The Impact of State Medical Malpractice Reform on Individual-Level Health Care Expenditures

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Background. Past studies of the impact of state-level medical malpractice reforms on health spending produced mixed findings. Particularly salient is the evidence gap concerning the effect of different types of malpractice reform. This study aims to fill the gap. It extends the literature by examining the general population, not a subgroup or a specific health condition, and controlling for individual-level sociodemographic and health status.

Methods. We merged the Database of State Tort Law Reforms with the Medical Expenditure Panel Survey between 1996 and 2012. We took a difference-in-differences approach to specify a two-part model for analyzing individual-level health spending. We applied the recycled prediction method and the bootstrapping technique to examining the difference in health spending growth between states with and without a reform. All expenditures were converted to 2010 U.S. dollars.

Results. Only two of the 10 major state-level malpractice reforms had significant impacts on the growth of individual-level health expenditures. The average annual expenditures in states with caps on attorney contingency fees increased less than that in states without the reform ($p < .05$). Compared with states with traditional contributory negligence rule, the average annual expenditures increased more in both states with a pure comparative fault reform ($p < .05$) and states with a comparative fault reform that barred recovery if the plaintiff's fault was equal to or greater than the defendant's ($p < .05$).

Conclusions. A few state-level malpractice reforms had significantly affected the growth of individual-level health spending, and the direction and magnitude of the effects differed by type of reform.

Key Words. Health regulation, state health policies, health care costs

In the newest round of the long-standing debate over statutory tort reform, some experts have raised concerns about increased provider exposure to medical malpractice liability, putatively brought about by the Affordable Care Act (ACA) (Chirba and Noble 2013). To address these concerns, policy makers

have taken actions at both state and national levels, as reflected by recent tort reform laws passed in Michigan and North Carolina (Letourneau 2013; Donovan 2015) and by the 2015 Medicare Access and CHIP Reauthorization Act (MACRA) at the federal level. Since MACRA includes only minor provisions touching on malpractice (Pear 2015; Schencker 2015), advocates have called for a stronger federal tort reform law (Sage and Hyman 2014). Republicans in Congress, including Tom Price, who just became the new secretary of Health and Human Services, have also attempted to push tort reform either as a part of their plan for replacing the ACA (Price 2015; Terhune 2017) or as a separate legislation (e.g., the one approved by the House Judiciary Committee in February 2017) (Kaplan 2017). Given the prospect of renewed Congressional debate over medical malpractice reform (Mello, Kachalia, and Studdert 2017), interest is once again focusing on empirical evidence about malpractice reforms at the state level, which may inform the policy debate at the federal level (Chirba and Noble 2013).

Prior research has examined the impact of state-level tort reforms on malpractice liability, provider behavior, and health care costs (for an updated summary, see Mello and Kachalia 2016). The most consistent findings in the literature show that caps on noneconomic damages reduced payout per claim (Danzon 1986; Frech, Hamm, and Wazzan 2006; Guirguis-Blake et al. 2006; Avraham 2007; Waters et al. 2007). Liability effects associated with other types of tort reform have been inconsistent (Mello 2006; Mello, Kachalia, and Goodell 2011). Past studies of the impact of malpractice reforms on health care costs have also produced mixed findings. For example, one often-cited study found that “direct” tort reforms, such as caps on damage awards, reduced expenditures for Medicare patients hospitalized for myocardial infarction or ischemic heart disease (Kessler and McClellan 1996). However, when two subsequent studies applied the same method to examining Medicare patients with other diagnoses, neither of them found any evidence that tort reforms reduced Medicare spending (Congressional Budget Office 2004; Sloan and Shadle 2009).

Given the limited and mixed findings in the literature, several commentators have called for further research on the relationship between state-level tort reforms and health spending (Hermer and Brody 2010; Studdert, Mello,

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and Brennan 2010; Grace and Leverty 2012; Sage 2012; Chirba and Noble 2013). The current study is a response to that call. We recognize that the logic of statutory tort reform is to reduce litigation risk and malpractice insurance rates for providers, and by extension, corresponding pressure for defensive medical practice. In turn, if a tort reform succeeds in achieving these aims, then downstream reductions in health spending theoretically ought to follow. However, few empirical studies have confirmed this logic. For example, one study found that caps on noneconomic damages led to lower health spending (Hellinger and Encinosa 2006). But that study analyzed aggregate data at the state level without controlling for individual-level sociodemographics and health status. Overall, the literature provides mixed results about the effect of caps on noneconomic damages on health spending (Mello and Kachalia 2016). There is also an evidence gap concerning the effect of different types of tort reform on health care costs. Intriguingly, prior research indicated that a few types of tort reform, such as the joint-and-several liability reform, might sometimes have a perverse effect, by increasing a provider's risk of being sued rather than decreasing it (Carvell, Currie, and MacLeod 2012). This perverse effect underlines the likelihood that not all versions of tort reform will have the intended effects, especially in regard to their downstream effect on medical costs. This study aims to investigate exactly the downstream cost effect of different tort reforms. In particular, we have endeavored to overcome prior research's limitations in three ways. First, we will focus on the general population, not a subgroup or a specific health condition. Since prior studies chose to examine subpopulations or conditions that are most prone to malpractice litigations (Kessler and McClellan 1996; Sloan and Shadle 2009), their findings do not represent the full picture, especially due to not depicting the impact of tort reforms on subpopulations or conditions at medium or low risk of malpractice litigations. To draw a full picture of the impact of tort reform, our research will instead focus on effects in the general population because medical malpractice reforms adopted by states do not seek to target any specific clinical subpopulations or conditions, but instead aim to reduce malpractice litigation risk for all physicians treating all conditions among the general population. To our knowledge, our study will be the first to use a nationally representative sample of the general population to examine the impact of tort reforms on health spending. Second, unlike prior research (Hellinger and Encinosa 2006), we will control for individual-level sociodemographics and health status. Third, we will investigate a more extensive set of malpractice reform measures at the state level. We hypothesized that most measures are expected to reduce the growth of health spending, while the cost effects of

several measures (e.g., joint-and-several liability reform and comparative fault reform) are uncertain and are subject to empirical investigation (see Table 1 for a brief hypothesis for each measure).

METHODS

Sources of Data

Data about state-level malpractice reforms for 1996–2012 are from the Database of State Tort Law Reforms (Avraham 2014). The database is considered the most current and comprehensive state-level tort reform dataset (Grace and Levery 2012), which tracks different types of reforms, as well as their evolving status (i.e., adoption/Constitutional challenge/revision) over time.

Data about individual-level health care costs are from the Medical Expenditure Panel Survey (MEPS), which is a nationally representative survey (Agency for Healthcare Research and Quality 2003). The study sample includes 522,003 MEPS participants in 1996–2012.

The above two databases were merged and analyzed for the current study at the AHRQ Data Center at Rockville, MD.

Variables of Malpractice Reforms

The Database of State Tort Law Reforms (Avraham 2014) includes 11 types of malpractice reform measures. Because one of the measures (caps on total damages) was not changed by any states during our study period between 1996 and 2012, it was not included in our study. We chose to study the other 10 types of reform measures, including caps on noneconomic damages, caps on punitive damages, split recovery reform, collateral source rule reform, punitive evidence reform, caps on attorney contingency fees, joint-and-several liability reform, patient compensation fund reform, mandatory periodic payments reform, and comparative fault reform (see Table 1 for brief explanations of the reforms). Following the taxonomy outlined by prior research (Avraham 2014), eight of the reforms were indicated by binary variables, while two of them were measured by categorical variables, including mandatory periodic payments, which was indicated by three dummy variables (no periodic payment reform; a reform granting courts the discretion of whether to apply periodic payments; a reform requiring courts to apply periodic payments); and comparative fault reform, which had three dummy variables (traditional contributory negligence rule; a pure comparative fault reform; and a

Table 1: Description of Malpractice Reforms Analyzed in the Study

<i>Reform</i>	<i>Description</i>	<i>States with the Reform by the End of 2012</i>	<i>Hypothesized Impact on Growth of Individual-Level Health Spending</i>
Caps on noneconomic damages	Noneconomic damages payable are capped at a statutorily specified dollar amount	25	Reduced spending growth due to fewer incentives for the plaintiff and his/her lawyer and less pressure for defensive medicine
Caps on punitive damages	Punitive damages payable are capped at a statutorily specified dollar amount	31	Reduced spending growth due to fewer incentives for the plaintiff and his/her lawyer and less pressure for defensive medicine
Split recovery reform	Punitive damages payable are split between the state and the plaintiff, based on statutorily specified percentages, as a way to reduce the economic incentive for the plaintiff's lawyers to obtain a punitive damages award while providing the state with additional revenue	8	Reduced spending growth due to fewer incentives for the plaintiff and his/her lawyer and less pressure for defensive medicine
Collateral source reform	Total damages payable in a malpractice suit are subject to reduction by all or part of the dollar value of collateral source payments (e.g., workers' compensation) to the plaintiff	34	Reduced spending growth due to fewer incentives for the plaintiff and his/her lawyer and less pressure for defensive medicine
Punitive evidence reform	Punitive damages may only be obtained by plaintiff after meeting an enhanced burden of proof (e.g., "clear and convincing evidence") as defined by statute	35	Reduced spending growth due to more work for the plaintiff's lawyer and less pressure for defensive medicine
Caps on attorney contingency fee	The proportion of an award that a plaintiff can contractually agree to pay an attorney is capped at a statutorily specified level	19	Reduced spending growth due to fewer incentives for the plaintiff and his/her lawyer and less pressure for defensive medicine

Continued

Table 1. *Continued*

<i>Reform</i>	<i>Description</i>	<i>States with the Reform by the End of 2012</i>	<i>Hypothesized Impact on Growth of Individual-Level Health Spending</i>
Joint-and-several liability reform	Joint-and-several liability reform is abolished for noneconomic or total damages, either for all claims or for claims in which defendants did not act in concert	40	Uncertain due to less pressure for defensive medicine but more incentives for the plaintiff to sue all possible defendants to maximize the compensation potential
Patient compensation fund reform	Physicians receive government-administered excess malpractice liability insurance, generally financed through a tax on malpractice insurance premiums	13	Reduced spending growth due to less liability for providers and less pressure for defensive medicine
Periodic payment reform	Periodic payments reform requires that part or all of damages must be disbursed in the form of an annuity that pays out over time The reform has three levels as indicated by the three dummy variables below		Uncertain due to two competing rules, of which one might save the defendant money by discounting the total payment over the years and the other might adjust the payment upward or downward through review of the victim's health condition
	No periodic payment reform	20	
	A reform granting courts the discretion of whether to apply periodic payments	11	
	A reform requiring courts to apply periodic payments	20	
Comparative fault reform	Comparative fault reform relates to the extent to which a plaintiff may recover damages when he/she, along with the defendant(s), has been negligent and contributed to causing the injury		Increased spending growth due to higher liability exposure and higher pressure for defensive medicine
	Traditional contributory negligence rule	5	
	A pure comparative fault reform	14	
	A reform that barred recovery if the plaintiff's fault was equal to or greater than the defendant's	32	

Note. See Table S1 for a more detailed description of the reforms.

reform that barred recovery if plaintiff's fault was equal to or greater than the defendant's). Since the Database of State Tort Law Reforms tracks changes in malpractice reform between 1980 and 2012, it allows us to identify the status of the above reforms in 1996, the beginning of our study period. If a reform was adopted before 1996 and remained effective in 1996, then we set the dummy variable for the reform to 1. We set the dummy variable to 0 for either of the two scenarios—(1) the reform was repealed before 1996; (2) the reform was not adopted before 1996. After identifying the status of each reform in 1996, we followed prior studies (Kessler, Sage, and Becker 2005) to code the reform variables in our study period between 1996 and 2012. If a state, k , adopted any of the above reforms in year t , then we set the binary/dummy variable $M_{ktj} = 1$ ($j = 1, 2, \dots, 10$, indicating one of the 10 reforms) for the year of adoption t and all years subsequent to t ; $M_{ktj} = 0$ for all years before t . If, after adopting reforms, a state repealed its reforms, then we reset the variable $M_{ktj} = 0$ for the year of repeal t and all years subsequent to t .

Expenditure Variable

This study analyzed total annual expenditures at the individual level. Expenditures in the MEPS are defined as “the sum of direct payments for care provided during the year, including out-of-pocket payments and payments by private insurance, Medicaid, Medicare, and other sources” (Agency for Healthcare Research and Quality 2003). Expenditures were converted to 2010 U.S. dollars using the medical CPI information from the U.S. Bureau of Labor Statistics.

Statistical Analysis

We took a difference-in-differences (DID) approach by taking advantage of the natural experiment created by states' implementing or repealing different malpractice reforms at different times. We used Stata v12 to account for the MEPS sampling design. Specifically, to generate nationally representative estimates, our analyses are weighted by using the MEPS sampling design variables that incorporate sampling strata, primary sampling units, and personal-level weights. We specified a two-part model, which has been widely used for analyzing health expenditures (Duan et al. 1983; Manning 1998; Ai and Norton 2000; Manning and Mullahy 2001; Bao 2002). The model's first part is a logistic model on the probability of using any health services, and the second part analyzes annual expenditures for users of

health services. As per the recommendations by prior research (Manning and Mullahy 2001), we compared several alternative approaches for the second part and chose a generalized linear model with Poisson distribution and log link, which has been used by prior research on health expenditures (Buntin and Zaslavsky 2004).

Each part of the model includes the above variables for state-level malpractice reforms and a considerable number of individual-level covariates that were classified into three groups based on the Andersen Model of Health Care Seeking Behavior (Andersen 1995), including (1) *predisposing factors*, such as age, sex, race/ethnicity, marital status, and education; (2) *need factors*, such as self-reported physical and mental health status; (3) *enabling factors*, including family income, insurance status, and rural versus urban residence. The model also includes dummy variables for years and states.

For the state-level malpractice reforms that were found to be statistically significant in one or two parts of the two-part model, we subsequently applied the recycled prediction method and the bootstrapping technique, in order to examine the statistical significance of the difference in health expenditure growth between states with and without a reform. First, based on predictions from our two-part model with the full set of covariates, including all reform dummies, we estimated expenditures by using the formula below:

$$Y_{\text{hat}_{ij}} = P_{ij} * E_{ij} \quad (\text{F1})$$

P: probability of using health services by the i^{th} person in year j predicted from the first part.

E: annual expenditures by the i^{th} person in year j predicted from the second part.

Then, we applied the recycled prediction method by taking multiple steps—(1) setting the year dummy for 1996 equal 1 and all other year dummies equal 0; (2) setting a specific reform variable (e.g., caps on attorney contingency fees) equal 1 for the entire sample, and using (F1) to predict expenditures; (3) setting the variable equal to 0 for the entire sample and predicting expenditures again; and (4) repeating the process for 2012. We accounted for the MEPS sampling design and calculated the DID estimates of expenditure growth using the predicted expenditures. Then, we bootstrapped the process for 1,000 repetitions and used the bootstrap results to determine 95 percent confidence intervals around the estimated mean difference in expenditure growth between states with and without a reform between 1996 and 2012.

To provide contextual information about health spending growth in our study period, we used the recycled prediction method to predict and calculate the average growth of annual per capita expenditures among all states between 1996 and 2012.

We also conducted a series of sensitivity checks. The first was a randomization test, an approach that mitigates the bias in the estimated standard errors in DID analyses and that has been increasingly used for analyzing how state policies affect individual-level behavior (Bertrand, Duflo, and Mullainathan 2002; Erikson, Pinto, and Rader 2010). For our randomization test, we took the observed data as given and generated a new data file by randomly assigning states to adopting a malpractice reform in a randomly selected year. (If the reform has no effects, then, on average, the random assignment will have no consequences for the differences in expenditures across states.) Then, we used the new data to estimate the two-part model. We had the process bootstrapped for 1,000 repetitions to examine the distribution of the coefficient of each reform. Like prior research (Bertrand, Duflo, and Mullainathan 2002), we formed a two-tailed test at the 0.05 level by identifying the 2.5th and 97.5th percentile of the coefficient distribution and using these values as cutoffs. If a coefficient estimated in the original two-part model lies outside these two cutoff values, then we reject the hypothesis that it is equal to zero. Otherwise, we do not reject it.

Other sensitivity checks included (1) lagging the tort reform variables by 1 year since some of the reforms become effective midyear, while other laws may lag in implementation; and (2) estimating a reduced model which only included those reforms that were changed most frequently during the study period. Specifically, we considered a reform measure as changed most frequently if it was adopted or repealed for five times or more in the study period. For example, caps on noneconomic damages were changed 17 times during the study period, including 12 times of adoption and 5 times of repealing; (3) coding caps on noneconomic damages as a categorical variable. We constructed two versions of categorical variable. For version 1, the variable was coded as no cap, capped at \$400,000 or less, and capped at \$400,001 or higher; for version 2, the variable was coded as no cap, capped on \$500,000 or less, and capped at \$500,001 or higher. We ran sensitivity checks separately by using one of these two versions plus all other reform variables and covariates; and (4) including chronic conditions in our analysis. Since the MEPS did not have questions about chronic conditions until 2000, our main model did not include variables about chronic conditions. As a sensitivity check, we restricted our sample to the 2000–2012 MEPS files and added to our analysis

10 variables about chronic conditions, including diabetes, asthma, high blood pressure, coronary heart disease, angina, myocardial infarction, other heart diseases, stroke, emphysema, and joint pain.

RESULTS

Descriptive Results

As indicated in Table 1, there are substantial variations in malpractice reforms across states. While some reforms were implemented by most states by the end of 2012, others were less likely to be adopted. For example, 40 states implemented joint-and-several liability reform, compared with eight states adopting split recovery reform.

Findings from Multivariate Analyses

Results from our two-part model indicate that three types of malpractice reform—caps on attorney contingency fees, patient compensation funds, and comparative fault reform—have significant coefficients in one or two parts of the two-part model. (See Table S2.) For the three types of reform, we then applied the recycled method and the bootstrapping procedure, to further examine their impacts on individual health expenditures, and the results are summarized in Table 2. On average, annual per person expenditures increased \$3,548 among all states between 1996 and 2012. The expenditures increased less in states with caps on attorney contingency fees than in states without the reform (\$3,183 vs. \$3,942), with a difference of $-\$759$. That difference is statistically significant ($p < .05$) since its 95 percent confidence interval generated from the bootstrapping process does not include zero.

Table 2 also indicates that health care expenditures increased less in states with patient compensation funds than in states without this reform (\$3,305 vs. \$3,615), and the difference of $-\$310$ is not statistically significant ($p > .05$).

Finally, as shown in Table 2, compared with states with traditional contributory negligence rule, the increase in average annual per person expenditures was significantly *higher* in either states with a pure comparative fault reform (\$1,328, $p < .05$) or states with a comparative fault reform that barred recovery if the plaintiff's fault was equal to or greater than the defendant's (\$1,431; $p < .05$).

Table 2: Impact of Malpractice Reforms on Individual Health Care Spending

<i>Reform Measure</i>	<i>Increase in Annual Individual Spending in 1996–2012 in States with the Reform (\$)</i>	<i>Increase in Annual Individual Spending in 1996–2012 in States without the Reform (\$)</i>	<i>Difference in Increase in Annual Individual Spending between States with and without the Reform (\$)*</i>	<i>95% of Confidence Interval[†]</i>		<i>Probability of the Mean Difference Equal 0 (p)</i>
Caps on attorney contingency fee	3,183	3,942	−759	−1,801	−134	<.05
Patient compensation fund reform	3,305	3,615	−310	−1,013	453	>.05
Comparative fault reform						
Traditional contributory negligence rule			Reference			
A pure comparative fault reform	3,682	2,354	1,328	171	2,488	<.05
A reform which barred recovery if the plaintiff's fault was equal to or greater than the defendant's	3,785	2,354	1,431	505	2,387	<.05

Notes. All expenditures were measured in constant 2010 U.S. dollars. The statistical inference is based on the 95% confidence interval, and the inference indicates whether a *p* value is <.05, not the exact *p* value.

*Calculated after applying the method of recycled predictions to each part of the two-part model.

†Calculated from bootstrapping procedure of the recycled predictions.

Sensitivity Check Findings

As a sensitivity check, our randomization test confirmed the effects we found in our two-part model. For example, the coefficient of caps on attorney contingency fees from the original GLM is −0.243, which lies outside the two cutoff values (−0.185, 1.738) from the randomization test, and leads us to reject the null hypothesis of no significant effect for caps on attorney contingency fee, an

inference that is consistent with the original two-part model. In the other sensitivity analyses, the results from our original model did not change substantially (see details in Tables S3–S7).

CONCLUSION AND DISCUSSION

We found that only two of 10 major types of state-level malpractice reforms had a significant impact on individual-level health care expenditures, and the direction and magnitude of those effects differed by type of reform. While caps on attorney contingency fees are associated with lower personal-level health spending, comparative fault reform appears to be associated with increased expenditures. These results are consistent with our hypotheses.

Our findings are complex but plausible, given some insights into malpractice liability and physician behavior below.

Caps on attorney contingency fees aim to make malpractice cases less financially attractive to plaintiff attorneys and consequently make it more difficult for would-be plaintiffs to find capable attorneys to represent them. In turn, the intended downstream effect is to reduce litigation against physicians and the pressure for them to engage in defensive medicine. Our finding of reduced growth of health expenditures associated with caps on attorney contingency fees is consistent with this kind of logic. We noted that the coefficients of caps on attorney contingency fees were of opposite direction in the first versus second part of the two-part model (Table S2). While we caution against overinterpretation, the results could suggest a complicated effect of this reform on health spending. On the one hand, it reduces physicians' liability risk by making it less financially attractive for attorneys to pursue malpractice lawsuits. Consequently, physicians may become less defensive and more willing to schedule preliminary or follow-up appointments with their patients, a scenario that was consistent with the positive coefficient for caps on attorney contingency fees in the first part of the two-part model, indicating a higher probability that patients use any health services. On the other hand, as the reform gives physicians the impression of a reduction in litigation risk, it may reduce the pressure on physicians to engage in other sorts of defensive practice, such as ordering expensive diagnostic tests to detect low-likelihood clinical problems. This is what people tend to anticipate as the impact of tort reforms on defensive medicine and is presumably why the expenditures are reduced among users of health care services in states with caps on attorney contingency fees, as we observed in the second part of the two-part model. By putting together

these two effects depicted by the two parts of our model, our results from the recycled prediction process revealed that overall, caps on attorney fees are associated with reduced growth of health spending for the general population. While the overall effect on spending is in the hypothesized direction, we recognize that our discussion above is just one plausible explanation, and to our knowledge, our study is the first to use the two-part model to analyze the effect of tort reform on health spending by the general population. Further research is needed to understand the mechanism by which tort reform is impacting any use of health care margin and why those impacts might be in the opposite direction as the impacts on the amount of health care use margin.

Comparative fault reforms relate to the extent to which a plaintiff may recover damages when he/she, along with the defendant(s), has been negligent and contributed to causing the injury. In states without any comparative fault reforms, the default rule is contributory negligence, which means that if a plaintiff is even 1 percent at fault for the injury, the plaintiff will be totally barred from recovering damages. That is a more physician-friendly rule than either of the two types of comparative fault reforms examined here. Those reforms were adopted specifically because the traditional rule was felt to be too harsh on plaintiffs. So, after a state adopted the reforms, we would expect to see physicians bristling at their increased liability exposure, practicing more defensive medicine, and leading to higher spending. That is what this study found.

On the other hand, our results differ from prior studies, in showing that several major tort reforms, such as caps on noneconomic damage, have no significant effects on health expenditures. For example, Hellinger and Encinosa reported that caps on noneconomic damage reduced health care expenditures (Hellinger and Encinosa 2006). Their study, however, examined only four reforms, compared with 10 reforms in our analysis. More important, they relied on state-level aggregated data for health expenditures, with no information about individual health status, unlike our study which analyzes individual-level expenditures and controls for individual-level sociodemographic and health status. As another example, Kessler and McClellan found that a group of “direct” malpractice reforms reduced medical expenditures (Kessler and McClellan 1996). However, their study focused on two conditions among one specific population (i.e., myocardial infarction or ischemic heart disease among Medicare beneficiaries), compared with all health conditions and the general population in our study. Their analysis classified eight malpractice reforms into two groups—reforms that directly reduce expected malpractice awards (e.g., caps on noneconomic damage) and reforms that reduce awards only indirectly (e.g., joint-and-several liability

reform)—instead of examining each of the reforms individually, as we did in this study. We believe that there is additional insight to be gained from examining different types of tort reforms separately, in part because some reforms may well be more effective than others, and in part because the states often enact or repeal specific reforms at different points over time, rather than switching them all on and off as a group. There is no clear pattern of types of reforms that tend to be enacted (or repealed) together. For example, Pennsylvania passed two tort reform laws during our study period. First, in 1997, the state adopted only one reform measure (e.g., caps on punitive damages). Then, in 2001, it enacted several reform measures, including split recovery reform, collateral source reform, periodic payments reform, and joint-and-several liability reform. Other states have made more frequent changes to their malpractice liability systems. For example, Illinois passed a tort reform law in 1997 to repeal two reform measures (caps on noneconomic damages, joint-and-several liability reform) and to change the comparative fault reform from a reform that barred recovery if the plaintiff's fault was equal to or greater than the defendant's to a pure comparative fault reform. Then, the state adopted again the reform measure of caps on noneconomic damages in 2005. Finally, it repealed again this reform measure in 2009.

In this study, we found that, on average, annual per person expenditures increased by \$3,548 across the nation between 1996 and 2012. In comparison, the National Health Expenditure Accounts from the Centers for Medicare and Medicaid Services indicated an increase of \$4,168 in per capita personal health care expenditures (Centers for Medicare and Medicaid Services 2017), which is higher than that found in this study. The discrepancy can be explained by three factors. First, our results adjusted for the overall sociodemographic characteristics of the pooled MEPS data in 1996–2012. Second, in our study, all expenditures are measured in constant 2010 dollars. Third, the MEPS data do not include expenditures on specific medical events, such as payments for over-the-counter drugs (Agency for Healthcare Research and Quality 2003), which have increased rapidly in the past two decades (Blenkinsopp and Bradley 1996; Qato et al. 2008).

This study has several limitations. First, it does not examine how the reforms affect health outcomes. Second, although our 17-year study period is relatively long, we were unable to examine the impact of the reforms for states that adopted them before 1996. Third, the two-part model is appropriate statistically for analyzing health care expenditures by the general population. While it is relatively easy to understand the second part of the two-part model (i.e., the impact of tort reform on expenditures incurred by health care users), it may be worth

noting rationale for the first part, which shows the effect of tort reform on whether an individual uses any health care services in a year. One possible explanation is that different types of tort reforms could lead to increases or decreases in defensive medicine at the end of the prior year and subsequently could impact whether an individual patient comes back for a follow-up appointment in the next year. However, we acknowledged that our analysis has not examined directly expenditures related to defensive medicine. Fourth, while the database about tort reforms used in this study is considered the most comprehensive (Grace and Leverty 2012), we recognize that it represents just one way to code tort reform measures. For example, it does not provide detailed information about caps on attorney fees (e.g., percent of award or specified dollar amount). More research is warranted to evaluate different coding systems of the reforms. Last, future studies need to examine potential policy endogeneity—the possibility that states with rapidly increasing malpractice litigation expenses and health care costs are more likely to adopt reform laws. While prior studies have investigated the endogeneity between tort reform and other health outcomes, such as medical malpractice claims (Durrance 2010), there is a lack of empirical evidence about endogeneity between tort reform and health care spending more broadly. Moreover, assuming for the sake of argument that such endogeneity does exist, the direction in which it would bias the estimates in our study is not clear. On the one hand, the trend of increasing health care costs might continue, biasing the estimates toward not finding an effect; on the other hand, there might be a regression to the mean, biasing the estimates toward finding an effect. It remains an empirical question for future research to determine how endogeneity might bias the estimates in this kind of research.

Our estimates help shed some light on the effect of tort reform on Americans' health spending. To the extent that our findings are confirmed by future studies, our results have three important policy implications. First, we found that 8 of the 10 types of reforms examined in the study do not have significant impacts on individual health spending, suggesting that tort reform may have only a limited contribution to cost containment in the nation (Thomas, Ziller, and Thayer 2010). Second, caps on attorney contingency fee, the reform measure that is found by this study to reduce individual health expenditures, are not so commonly adopted by states since it was implemented by 19 states. Our study finding suggests that other states (or the federal government) may want to implement this type of reform to rein in costs. Finally, policy makers may want to reflect on the effect of comparative fault reform in the domain of medical malpractice, given our finding that expenditures appear to be moving opposite to the desired direction in those states that have already adopted the

reform. We recognize that the reform typically affects all branches of tort law, not just medical malpractice, and medical malpractice claims account for a minority of personal injury claims in state courts. Thus, we stress that our results are only related to the impact of comparative fault reform in the domain of medical malpractice. Our results suggest that physicians may be responding with more defensive behavior in the presence of comparative fault reform, rather than less. This finding could make intuitive sense, for example, if the reform leads plaintiffs more aggressively to include as many potential defendants as possible in their claims, in order to maximize the amount that they can recover. By extension, physicians broadly could be placed at greater risk of being sued, even if the potential for damage awards against any single physician is reduced. Our finding suggests that the behavioral impact of tort reforms on physicians' defensive practice may be subtle and less straightforward than the simple intuition that tort reforms necessarily lead to direct and corresponding reductions in defensive practice.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the supporting information tab for this article:

Appendix SA1: Author Matrix.

Table S1: A Detailed Description of Malpractice Reforms Analyzed in the Study.

Table S2: Results from the Two-Part Model.

Table S3: Results from the Sensitivity Analysis with Each Reform Variable Lagged by One Year.

Table S4: Results from the Sensitivity Analysis with the Most Frequently Changed Reforms.

Table S5: Results of Coding Caps on Noneconomic Damages as a Categorical Variable (Version 1).

Table S6: Results of Coding Caps on Noneconomic Damages as a Categorical Variable (Version 2).

Table S7: Results of Including Chronic Conditions in the Main Model.