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Does Medical Malpractice Reform Help States Retain Physicians and Does It Matter?

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Abstract: Many states have passed medical malpractice law reforms in an effort to retain and attract physicians. However, it is unclear what the net public health effect of such reforms is. While reforms are likely to help states retain doctors, they also diminish incentives to provide a high level of health care. We provide empirical evidence that some malpractice reforms have helped states retain doctors while others have not. However, retention of doctors comes at a cost. We show that some malpractice law reforms have lowered the level of care provided, as indicated by an increase in infant mortality. This suggests that some of the tort reforms lead to worsening health outcomes.

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1. INTRODUCTION

Medical malpractice law looms large in tort reform debates in the United States. Supporters of malpractice law reform claim states that implement reform will attract more doctors, improving access to healthcare for the state's residents. One example is Stein (2003) who suggests that "Americans are beginning to feel the effects of double digit increases in medical malpractice insurance premiums, which are prompting doctors to flee states with the highest premiums, refuse to perform high risk procedures, retire early out of frustration or stage protests such as the one underway in West Virginia (Stein 2003)."

Opponents of malpractice reform suggest that reforms do not significantly affect insurance rates or physician location decisions. For example, the American Trial Lawyers of America (ATLA) states that "[t]he claim that doctors are leaving their practices because they can't afford insurance is all hype (ATLA 2003)." Further, a standard law and economics analysis would suggest that reforms induce medical professionals to provide a lower level of care.

Theoretical considerations support both views. Effective reforms will lower the cost of practicing medicine in a given state. This effect attracts more physicians to the state and induces the physicians presently practicing in the state to remain there. As a consequence, access to medical care increases, leading to improved public health. However, a less stringent tort law reduces doctors' incentives to provide an efficient level of care to their patients, and this weakened incentive structure might lead to worsening public health outcomes. Thus, the net effect of medical malpractice reform is theoretically ambiguous.

To date, there is little systematic evidence supporting either view. Moreover, the theoretical predictions of reform are not clear-cut. If malpractice premiums have been rising for

reasons other than increasing tort judgments, malpractice reform might do little to keep physicians in a state.[1] Even if tort judgments are the driving factor in increasing insurance rates, depending upon the elasticity of demand for health services and the supply elasticity of physicians, physicians might be able to pass any cost increase on to their customers.[2]

In this paper, we examine whether tort reform leads to an increase in access to healthcare as represented by the number of physicians in a state, and whether tort reform leads to worsening health outcomes, using infant mortality as our health metric. We introduce more precise measures of tort reform than are used in existing research. Previous work on the effect of tort reform on health outcomes (e.g., Kessler and McClellan 1996; Kessler, Sage, and Becker 2005) examines reform somewhat generically, focusing on broad categories such as direct and indirect reforms. In this paper we unbundle these categories by examining specific types of medical malpractice reforms. This allows us to determine which aspect of reform causes any change in outcomes.

To examine the effect of malpractice reform on location decisions, we use annual data on the number of doctors by specialty in each state for the period from1980 to1998. We show that some malpractice reforms, specifically caps on non-economic damages, increase the number of physicians choosing to locate in a given state. However, a number of other reforms, such as abolishing joint and several liability and establishing victims' compensation funds, might work

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[1]See, for example, Americans for Insurance Reform (2003) which argues that increasing rates are primarily an artifact of the financial performance of insurance companies' portfolios.

[2]A General Accounting Office (2003, Highlights Section) study of malpractice insurance premiums determined that increasing losses on medical malpractice claims "appear to be the primary driver of rate increases in the long run."

against attracting more physicians. After controlling for potential simultaneity in the adoption of medical malpractice reforms, capping non-economic damages is the only reform that has a consistent systematic effect on doctor location. This represents the first systematic examination of the effects of specific kinds of reforms on the physician location decision.

For health outcomes, we use annual data on state infant mortality from 1980 to 1998. We find that reversing the collateral source rule leads to an increase in infant mortality rates. Further, we find that reforms that attract more doctors to a state might improve the black infant mortality rate but they do not affect infant mortality in the white population.

In section 2, we present the theoretical issues involved in the effect of malpractice reform on both a physician's location decision and his care decision. In section 3, we provide empirical evidence that malpractice does affect physician location decisions. Section 4 presents evidence from infant mortality rates suggesting that malpractice reform does lower the level of physician care but there might be some countervailing effects as non-economic damage caps improve access to care in under-served communities. Section 6 concludes.

2. AMBIGUOUS EFFECT OF REFORM ON LOCATION AND CARE DECISIONS

2.1 The Location Decision

A doctor's decision about where to locate is influenced by the costs of practicing medicine. Part of these costs are premiums for medical malpractice insurance which represent a significant proportion of a doctor's operating costs. For example, in Florida, obstetricians pay in excess of $200,000 per year in malpractice premiums (Coble 2003).

The price of malpractice insurance is presumably influenced by the dollar value of

malpractice judgments assessed against the doctors in the insurance pool. To the extent that tort reform reduces these judgements,[3] the price of the insurance decreases. This reasoning suggests that the costs of practicing medicine is lower in states that pass effective reforms, making those states relatively attractive to doctors.

However, the empirical relationship between liability exposure and medical malpractice premiums is not well established. Zuckerman, Bovbjerg, and Sloan (1990) find that liability caps reduce malpractice premiums, while some other studies (Baicker and Chandra 2004; Black, Silver, Hyman, and Sage 2005) find no systematic relationship between metrics of liability and malpractice premiums.[4] Further, Baicker and Chandra (2004) also find no strong systematic link between the level of malpractice premiums and location for most doctors, though they concede that there may be a marginal effect on entry and exit, and the effect may be large in rural communities.[5] For infra-marginal doctors, Dranove and Gron (2005) find that neurosurgeons in Florida significantly cut back on the volume of brain surgeries performed when malpractice premiums increase.

In addition to the monetary costs, doctors face non-financial costs of malpractice litigation that are affected by tort reform (Kessler and McClellan 1996). These costs include

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[3]Danzon (1982, 1986) and Sloan, Mergenhagen, and Bovbjerg (1989) find that liability caps and required offset from collateral sources reduce payments per insurance claim.

[4]Studies examining the relationship between liability exposure and medical malpractice insurance rates, including those cited here, generally suffer from a lack of data on open claims which insurers usually will not disclose.

[5]Interestingly, in a structural model, Erus (2004) finds that malpractice liability insurance premiums are not a statistically significant predictor of where doctors choose to practice, in general, though he does find a significant effect on where doctors begin their careers.

reputational harm induced by a lawsuit and time spent and unpleasantness defending against a claim. If malpractice reform reduces the number or size of claims brought, these non-insurance costs are accordingly reduced.[6] Mello, et. al. (2004) document that self-reported discontent among physicians in Pennsylvania was positively related to the liability environment. Thus, there are direct and indirect channels by which reform might attract physicians to a given state.

However, some qualifications apply. First, the extent to which high insurance cost states are less attractive to doctors depends on their ability to pass the cost of the insurance on to their patients in the form of higher fees (Danzon, Pauly, and Kington 1990). Secondly, patients know they bear greater financial risks in those states where reforms have been passed. Thus, depending on the elasticity of demand for medical services, patients in these states might demand less medical care. This decrease in demand for medical services could have a negative effect on a state's ability to attract physicians.

2.2 The Care Decision

The analysis of the negligence rule in tort implies that a standard of care satisfies the usual marginal conditions for efficiency (Posner 1998). That is, liability is the rule if care is not provided up to the point where the marginal benefit of care equals the marginal cost. In the medical context, this suggests that all care should be taken up to the point where the marginal value of the health improvement equals the marginal cost of the additional medical care.

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[6]Although, it is interesting to note that Hughes and Savoca (1999) find that reforms which increase procedural hurdles for a plaintiff to bring a medical malpractice claim significantly increase the longevity of a malpractice dispute. Thus, there might be a trade-off for doctors between the number of claims and the duration of adjudicating any particular claim.

The incentive structure of the liability rule is somewhat attenuated since doctors are generally indemnified against any losses arising out of a malpractice judgment. Because malpractice insurance premiums are generally not experience rated (Sloan 1990), there might be little correspondence between the doctor's care level and the financial cost he bears. However, insurance companies have the ability to refuse to provide coverage to individuals with particularly poor records. Also, there might be some internal monitoring of physician activity by managed care organizations, hospitals, or colleagues who potentially face litigation risk arising from a doctor's negligent behavior. Further, as Kessler and McClellan (1996) note, there are significant non-financial costs to negligent behavior that might induce a physician to provide the required standard of care. Regardless of the ultimate effect on efficiency, to the extent that malpractice reforms lower the magnitude or frequency of malpractice claims, reforms will lead to a lower level of care provided.

It is not clear, however, that this lower level of care will necessarily lead to identifiable decreases in health outcomes. In the absence of restrictions on non-economic damages or other tort reforms, doctors might have the incentive to engage in so-called "defensive medicine." Kessler and McClellan (1996) provide results suggesting that, in the case of heart disease, fear of liability induces doctors to engage in treatment and diagnostic procedures that provide little or no value in terms of improved health outcome. When malpractice reform is implemented, doctors may reduce their level of defensive medicine with no adverse health consequences. Thus, without malpractice reform, doctors have an incentive to provide care that has zero marginal benefit to the patient. If doctors practice defensive medicine, reform has the potential to change physician behavior with no negative public health consequences.

3. DATA AND EMPIRICAL MODELS

Medical malpractice reform has the potential of allowing states to attract and retain physicians. The reform activities of states over the last few decades provides the opportunity to examine what reform has achieved in practice. We use American Medical Association (AMA) data for the period 1980-1998 on the number of doctors by state and by specialty[7] to examine the location decisions of doctors. These panel data allow us to perform a difference-in-difference analysis that uses the adoption of various tort reforms by states as exogenous shocks to the litigation loss exposure faced by physicians. This method allows us to identify the effect of changes in laws on the number of doctors per capita in each specialty for every state. In addition to state fixed effects and year effects, we will control for additional covariates that might influence doctors' choice of location.

We focus on reforms such as caps on non-economic damages, caps on total medical malpractice damages, the abolition of the collateral source rule, the abolition of joint and several liability, restrictions on the contingency fees charged by plaintiff's attorneys, requirements for defendants to set up annuities to pay victims over time, and the adoption of victims' compensation funds.

The caps on non-economic damages apply to damages beyond the loss of future income, medical expenses, and the like. We measure this reform as a dummy variable taking the value of one if a cap has been adopted by the state in a given year and zero otherwise. We also examine the effect of the level of the cap, analyzing separate specifications in which the dummy takes the

[7]The AMA maintains this data and doctors self-select their specialization.

value of one only if the cap is set at $500,000 or lower, and specifications where the dummy only takes the value of one if the cap is set at $250,000 or lower. Total malpractice caps are analyzed with a dummy taking the value of one if any cap is in place in the state for a given year and zero otherwise.

The collateral source rule is measured through a dummy variable taking the value of one if a state has reversed the normal rule that disallows offsetting the damages owed by the tortfeasor by any amount for which the victim has already been compensated or will be compensated for by an alternate insurer. We measure joint and several liability with a dummy variable taking the value of one if the state has abolished joint and several liability under which a victim can collect damages from any party that is potentially jointly responsible for a given harm. Fee restrictions are measured with a dummy variable taking the value of one if the state has placed any restriction on attorney's fees in medical malpractice cases. Similarly, if the state requires that all or some of a judgment be paid in the form of an annuity, our periodic variable takes the value of one. Lastly, if the state has established a no-fault fund that compensates individuals suffering adverse medical consequences, our fund indicator takes the value of one.

Among the covariates, we include a variable measuring the percent of the state's population with no insurance coverage, expecting a positive relationship between coverage and the number of doctors. We also control for the effect of a relatively old population by including a variable measuring the percent of a state's population aged sixty-five or older, expecting a positive relationship between the number of doctors and this variable. Further, we include a measure of the percent of the state population with a secondary education, expecting that a more educated population demands more medical services. We expect the same with a variable

measuring the per capita income of the state. We also include measures of the per capita governmental transfer payments made to individuals in the state, as well as per capita medical transfer payments, expecting positive correlations with both of these measures. We also include per capita alcohol sales as a general measure of the health preferences of the state's residents, hypothesizing a negative correlation between the number of doctors and alcohol sales and the level of doctors. Lastly, we include a variable measuring the percent of the state population that is black to control for any systematic differences in the demand for health services by race.

We estimate the regression

$$Docs_{ist} = \Psi R_{st} + \Theta X_{st} + \lambda_{is} + \tau_t$$

where the *R* vector captures the reform indicators described above, X_{st} represents a vector of covariates, λ_{is} is a time invariant fixed effect for each specialty by state, and τ represents year effects. The dependent variable is the number of doctors in state *s* that list specialty *i* as their primary specialization[8] per 100,000 state residents. We estimate the regressions with population weights.

Table 1 presents the means and standard deviations for the variables employed in our analysis. We coded state medical malpractice reforms according to the information provided by the American Tort Reform Association (ATRA).[9]

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[8]The AMA recognizes 232 distinct specializations.

[9]This information is available at http://www.atra.org/issues/index.php?issue=7338 . Our primary results are robust to the coding provided by the National Conference of State Legislatures, which differs slightly.

3.1 Results for Location Decisions

In Table 2, we provide results examining per capita doctors within each specialty by state. Given that we then have multiple observations for each state each year, we cluster our standard errors by state. Of the examined reforms, only caps on non-economic damages and the existence of victims' compensation funds have statistically significant effects on doctor location. We find that the existence of a non-economic damage cap increases per capita doctors per specialty by 0.02 per 100,000 population ($p = 0.02$), an increase of about 2 percent. The establishment of a victims' compensation fund decreases the number of doctors by about 0.03 per 100,000 population ($p = 0.01$), a decrease of 3.7 percent. Interestingly, victims' funds tend to be funded by taxing doctors, making the negative effect of funds on location intuitively plausible.

To examine whether the level of the non-economic damage cap matters, we re-estimated the regression presented above allowing for separate dummies for those states with caps set at $500,000 or below and those states with caps set at $250,000 and below. Note that the states with caps less than or equal to $250,000 are also counted among the states with caps set at $500,000 or below. We find that the coefficient on $500,000 caps has a statistically significant positive effect on the number of doctors ($p = 0.03$). The $250,000 caps appear to generate a small additional positive effect on the number of doctors in a state, but the effect is not statistically significant ($p = 0.52$).

Examining data aggregated at the state-specialty level precludes us from examining more interesting temporal effects such as state-specialty trends.[10] To allow for different temporal

[10]Doing so would add an additional 11,600 covariates which taxes Stata's abilities on a machine with standard RAM endowments.

effects, we also examined our data aggregated at the state level. In Table 3, we present results from the regression described above in which we merely have state fixed effects, instead of state-specialty effects. We once again find statistically significant effects of non-economic damage caps and victims' funds. In this specification, we also find that abolishing joint and several liability has a statistically significant negative effect on the number of doctors per 100,000 population.

We find that non-economic damage caps are associated with an increase of 4.7 doctors per 100,000 people ($p = 0.00$), which represents an increase of 2.4 percent. This relative effect is similar to the one we find in the state-specialty regressions presented in Table 3. Victims' funds are associated with a decline of 7.3 doctors per 100,000 people ($p = 0.00$). Once again, the relative effect is about 3.7 percent.

Abolishing joint and several liability leads to a decline of 2.9 doctors per 100,000 population ($p = 0.01$), a relative change of 1.5 percent. Interestingly, even though the AMA supports the abolition of joint and several liability, these results suggest that doctors actually favor the rule. Presumably, in a situation where joint and several liability would apply, doctors benefit from having additional defendants (with potentially deeper pockets) from whom plaintiffs are more likely to collect such as hospitals.

Also presented in Table 3 are specifications that account for serial correlation in the relationships. As documented in Bertrand, Duflo, and Mullainathan (2004), inertia in state policy decisions can lead to underestimates of standard errors in treatment effects. One of the remedies they suggest for this problem is equivalent to using Newey-West heteroskedastic autocorrelation consistent (HAC) standard errors (Stock and Watson, 2003). Statistical significance of non-

economic damage caps, victims' compensation funds, and abolishing joint and several liability does not change when HAC standard errors are estimated.

We also estimated regressions that allowed for state-specific trends with robust standard errors and HAC standard errors. In this specification, victims' funds lose statistical significance, while the coefficient on periodic payments became statistically significant ($p = 0.05$), generating a negative coefficient that implies a decline in per capita doctors of 1 percent. While the abolition of joint and several liability retained a statistically significant coefficient ($p = 0.05$), the sign flipped. In this specification, it appears as though abolishing joint and several liability leads to an increase in doctors per capita of 1 percent.

The only reform measure that remains statistically significant and retains its sign is the adoption of non-economic damage caps ($p = 0.05$). In this specification, the adoption of a non-economic cap leads to an increase of 1.7 in the number of doctors per 100,000 people, an increase of about 1 percent.

Allowing for separate dummies for $500,000 and $250,000 damage caps generates results similar to those in Table 2. Once again, we find that both caps generated positive effects on the number of doctors per capita, but only the $500,000 cap coefficient is statistically significant in any of the specifications.

On the basis of OLS regressions then, only non-economic damage caps appear to have a consistent systematic effect on doctor location decisions. However, while there appears to be a positive relationship between caps and the number of doctors practicing in a given state, the direction of causality is not clear. For example, even though we control for idiosyncratic differences across states and state specific temporal effects, it may still be the case that the AMA

has more influence among state legislatures in states with large numbers of doctors. If this is true, we might expect these states to be more likely to adopt caps due to rent seeking on the part of the AMA.

To investigate this possibility, we provide some instrumental variables estimates in Table 4. It is relatively difficult to instrument seven separate reform measures since this would require at least eight instruments in order to perform exogeneity tests. To mitigate this problem, we take the intermediate course of only instrumenting those reforms that appeared to be important in at least one of the OLS specifications described above (i.e., non-economic damage caps, abolishing joint and several liability, periodic payments, and victims' funds), including the other reforms as exogenous controls.

For our instruments, we choose: 1) the percent of state population who are Roman Catholics; 2) the percent of state population who are Mormons; 3) whether or not state legislators are term limited; 4) whether the state has adopted some kind of class action reform; and 5) whether the state has adopted some products liability reform.

Our religion instruments are meant to capture some of the underlying influences in the political process. Specifically, given the traditional focus on social justice concerns among Catholics, we expect to find that states with large proportions of Catholics are less likely to adopt medical malpractice reform.[11] Mormons, on the other hand, tend to align on the conservative side of issues, including tort reform. In principle, it does not seem as though religious composition of a state should have an independent effect on doctor location, particularly given

[11]See for example, the Center for a Just Society, which argues against tort reform on the basis of Judeo-Christian principles http://www.ajustsociety.org/ .

that we independently control for income and other demographic effects.

Term limits allow legislators to make decisions untempered by voter control, so we might expect them to have some effect on the passage of tort reforms that is unrelated to other state characteristics that may influence physician location decisions. Class action and products liability reforms might capture a state's willingness to adopt tort reform in general, but there is no reason to believe that doctor's location decisions are particularly influenced by state policy in either of these areas.

As shown in Table 4, our instruments are strong predictors of each of our medical malpractice reforms except periodic payments. Our first stage F statistics for the instruments is 12.50 for adoption of non-economic damage caps, 14.48 for the abolition of joint and several liability, 2.62 for periodic payments, and 8.85 for victims' funds.

In the most general IV specification, we instrument all four of the potentially important reforms and control for the three other reforms. We find that only non-economic damage caps have a statistically significant effect ($p = 0.04$). We find that, after controlling for potential simultaneity, our estimate of the effect is substantially larger than that estimated in the OLS regressions. Specifically, we find that non-economic damage caps are associated with an increase of 72 doctors per 100,000 population, a 37 percent increase. The test of overidentifying restrictions generates a p value of 0.29, suggesting that our instruments are exogenous and the estimated treatment effect is not likely to be an artifact of simultaneity.

We also present a few other robustness checks for the IV estimation. In the specification presented in Table 4, we instrument only the four potentially important reforms and drop the other three. We again find that only non-economic damage caps have a statistically significant

effect (p = 0.01), generating an increase of 66 doctors per 100,000 population, a 37 percent relative effect.

We also performed one specification where we only instrument non-economic damage caps and control for all other medical malpractice reforms. Here we find that non-economic damage caps are statistically significant (p = 0.00) and lead to an increase of 51 doctors per 100,000 population, a 26 percent increase.[12] In the next specification, we instrument non-economic damage caps and drop all other reforms from the equation. Once again, we find a statistically significant (p = 0.00) positive effect, implying an increase of 40 doctors per 100,000 population (a 20 percent increase). Lastly, we present a specification in which we instrument non-economic damage caps, drop all other reforms, and include state-specific trends. Once again, we find a statistically significant (p = 0.00) increase of 19 doctors per 100,000 population, a 10 percent increase. In each of these specifications, the F value for the instruments in the first stage equation explaining the adoption of non-economic damage caps is well above the standard cut-off of 10 and the tests of overidentifying restrictions uniformly suggest that the instruments do not directly affect doctor location.

3.2. Results for Health Outcomes: Infant Mortality Rates

The foregoing analysis suggests one potential benefit to passing non-economic damage caps. That is, doing so appears to attract more doctors to a state. If access to physicians improves public health, non-economic damage caps would appear to be welfare enhancing.

[12]Note in this specification, a few of the other (non-instrumented) reforms generate statistically significant coefficients. We do not place great confidence in these effects given the potential for endogeneity.

However, tort reform of this type, as well as the other medical malpractice reforms that states pass, could erode a physician's incentives to exercise proper care. Therefore, a useful welfare analysis can not stop at examining physician location decisions. Instead, we need to investigate the net effect of reforms on some metric of public health.

To do this, we focus on the effect of reforms on infant mortality using data on the death rates of children during the first 6 days after birth, again using a difference-in-difference design that employs the same reform measures.[13] We focus on this measure of public health due to the conventional wisdom that obstetricians are especially sensitive to liability exposure in their location decisions. Further, CDC collects very detailed infant mortality data and the metric is relatively well defined.

Because of evidence of racial discrepancies in infant mortality rates (Leslie, Galvin, Diehl, Bennett, and Buescher 2003), we examine white and black mortality separately. We use many of the same covariates, changing the age measure to the percent of the state population aged 15-19 because young mothers experience more difficult pregnancies (Phipps, Sowers, and DeMonner, 2002). We also add a measure of the abortion rate to control for the possibility that abortion might be a way to prevent post-birth mortality.[14] We present these results in Table 5.

For white infant mortality, we do not find a beneficial effect of the increased access to physicians occasioned by the passage of non-economic damage caps. We do find however that joint and several liability reform worsens the white infant mortality rate and the effect is

[13]All of the results that follow are robust to examining 28-day and 1-year mortality rates as well.

[14]See Gruber, Levine, and Staiger (1999).

statistically significant (p = 0.00). Joint and several liability reform is associated with an increase of 14 deaths per 100,000 live white births, a 3 percent relative effect. The effect loses statistical significance when we include state-specific trends.

We present the same specifications for black infant mortality rates. Among blacks, we do not find any statistically significant effect of joint and several liability reform. We do, however, find statistically significant effects of non-economic damage caps, collateral source reform, and restrictions on contingency fees, though only the collateral source effect is robust to all specifications.

In the specifications that do not include state-specific trends, we find that non-economic damage caps reduce black infant mortality by 55 deaths per 100,000 live black births, a relative effect of nearly 6 percent. When state-specific trends are included, the effect is no longer statistically significant.

Collateral source reform leads to statistically significant increases in black infant mortality in all specifications. The estimated effect suggests an increase of 44 deaths per 100,000 live black births, a relative effect of 5 percent when state-specific trends are not included and 73 additional deaths per 100,000 live black births (an 8 percent increase) when we control for state-specific trends.

Restrictions on contingency fees are associated with a decline of 69 deaths per 100,000 live black births, a 7 percent decrease, when state-specific trends are not included. This effect disappears when we control for state-specific trends.

The only effect that is consistent across specifications then is a negative effect of collateral source reform within the black community. Presumably, reversing the collateral source

rule somehow induces lower levels of care among professionals in the healthcare industry. Unfortunately, do to the aggregate nature of the data, it is not possible to pinpoint where the care reductions are coming. It could be care levels among obstetricians or pediatricians, or the hospitals themselves may reduce healthcare inputs (e.g., nurse staffing levels, capital improvements, etc.) when the collateral source rule is eliminated. Given the indirect nature of the collateral source rule, it might seem odd that it alone would have consistent health effects. One potential explanation of this result comes from qualitative research done on attorneys' incentives to bring suits. Baker (2001) argues that many low stakes medical malpractice suits are eliminated when states reform the collateral source rule. If a given adverse event does not justify non-economic damages, if hospital bills are eliminated from the potential judgment, there will be no reason for a lawyer to bring a claim. The elimination of these nuisance suits may induce doctors or hospitals to cut back care levels or reduce defensive medicine.

Regarding whether attracting doctors matters, it is interesting that the only reform that consistently attracts more doctors, non-economic damage caps, seems to have no effect on white infant mortality rates. Arguably, this is because access to care is already high for most white individuals. However, in the black community, there is more evidence that increasing access to care, by passing non-economic damage caps, does substantially improve health outcomes.

To investigate this possibility further, we performed some IV analysis of infant mortality rates, using the percent Catholic and percent Mormon instruments described above. In Table 6, we present four IV specifications. For whites, there is no statistically significant effect of non-economic damage caps on infant mortality, though in some specifications, our instruments do not appear to satisfy the orthogonality condition. For black infant mortality rates, the instruments

continue to perform well in the first stage, and we find a statistically significant ($p = 0.00$) negative effect of non-economic damage caps on black infant mortality. The coefficient implies that non-economic damage caps reduce black infant mortality rates by between 310 and 330 deaths per 100,000 live black births. This represents a relative reduction of more than 33 percent. The test of overidentifying restrictions indicates that our instruments do not affect black infant mortality rates directly.

When we include state specific trends, our instruments are no longer strongly predictive in the first stage (first stage F statistics of 2.8 and 8.6 respectively), but we do find reductions in black infant mortality rates that are statistically significant at the 6 and 9 percent levels.

If there really is a causal effect of non-economic damage caps on public health in black communities, it presumably works through the access channel. That is, in marginalized communities, having more doctors may make the difference between whether or not a pregnant mother receives prenatal care. This explanation is consistent with the results of Dubay, Kaestner, and Waidmann (2001) who find that decreasing malpractice liability costs are associated with a lower incidence of late prenatal care for pregnant women. Though the relationship is statistically significant for both white and black women, the effect is larger for black women.[15] The result would also suggest that damage caps may actually serve to reduce health disparities between blacks and whites.

[15]Interestingly, Aizer, Lleras-Muney, and Stabile (2004) find that even though pregnant black women change their behavior very little when access to care is expanded relative to pregnant white women, the health benefits from increased access are much larger for black babies than for white babies.

6. CONCLUSION

Many states have passed medical malpractice reforms in efforts to attract and retain physicians. We provide evidence that some of these reforms, particularly caps on non-economic damages, are effective in achieving this goal, but that some kinds of reform are counter-productive, such as abolishing joint and several liability, and establishing victims' compensation funds.

However, it is not clear that achieving this goal advances the public health. While reforms might be an effective strategy for increasing the level of doctors in a given state, they also lower the standard of care provided. We provide evidence that this is the case for infant mortality, showing that collateral source reform leads to a statistically significant increase in infant mortality rates in the black community. However, reforms that increase the number of doctors practicing in a state have the potential to improve health outcomes, at least in the case of infant mortality for black communities.

Future research should examine whether our results are peculiar to using infant mortality as a health metric. Different types of tort reform might have different impacts on various health outcomes and medical treatments. Research that uses the specific tort reform measures provides insights for future state and federal malpractice policy and might stimulate theoretical work modeling the various components of tort reform.

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Table 1
Descriptive Statistics

Variable	Description	Mean	Std. Dev.	Source
Physicians	Number of Physicians in Each State by Specialty	43.72	255.34	AMA
Physicians per 100,000 pop	Physicians/State Population (in 100,000's)	0.85	3.33	AMA
Total Physicians	Number of Physicians in Each State	10,099.07	12,500.90	AMA
Total Physicians per 100,000 pop	Total Physicians/State Population (in 100,000's)	196.30	65.50	AMA
White 6-Day Infant Mortality Rate	Number of White Infant Deaths Occurring during First 6 Days after Birth per 100,000 White Births	427.44	123.97	CDC
Black 6-Day Infant Mortality Rate	Number of Black Infant Deaths Occurring during First 6 Days after Birth per 100,000 Black Births	945.05	635.30	CDC
Non-economic Damages Cap	= 1 if State has Passed a Cap on Non-Economic Damages in Medical Malpractice Cases	0.22	0.42	ATRA
Non-economic Damages Cap ($500,000 nominal)	= 1 if State has Passed a Cap of $500,000 or less on Non-Economic Damages in Medical Malpractice Cases	0.21	0.40	ATRA
Non-economic Damages Cap ($250,000 nominal)	= 1 if State has Passed a Cap of $250,000 or less on Non-Economic Damages in Medical Malpractice Cases	0.06	0.24	ATRA
Medical Malpractice Cap	= 1 if State has Passed a Cap on Total Medical Malpractice Damages	0.08	0.27	ATRA
Collateral Source	= 1 if State has Abolished the Collateral Source Rule	0.45	0.50	ATRA
Joint and Several	= 1 if State has Abolished Joint and Several Liability	0.29	0.46	ATRA
Contingency	= 1 if State has Adopted Limitations on Fees Charged by Plaintiff's Attorney	0.36	0.48	ATRA
Periodic	= 1 if State has Mandated Periodic Payments of Judgments	0.42	0.49	ATRA
Victims' Fund	= 1 if State has Established a Victim Compensation Fund	0.20	0.40	ATRA
Uninsured	Percent of State Population with no Insurance	0.17	0.05	BLS
Per 65+	Percent of State Population that is 65 or Older	0.12	0.02	Census
Per 15-19	Percent of State Population Between Ages of 15 and 19	0.07	0.01	Census
Secondary Education	Percent of State Population with High School Education	0.76	0.08	Census

Income	Personal Income (in $1,000's) Deflated by CPI	13.98	2.53	BEA, BLS
Transfers	Per Capita Governmental Transfer Payments Deflated by CPI	1.73	0.39	BEA, BLS
Medical Transfers	Per Capita Medical Transfer Payments Deflated by CPI	0.25	0.15	BEA, BLS
Alcohol	Alcohol Sales (in gallons of ethanol) per Person aged 14 and older	2.51	0.69	NIH
Per Black	Percent of State Population that is Black	0.11	0.12	Census
Abortion Rate	Number of Legal Abortions Performed in State per 1,000 Women Aged 15-44	23.34	19.34	AGI
Catholic	Percent of state population self-identified as belonging to the Roman Catholic Church	0.19	0.13	Jones, et.al. (2002)
Mormon	Percent of state population self-identified as belonging to the Mormon Church	0.03	0.10	Jones, et.al. (2002)
Term Limit	=1 if state legislators are term limited	0.10	0.30	NCSL
Class Action	=1 if state has adopted class action reform	0.00	0.06	ATRA
Products Liability	=1 if state has adopted products liability reform	0.13	0.33	ATRA

AGI: Alan Guttmacher Institute
AMA: American Medical Association
ATRA: American Tort Reform Association
BEA: Bureau of Economic Analysis
BLS: Bureau of Labor Statistics
CDC: Centers for Disease Control
Census: Census Bureau
NCSL: National Conference of State Legislatures
NIH: National Institutes of Health

Table 2
Effect of Malpractice Reform on Number of Physicians per 100,000 Population by State and Specialty
(Clustered Standard Errors in Parentheses)

Non-Economic Damages Cap	0.020 (0.009)	–
Non-Economic Damages Cap ($500,000 nominal)	–	0.022 (0.010)
Non-Economic Damages Cap ($250,000 nominal)	–	0.007 (0.011)
Medical Malpractice Cap	0.001 (0.011)	-0.001 (0.010)
Collateral Source	-0.002 (0.008)	-0.002 (0.008)
Joint and Several	-0.012 (0.009)	-0.013 (0.009)
Contingency	0.006 (0.008)	0.005 (0.008)
Periodic	0.000 (0.007)	0.000 (0.007)
Victim Fund	-0.032 (0.012)	-0.032 (0.012)
Uninsured	0.064 (0.085)	0.065 (0.086)
Per 65+	2.658 (1.271)	2.687 (1.260)
Secondary Education	1.047 (0.225)	1.057 (0.229)
Income	0.024 (0.007)	0.024 (0.007)
Transfers	0.056 (0.053)	0.058 (0.053)
Medical Transfers	0.105 (0.108)	0.108 (0.108)
Alcohol	-0.032 (0.033)	-0.033 (0.033)
Per Black	3.731 (0.940)	3.693 (0.940)
State-Spec. Effects	Yes	Yes
Year Effects	Yes	Yes
Clustering	State	State
Adjusted R^2	0.977	0.977

Note: All specifications are estimated with population weights. The time period is 1980-1998.

Table 3
Effect of Malpractice Reform on Number of Physicians per 100,000 Population by State
(Robust Standard Errors in Parentheses)
[Newey-West HAC Standard Errors in Brackets]

Non-Economic Damages Cap	4.715 (1.039) [1.403]	–	1.652 (0.738) [0.848]	–
Non-Economic Damages Cap ($500,000 nominal)	–	5.025 (1.196) [1.615]	–	1.982 (0.835) [0.955]
Non-Economic Damages Cap ($250,000 nominal)	–	1.704 (1.580) [2.091]	–	0.305 (1.647) [2.107]
Medical Malpractice Cap	0.188 (1.638) [2.004]	-0.323 (1.659) [1.966]	1.917 (1.499) [1.573]	1.765 (1.504) [1.575]
Collateral Source	-0.347 (0.956) [1.292]	-0.466 (0.960) [1.298]	-0.619 (0.511) [0.612]	-0.653 (0.516) [0.614]
Joint and Several	-2.854 (1.016) [1.408]	-2.937 (1.014) [1.404]	1.612 (0.638) [0.817]	1.511 (0.645) [0.827]
Contingency	1.298 (1.100) [1.420]	1.263 (1.100) [1.419]	0.432 (0.731) [0.882]	0.491 (0.741) [0.899]
Periodic	0.077 (0.889) [1.163]	-0.003 (0.912) [1.192]	-1.299 (0.557) [0.652]	-1.381 (0.569) [0.667]
Victims' Fund	-7.343 (1.336) [1.794]	-7.462 (1.373) [1.854]	-1.385 (1.196) [1.150]	-1.527 (1.202) [1.172]
State Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
State-Specific Trends	No	No	Yes	Yes
Adjusted R^2	0.987	0.987	0.998	0.998

Note: Although the coefficients for covariates are not presented, all specifications include the variables Uninsured, Per 65+, Secondary Education, Income, Transfers, Medical Transfers, Alcohol, and Per Black. Coefficients for these covariates are similar in sign and statistical significance to the results presented in Table 2, and are available upon request. All regressions are estimated with population weights. The time period is 1980-1998.

Table 4
Effect of Malpractice Reform on Number of Physicians per 100,000 Population by State
Instrumental Variables Analysis
(Robust Standard Errors in Parentheses)

Non-Economic Damages Cap (Instrumented)	71.948 (34.165)	65.834 (26.729)	50.764 (6.874)	39.955 (5.422)	19.133 (3.613)
Joint and Several (Instrumented)	-19.501 (24.358)	-15.609 (21.021)	–	–	–
Periodic (Instrumented)	-93.377 (102.981)	-62.858 (49.398)	–	–	–
Victim Fund (Instrumented)	26.884 (74.637)	31.633 (64.159)	–	–	–
Medical Malpractice Cap	10.120 (29.108)	–	-15.741 (5.874)	–	–
Collateral Source	17.312 (24.827)	–	-1.134 (1.889)	–	–
Joint and Several (Not Instrumented)	–	–	-7.298 (2.296)	–	–
Contingency	9.073 (24.763)	–	-6.599 (3.643)	–	–
Periodic (Not Instrumented)	–	–	-10.202 (2.768)	–	–
Victims' Fund (Not Instrumented)	–	–	-26.295 (5.144)	–	–
State Effects	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes
State Specific Trends	No	No	No	No	Yes
First Stage F for Non-Econ Cap	12.500	16.700	29.180	35.430	26.690
First Stage F for Joint and Several	14.480	14.780	–	–	–
First Stage F for Periodic	2.620	5.770	–	–	–
First Stage F for Victim Fund	8.850	8.890	–	–	–

Hansen J-Statistic	1.111	1.340	0.542	1.018	2.742
[p-value]	[0.292]	[0.247]	[0.461]	[0.313]	[0.098]

Note: Although the coefficients for covariates are not presented, all specifications include the variables Uninsured, Per 65+, Secondary Education, Income, Transfers, Medical Transfers, Alcohol, and Per Black in both the first and second stages. Reforms are instrumented using the variables Catholic, Mormon, Term Limit, Class Action, and Products Liability. Coefficients for these covariates in the second stage are similar in sign and statistical significance to the results presented in Table 3. All regressions are estimated with population weights. The time period is 1980-1998.

Table 5
Effect of Malpractice Reform on 6-Day Infant Mortality Rate per 100,000 Births
(Robust Standard Errors in Parentheses)
[Newey-West HAC Standard Errors in Brackets]

	White Infant Mortality Rate		Black Infant Mortality Rate	
Non-Economic	5.873	2.889	-55.441	-5.743
Damages Cap	(5.269)	(6.374)	(23.309)	(26.236)
	[5.502]	[6.360]	[29.616]	[28.072]
Medical Malpractice Cap	7.948	-4.634	24.018	-48.308
	(13.294)	(17.549)	(44.851)	(56.138)
	[13.805]	[16.345]	[40.296]	[45.297]
Collateral Source	7.077	13.751	44.441	72.519
	(5.466)	(7.117)	(19.457)	(25.531)
	[5.541]	[7.392]	[19.978]	[26.663]
Joint and Several	14.018	9.425	24.480	16.581
	(4.461)	(5.444)	(17.833)	(22.147)
	[4.901]	[5.558]	[20.102]	[22.585]
Contingency	-11.084	-13.431	-68.555	-18.433
	(7.310)	(8.310)	(29.301)	(31.333)
	[7.711]	[9.112]	[30.703]	[29.894]
Periodic	-1.483	1.502	-19.526	9.730
	(6.336)	(7.205)	(25.100)	(31.263)
	[6.422]	[7.123]	[26.558]	[31.720]
Victims' Fund	-13.612	17.439	11.619	-4.308
	(7.915)	(13.012)	(25.883)	(38.693)
	[8.323]	[13.801]	[29.337]	[39.786]
Uninsured	-82.405	-0.858	-44.229	151.000
	(73.759)	(82.939)	(274.372)	(294.669)
	[72.416]	[82.856]	[277.325]	[301.786]
Per 15-19	223.757	232.225	5,872.344	5,959.739
	(566.490)	(957.377)	(2,740.692)	(3,768.993)
	[608.075]	[947.943]	[3,148.159]	[4,086.438]
Secondary Education	-438.250	-17.762	575.411	1,114.040
	(114.950)	(289.203)	(492.907)	(885.073)
	[119.666]	[294.358]	[557.344]	[900.076]

Income	-5.773	0.533	27.971	21.317
	(3.447)	(4.143)	(13.475)	(17.067)
	[3.531]	[4.102]	[14.408]	[17.167]
Transfers	-36.495	-11.906	119.079	147.150
	(26.886)	(42.895)	(104.650)	(179.212)
	[27.613]	[42.258]	[121.117]	[182.108]
Medical Transfers	-65.544	-66.828	-391.532	-129.690
	(54.134)	(66.759)	(222.229)	(294.290)
	[57.612]	[66.278]	[255.126]	[304.021]
Alcohol	41.226	12.346	-15.909	26.323
	(14.901)	(21.605)	(55.842)	(79.820)
	[14.767]	[21.167]	[56.944]	[83.650]
Per Black	191.301	1088.560	500.888	5,434.686
	(469.984)	(1063.028)	(1,483.678)	(2,894.703)
	[510.236]	[1076.609]	[1,764.393]	[3,358.932]
Abortion Rate	-0.031	-0.007	-2.601	-1.163
	(0.666)	(0.774)	(2.373)	(2.898)
	[0.706]	[0.787]	[2.625]	[3.121]
State Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
State Specific Trends	No	Yes	No	Yes
Adjusted R^2	0.906	0.926	0.758	0.814

Note: The regressions were estimated with the appropriate population weights. The dependent variable is the number of babies of each race that died within 6 days of birth per 100,000 births. The time period analyzed was 1980-1998.

Table 6
Effect of Malpractice Reform on 6-Day Infant Mortality Rate per 100,000 Births
Instrumental Variables Analysis
(Robust Standard Errors in Parentheses)

	White Infant Mortality Rate				Black Infant Mortality Rate			
Non-Economic Damages Cap (Instrumented)	9.773 (21.108)	-6.293 (18.042)	-38.933 (35.039)	-39.530 (29.776)	-329.515 (95.891)	-310.090 (74.137)	-564.380 (334.404)	-417.053 (220.929)
Medical Malpractice Cap	6.328 (16.766)	–	6.536 (21.970)	–	125.065 (71.656)	–	231.111 (188.662)	–
Collateral Source	4.640 (5.667)	–	12.581 (6.842)	–	37.345 (25.072)	–	-12.658 (67.479)	–
Joint and Several	10.841 (4.808)	–	16.981 (9.633)	–	58.947 (27.662)	–	166.126 (105.482)	–
Contingency	-13.804 (8.166)	–	-18.202 (8.418)	–	-31.245 (34.452)	–	-53.657 (58.248)	–
Periodic	-4.620 (7.858)	–	5.063 (8.660)	–	69.391 (46.247)	–	138.168 (99.645)	–
Victims' Fund	-14.779 (10.875)	–	19.941 (17.617)	–	107.359 (48.727)	–	119.124 (103.069)	–
State Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Trends	No	No	Yes	Yes	No	No	Yes	Yes
First Stage F for Non-Econ. Damage Cap	31.16	38.93	19.50	27.45	17.87	18.83	2.78	8.55
Hansen J Statistic [p-value]	5.695 [0.017]	5.113 [0.024]	1.060 [0.303]	1.222 [0.269]	0.002 [0.967]	0.073 [0.787]	0.496 [0.481]	1.725 [0.189]

Note: Although the coefficients for covariates are not presented, all specifications include the variables Uninsured, Per 15-19, Secondary Education, Income, Transfers, Medical Transfers, Alcohol, and Abortion Rate in both the first and second stages. Coefficients for these covariates in the second stage are similar in sign, magnitude, and statistical significance to the results presented in Table 5. All regressions are estimated with the appropriate population weights. The time period is 1980-1998.