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Working Paper 23446
http://www.nber.org/papers/w23446

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2017

We are grateful to Christoph Engel, Jonah Gelbach, Eric Helland, Anupam Bapu Jena, Daniel Ho, Kristin Madison, Susanne Prantl, Max Schanzenbach, Urs Schweizer and various seminar participants and colleagues for helpful comments. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed a financial relationship of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w23446.ack

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The Effect of Malpractice Law on Physician Supply: Evidence from Negligence-Standard Reforms

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NBER Working Paper No. 23446
May 2017
JEL No. I18,K13

## ABSTRACT

We explore whether the composition of the physician work force is impacted by the clinical standards imposed on physicians under medical liability rules. We theorize that physicians of particular backgrounds will be attracted to regions when the malpractice laws of those regions favor the type of medicine characteristic of those backgrounds. To test this prediction, we rely on a quasi-experiment made possible by states shifting from local to national customs as the basis for setting standards at court, a distinction that captures meaningful differences in the clinical expectations of the law in light of the well documented phenomenon of regional variations in medical practices. Using data from the Area Health Resource File from 1977 to 2005, we find that the rate of surgeons among practicing physicians increases by 2-2.4 log points following the adoption of national-standard laws in initially low surgery-rate regions—i.e., following a change in the law that effectively expects physicians to increase practice intensities. We find that this response is nearly three times greater in rural counties. We also find that this supply effect is unidirectional, with no evidence to suggest that surgeons retreat when initially high-surgery-rate regions change their laws so as to expect less intensive practice styles.

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An online appendix is available at http://www.nber.org/data-appendix/w23446

Along with health care spending and quality, patient access to care is frequently identified as one of the key criteria by which we assess the effectiveness of the U.S. health care system. Critical to the patient-access pillar is the supply of physicians willing and able to provide care in the first instance, along with the geographic distribution and the specialty case mix of this supply. Numerous reform proposals over the years have focused on these various dimensions to physician supply.[1] Among these proposals—especially those put forth by Republican lawmakers—is tort reform. That is, commentators frequently cite the medical liability system as a significant threat to patient access, with liability fears and high premiums presumably causing physicians to shut down or relocate their practices or refrain from entering the industry in the first place. If true, then reforms to the liability system may therefore improve patient access to care. The reform most commonly touted for such purposes is a cap on non-economic damages awards at trial—i.e., a limit on the amount of pain and suffering awards that successful plaintiffs may recover.

Interestingly, however, the literature has failed to produce consistent evidence of an increase in physician supply following the hypothesized reductions in liability pressure that may result from damage cap adoptions. This lack of conclusive evidence has led some scholars to conclude that liability risk is unlikely to be a factor that shapes physician location decisions and that such decisions are likely to be driven entirely by other factors such as patient demand (Paik er al. 2016). Any such conclusion, however, is perhaps premature and places a particular burden on the validity of using state damage caps to evaluate physician responsiveness to the liability system.

Indeed, there may be reasons to doubt the power of damage caps as a methodological instrument for these purposes. Underlying this doubt is evidence suggesting that physicians are financially insulated from, and thus potentially insensitive to, damages levels at trial, especially in

[1] For recent policy discussions and empirical analyses surrounding physician supply, see, for example, Kirch and Petelle (2017), Gudbranson et al (2017), Strange (2014), Garthwaite (2012); Chernew et al. (2009), Staiger et al. (2009), and Institute of Medicine (2008).

light of the often discussed fact that medial liability policies are generally not experience rated (Sloan 1990, Zeiler et al. 2007). A damage-cap reform could theoretically alter a physician's perceived liability threat to the extent caps discourage plaintiffs and plaintiffs' attorneys from filing suit in the first place and to the extent that physicians face a number of uninsurable costs as a result of malpractice liability—e.g., reputational and psychological damage (Jena et al. 2011; Frakes and Jena 2016).[2] The literature, however, has failed to systematically and consistently document a strong relationship between damage caps and the frequency of suits.[3] Moreover, even if damages caps do indeed lead to actual reductions in the likelihood of suit, other evidence suggests that they do not meaningfully change the perception of risk among physicians (Carrier et al., 2010). Ultimately, damage caps may be too marginal of a shock to the liability system to powerfully inform on the *full* impacts of that system.[4]

Beyond the possibly marginal nature of caps, remedy-focused reforms of this nature can only hope to inform the impacts of the present structure of liability rules. Over the years, however, policymakers and scholars have discussed implementing more structural reforms to the system—for instance, retreating from using physician custom as the exclusive basis to determining liability standards and instead extending safe harbors to physicians who comply with specified clinical practice guidelines. Safe-harbor proposals of this nature may even be prominent among the liability reforms likely to be considered by the new administration (Mello et al. 2017). How

[2] Subject to certain exceptions, payments made on behalf of physicians to settle claims or to satisfy judgments must, under federal law, be registered in the National Practitioner Data Bank (NPDB), an electronic repository which is made available to hospitals and certain other health care entities. The NPDB was established by the Health Care Quality Improvement Act of 1986, as amended (42 U.S.C. 11101 et seq.). This repository may reinforce any reputational consequences of malpractice liability.

[3] See Mello and Kachalia (2016) for survey of this literature. A couple of recent studies have found that damage caps are associated with a reduction in the frequency of malpractice suits (Paik et al. 2013, Avraham 2007). Other studies, however, have found no such association (Donohue and Ho 2007, Zuckerman et al. 1990).

[4] Reinforcing the point, the literature on the link between damage caps and healthcare spending also demonstrates a generally weak relationship (though with some variation across studies). For recent surveys on this literature, see Frakes (2015).

physicians respond to more substantive reforms of this nature may not be so clear based on our experiences to date with the implementation of remedy-centric reforms.

Given these limitations with the damages cap approach, we elect in this paper to consider variation along a separate dimension in the law—that is, variation in the standards of care to which physicians are held. Physicians, after all, are only liable for harm that they cause to patients when their behavior fails to satisfy certain clinical standards determined at trial, a structural feature of the law not appreciated by the existing studies exploring the link between liability forces and physician supply. Analogizing medical liability to speeding tickets, the adoption of damage caps is akin to new traffic laws aimed at modifying the magnitude of speeding ticket fines, whereas reforms to medical liability standards are akin to new traffic laws aimed at changing the speed limits triggering the law in the first instance. To acquire variation along this dimension, we rely upon a large scale, fundamental change in liability standards that the majority of states underwent in the latter half of the 20$^{th}$ Century—that is, the shift from a "locality rule," whereby the liability standards to which physicians were held were determined according to local physician customs, to a national-standard rule, whereby geographical limitations in the liability-standard determinations were removed entirely (Frakes 2013).

In the healthcare context, the difference between local and national customs is potentially significant in light of the rampant regional variations that exist in medical practices, a phenomenon which has long garnered the attention of medical scholars and health economists (Wennberg and Gittelsohn 1973; Chandra and Staiger, 2007; Skinner 2011; Finkelstein et al 2016). Put simply, physicians in different regions practice very different styles of medicine. With substantial regional variations in practices in mind, the abandonment of a locality rule—for instance, a change which

expects Mississippi physicians to begin practicing like physicians in others regions—represents a potentially meaningful change in the legal expectations placed upon physicians.

In this paper, we explore whether such an alteration of the clinical standards demanded of physicians, in turn, impacts the inclinations of physicians of certain clinical types to locate their practices in the relevant jurisdictions. Broadly speaking, if the laws of a particular region favor a certain style of medicine, do we see physicians trained in that style of medicine being attracted to that region? In approaching this question, we follow Chandra and Staiger (2007) and characterize physician behavior as following one of two styles: (1) a low-intensity approach (e.g., medical management of disease) or (2) a high-intensity approach (e.g., surgical intervention). With this binary clinical distinction in mind, consider the traditional locality rule as a starting point. If legal standards reinforce local customs and if those customs, for instance, favor medical management over intensive clinical approaches, then perhaps relatively few surgical specialists will be inclined to set up their practices in the relevant locality. Similarly, hospitals would perhaps be disinclined to recruit surgeons in areas whose local liability-standard rules call for low-intensity practice styles. Should those jurisdictions embrace national medical liability standards, however, these isolating forces may relax and we may observe more surgical specialists attracted to those regions, whether through self-recruitment or hospital-recruitment mechanisms.

Dynamics of this nature have been ignored by the existing literature. Those studies that have considered differential impacts of medical malpractice across physician type have done so by simply noting that malpractice pressure more strongly discourages the supply of physicians in specialties that face a higher incidence of malpractice suits (Klick and Stratmann, 2007). More generally, the literature has treated liability "pressure" in a vague and abstract sense without appreciating the clinical standards underlying that pressure.

Using data on physician population counts from the Area Health Resource File from the mid-1970s to the mid-2000s and drawing on locality rule abdications from 16 states over this time period, we find evidence consistent with the prediction that the physician mix between surgeons and non-surgeons in a region may be a function of the prevailing malpractice-standard rules in that region—i.e., a function of whether prevailing rules expect that physicians practice at intensive or non-intensive practice styles. More specifically, we find that when counties with initially low-intensity medical practices—as captured by initially below-average rates of surgery—experience a shift from a locality rule to a national-standard rule, those counties observe a corresponding shift towards relatively greater numbers of surgical specialists practicing within their borders and relatively fewer non-surgeons (at a magnitude of a roughly 2-2.4 log-point increase in the proportion of surgeons). However, we find that this supply effect may be unidirectional, with no evidence to suggest that surgeons retreat when initially high surgery-rate regions change their laws so as to expect less intensive practice styles.

Concerns over the influence of liability forces on physician location decisions have been particularly paramount when discussing physicians practicing in rural regions (Matsa 2007). With this backdrop in mind, we separately estimate our difference-in-difference specifications across counties in different quartiles of population density. Though we generally find a shift towards more surgical specialists in all initially low-intensity counties, we find that this shift is especially strong in the most rural population-density group. In this sub-group, we estimate a roughly 6 log-point increase in the proportion of surgeons among the county's total physicians following a national standard rule adoption.

These findings complement our prior research (Frakes, Frank and Seabury 2015), where we found that treatment utilization patterns are sensitive to the adoption of national-standard rules.

In this prior work, we likewise tested the impacts of national-standard-rule adoptions in counties with initially below-average rates of surgery, using data on total inpatient and outpatient surgery counts from the Area Health Resource File.[5] In such counties, we estimated a roughly 9 log-point increase in the surgery rate following the relevant change in the law (with an even stronger response in the most rural population-density quartile). As such, not only do we observe an increase in surgeries, we also observe an increase in the proportion of surgeons among practicing physicians in those counties experiencing a change in the law that alters clinical expectations in the direction of greater practice intensity.

Collectively, the findings from the present paper and from Frakes, Frank and Seabury (2015) and Frakes (2013) raise a question regarding the mechanisms underlying the treatment intensity responses documented in these prior works. Was the convergence in practices documented in these studies likely attributable to given physicians modifying their practices in connection with altered clinical expectations—e.g., do given physicians practice more intensively when an initially low-intensity regions adopts a national standard rule that expects physicians to follow more intensive styles? Or was the convergence more attributable to a relative attraction of surgical specialists to the affected regions upon the adoption of a national-standard rule?[6] While we do find evidence supporting the former interpretation, our analysis suggests that some of the changing clinical landscape may stem from corresponding impacts on the composition of physician supply.

[5] Frakes, Frank and Seabury (2015) builds upon Frakes (2013) which had focused on state-year level data instead of county-year level data and which had shown that the abandonment of locality rules and the adoption of national-standard rules led to substantial regional convergence in various obstetric, cardiac, and diagnostic procedures using data from the National Hospital Discharge Surveys. Extending the analysis to a sub-state unit was helpful given that locality rules themselves operated on sub-state bases.

[6] Frakes (2013) controlled for changes in prevailing physician concentration rates—e.g., OB/GYN concentration rates in the case of the obstetrics utilization convergence analysis—in order to rule out that the observed convergence in clinical behaviors resulting from national-standard-rule adoptions could be explained by spurious fluctuations in physician supply patterns. However, Frakes (2013) did not consider whether the regional convergence in clinical practice styles that were driven by the geographical standardization of malpractice-standard rules was accompanied by an associated convergence in the relative physician mix servicing those regions.

The paper proceeds as follows. In Part I, we survey the literature on the relationship between physician supply and medical malpractice law, while also providing background on the evolution of liability standards in U.S. courts over time. In Part II, we describe the data sources that we use and in Part III we outline our empirical framework. Part IV describes the results. Finally, in Part V, we conclude.

## I. Literature Review and Background

### *A. Physician Supply and Medical Malpractice Law Literature*

As alluded to in the introduction, a large literature has investigated whether medical liability forces are associated with physicians' supply and location choices, where most such studies approach this inquiry through the estimation of difference-in-difference specifications drawing on the adoption of non-economic damages caps and related remedy-focused reforms.[7] Helland and Seabury (2015), Paik et al. (2016) and Mello and Kachalia (2016) provide recent surveys of this literature. Overall, the findings are mixed, though some studies have found that caps increase physician supply in certain circumstances. Klick and Stratmann (2007), for instance, fail to find increases of this nature on overall supply levels when estimating difference-in-difference specifications using physician population data from 1981-2000; however, the authors do find positive effects when estimating triple-differences specifications that differentiate between high- and low-risk specialties. Drawing upon more recent reforms, Helland and Seabury (2015) find similar triple-differences results, while Pesko et al. (2017) similarly find stronger supply responses to caps among high-risk physicians (especially younger high-risk physicians). Matsa (2007) finds no association between caps and overall supply levels when estimating difference-in-difference specifications; however, Matsa does find an increase in supply following damage cap

[7] Medical malpractice studies have also employed this methodological approach to answer other questions, such as whether physicians practice defensive medicine. See, for example, Kessler and McClellan (1996).

adoptions in the quartile of counties with the lowest population density. Encinosa and Hellinger (2005) likewise find an effect that is strongest among rural counties.

In a comprehensive investigation into the supply effects of the most recent wave of non-economic damages caps, Paik et al. (2016) apply a range of methodological approaches and fail to find the same pattern of results depicted in the above studies. They find no evidence that caps are associated with an increase in physician supply, whether in the aggregate, among high-risk physicians or among rural physicians. Other studies (see, for example, Yang et al. 2008 and Hyman et al. 2015) similarly find no evidence supportive of the conventional wisdom that liability pressure impacts physicians' decision regarding where and whether to practice.[8]

### *B. Evolution of Malpractice Standard Rules*

Rather than engaging in an abstract exercise of what a "reasonable" physician would do under the circumstances—i.e., the classical approach to setting liability standards in tort law more broadly—courts in the United States have generally deferred to customary market practices as the basis of determining the standards generally expected of physicians in malpractice cases. Where courts have differed, however, is with respect to which physicians one should look to in determining operable customs. Historically, courts followed a locality rule, whereby appropriate standards of care were based on local customs—i.e., the care that other physicians in the same area would customarily provide.

In the latter part of the 20th Century, a number of states abandoned locality rules in favor of requirements that physicians comply with national standards of care. Frakes (2013) reviewed both case and statutory law and documented the evolution of each state's malpractice standard-of-

[8] Though not drawing on the effects of tort reforms, Baicker and Chandra (2005) find a similarly weak relationship between malpractice forces and physician supply by instead drawing on variations in medical liability premiums. Other studies, however, continue to document some evidence consistent with the conventional wisdom. See, for instance, Kessler et al. (2005).

care laws from the mid-1970's to the present. Table 1 documents this legal evolution (replicating a corresponding table from Frakes, Frank and Seabury 2015). Specifically, the table identifies 17 states that amended their standard-of-care laws—along the national vs. non-national dimension—during the study period (16 of which indicate national-standard law adoptions).[9]

As discussed in the introduction, the intensity by which physicians practice medicine is known to be a function, in part, of geography, with some regions characterized by more intensive, surgical approaches to medicine and others characterized by approaches focused to a greater degree on medical management. When the geographical scope of liability standards changes—e.g., as a result of national-standard adoptions—one might therefore expect that the law begins to place different weight on the intensity of medical practices that it expects of physicians in the affected regions. These altered expectations, in turn, may bear on the attractiveness, or suitability, of surgical specialists practicing in those regions. We draw upon this evolution of liability standards in the U.S. to test this prediction in the analysis below.

For these purposes, we do not simply estimate the association between national-standard adoptions and prevailing rates of surgeons (unlike the reform specifications estimated in the damages cap papers discussed above). The impact of national-standard adoptions, after all, is likely to depend on the baseline—i.e., national standard reforms do not immediately map onto the intensive-versus-non-intensive spectrum. Some regions that switch from local to national-standard rules begin the sample with non-intensive approaches to medicine. It is in these regions where national standard adoptions may represent a change in clinical standards that may result in attracting more surgical specialists. Other regions experiencing the relevant reforms begin the

[9] Note that a few states take varying approaches to their standard-of-care requirements according to whether a physician is a general practitioner or a specialist. Following Frakes (2013), in our analysis we only use laws that either (1) apply to specialists only, or (2) take symmetrical approaches between general practitioners and specialists We also exclude Texas and Hawaii, as for these states there was conflicting case law that generated considerable uncertainty in the nature of their standard-of-care requirements. The results of this study are entirely robust to the assumption that Texas follows a national-standard law throughout the entire sample period, arguably the most accurate reading of its case law.

sample with levels of practice intensity at the top of the across-county practice-intensity distribution. If anything, national-standard reforms in these areas might be expected to result in a shift in the prevailing physician mix away from surgical specialists to the extent that national-standard adoptions in these regions arguably amount to the law condoning less-intensive clinical approaches. Accordingly, to map national-standard reforms onto a spectrum of the intensity of medical practices expected under the law, our empirical specifications effectively estimate differential impacts of national-standard reforms across counties depending on the relative county's placement in the initial practice-intensity distribution.

A causal interpretation of any estimated association between national standard rule adoptions and physician supply patterns may be compromised to the extent that national-standard adoptions are enacted in response to trends in physician supply. A recent analysis by Frakes and Jena (2016) largely appeases these concerns through a qualitative review of the circumstances surrounding these reforms. Frakes and Jena first point out that, given the critical nature of the local-versus-national standard distinction, each reforming state reserved this decision for the state's high court, as opposed to the legislature and as opposed to allowing the reform to grow organically via lower state court decisions. When deciding to abdicate the locality rule, each state high court made this decision while ruling upon the equities of a specific case, which itself had dealt with only a particular clinical context—e.g., the use of a local versus a national customary standard in dosing a particular anesthetic during childbirth. Accordingly, though the abdication held precedential value for all future clinical contexts—whether or not related to anesthetic dosing—the judges were likely primarily subjected to evidence pertaining to those single clinical contexts at issue in the respective abdication cases. Frakes and Jena conclude that these facts are

encouraging in that they suggest that national-standard adoption decisions were likely orthogonal to the broad-based trends under investigation in analyses such as the present.

Despite the fact that courts made these generally precedential decisions in the face of specific clinical contexts, Frakes and Jena (2016) acknowledge that judges could nonetheless have been provided with evidence on prevailing health care trends at large through information provided via briefs submitted by the litigants or by third parties. Frakes and Jena addressed this final concern by searching historical litigation records, finding no evidence to suggest that third parties submitted amicus briefs of this nature in the relevant abdication cases. As such, the circumstances surrounding liability standard reforms raises fewer concerns over external interest group pressures relative to reforms that are more legislatively based, such as non-economic damage caps. Frakes and Jena also reviewed the available briefs filed by the parties themselves, which suggest that the judges were driven by broader principles of equity, as opposed to recent marketplace trends.

Accordingly, one may plausibly view national-standard adoptions as exogenous events to test predictions that changes in liability standards that expect physicians to follow more intensive practice styles will, in turn, lead to relatively more physicians practicing in that jurisdiction that specialize in intensive approaches. Despite any alleviation of concerns over endogeneity that ensue from this qualitative analysis, in our empirical analysis below, we likewise estimate dynamic, event-study specifications that test more explicitly for markers that might be indicative of any violation of the parallel-trends assumption underlying difference-in-difference designs. As a final exercise in demonstrating the likely exogeneity of national-standard rule adoptions, we divide counties in quintiles based on their practice intensities (surgery rates) in the first year of our sample. As demonstrated by Table A1 of the Online Appendix, we find that treatment counties—

counties experiencing a locality rule abdication at some point over the same period—are evenly distributed across these initial-practice-intensity quintiles.

## II. Data and Empirical Specification

For our analysis, we use data on the number of physicians, the intensity of clinical practice and local area characteristics from the Area Health Resource File (AHRF, formerly the ARF). The AHRF physician 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 has compiled these into aggregate counts of physicians to the county level. The physician counts are broken down by specialty, although the information is missing for some intermittent years. We use linear projections to impute data for missing years. The AHRF also includes data on several sources of demographic and other information relevant for our study, including county-by-year level data on per-capita income, percent white/black, percent Hispanic origin, percent over 65 years of age, population density (population per square mile) and the percent falling into various education categories. In addition to the AHRF, we use as covariates data on other tort reforms from Ronen Avraham's Database of State Tort Law Reforms (DSTLR), 4th Edition (Avraham, 2012).[10]

We merge in information on malpractice standards rules and tort reforms at the state-year level to our county-by-year level data on physician population and demographics. We focus on the time frame explored in Frakes (2013)—i.e., 1977-2005—given that this is time period over which we have information on the evolution of liability standard rules. Table 2 provides summary statistics for the key variables discussed below. The mean number of physicians per 1,000 residents in the sample is roughly 1.6. The mean ratio of non-surgical specialists to total physicians

[10] The specific reforms we use include noneconomic damage caps, punitive damage caps, collateral source rule reform, joint and several liability reform and caps on attorney contingency fees.

is approximately 0.73. We elect to construct the clinical mix ratio with non-surgeons in the numerator as opposed to surgeons to decrease the incidence of zero-value observations at the county-year level and thereby allow for better behaved estimation in the difference-in-difference specifications.

With these data, we estimate the impact of switching from local to national standards in medical malpractice cases on the percentage of physicians practicing in a county that are not surgical specialists, along with the impact on the total number of physicians in the affected area.[11] As above, to translate national-standard reforms into reforms that expect physicians to practice more (less) intensively, we differentially estimate the impact of reforms depending on the respective county's initial placement along the distribution of surgery rates across counties, using information on county-year surgery counts (inpatient and outpatient) also provided by the AHRF.[12]

We implement our approach using the following empirical specification:

$$(1) \qquad Y_{c,t} = \alpha + \lambda_t + \delta_c + \beta_1 NS_{s,t} + \beta_2 HI_c + \beta_3\left(NS_{s,t} \times HI_c\right) + \beta_4 X_{c,t} + [\varphi_s t] + \varepsilon_{c,t}$$

where $c$ indexes county, $t$ indexes year, and s indexes state.; $NS_{s,t}$ represents an indicator for a national-standard law (note the use of the subscript $s$, as the standard law evolves on a state-year basis); $X_{c,t}$ contains additional county-year factors (see above);[13] $\lambda_t$ represent year effects which control for fixed differences across years and $\delta_c$ represent county effects which control for fixed differences across counties. In some robustness checks, we include state-specific linear time trends, $\varphi_s t$, which account for slowly-moving correlations between physician mix in a state and

[11] For these purposes, we focus on active, non-federal physicians.
[12] The AHRF collects these data from information provided in the American Hospital Association (AHA) annual survey. To form surgery rates, we follow Frakes, Frank and Seabury (2015) and normalize the number of observed surgeries by the number of physicians in the county. This allows us to capture the average practice style of a given physician operating in that region, which is what we are hoping to achieve in characterizing a region's baseline clinical style. Nonetheless, the findings do not differ substantially when instead normalizing by the county resident population.
[13] These characteristics include the percent of the population over age 65, percent non-white, percent Hispanic origin, average per-capital household income, the number of hospital beds per population, the percent of county residents falling into various education categories and the population density, as well as dummy indicators for the tort reform variables described above.

the adoption of a national-standard law. In our main approach to differentiating counties by initial practice intensity levels, we follow Frakes (2013) and Frakes, Frank and Seabury (2015) and focus separately on counties with initially above- and below-average treatment intensity rates—to this effect, $HI_c$ indicates that an initially high treatment-intensity county.[14] The term $\left(NS_{s,t} \times HI_c\right)$ represents the interaction between initially high-intensity areas and the national standard laws. In alternative specifications discussed below, we retreat from this binary treatment of the distribution of initial practice intensities and instead interact the national-standard reform indicator with dummy variables capturing quintiles of the initial practice-intensity distribution across counties.

There are two primary coefficients of interest. The term $\beta_1$ identifies the extent to which the adoption of a national-standard law is associated with a change in the relevant outcome variable for those areas that began the sample with initially low-intensity practice styles—i.e., those with $HI = 0$. The term $\beta_3$ identifies how the effect of the standard differs in areas that are initially high intensity. The sum $\beta_1 + \beta_3$ represents the combined effect of national standards in the initially high intensity areas. In other words, this interaction specification allows us to explore the effects of national standard adoptions separately on: (1) those areas with initially low practice intensities and with respect to which a national standard adoption arguably entails a heightening of the practice intensity expected of medical practices and (2) those areas with initially high practice intensities and with respect to which a national standard adoption arguably entails a reduction in the practice intensity expected of medical practices.

[14] The variable $HI_c$ drops out of the models with county fixed effects. Following Frakes and Jena (2016), we allocate each treatment county to the relevant group—initially-above or initially-below the national average—based on its relation to the national average in the several-year period leading up to the reform, as opposed to the actual beginning of the sample period. For treatment states that experience reforms in the middle of the sample period, this approach ensures that we accurately characterize the nature of the reform bearing on that state at the time of the law change. We note, however, that the results do not differ when instead determining initially high and low intensity treatment status based on treatment intensity data in 1977—i.e., the first sample year. We note that the results of this exercise are robust to an alternative framing of county-level practice intensities based on the initial allocation of surgeons and non-surgeons within each county as opposed to the initial rates of surgery applied in the respective county (results available upon request).

We test several versions of this model to check the sensitivity and highlight different results. We vary the inclusion of covariates, fixed effects and time trends to determine whether other confounders are possibly driving the results. We also stratify the results according to the population density of the county to determine whether the effects vary in urban or rural areas. We also estimate dynamic versions of this model to test for the presence of leads (i.e., pre-existing trends) or lags in the effect. In all cases we log the dependent variable, so the effect of the standard rules (which are dummy variables) can be interpreted in percentage terms. Also, for all regressions we produce variance estimates that are adjusted to allow for clustering at the state level.

In Table 3, we demonstrate the covariate balance within this design. The key component of this balance analysis is depicted in Column 1 of Panel A, whereby we (1) start by forming county-year predictions of the proportion of total physicians in the region that are non-surgeons, based on the results of a regression of actual county-year non-surgeon proportions on the set of county-year covariates and (2) then estimate the above specified difference-in-difference specification using the natural log of this predicted non-surgeon proportion as the dependent variable (leaving out the set of covariates from the regression). Since the predicted physician composition mix effectively characterizes the collective influence of the covariates, this approach offers an omnibus means of assessing covariate balance. The results suggest a substantial degree of balance, with national standard adoptions (whether in initially high or low practice intensity areas) having a near 0 log-point relationship with predicted values of the physician clinical mix. The remainder of Table 3 shows corresponding results when focusing on each individual covariate separately; though with greater noise in each individual case, these separate regressions support the conclusion that national-standard law adoptions do not appear to be related to various county-year demographic characteristics.

One methodological challenge for our analysis stems from the fact that there are many counties within the data that do not have a hospital at all (not surprisingly, these are concentrated largely in rural areas). Some have no hospital presence early in the sample but add a hospital in the later sample years. Including all counties in our specifications may compromise our ability to isolate physician recruitment and physician location decision mechanisms insofar as the lack of a hospital in the first place may leave little room for such mechanisms to operate. With this consideration in mind, we follow the same approach taken in Frakes, Frank and Seabury (2015) and estimate specifications that focus on those counties that on average over the sample period have at least some meaningful hospital presence, which we capture by having at least a mean of 50 hospital beds over the sample. By conditioning our analysis on some baseline level of inpatient facility supply, we may be able to identify a more continuous relationship between liability standards and the physician composition mix.[15]

## III. Results

### *A. Primary Results: Impacts of National-Standard Reforms on Physician Composition Mix*

In Table 4, we estimate the interaction specification indicated in equation (1) and explore whether changes in liability standards that expect physicians to practice more (less) intensively are associated with a shift towards relatively more (less) surgical specialists practicing in the affected regions. The coefficient of the national-standard-law indicator in this specification can be interpreted as the association between national standard law adoptions in initially low practice intensity counties and the proportion of physicians that are non-surgical specialists (among all non-federal patient-care physicians). In the naïve difference-in-difference specification with no controls (only county and year fixed effects), we estimate a 2.4 log-point reduction in the share of

[15] In the Online Appendix, we present results without making any such restrictions altogether. These alternative tables demonstrate that this sample selection choice has little to no consequence on the results.

non-surgeons, indicating a relative shift towards more surgical specialists as the affected counties experience a change in the law that calls upon physicians to follow more intensive practice styles. This estimate does not change as we include other tort reform measures (e.g., damage caps) and county-year covariates. The estimate falls slightly in absolute terms to -2.1 log points when including state-specific linear time trends.[16]

The impacts of national-standard law adoptions in initially high intensity counties arguably allows us to explore how physician supply patterns change when the negligence standard is altered so as to condone (or expect) the delivery of less intensive practices. To observe this effect from the interaction specification, one need only add the coefficient of the national standard indicator to the coefficient of the interaction variable. In the final row, we indicative the p-value corresponding to this sum (derived from an analogous specification that interacts the national-standard indicator with an indicator for an initially low-intensity county). Generally, the point estimate of the relevant sum is close to zero, and the estimate is statistically indistinguishable from zero. Only in the case of the inclusion of state-specific linear time trends (which we intend more as a robustness check than our primary specification), is the sum positive and significantly different from zero. As such, the evidence appears to suggest that changes in liability standards have a one-sided effect only. Increasing the intensity of practices expected of physicians may be associated with a compositional shift towards more specialists; however, once a region has already welcomed surgical specialists, a change in the negligence standard that leads to arguably less favorable treatment of high intensity practices, there does not appear to be a retreat from this presence of surgeons.

[16] The results are not sensitive to the use of logs of the dependent variable. When using the level of the non-surgeon proportion, we estimate a statistically significant 1.4 to 1.7 percentage point reduction in the non-surgeon proportion following national standard rule adoptions in initially low-intensity counties.

All told, this evidence suggests that there may be a fundamental asymmetry in the effect of the law—in low-intensity areas a national standard rule adoption attracts more surgeons, while in high-intensity areas it leads to fewer surgeries (Frakes, Frank and Seabury 2015) but has no real impact on the number of surgeons. There are a number of reasons why this might be the case. Perhaps it is relatively easier for a surgeon to take a less aggressive approach and perform fewer surgeries when the law is modified so as to expect less intensive approaches than it is for non-surgeons to perform more surgical interventions. Or perhaps there is relatively more unused capacity among surgeons, so it is easier for them to adjust practice styles or the volume of patients they treat.

### *B. Additional Specification Checks*

In the Online Appendix, we move beyond this binary treatment of initially high and initially low intensity regions and consider a more fine-grained response to changes in the liability standard that map onto changes in expected practice intensity. More specifically, we interact the national standard rule indicator with dummy variables representing each quintile of initial county-level practice intensities. This exercise suggests that the biggest swing in the compositional mix of physicians in the direction of surgical specialists occurs in those counties with the lowest initial practice intensities—i.e., for those counties arguably experiencing the largest swing in liability expectations. The estimated impacted of national standard adoptions weakens somewhat (in absolute terms) in the second quintile, falling by roughly 1.2 log points. By the time we reach the 3$^{rd}$, 4$^{th}$ and 5$^{th}$ quintiles, the estimated relationship between national-standard laws and the physician composition mix levels out and is nearly zero in magnitude.

In Figures 1 and 2, we present results from a dynamic, event-study counterpart to the primary specification discussed above. For these purposes, instead of estimating a single

interaction specification, we separately estimate the impact of national-standard-rule adoptions on the initially low-intensity samples and the initially-high-intensity samples. In each case, we include a number of leads and lags of the national-standard-rule adoption indicator and plot the coefficients of these leads and lags (doing so in a specification that includes county and year fixed effects, county-year coefficients and other state-year tort reforms). These coefficients allow us to trace out the differential non-surgeon-to-total-physician rate between treatment counties and control counties in the years leading up to and subsequent to the abandonment of a locality rule and adoption of a national-standard rule. Each coefficient is meant to reflect this differential relative to the reference period characterized by the time preceding the fourth year prior to the reform.

As demonstrated by Figure 1, we do not observe a discernable trend prior to the national-standard adoption which might otherwise call into question the parallel trends assumption underlying our difference-in-difference approach. There is a small spike in the non-surgeon proportion differential 4 years prior to the reform, but the differential remains flat in the preceding three years. Collectively, an F-test on the set of lead coefficients falls to reject the hypothesis that they are all equal to zero. At the time of the reform itself, the differential in the non-surgeon rate between treatment and control states falls by over 2.5 log points—reflecting a compositional shift towards surgical specialists—and remains at roughly this level over the ensuing years. The lack of a pre-existing trend prior to the reforms also alleviates concerns that we are merely picking up mean reversion in physician composition mixes. Further alleviating these concerns, we note that the shift in physician composition we observe in the year following the reform is over 7 times the magnitude of the average annual change in the percentage of non-surgeons among physicians over the full sample (focusing on initially low-intensity control counties; we depict this control trend in

Figure A1 in the Online Appendix). In other words, while there may be a general trend towards relatively more surgical specialists among initially low-intensity counties, this trend accelerates notably upon the adoption of negligence-standard reforms calling for regional convergence in liability standards.

In Figure 2, we present a corresponding event-study graph focusing on initially high-intensity counties—i.e., counties with respect to which national-standard reforms amount to a change in the law in which courts will condone or expect less intensive practice styles. The interaction specifications estimated above suggested little impact of national-standard adoptions on the physician composition of these counties, implying that the negligence standard may work more as a one-way ratchet on the surgical specialist mix of the physician population. Figure 2 confirms this conclusion, demonstrating on a year-by-year basis little relationship between physician composition and national-standard-rule adoptions among this group of counties.

### *C. Total Physician Concentration Rate*

In Table 5, we move beyond an investigation into the compositional mix of physicians in a region—i.e, the balance between surgical specialists and other physicians—and now estimate the impact of changing negligence standards on total physician concentration rates (county-year counts of patient-care non-federal physicians per county-year population). We take the same approach here as in Table 4 and interact national-standard adoptions with initial practice intensities of counties to allow us to ask what happens separately when we change standards to expect more intensive styles and when we change standards to expect less intensive approaches. In general, we find no evidence that changing expectations of clinical practices by switching to a national standard deters physicians—in the aggregate—from practicing in an area. That is, despite conventional fears to the contrary, we find no evidence of physician flight stemming from locality

rule abdications. None of the coefficients on national standards indicate a statistically significant reduction in the number of physicians practicing in an area. If anything, as we increase the demands on initially low intensity areas there may be some attraction of physicians (though the positive coefficients are generally not statistically significant). Moreover, we also do not find strong evidence of a change in physician concentration rates when national-standard laws are adopted in initially high intensity practice areas. Again, if anything, the results might suggest an attraction of physicians to such regions following the abandonment of locality rules.

Above, we found a shift towards relatively more surgeons and relatively fewer non-surgeons in regions when the law changes so as to expect more intensive standards. Is this change in the relative mix accomplished by a substitution effect or from a net addition of more surgeons to a region? It is perhaps difficult to say from the results depicted in Table 5. Though the findings do not document a statistically significant change in overall physician rates, the estimates are somewhat noisy.

*D. Results by Population Density Quartile*

In the physician supply and malpractice literature, there is a general perception that the sensitivity to malpractice pressure differs across geographic areas. Matsa (2007) found evidence of this and argued that it is due to rural physicians facing more uninsured malpractice costs and more elastic demand for health care. If physicians in rural areas are indeed more susceptible to malpractice pressure, we might expect that the effect of standards would be different in rural areas. There are other reasons to consider the effect of changes in the negligence standard on rural and urban areas separately. Much of the historical discussion surrounding the need for the locality rule emphasized its rationale within rural areas—suggesting, for example, that it might be impractical to expect rural areas to follow practice styles elsewhere. Moreover, one might presume that

practices in urban areas are heavily shaped by so many influences—competitive forces, information networks, etc.—that arguably may be more attenuated in rural environments. If this is the case, physicians may place greater weight on whatever signals are provided via the malpractice system regarding proper clinical practices (liability fears themselves aside) in rural regions. Altogether, these different factors suggest that we would expect the impact of a switch to national standards to be more pronounced in rural areas.

To test this, we follow Matsa and estimate our physician composition mix specifications for counties in each quartile of population density. We report these results in Table 6. Each column reports the results of a separate regression for each county population-density quartile. For the purposes of brevity, we show only the results of our primary specification, which includes county-year covariates, other state-year tort reforms and county and year fixed effects (though the results are not sensitive to the various specifications depicted throughout the columns of Table 3). Generally, the findings appear to support the notion that standards are more binding in rural areas. The results across population density quartiles are similar to the overall results, with a shift towards fewer non-surgeons as initially low-intensity areas adopt national standard rules. However, the relationship is indeed most pronounced in rural counties, with national standards being associated with an over 6 percent decrease in the proportion of physicians that are non-surgeons in initially low-intensity counties that fall in the bottom population-density quartile.[17] As before, the effect appears to be one sided, with the impact of national standard laws on initially high intensity counties (the sum of the 2 indicated coefficients) being near zero across each quartile.

In the Online Appendix, we replicate the event study analyses depicted in Figures 1 and 2 separately for each population density quartile. When focusing on the most rural counties that

[17] This stronger effect in rural counties is consistent with Frakes, Frank and Seabury (2015) which found stronger impacts in rural regions on patterns of surgery rates.

begin with initially low practice intensities, we again see that the shift in the physician mix away from non-surgeons does not materialize until after the abandonment of the locality rule and the adoption of a national standard rule, with a roughly 6.1 log-point fall in the differential in the non-surgeon proportion between treatment and control counties occurring with a 1-year lag following the change in the law. There is no evidence of this negative trend precipitating the reform itself. The figures for the remaining quartiles also generally suggest that the decline in the proportion of non-surgeons associated with the national standard reforms generally materialize at the time of or with a 1-year lag following the reform (though to a smaller degree than in the case of the most rural quartile).

### *E. Mechanisms of Response Behind Impacts of Reforms on Practice Patterns*

Frakes (2013) and Frakes, Frank and Seabury (2015) document a change in utilization of intensive medical treatments (e.g., cesarean sections, intensive cardiac interventions, overall rates of surgery) in connection with a shift from local to national standards. Can the swings in the physician composition mix documented in the present paper explain these previous treatment utilization findings? That is, are given physicians altering their practices in connection with changing standards or are observed treatment patterns explained by changes in the type of physicians practicing in an area? There is perhaps reason to believe that the findings from the previous national-standard studies reflect a great deal of the former explanation. For instance, as discussed at length above, national-standard adoptions in initially high-intensity regions do not appear to be associated with a change in the surgeon/non-surgeon mix, despite the fact that these prior studies found convergence in practice from both directions—i.e., that the rate of intensive treatments fell following reforms arguably calling for the delivery of less intensive practice styles. Even in the case of initially low-intensity regions, we note that the magnitude of the impacts of

reforms on surgery rates are higher in percentage terms relative to the percentage impacts on the rate of surgeons among the physician population. That is, Frakes, Frank, and Seabury (2015) report an 8-15 log-point increase in surgery rates following national standard reforms in initially low-intensity regions; whereas above we document only around a 2 log-point increase in the proportion of surgeons. Moreover, when we modify the specifications estimated in Frakes, Frank and Seabury (2015) to include controls for prevailing proportions of non-surgeons, the estimated relationship between national-standard reforms and surgery rates in initially low-intensity regions falls by only 1 log point.

As such, while changes in the negligence standard may indeed be associated with changes in the composition mix of physician supply, the bulk of the impact of such reforms may operate within existing means of supply.

## IV. Conclusion

Though the link between physician supply and medical malpractice law has been the subject of a considerable literature, scholars to date have largely overlooked a key aspect of malpractice law that may bear on this discussion—mainly, the negligence standard itself. The law places particular expectations on the type of practice that physicians must follow; and if physicians follow such standards, they may avoid liability altogether, even if their actions cause harm to patients. This fault-based approach to liability is at the core of the American system of tort law. Moreover, not only may this standard be used to help adjudicate disputes that arise, but it also has the potential to shape medical practices prospectively and even to shape the type of physicians attracted to the jurisdiction in the first instance. Those embracing a deterrent rationale for tort law, in fact, have this very goal in mind. Though the evidence put forth to date collectively suggests that treatment patterns themselves—taking as given the state of physician supply—are perhaps the

most sensitive to changes in the medical liability standard, the results of our present analysis suggest that the composition of physician supply may also respond. In particular, when standards change so as to favor more intensive clinical practices, we observe a relative shift towards more surgical specialists practicing in the affected region.

In discussing reforms aimed at bending the health care cost curve and improving patient access to care, policymakers frequently invoke the possibility of medical liability reform. Those in the past frequently turned to remedy-focused reforms, such as caps on non-economic damages awards. The evidence to date, however, has demonstrated uncertainty as to whether caps may be able to stem costs (Frakes 2015) and/or improve patient access. Legal scholars and policymakers, however, have long considered alternative reforms that focus less on the remedy side of liability and more on its substantive elements, including the manner in which liability standards are determined. Such alternatives include: (1) the establishment of liability safe harbors for physicians who comply with specified clinical practice guidelines (Mello 2001, Bovberg and Berenson 2012)[18] and (2) the establishment of specialized administrative health courts to resolve medical liability disputes, reform proposals that are frequently accompanied by proposals to shift from a custom-based standard of care to a broader avoidable medical-injury standard (Kachalia et al. 2008). Though national-standard rule adoptions are functionally distinct from these alternative proposals, our analysis can nonetheless be seen as demonstrating the general relevance of liability standards to physician supply decisions. As such, our findings build upon the existing damages-cap-focused literature by shedding light on the potential of reforms to the liability standard in shaping the specialty landscape of local physician supply chains. To the extent states (or even the

[18] Peter Orszag, former director of the Office of Management and Budget, argued for liability reforms of this nature in a series of opinion-editorial pieces. See for example, Malpractice Methodology, NY Times A39 (Oct 21, 2010); To Fix US Budget, Reform Medical-Malpractice Law, Bloomberg View (March 5, 2013); A Better Fix for Medical Malpractice, Bloomberg View (February 25, 2014).

federal government) proceed to experiment with alternative structures of this nature, future research may further illuminate their impact on physician supply.

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Figure 1. Impact of National-Standard Adoptions on Non-Surgeon Rates in Initially Low-Intensity Counties: Event-Study Analysis

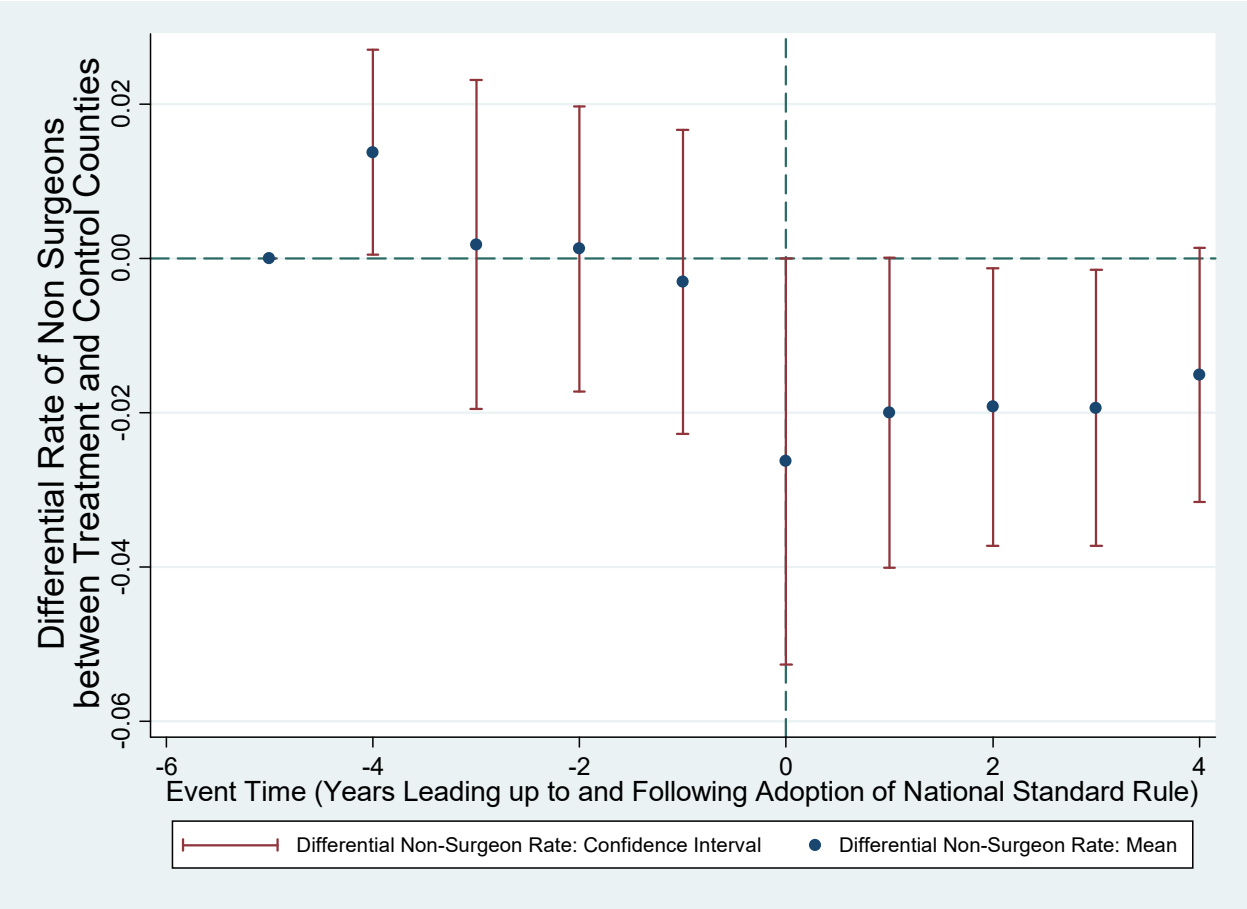

Figure 2. Impact of National-Standard Adoptions on Non-Surgeon Rates in Initially High-Intensity Counties: Event-Study Analysis

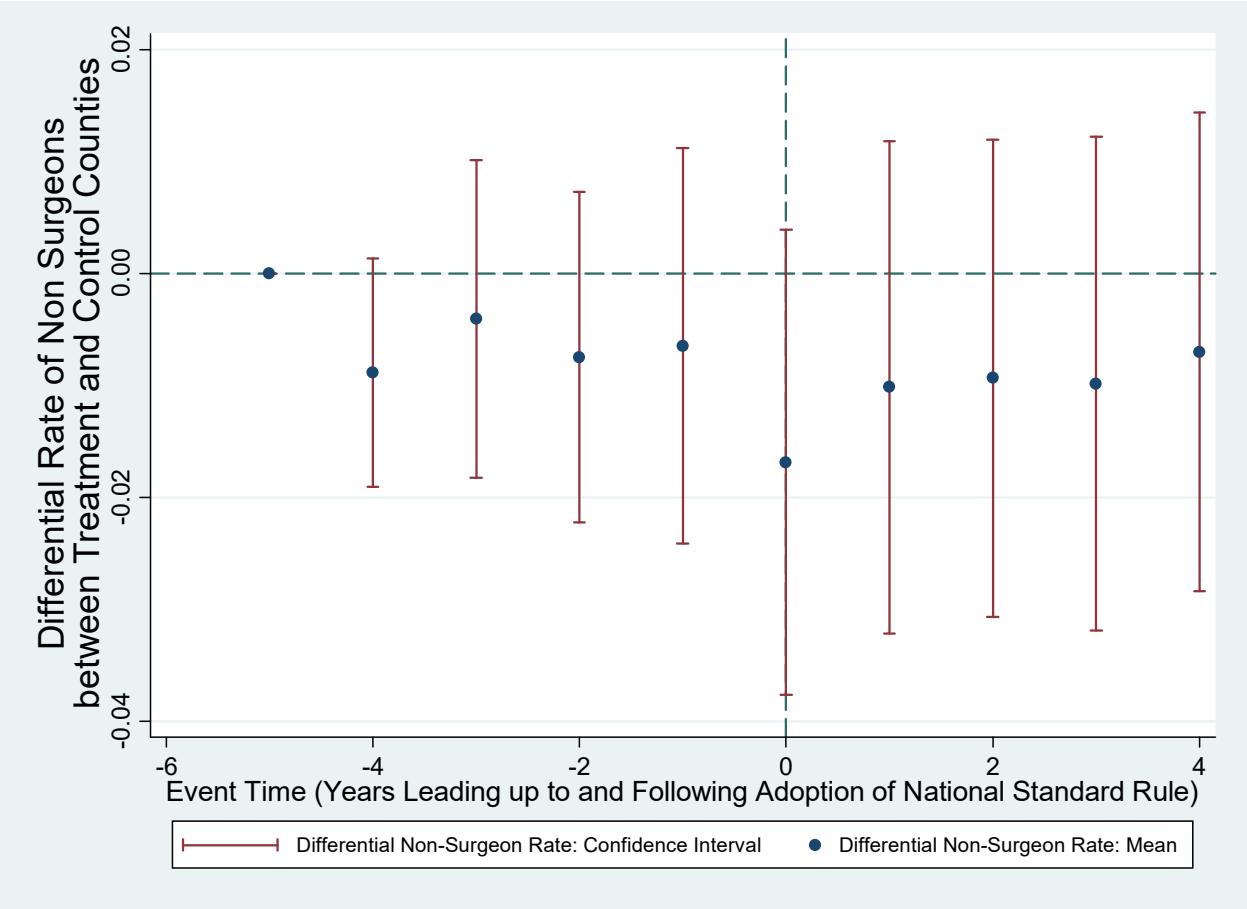

Table 1. Timing of Adoptions and Repeals of National Standard-of-Care Requirements (replicating Table 1 form Frakes, Frank and Seabury 2015)

| State | Year | Source(s) |
|---|---|---|
| Alabama | 1980 | Zellis v. Brown, 382 So. 2d 528 (March 1980).[1] |
| Colorado | 1983 | Green v. Thomas, 662 P.2d 491 (November 1982); Short v. Kinkade, 685 P.2d 210 (December 1983).[2] |
| Connecticut | 1984 | Logan v. Greenwich Hospital Association, 191 Conn. 282 (September 1983).[3] |
| Delaware | 1999 | 18 Del.C. § 6801 (amendment effective July 1998). |
| D.C. | 1980 | Morrison v. Macnamara, 407 A.2d 555 (October 1979). |
| Indiana | 1992 | Vergara v. Doan, 593 N.E. 2d 185 (June 1992). |
| Maryland | 1994 | Md Code Ann, [Cts&Jud Proc] §3-2A-02(c) (effective July 1993).[4] |
| Mississippi | 1983 | King v. Murphy, 424 So. 2d 547 (Nov. 1982); Hall v. Hilbun, 466 So. 2d 856 (Feb. 1985).[5] |
| Montana | 1985 | Aasheim v. Humberger, 215 Mont. 127 (February 1985). |
| Nevada | 1979 | Orcutt v. Miller, 595 P2d 1191 (June 1979). |
| New Mexico | 1978 | Pharmaseal Lab., Inc. v. Goffe, 90 N.M. 753 (September 1977). |
| Oklahoma | 1984 | 76 O.S. Supp. 1983 § 20.1 (effective September 1983). |
| Rhode Island | 1998 | Sheeley v. Memorial Hospital, 710 A. 2d 161 (April 1998). |
| South Carolina | 1981 | King v. Williams, 276 S.C. 478 (June 1981). |
| South Dakota | 1988 | Shamburger v. Behrens, 418 N.W.2d 299 (January 1988). |
| West Virginia | 1986 | Paintiff v. City of Parkersburg, 176 W. Va. 469 (March 1986); W. Va. Code § 55-7B-3 (effective 1986).[6] |
| Wyoming | 1981 | Vassos v. Roussalis, 625 P.2d 768 (March 1981).[7] |

Notes:

1. The Zellis decision was decided by a plurality and its holding did not turn directly on this geographical distinction. Nonetheless, the Supreme Court of Alabama in Zellis provided a strong, direct indication of their intention to abandon the use of a locality rule in Alabama. This stance was subsequently strengthened by the Court's decision in Bryant v. Otts, 412 So. 2d 254 (1982). The results are robust to the use of 1982 as the relevant date of adoption.

2. It was not until Jordan v. Bogner, 844 P.2d 664 (January 1993) when the Supreme Court of Colorado spoke definitively on the geographical scope of the standard of care owed by a specialist physician. However, several earlier cases, including those indicated, adopted requirements that specialist physicians are to be judged by a standard commensurate with that of a reasonable physician practicing in that specialty. This approach did not limit the standard to particular geographical bounds, a fact emphasized by subsequent case law. In describing the standard to be applied to non-specialist physicians, Colorado case law continued to refer to the use of a community standard. As such, in indicating that specialists are not subject to a locality rule, the Supreme Court in the 1993 Bogner decision cited these earlier decisions as the applicable law and did not indicate that it was adopting a new approach. For these reasons, I use these earlier rulings, Green and Kinkade, as the turning point in Colorado's national-standard requirement for specialist physicians. The findings presented however are robust to the alternative use of 1993 as the year of adoption and to the exclusion of Colorado entirely.

3. This position was subsequently codified in Conn. Gen. Stat. § 52-184c.

4. Maryland had, prior to this time, adopted a national-standard requirement in Shilkret v. The Annapolis Emergency Hospital Association, 276 Md. 187 (October 1975).

5. King expanded the geographical scope of the standard-of-care requirements to include at least the entire state of Mississippi plus a reasonable distance beyond the boundaries of the state. For the purposes of this empirical analysis (structured around state-year cells) I consider this breaking of state boundaries as an abandonment of the locality rule. However, the findings are robust to the use of 1985 as the relevant national-standard adoption year, at which time the court fully embraced a national standard in Hilbun.

6. With respect to specialists, the abolition of the substantive components of the locality rule in West Virginia may arguably be seen as having begun years before with the West Virginia Supreme Court's decision in Hundley v Martinez, 151 W. Va. 977 (1967). The presented results are robust to the exclusion of West Virginia.

7. In Vassos, the Wyoming Supreme Court stated that "a physician or surgeon must exercise the skill, diligence and knowledge, and must apply the means and methods, which would reasonably be exercised and applied under similar circumstances by members of his profession in good standing and in the same line of practice… The skill, diligence, knowledge, means and methods are not those 'ordinarily' or 'generally' or 'customarily' exercised or applied, but are those that are 'reasonably' exercised or applied." Subsequent case law viewed this 1981 decision as the turning point in the abandonment of the locality rule. Wyoming subsequently codified the use of a national standard in 1986.

Table 2. Summary Statistics for Key Variables

| | *Mean (Standard Deviation)* |
|---|---|
| Physicians Per 1,000 Population | 1.56<br>(0.83) |
| Proportion of Non-Surgical Specialists (among Total Physicians) | 0.73<br>(0.06) |
| National Standard Rule | 0.64<br>(0.48) |

Note: Statistics are from a sample of county-year records from the Area Health Resource File and covers the years 1977-2005.

Table 3. Covariate Balance: Relationship between National Standard Adoptions and Covariates, Separately for Initially High- and Low-Intensity Counties

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Panel A. 1st set of Covariates** | Omnibus Covariate: Predicted Proportion of Non-Surgeons (Logged) | Percent Black (Logged) | Percent Hispanic (Logged) | Percent Over-65 (Logged) | Real Income Per Capita (Logged) |
| National-Standard (NS) Law Dummy | -0.000 | 0.021 | 0.039 | 0.023 | 0.051 |
| | (0.003) | (0.062) | (0.070) | (0.025) | (0.030) |
| NS Law * Above-Average County | -0.001 | -0.062 | -0.080 | 0.021 | -0.089*** |
| | (0.003) | (0.047) | (0.103) | (0.020) | (0.030) |
| **Panel B. 2nd Set of Covariates** | Population Density (Logged) | Education Group 1 (Less than 9th Grade, Logged) | Education Group 2 (some High School, Logged) | Education Group 3 (some College, Logged) | |
| National-Standard (NS) Law Dummy | -0.024 | -0.046 | -0.000 | -0.005 | |
| | (0.049) | (0.077) | (0.022) | (0.025) | |
| NS Law * Above-Average County | -0.021 | -0.042 | 0.023 | -0.025 | |
| | (0.045) | (0.037) | (0.015) | (0.022) | |

Note: Each column in each panel represents results from a regression of the indicated dependent variable on a dummy variable indicating the incidence of a national-standard law along with the interaction of that indicator with another dummy variable indicating whether the relevant county is an initially above-average-intensity county. Regressions include county and year fixed effects. Data are at the county-year level. An above-average county is defined as a county that initially had higher than average utilization of inpatient and outpatient surgical procedures (coefficient dropped with inclusion of county fixed effects). Regressions are weighted by the size of the relevant county-year cell (by population). Robust standard errors are reported in parentheses, adjusted to allow for correlation (clustering) at the state level. A *, ** or ***, indicates statistical significance at the 10%, 5% or 1% level, respectively.

Table 4. Relationship between National-Standard Laws and Proportion of Physicians that are Not Surgical Specialists (Logged), Separately for Initially High- and Low-Intensity Counties

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| National-Standard (NS) Law Dummy | -0.024** | -0.023** | -0.024** | -0.021** |
| | (0.010) | (0.009) | (0.010) | (0.008) |
| NS Law * Above-Average County | 0.016 | 0.019* | 0.020* | 0.035*** |
| | (0.012) | (0.011) | (0.011) | (0.011) |
| P-value of sum of above 2 coefficients | 0.351 | 0.649 | 0.597 | 0.037 |
| County-Year Covariates | NO | YES | YES | YES |
| Covariate Tort Reforms | NO | NO | YES | YES |
| State-Specific Linear Time Trends | NO | NO | NO | YES |

Note: Table reports the difference-in-differences estimates of the impact of national standards laws on the proportion of physicians that are not surgical specialists (logged) among the physicians in the respective county-year cell. Data are at the county-year level. Regressions include county and year fixed effects. An above-average county is defined as a county that initially had higher than average utilization of inpatient and outpatient surgical procedures (coefficient dropped with inclusion of county fixed effects). County-year covariates include the percent of the population over age 65, percent non-white, percent Hispanic origin, average per-capital household income, the number of hospital beds per population, the percent of county residents falling into various education groups, and the population density. Robust standard errors are reported in parentheses, adjusted to allow for correlation (clustering) at the state level. A *, ** or ***, indicates statistical significance at the 10%, 5% or 1% level, respectively.

Table 5. Relationship between National-Standard Laws and Physician Concentration Rate (Logged), Separately for Initially High- and Low-Intensity Counties

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| National-Standard (NS) Law Dummy | 0.067* | 0.030 | 0.020 | 0.028 |
| | (0.039) | (0.043) | (0.045) | (0.031) |
| NS Law * Above-Average County | -0.029 | 0.015 | 0.033 | -0.019 |
| | (0.032) | (0.041) | (0.048) | (0.030) |
| P-value of sum of above 2 coefficients | 0.254 | 0.077 | 0.118 | 0.404 |
| County-Year Covariates | NO | YES | YES | YES |
| Covariate Tort Reforms | NO | NO | YES | YES |
| State-Specific Linear Time Trends | NO | NO | NO | YES |

Note: Table reports the difference-in-differences estimates of the impact of national standards laws on the aggregate number of physicians per capita (logged). Data are at the county-year level. Regressions include county and year fixed effects. An above-average county is defined as a county that initially had higher than average utilization of inpatient and outpatient surgical procedures (coefficient dropped with inclusion of county fixed effects). County-year covariates include the percent of the population over age 65, percent non-white, percent Hispanic origin, average per-capital household income, the number of hospital beds per population, the percent of county residents falling into various education groups, and the population density. Robust standard errors are reported in parentheses, adjusted to allow for correlation (clustering) at the state level. A *, ** or ***, indicates statistical significance at the 10%, 5% or 1% level, respectively.

Table 6. Relationship between National-Standard Laws and the Proportion of Physicians that are Not Surgical Specialists (Logged), By Population Density Quartile

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 1ST POPULATION DENSITY QUARTILE (RURAL) | 2ND POPULATION DENSITY QUARTILE | 3RD POPULATION DENSITY QUARTILE | 4TH POPULATION DENSITY QUARTILE (URBAN) |
| National-Standard (NS) Law Dummy | -0.062* | -0.019 | -0.032 | -0.021** |
| | (0.033) | (0.013) | (0.024) | (0.009) |
| NS Law * Above-Average County | 0.075*** | 0.015 | 0.020 | 0.017 |
| | (0.025) | (0.038) | (0.026) | (0.011) |
| P-value of sum of above 2 coefficients | 0.241 | 0.884 | 0.234 | 0.682 |

Note: Table reports the difference-in-differences estimates of the impact of national standards laws on the proportion of physicians that are not surgical specialists (logged) among the physicians in the respective county-year cell. Data are at the county-year level. Regressions include county and year fixed effects and county-year controls for the percent of the population over age 65, percent non-white, percent Hispanic origin, average per-capital household income, the number of hospital beds per population, the percent of county residents falling into various education groups, and the population density. An above-average county is defined as a county that initially had higher than average utilization of inpatient and outpatient surgical procedures (coefficient dropped with inclusion of county fixed effects). Robust standard errors are reported in parentheses, adjusted to allow for correlation (clustering) at the state level. A *, ** or ***, indicates statistical significance at the 10%, 5% or 1% level, respectively.