Impact of Malpractice Reforms on the Supply of Physician Services

Daniel P. Kessler, PhD, JD
William M. Sage, MD, JD
David J. Becker, BA

Determines and debates about medical malpractice have recurring themes, with tort reformers emphasizing the threat that liability crises pose to the cost and availability of medical services and tort defenders emphasizing the importance of liability to medical quality. Effects on access to health care are of particular concern during “malpractice crises,” when rising liability insurance premiums and uncertain coverage are said to induce physicians to avoid high-risk patients or procedures, relocate to other communities, or leave practice altogether. Even between such crises, however, malpractice climate remains one of many factors determining how many physicians enter the medical profession, what specialties they choose, and where they practice.

We investigated whether and how liability pressure affects long-term trends in physician supply from state to state. We used data from the American Medical Association’s Physician Masterfile on the number of physicians in active practice in each state for each year from 1985 through 2001. We modeled the number of physicians in a state at a point in time as a function of state fixed effects, time fixed effects, time-varying state characteristics, and the presence or absence of certain malpractice reforms. We divided liability-reducing malpractice reforms into 2 types: reforms that directly reduce expected malpractice awards and reforms that reduce awards only indirectly. We estimated the simple average effect of liability-reducing reforms on physician supply. We also estimated how the effect of reforms varies over time, across different health care markets, and for different types of physicians.

METHODS
We modeled the determinants of the supply of physician services in the United States from 1985 to 2001. In each state s in year t, we measured supply by the total number of physicians in the state and by the number of physicians with 20 or more vs less than 20 years of experience (defined as the difference between the current year and year of graduation from medical school). We began our analysis in 1985, and we omitted 1990 from our analysis because physician-level data were unavailable for years before 1985 and for 1990. In addition, we modeled the decisions of 2 subpopulations of physicians whose supply decisions are likely to be particularly sensitive to malpractice pressure. First, we measured the supply of physicians in nongroup practice settings because these physicians may bear a greater share of at least the financial burden of malpractice.

Context Proponents of restrictions on malpractice lawsuits claim that tort reform will improve access to medical care.

Objective To estimate the effects of changes in state malpractice law on the supply of physicians.

Design Differences-in-differences regression analysis that matched data on the number of physicians in each state between 1985 and 2001 from the American Medical Association’s Physician Masterfile with data on state tort laws and state demographic, political, population, and health care market characteristics.

Main Outcome Measure Effect on physician supply of “direct” malpractice reforms that reduce the size of awards (eg, caps on damages).

Results The adoption of “direct” malpractice reforms led to greater growth in the overall supply of physicians. Three years after adoption, direct reforms increased physician supply by 3.3%, controlling for fixed differences across states, population, states’ health care market and political characteristics, and other differences in malpractice law. Direct reforms had a larger effect on the supply of nongroup vs group physicians, on the supply of most (but not all) specialties with high malpractice insurance premiums, on states with high levels of managed care, and on supply through retirements and entries than through the propensity of physicians to move between states. Direct reforms had similar effects on less experienced and more experienced physicians.

Conclusion Tort reform increased physician supply. Further research is needed to determine whether reform-induced increases in physician supply benefited patients.

See also pp 2609 and 2660.
pressure. The nongroup designation excluded physicians who reported that they were members of a group, were members of a health maintenance organization, were hospital-based, or were in public-sector practice but included physicians who reported that they were members of a partnership. Second, we measured the supply of physicians in 5 “high-risk” specialties: obstetrics/gynecology, surgery (including surgical subspecialties), anesthesiology, emergency medicine, and radiology. These specialties paid the highest reported malpractice premiums in 1994, a year in the middle of our study period for which comprehensive premium data by specialty were available.

We modeled the supply of care as a function of state and year fixed effects ($\alpha$ and $\theta$), the natural log of the population of state $s$ in year $t$ ($P_s$), the political parties of the governor and each house of the legislature of state $s$ in year $t$ ($W_s$), the number of residency programs and the number of residents per capita in state $s$ in year $t$ ($R_{st}$ and $R_{2st}$, respectively, with $R_s$ defined as the 2-element vector containing $R_{1st}$ and $R_{2st}$), managed care enrollment per capita in state $s$ at year $t$ ($M_s$), and state malpractice laws ($L_s$).

Our models identified the effect of state malpractice laws by comparing the change in physician supply in states that altered their laws between 1986 and 2001 to the change in supply in states that did not. As in previous work by one of us (D.P.K.), this involved using differences-in-differences between reforming and nonreforming states to identify effects.

Our differences-in-differences approach has advantages and disadvantages. By identifying the effect of interest based only on states that changed their laws between 1986 and 2001, we can control completely for fixed differences between states and for national trends that affect all states, as well as for the time-varying characteristics of states affecting physician supply that are most likely to be correlated with states’ propensity to adopt legal reforms. However, we cannot assess the impact of reforms adopted in 1985 or earlier. For example, the effect of a reform adopted in 1985 (that remained in force through 2001) would be indistinguishable from other fixed differences between states.

We categorized state malpractice laws according to the presence of 2 types of reforms: reforms that directly reduce expected malpractice awards and reforms that reduce awards only indirectly (Table 1). “Direct” reforms include caps on damage awards, abolition of punitive damages, abolition of mandatory prejudgment interest, and collateral source rule reforms. “Indirect” reforms include caps on attorney contingency fees, mandatory periodic payment of future damages awards, joint-and-several liability reforms, statute of limitations reforms, and patient compensation funds. We chose to group reforms into these 2 categories because several studies in the literature have found that reforms that directly reduce expected malpractice awards have the largest effect on malpractice pressure and physician behavior.

We defined our 2 law variables as follows. If a state adopted any direct reform between 1986 and 2001, then we set the binary variable $L_{1st}$ to 1 for the year of adoption and all years subsequent to it; $L_{1st}=0$ for all years before $t$. If, after adoption of reforms, a state repealed all its direct reforms between 1986 and 2001, then we reset the binary variable $L_{1st}$ to 0 for the year of repeal and all years subsequent to it; $L_{1st}=1$ for the year of adoption and all years after adoption but before $t$. For all other states, $L_{1st}=0$ for all years. We defined $L_{2st}$ similarly for indirect reforms. $L_{2st}$ was defined as the 2-element vector containing $L_{1st}$ and $L_{2st}$.

### Table 1. Legal Reforms Used in the Analysis

<table>
<thead>
<tr>
<th>Reform</th>
<th>Description of Reform</th>
<th>Potential Impact on Liability</th>
</tr>
</thead>
<tbody>
<tr>
<td>Caps on damage awards</td>
<td>Either noneconomic (pain and suffering) or total damages payable are capped at a statutory specified dollar amount</td>
<td>Direct</td>
</tr>
<tr>
<td>Abolition of punitive damages</td>
<td>Medical malpractice defendants are not liable for punitive damages under any circumstances</td>
<td>Direct</td>
</tr>
<tr>
<td>No mandatory prejudgment interest</td>
<td>Interest on either noneconomic or total damages accruing from either the date of the injury or the date of filing of the lawsuit is not mandatory</td>
<td>Direct</td>
</tr>
<tr>
<td>Collateral source rule reform</td>
<td>Total damages payable in a malpractice suit are subject to reduction by all or part of the dollar value of collateral source payments to the plaintiff</td>
<td>Direct</td>
</tr>
<tr>
<td>Caps on contingency fees</td>
<td>The proportion of an award that a plaintiff can contractually agree to pay an attorney is capped at a statutorily specified level</td>
<td>Indirect</td>
</tr>
<tr>
<td>Mandatory periodic payments</td>
<td>Part or all of damages must be disbursed in the form of an annuity that pays out over time</td>
<td>Indirect</td>
</tr>
<tr>
<td>Joint-and-several liability reform</td>
<td>Joint and several liability is abolished for noneconomic or total damages, either for all claims or for claims in which defendants did not act in concert</td>
<td>Indirect</td>
</tr>
<tr>
<td>Patient compensation fund</td>
<td>Physicians receive government-administered excess malpractice liability insurance, generally financed through a tax on malpractice insurance premiums,</td>
<td>Indirect</td>
</tr>
<tr>
<td>Statute of limitations reform</td>
<td>Plaintiffs are precluded from making a claim more than 3 y after the underlying injury or act, without regard to the discoverability of the injury (except for injuries caused by foreign objects or injuries to minors and incompetent patients)</td>
<td>Indirect</td>
</tr>
</tbody>
</table>

*Source: Kessler and McClellan.*

©2005 American Medical Association. All rights reserved.

(Reprinted) JAMA, June 1, 2005—Vol 293, No. 21 2619
We began by estimating Poisson models of the following form:

$$\text{Prob}(\text{Number of physicians}_t = N_t) = \frac{e^{\lambda_t} N_t^{\lambda_t}}{N_t!}$$

where \(\ln(\lambda_t) = \alpha_t + \theta_t + \beta P_t + W_t \gamma + R_t \rho + M_t \delta + L_t \phi\). Although we were unable to distinguish the effect of reforms adopted in 1985 or earlier from differences caused by other factors influencing either the level or growth of physician supply, we estimated different baseline time trends \(\theta\) for states adopting direct and indirect reforms before 1986 (which generally were adopted before 1980) and for nonadopter states to impose as few constraints as possible on the empirical model. Because it is impossible to consistently estimate the fixed effects in a nonlinear model of this form, we conditioned them out of the likelihood function according to the method described by Hausman et al.\(^9\) In this model, \(E(N_t) = P_t, W_t, R_t, L_t = \lambda_1\), so \(\ln(E(N_t)) = \ln(L_1) = \phi\). In other words, \(\phi\) represents the approximate percentage change in the supply of physicians that results from tort reform. We calculated the SE of \(\phi\), allowing the number of physicians to be correlated within a state over time; we assumed only that the number of physicians is independent across states.\(^10\)

We also estimated 3 sets of expanded Poisson models. The first set of models estimated separately the long-term and short-term effects of reforms. In these models, we denoted the existence of direct reforms by using 2 binary variables. If a state adopted any direct reform between 1986 and 2001, then we set the binary variable \(L_{1st} = 1\) for the year of adoption, \(t + 1\), and \(t + 2\); \(L_{1st} = 0\) for all years before \(t\). We set the binary variable \(L_{2nd} = 1\) for all years after \(t + 2\); \(L_{2nd} = 0\) for all years through \(t + 2\). If, after adoption, a state repealed all its direct reforms between 1986 and 2001, then we reset the binary variables \(L_{1st} = L_{2nd} = 0\) for the year of repeal and all years after \(t\); \(L_{1st} = 1\) for the year of adoption and the 2 subsequent years but before \(t\); \(L_{2nd} = 1\) for all years after the second year after adoption but before \(t\). For all other states, \(L_{1st} = L_{2nd} = 0\) for all years. We defined \(L_{3rd}\) and \(L_4\) similarly for indirect reforms. \(L_4\) was defined as the 4-element vector containing \(L_{1st}, L_{2nd}, L_{1st}\), and \(L_{4th}\).

The second set of models allowed the effect of law reforms to vary in high vs low managed care environments. As discussed in previous work by one of us (D.P.K.), reductions in liability that reduce defensive practices in a conventional tort and insurance environment may be either more or less beneficial in an environment that is influenced by managed care or may even be socially harmful.\(^11\) In these models, we defined \(\ln(\lambda_t) = \alpha_t + \theta_t + \beta P_t + W_t \gamma + R_t \rho + M_t \delta + L_t \phi + M_4 \phi^L \phi_4\), where \(\phi\) is the differential effect of reforms in high managed care environments.

The third set of models decomposed the effect of reforms into 2 parts: the part caused by the movement of existing physicians between states and the part caused by the entry of new and the retirement of existing physicians. Identifying how much of the net effect of reforms is due to moves vs entries and retirements is important because the welfare consequences to the country as a whole of aggregate changes in supply (through entries and retirements) are different from the consequences of reallocation of physicians across states. To do this, we defined a “moving” physician as one who was in active practice in \(t\) and \(t + 1\) as one who was in active practice in \(t\) and \(t + 1\) but not in \(t + 1\) (including immigrating physicians); we defined a “retiring” physician as one who was in active practice in \(t\) but not in \(t + 1\) (including emigrating physicians).

We used data from 4 sources. First, we used data from the American Medical Association Physician MasterFile on the number of physicians involved in direct patient care. The Physician MasterFile represents the most comprehensive data available on physician supply for the years of our study.\(^12\) Second, we used data on malpractice laws and state political characteristics from Kessler and McClellan,\(^3\) updated through 2001.

Third, we used data on the number of residency programs and the number of residents per capita in each state for each year from 1985-2001 from the National Graduate Medical Education Census. Fourth, we used data on state managed care enrollment from InterStudy Publications. Enrollment rates per capita were calculated by dividing the number of enrollees (exclusive of preferred provider organization members and supplementary Medicare enrollees) by the population.

RESULTS

Table 2 previews our basic differences-in-differences analysis by reporting unadjusted 1985-2001 percentage changes in the number of physicians from states adopting and not adopting reforms during our study period. Column 5 of Table 2 presents the percentage change in physician supply in states with direct reforms only compared with nonadopting states; column 6 presents the change in supply in states with indirect reforms only compared with nonadopting states; and column 7 presents the change in supply in states with direct and indirect reforms compared with nonadopting states. Column 5 of the first row of Table 2 shows our basic result: physician supply increased more rapidly, by 8.2%, in states adopting direct reforms only vs no reforms.

Trends in physician supply differed by specialty. On an unadjusted basis, states with direct reforms only vs no reforms showed less-than-average differential increases in the supply of physicians in the 5 high-malpractice-premium specialties (with surgeons reporting no differential increase at all), although states with direct and indirect reforms showed greater-than-average differential increases in the supply of 2 of the 5 high-premium specialties, anesthesiology (12.3%) and radiology (11.1%). In contrast, overall physician supply increased 3.4% less rapidly in states with indirect reforms only.

Malpractice reform increased growth more in the supply of physicians with 20 or more years of experience (as mea-
sured by years since completion of medical school) than growth in the supply of physicians overall. The supply of experienced physicians in states adopting direct reforms only increased by 87.8% from 1985-2001 compared to an increase in supply of 69.1% in non-adopting states, a difference of 18.7%. This result persisted for all of the high-malpractice-pressure specialties. The number of nongroup physicians shrank during the period. The unadjusted differences-in-differences effect of direct reforms on nongroup physicians was slightly smaller than the effect on all physicians.

These simple comparisons do not account for differences in trends in population, states’ market and political characteristics, and differences in malpractice law that predate the start of our study period. We explore the importance of these factors in the regression analysis that follows.

Table 3 presents estimates of the effects of direct and indirect reforms on state/year counts of the number of physicians, holding all else constant, from our basic econometric model. All models underlying the results in Table 3 and subsequent tables are based on a sample of size 800 (50 states × 16 years; 1985-2001, except 1990). States adopting direct reforms during the study period experienced statistically significantly greater increases in the supply of physicians than states that did not. In particular, physician supply in direct-reform states expanded by approximately 2.4% more during the study period than did supply in nonreform states, all else being held constant (SE, 0.24%). Supply in indirect reform states, in contrast, contracted by a smaller amount in absolute value (approximately 1.29%; SE, 0.24%). A 1% increase in managed care enrollment per capita led to a 0.13% decrease in physician supply (SE, 0.01%). The effect of direct reforms on the supply of nongroup physicians was substantially larger than the effect on all physicians (approximately 3.9% compared with 2.4%).

Does malpractice climate have a greater effect on nongroup physicians because they can transition out of nongroup status or because physicians who are not in groups are more likely to move or retire (and not be replaced by physi-
Physicians entering practice? To distinguish between these possibilities, we reestimated the model underlying Table 3 but limited the universe of physicians to those who were in the sample and in the same state for all of the study years, thereby excluding all moves, entries, and retirements. By using the number of non-group physicians in every state/year as the dependent variable, we found that direct reforms increased growth in physician supply by a smaller amount, leading us to conclude that the differential responsiveness of the supply of non-group physicians was the result of non-group becoming group physicians in nonreform states (data not shown).

Table 3 reports 3 other key findings. First, direct reforms had a similar effect on the supply of less vs more experienced physicians. Second, the net effect of indirect reforms on physician supply masked 2 competing effects: a negative effect on the supply of less experienced physicians and a positive effect on the supply of more experienced physicians. Third, the effect of managed care is larger for less experienced physicians.

Table 4 presents estimates of the effects of reforms on the supply of non-group physicians in 5 “high-risk” specialties. We restricted the analysis to nongroup physicians to isolate the effect of specialty. The proportion of physicians employed in a group vs nongroup setting differs by specialty and may affect the incidence of malpractice pressure. Thus, differences by specialty in the effects of malpractice pressure on the supply of group and nongroup physicians together may represent a combination of the effect of specialty and differences by specialty in the proportion of physicians employed in a group setting.

The point estimates of direct reforms for 3 of the 5 high-premium specialties exceeded the average effect of reforms for all nongroup physicians. For example, direct reforms led to increased growth in the supply of emergency medicine physicians of approximately 11.5%, almost 3 times the magnitude of the average nongroup effect of 3.9%. Effects for anesthesiology and radiology were also larger than the average effect, although the effect for radiology was statistically significant only at the 10% level (P = .10). The effect of direct reforms on the supply of surgeons was smaller than the average effect and statistically nonsignificant (P = .15).

Table 5 presents estimates of the long-run vs short-run effects of reforms and shows that reforms take time to reach their equilibrium impact. The magnitude of the effect of direct reforms long after their adoption is always greater than the magnitude of their effect soon after adoption, which is consistent with the estimates representing causal effects of law reforms rather than differences in trends in unobserved characteristics of states. For example, states adopting direct reforms experienced small and nonsignificant immediate changes in physician supply but approximately 3.3% greater growth in physician supply 3 or more years after adoption of reforms compared with states that did not. The group/nongroup and less experienced/more experienced effects of direct reforms followed the same pattern, with significantly greater magnitudes 3 or more years after adoption than within 2 years of adoption.

Table 6 presents estimates of the effect of reforms and managed care enrollment on physician supply, allow-
ing the effect of reforms to vary in high- and low-managed care environments. Table 6 shows that direct reforms had a statistically significantly greater effect on physician supply in high care vs low managed care states ($P < .001$). High levels of managed care either increase the level of malpractice pressure that physicians bear or increase the disutility of a given amount of malpractice pressure. The opposite was true of indirect reforms for physicians in aggregate and for less experienced physicians.

Table 7 presents estimates of the effect of reforms and managed care enrollment on physician supply, decomposing changes in supply into 1 of 2 types: retirements or entries and moves. Table 7 shows that virtually all the effect of direct reforms was due to increased entry and decreased retirement of physicians in reforming states rather than the movement of existing physicians from nonreforming to reforming states. The positive effect of direct reforms on entry (ie, on less experienced physicians) was smaller in magnitude than the negative effect of direct reforms on retirements (ie, on more experienced physicians). The positive effect of indirect reforms on the supply of physicians through moves was counterbalanced by a substantial negative effect of indirect reforms on entry, which is consistent with the results in Table 3; indirect reforms are more highly valued by physicians who have been in practice compared with those who have not.

We estimated several alternative models (not included in the tables) to further investigate how malpractice liability affects physician supply. First, we estimated a model that included a separate law variable for states that adopted both direct and indirect reforms. Adopting both direct and indirect reforms had a small ($<0.01$) and statistically insignificant effect on supply ($P = .99$) over and above the independent effects. Second, we estimated a model that allowed the effect of caps on damages to differ from the effect of other direct reforms. Caps on damages have a statistically significantly larger effect than all other direct reforms (3.0% compared with 0.64%; $\chi^2$ testing equality of effects = 25.7). Third, we estimated models that allowed the effect of managed care to be nonlinear and that were based on a subset of the years in our full analysis. Although the estimated effect of direct reforms was robust to these specifications, the estimated effects of indirect reforms and managed care were not.

**COMMENT**

In this study, we developed new empirical evidence on the relationship between malpractice climate and the supply of medical care. We compared trends in the supply of physicians in states that adopted and did not adopt law reforms limiting malpractice liability between 1985 and 2001. We found greater growth in physician supply in states that adopted reforms directly limiting liability than in states that did not. This basic result accords with other work published by Hellinger and Encinosa.13 In our study, physician supply in direct-reform states expanded by 2.4% more during the study period than did supply in nonreform states, controlling for fixed differences across states, population, market and political characteristics, and other differences in malpractice law. Direct reforms have a larger effect on supply 3 or more years after their adoption (3.3%) compared with 2 or fewer years after adoption ($-0.01$).

Our estimates of the effect of malpractice climate on physician supply were consistent with previous research finding that physicians practice “defensive medicine,” changes in practice based on fear of litigation that have little or no medical benefit for patients.5,11,13-16 Defensive medicine includes declining to supply care that has medical value to reduce the risk of malpractice liability. It can manifest itself both in across-the-board decisions by physicians to refrain from performing certain procedures or treating certain diseases and in case-by-case decisions not to treat particular patients. Avoidance behaviors of this sort have received significantly less academic attention than defensive medicine manifested as excessive testing or unnecessary procedures.

Malpractice climate is one of many determinants of the physician workforce, which accounts for its relatively modest impact in our study. Overall supply and the specialty and geographic distributions of physicians may be modified at several junctures: initial choice of career or specialty, retraining, relocation, and retirement. These decisions are influenced by various economic, experiential, and non-experiential factors, which them-

---

**Table 7. Effect of State Tort Reforms and Managed Care Enrollment on Physician Supply, 1985-2001, Decomposing Changes in Supply Into Retirements/Entries and Moves**

<table>
<thead>
<tr>
<th></th>
<th>Retirements/Entries Only</th>
<th></th>
<th>Moves Only</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Full Sample</td>
<td>&lt;20 Years’ Experience</td>
<td>&gt;20 Years’ Experience</td>
</tr>
<tr>
<td>Direct reforms</td>
<td>2.32 (0.24)</td>
<td>1.82 (0.33)</td>
<td>2.81 (0.35)</td>
</tr>
<tr>
<td>Indirect reforms</td>
<td>-2.61 (0.24)</td>
<td>-4.84 (0.33)</td>
<td>-0.01 (0.35)</td>
</tr>
<tr>
<td>Managed care enrollment</td>
<td>0.03 (0.01)</td>
<td>0.00 (0.01)</td>
<td>0.02 (0.02)</td>
</tr>
</tbody>
</table>

©2005 American Medical Association. All rights reserved.
selves are products of individual, community, and market characteristics, as well as government policies.\textsuperscript{1,17-23} To put our estimates in context, Rizzo and Blumenthal\textsuperscript{24} found that a 1\% increase in physicians' wages led to a 0.2\% to 0.3\% increase in hours worked. Thus, the 3.3\% increase in physician supply from direct malpractice reform is roughly equivalent to the increase in labor supply that would result from an 11\% increase in wages ($11 = 3.3/0.3$).

Our results illuminate the mechanisms by which malpractice liability reduces growth in physician supply. Physician characteristics such as practice structure, specialty, payer mix, and stage of career mediate the relationship between malpractice climate and supply. In our study, the estimated effect of direct reforms was greater among physicians who practiced in nongroup settings, which arose out of the movement of physicians into group settings in nonreform states. This is consistent with the lesser ability of smaller practices to spread liability insurance costs among many physicians, cushion premium volatility with high patient volume, or share risk with hospitals or other health care institutions.

Malpractice insurance is priced according to location and specialty rather than individual physician quality or loss experience.\textsuperscript{25-26} All else being equal, one therefore would expect greater supply effects in specialties known to pay the highest malpractice premiums. Our point estimates show that reforms had a greater-than-average effect on the supply of physicians in 3 of 5 specialties paying the highest malpractice premiums. The effect of reforms on the supply of obstetrics and gynecology and general surgical practitioners were smaller than, although not statistically distinguishable from, the average effect.

In our study, direct reforms had a greater effect on retirements and entries to the profession than on the propensity of physicians to move between states. This finding supports the argument that the supply effects of direct reforms will persist, at least to some degree, even if all states adopt reforms.

The positive effects of direct reforms on physician supply are greater in high- vs low managed care states. The disutility to physicians of managed care and malpractice pressure together may lead them to alter their careers more than either factor alone, although we cannot determine what aspect of the situation is the proverbial "last straw.” Because managed care enrollment rose throughout the 1980s and 1990s, effects measured in high managed care states may be a better approximation of future consequences of malpractice reform than effects in low managed care states.

Malpractice reform affects the organization of physician services beyond simply increasing supply. We found that the differential responsiveness of the supply of nongroup physicians appears to be the result of nongroup becoming group physicians in nonreform states. Put another way, liability pressure is a contributing factor to the increasing corporatism of US medicine. We also found that indirect reforms increased supply growth of more experienced physicians but decreased supply growth of less experienced physicians. This change would occur if more experienced physicians valued indirect reforms more highly than their less experienced counterparts, and the decline in earnings or partnership opportunities associated with a greater supply of more experienced physicians in states with indirect reforms discouraged entry of new graduates. Further investigation of these effects is needed.

Policy makers should be cautious about the prescriptive implications of our analysis. The goals of our study were narrowly defined, and our approach has significant limitations. First, although we controlled for fixed differences between states, national trends that affected all states, and time-varying characteristics of states, we were unable to assess the impact of reforms for states that adopted them before 1986. On one hand, if tort reform that originated in the malpractice crisis of the 1970s (eg, California's Medical Injury Compensation Reform Act) has persistent supply effects, then our study will underestimate differences between reform and nonreform states. On the other hand, supply gains from reforms adopted after the first wave may be more representative of potential future effects.

Second, we cannot exclude the possibility that the increase in physician supply we observed in states adopting reforms during our study period was simply a consequence of those states having more room for growth, because those states had fewer physicians at baseline. We controlled for differences in baseline levels of supply but not for differences in baseline growth rates in supply.

Third, endogeneity bias may have led us either to underestimate or overstate the effect of reforms. If decreases in physician supply lead states to adopt reforms, endogeneity bias would lead us to underestimate the effect of reforms. On the other hand, if increases in physician supply and the adoption of reforms are both caused by an unobserved factor, such as population preferences for litigation and medical services, endogeneity bias would lead us to overstate the effect of reforms.

Fourth, we estimated only the total effect of law reforms on physician supply. We did not separately identify the effect of malpractice pressure through which insurance premiums, the frequency of claims or awards, the amount of awards, and the nonfinancial impact of litigation would require significant additional econometric assumptions.\textsuperscript{27} Because we did not make these assumptions, our results are more robust but less detailed.

Fifth, we estimated the effect of law reforms only on the number of physicians, not on the total hours worked by physicians. If hours worked per physician decrease with the number of physicians, then our estimates overstate the total effect of reforms.
Sixth, we did not assess the impact of malpractice-induced supply changes on cost, quality, or access. Health policy analysts do not agree on the welfare implications of having more health care providers. Reforms-induced expansion in supply could either decrease or increase health care costs; competition among health care providers might lead to lower prices and more wasteful care, or additional physicians might induce demand for their own services beyond the point at which they are medically necessary. Similarly, increased supply could lead to higher quality through competition but could also lead to lower quality if the physicians who exit as a result of malpractice pressure are disproportionately less skilled. Finally, access to health care depends on patient characteristics and local distributions of specialists, as well as on statewide aggregate numbers of providers, and may differ between acute malpractice crises and noncrisis periods. In one of the few recent studies of this topic, Dubay et al reported that malpractice pressure results in a small but significant reduction in access to prenatal care.

Finally, our research does not address the fact that there are tradeoffs between the potential benefits of direct reforms, such as greater growth in physician supply, and their potential costs, such as reduced compensation for medical error. Health policy scholars have proposed alternative ways of improving the overall performance of the malpractice system. Empirical investigation of these approaches is an important topic for future research.

Author Contributions: Dr. Kessler had full access to all of the data in the study and takes responsibility for the integrity of the data and the accuracy of the data analysis.

Study concept and design: Kessler, Sage.

Acquisition of data: Kessler.

Analysis and interpretation of data: Kessler, Sage, Becker.

Drafting of the manuscript: Kessler, Sage, Becker.

Critical revision of the manuscript for important intellectual content: Kessler, Sage, Becker.

Statistical analysis: Kessler, Becker.

Obtained funding: Sage.

Administrative, technical, or material support: Kessler.

Study supervision: Kessler.

Financial Disclosures: None reported.

Funding/Support: This work was supported by the Project on Medical Liability in Pennsylvania funded by The Pew Charitable Trusts (grant 2002-00279).

Role of the Sponsor: The Pew Charitable Trusts had no role in the design or conduct of the study; analysis and interpretation of data; or preparation, review, or approval of the manuscript.

Acknowledgment: We thank Columbia student Nathanial B. Chase for research assistance.

REFERENCES

7. Danzon PM. New evidence on the frequency and severity of malpractice claims. Law Contemp Probs. 1986;5:57-84.