just as we estimate effects for particular specialties. However, we find that the state-level estimates vary widely and without a clear, discernible pattern. This is perhaps not surprising. Suppose the true treatment effect is $\theta^* + \epsilon_s$, where ϵ_s is a parameter that represents differences in the treatment effect across states. This term could be a function of different state characteristics, such as the level of managed care in the state, or the average treatment intensity or local standards. Or it simply could be random noise that comes from the uncertainty inherent to all such observational studies. As noted by Angrist (2004), "the theory of parameter heterogeneity runs quickly into the sandpile of sampling variance and specification uncertainty."

Given the widespread variation in the state-specific estimates, the most convincing evidence for the effect of malpractice liability reforms is the lesson from the existing evidence base. That is, when a state adopts a reform such as a noneconomic damage cap, the most likely outcome is that it will experience an increase in the supply of some physicians, particularly those in high risk specialties over and above what they would experience in the absence of a cap.

Exactly how big that increase is or would be is subject to uncertainty, but the structure of the physician labor market, with its high cost of entry and exit, suggest there is a limit to the size of the effect. This does not mean that we should ignore heterogeneity across states. In cases where we have a specific theory about why a particular reform might be more or less effective in a particular state, this offers a testable hypothesis that can be tested using the state-level differences. But in the absence of such theory, or the application of more advanced econometric techniques suited for this approach,[19] the national estimates of average treatment effects provide the best guide as to the expected effects of any given policy.

[19] In particular, some of the newer methods using synthetic control groups seem to hold some promise for identifying state-specific treatment effects (for example, see Abadie and Gardeazabal, 2003 and Abadie, Diamond and Hainmueller, 2010).

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Table 1. Summary of Key Papers on Tort Reform and Physician Labor Supply

Study	Empirical Strategy	Main Findings
Kessler et al. (2005)	Difference-in-differences	There was a 2.4% increase in the number of physicians in a state associated with "direct" reforms
Encinosa and Helliger (2005)	Difference-in-differences	There was a 2.2% increase in the number of physicians associated with the adoption of a cap, mostly found 3+ years after enactment.
Baicker and Chandra (2005)	Difference-in-differences	There was negative relationship between the number of obstetricians, internal medicine, surgeons and doctors over 55 and medical malpractice premiums and multiple measures of liability although the results are inconsistent.
Klick and Stratmann (2007)	Difference-in-difference-in-differences	Noneconomic damage caps led to a 2.9% increase in physicians in high risk specialties and a 1.3% increase in physicians in low risk specialties.
Matsa (2007)	Difference-in-difference-in-differences	Noneconomic damage caps led to a 4-7% increase in the number of physicians in rural counties.
Helland and Showalter (2009)	Difference-in-differences	An increase in liability risk led to an increase in the number of hours worked.

Table 2. Summary Statistics

	Mean	Std. Dev.	Min	Max
Active Physicians per 100,000	240.43	59.49	135.9	478
Total Physicians in Patient Care per 100,000	215.78	47.59	129.5	388
Physicians by Specialty				
Allergy & Immunology	1.09	0.38	0.3	2
Anesthesiology	11.45	2.64	5.2	21
Cardiovascular Disease	6.07	2.06	1.7	12
Colorectal Surgery	0.33	0.18	0	1
Dermatology	2.91	0.96	0.8	6
Diagnostic Radiology	7.14	1.59	3.8	14
Emergency Medicine	8.35	2.48	2.5	16
Family Practice	27.75	8.97	12.5	51
Gastroenterology	3.19	1.09	1.1	7
General Internal Medicine	31.53	13.99	10.2	78
General Practice	31.83	9.05	15.8	56
General Surgery	12.17	2.69	7.4	21
Neurological Surgery	1.74	0.42	0.5	3
Neurology	3.72	1.27	1.3	10
Obstetrics and Gynecology	11.34	2.74	5.6	19
Ophthalmology	5.69	1.31	2.7	10
Orthopedic Surgery	11.34	2.74	5.6	19
Pathology	5.1	1.51	2	11
Pediatrics	15.22	5.5	5.2	30
Plastic Surgery	1.9	0.59	0.4	3
Psychiatry	11.13	5.37	3.8	30
Pulmonary Dis	2.48	0.9	0.8	6
Rad Oncology	1.26	0.32	0.5	3
Radiology	2.59	0.63	1.1	6
Thoracic Surgery	1.08	0.65	0	3
Urology	3.28	0.6	1.3	5
Reform Variables				
Caps Noneconomic Damages	0.39	0.49	0	1
Caps Punitive Damages	0.49	0.5	0	1
Caps Total Damages	0.12	0.32	0	1
Split Recovery Reform	0.12	0.33	0	1
Collateral Source Reform	0.63	0.48	0	1
Punitive Evidence Reform	0.67	0.47	0	1
Periodic Payments-discretion	0.21	0.41	0	1
Periodic Payments-mandatory	0.37	0.48	0	1
Contingency Fee Reform	0.36	0.48	0	1
Joint and Several Liability Reform	0.73	0.44	0	1
Patient Compensation Fund Reform	0.24	0.43	0	1
Observations	816			

Notes: Data are at the state-year level. Specialty-specific counts represent physicians active in patient care.

Table 3. Regression Estimates of the Impact of Noneconomic Damage Caps on the Number of Physicians Per Capita

	(1)	(2)	(3)	(4)	(5)	(6)
Top 5 Highest Risk Specialties × Noneconomic Cap		0.066**		0.025**		
		(0.028)		(0.010)		
Top 10 Highest Risk Specialties × Noneconomic Cap			0.034***		0.015*	
			(0.007)		(0.009)	
Liability Risk × Noneconomic Cap						0.549***
						(0.154)
Direct effect of noneconomic damage cap	-0.031***	-0.043***	-0.051***	-0.039***	-0.037***	-0.081***
	(0.012)	(0.010)	(0.012)	(0.012)	(0.012)	(0.017)
Other Reform Variables						
Caps on Punitive Damages	-0.005	-0.006	-0.006	-0.005	-0.005	-0.006
	(0.013)	(0.009)	(0.013)	(0.013)	(0.013)	(0.013)
Caps on Total Damages	-0.185**	-0.186***	-0.186**	-0.186**	-0.185**	-0.186**
	(0.075)	(0.013)	(0.074)	(0.075)	(0.075)	(0.075)
Split Recovery Reform	-0.012	-0.012	-0.012	-0.012	-0.012	-0.012
	(0.017)	(0.014)	(0.016)	(0.017)	(0.017)	(0.016)
Collateral Source Reform	-0.009	-0.009	-0.009	-0.009	-0.009	-0.009
	(0.016)	(0.010)	(0.016)	(0.016)	(0.016)	(0.016)
Punitive Evidence Reform	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016
	(0.023)	(0.010)	(0.023)	(0.023)	(0.023)	(0.023)
Periodic Payments-discretion	-0.009	-0.009	-0.009	-0.009	-0.009	-0.009
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Periodic Payments-mandatory	-0.026*	-0.026*	-0.026*	-0.026*	-0.026*	-0.026
	(0.016)	(0.014)	(0.016)	(0.016)	(0.016)	(0.016)
Contingency Fee Reform	-0.077***	-0.077***	-0.077***	-0.077***	-0.077***	-0.077***
	(0.029)	(0.008)	(0.029)	(0.029)	(0.029)	(0.029)
Joint and Several Liability Reform	0.025*	0.025**	0.025*	0.025*	0.025*	0.025*
	(0.014)	(0.010)	(0.014)	(0.014)	(0.014)	(0.014)
Patient Compensation Fund Reform	0.017	0.017*	0.017	0.017	0.017	0.017
	(0.031)	(0.009)	(0.031)	(0.031)	(0.031)	(0.031)

Source of specialty risk used	None	Klick and Stratman (2007)	Klick and Stratman (2007)	Jena et al. (2011)	Jena et al. (2011)	Jena et al. (2011)
Observations	20,884	20,884	20,884	20,884	20,884	20,884
R-squared	0.945	0.945	0.945	0.945	0.945	0.945

Notes: The table reports the results of regressions of the numbers of physicians per capita against various measures of tort reform plus other covariates. Physicians are broken into high-risk and low risk specialties according to classifications used in two papers: Klick and Stratman (2007) and Jena et al. (2011). Robust standard errors are reported in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Regression Estimates of the Impact of Noneconomic Damage Caps on the Number of Physicians Per Capita prior and post adoption

	(1)	(2)	(3)	(4)	(5)
Direct effect of noneconomic damage cap (3 years prior to adoption)		-0.015			
		(0.016)			
Direct effect of noneconomic damage cap (2 years prior to adoption)		-0.013	-0.021		
		(0.023)	(0.017)		
Direct effect of noneconomic damage cap (1 year prior to adoption)		-0.009	-0.003	-0.004	
		(0.021)	(0.020)	(0.014)	
Direct effect of noneconomic damage cap	-0.031***	-0.012	-0.020	-0.028	-0.015
	(0.012)	(0.022)	(0.023)	(0.022)	(0.017)
Direct effect of noneconomic damage cap (1 year post adoption)		-0.005	-0.001	-0.007	-0.004
		(0.019)	(0.019)	(0.016)	(0.016)
Direct effect of noneconomic damage cap (2 years post adoption)		-0.006	-0.009		-0.001
		(0.017)	(0.016)		(0.015)
Direct effect of noneconomic damage cap (3 years post adoption)		-0.010			-0.011
		(0.017)			(0.015)
Observations	20,884	13,962	16,695	18,090	18,086
R-squared	0.945	0.946	0.943	0.944	0.944

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

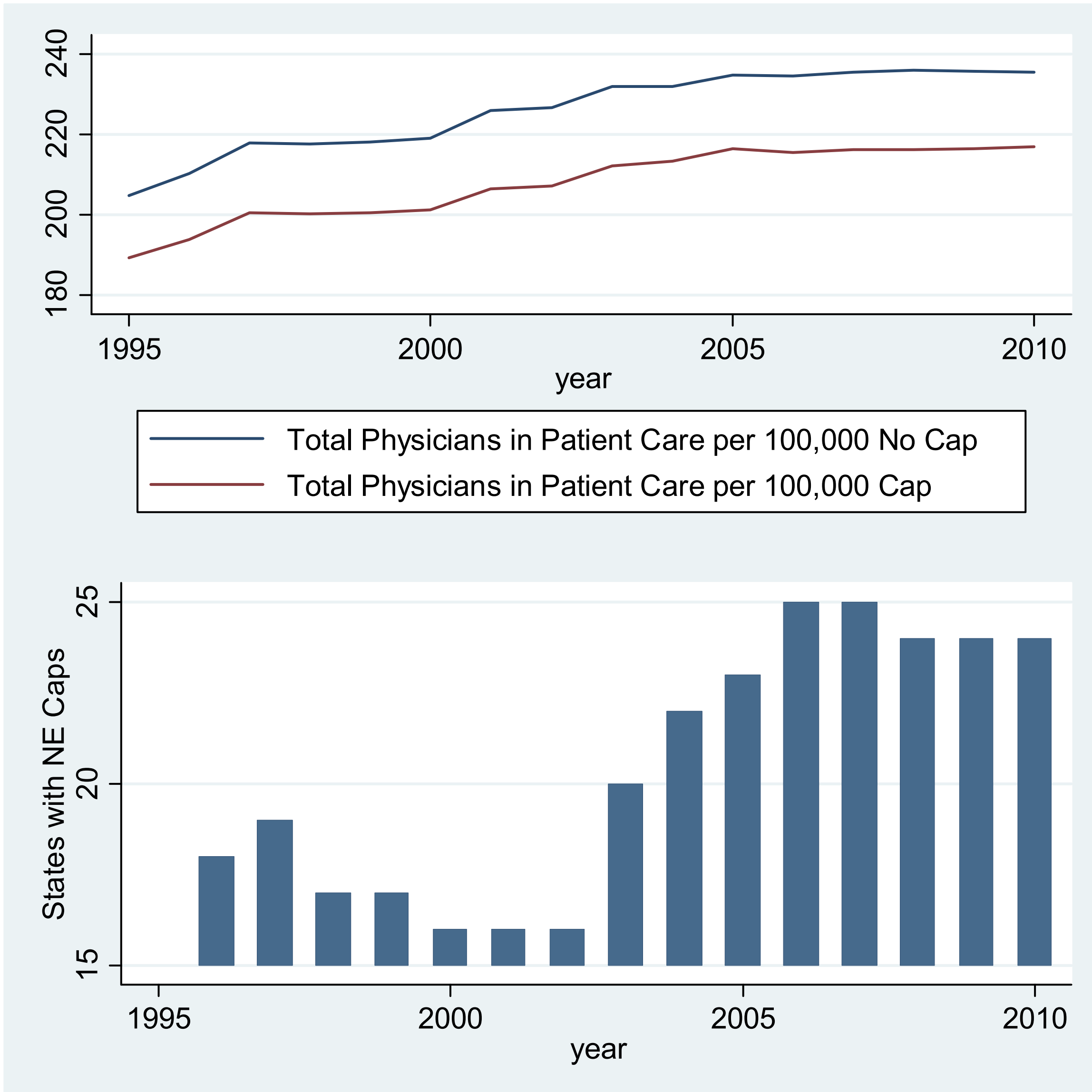

Figure 1. Annual Trends in Physicians and the Adoption of Noneconomic Damage Caps

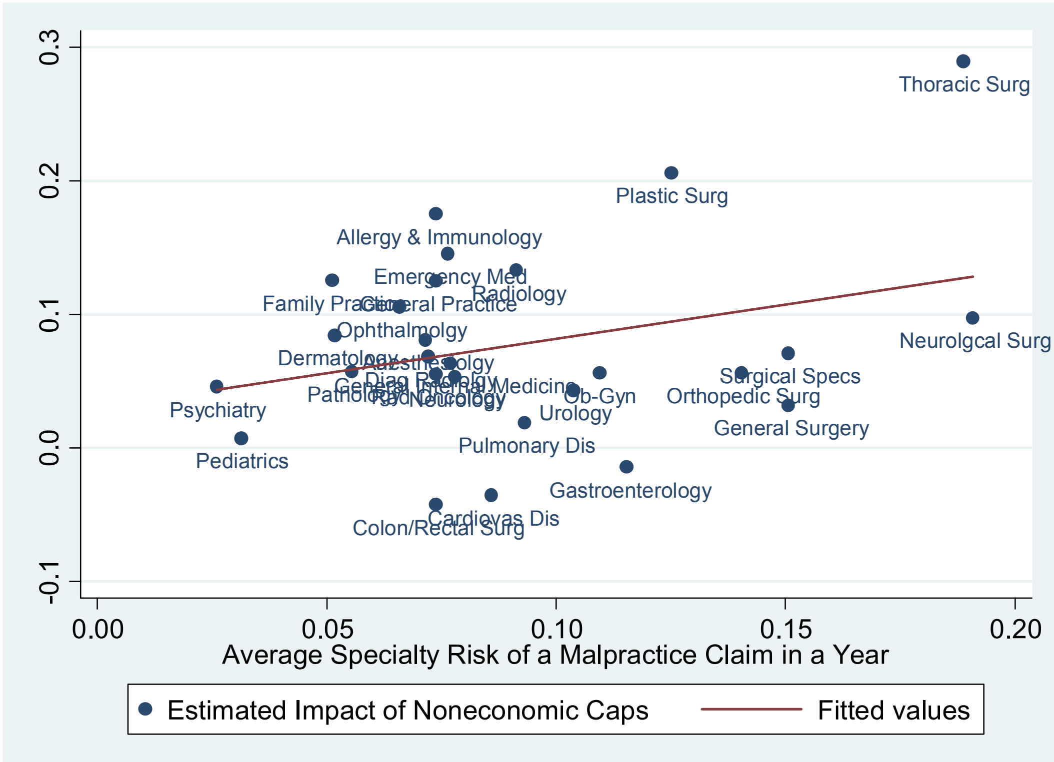

Figure 2. The Estimated Effect of a Noneconomic Damage Cap on Physician Supply by Specialty According to Specialty Liability Risk

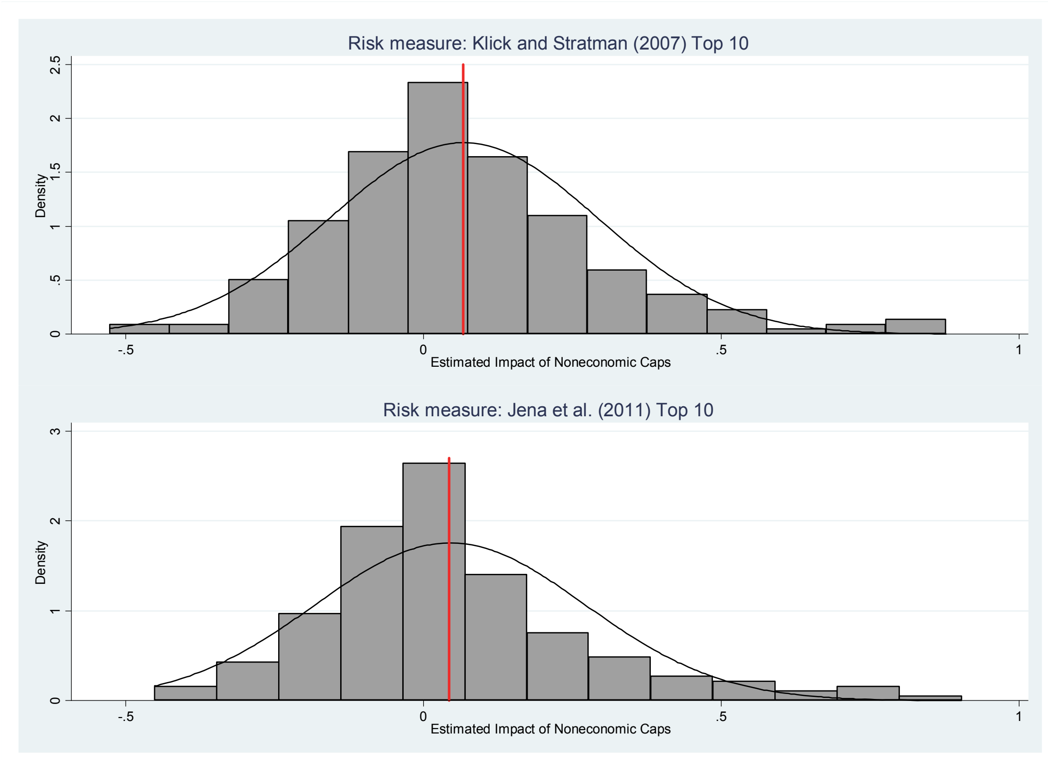

Figure 3. The Distribution of State-Specific Estimates of the Impact of Noneconomic Damage Caps on Physician Supply in High-Risk Specialties