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Task Force on Medical Malpractice (ATF-MM)

Sample:

Record of Comm. Proceedings ... RCP
- 05hr_AC-Ed_RCP_pt01a
- 05hr_AC-Ed_RCP_pt01b
- 05hr_AC-Ed_RCP_pt02

- *Appointments* ... *Appt*
- **

- *Clearinghouse Rules* ... *CRule*
- **

- *Committee Hearings* ... *CH*
- **

- *Committee Reports* ... *CR*
- **

- *Executive Sessions* ... *ES*
- **

- *Hearing Records* ... *HR*
- **

- *Miscellaneous* ... *Misc*
- **05hr_ATF-MM_Misc_pt42**

- *Record of Comm. Proceedings* ... *RCP*
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Does Medical Malpractice Reform Help States Retain Physicians and Does It Matter?

Jonathan Klick
Associate Director, Liability Project
The American Enterprise Institute

Thomas Stratmann
Professor of Economics
George Mason University

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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, it is possible 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) 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, work against attracting more physicians. This represents the first systematic examination of the effects of

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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."

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 for both whites and blacks. Further, we find that reforms that attract more doctors to a state improve the black infant mortality rate but 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 reform does help states retain physicians. Section 4 presents evidence from infant mortality rates suggesting that malpractice reform does lower the level of physician care, section 5 discusses how our research relates to the literature on defensive medicine, and section 6 concludes.

2. The Ambiguous Effect of Malpractice Reform on the 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 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.[4]

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 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.[5] 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.

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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]Zuckerman, Bovbjerg, and Sloan (1990) find that liability caps reduce malpractice premiums.

[5]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.

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.

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 Model

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[6] 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, 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

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

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 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 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[7] per 100,000 state residents. We estimate the regressions with population weights.

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[7]The AMA recognizes 232 distinct specializations.

Table 1 presents the means and standard deviations for the variables employed in our analysis. Table 2 indicates when states implemented their malpractice reforms. As discussed previously, and as is evident from the table, the malpractice reform entails seven different elements and for each element we find variation across states and over time that allows for a statistical analysis.

3.1 Results for Location Decisions

The results for the regressions examining all doctors are presented in Table 3. The effect of caps on non-economic damages in general and those set at $500,000 is positive on the number of doctors per capita, and the result is statistically significant (Table 3, columns 1,2,4, and 5). That is, states adopting these caps have three percent more doctors per 100,000 residents. Interestingly, the effect of adopting caps set at $250,000 or below is not statistically significant although it is still positive (Table 3, columns 3 and 6).

The effect of caps on total medical malpractice damages is negative, leading to a loss of more than one percent of a state's doctors per capita, but the result is generally not statistically significant. Reform of the collateral source rule and the adoption of periodic payments have no statistically significant effect on the number of doctors. Similarly, eliminating joint and several liability has an insignificant negative effect on the number of physicians per capita. This result suggests that doctors might view joint and several liability as a type of litigation shield under which deeper pockets, such as a hospital, might be available to pay any assessed damages.

Restrictions on attorney's fees and the creation of victims' compensation funds are both negatively related to the number of doctors in a given state. While the negative correlation for

victims' funds makes sense given that funds are financed through fees assessed on doctors, there is no clear *a priori* explanation for the negative association with fee restrictions. One possibility suggested by Helland and Tabarrok (forthcoming) is that limitations on contingency fees actually increase the likelihood that frivolous lawsuits will be filed. They provide some empirical evidence for this claim using data on medical malpractice claims litigated in Florida.

The effects of the covariates are generally in line with our predictions. We find that states with relatively old populations have more doctors per capita, as do relatively well educated populations. The effects of income and transfer payments are both positive and statistically significant. States with high rates of alcohol sales have fewer doctors, and the result is statistically significant. States with relatively large black populations also have relatively more doctors. The effect of medical transfer payments is not statistically significant.

Since we have multiple observations for each state in each year, following Moulton (1990) and Bertrand and Duflo (2002), we examine whether our treatment results are robust to clustering observations by state and find our results substantially unchanged in either sign or statistical significance (Table 3, columns 4, 5, and 6).

One concern regarding these estimates is that the adoption of tort reform measures may be endogenous to the number of doctors in the state. For example, if doctors like malpractice reform in general, politicians in states with large numbers of doctors might find it politically expedient to adopt reforms. Such a possibility would induce a simultaneity bias that would cast doubt on the causal interpretation of our results. To tests for this possibility, we performed the same analyses using a two stage least squares approach, allowing for the endogeneity of the malpractice reform variables. In this analysis we use political variables as our instruments, such

as indicators of whether the state legislatures were controlled by the Democratic party,[8] whether corporations were prohibited from making political contributions,[9] as well as other political variables. While it appears as though we have strong instruments, as implied by first stage F statistics and overidentification tests, the coefficients on our reform measures do not change signs or statistical significance. Hausman tests on our estimates indicates we can not reject the hypothesis that the malpractice reforms are exogenous, suggesting that the OLS estimates are unbiased and efficient.

Table 4 replicates our analysis using real dollar amounts for the damage caps instead of indicators. While a few states do tie their caps to inflation, the majority have no such allowance. Our findings in Table 4 are similar to the previous findings in Table 3. The coefficient on caps set at $500,000 is positive and statistically significant, but coefficient on $250,000 caps remains an insignificant determinant of the number of doctors in a given state.

Because doctors self-select their specialty in the AMA data, there might be a concern that the data do not accurately reflect areas of practice. For example, there might be little incentive for an obstetrician-gynecologist (OBG in the AMA data) to change his listed specialty to just gynecology (GYN in the AMA data) even if he decides to stop delivering babies due to liability issues. Therefore, we next examine whether our previous results hold up when we discard the information regarding specialty in the data. To do this, we re-examine our data at the state level, using total physicians per 100,000 residents as our dependent variable.

Table 5 presents regressions using this dependent variable and nominal damage cap

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[8]This instrument has a statistically significant negative effect on adoption of reforms.

[9]This instrument has a statistically significant negative effect on reform adoption.

amounts. All regressions include year and state fixed effects. The last three columns of Table 5 also include state specific time trends. Again we find that non-economic damage caps in general (Table 5, column 1) and caps set at $500,000 are positively related to the number of doctors per capita, and this relationship is statistically significant, representing a 3 percent increase in total physicians per 100,000 state residents (Table 5, column 2). Although the coefficient estimated for $250,000 caps is positive, it is not statistically significant (Table 5, column 3). Of the other reforms, abolishing joint and several liability, restricting attorney's fees, and establishing victims' compensation funds all lead to fewer physicians per capita, and the relationships are statistically significant.

Our results are robust when we include state specific trends. The coefficients on caps in general and $500,000 caps remain positive and statistically significant (Table 5, column 5). The $250,000 cap coefficient, however, turns negative and becomes statistically significant (Table 5, column 6). The signs and significance of attorney's fee restrictions and victims' compensation funds are robust to the inclusion of trends.

Table 6 examines caps using real dollar amounts. In these regressions, all non-economic damage caps are positively related to the per capita number of doctors in the state. The results are statistically significant for $500,000 (real) caps either with or without state specific trends. The coefficient estimated for the $250,000 (real) caps is statistically significant when trends are included, but insignificant when they are omitted. Contingency fee restrictions and establishment of victims' compensation funds uniformly lead to statistically significant reductions in the number of doctors per capita.

In summary, the results show that enacting caps on non-economic damages is an

effective way to attract and retain physicians. On the other hand, restricting contingency fees, abolishing joint an several liability, and establishing victims' compensation funds might be counterproductive in terms of getting physicians to locate in a given state. General malpractice damage caps, collateral source reform, and mandating periodic payments are not systematically related to the per capita number of physicians in a state.

3.2. Results for Health Outcomes: Infant Mortality Rates

To analyze the public health consequences of malpractice reform, we need not only examine the benefit side (i.e., whether reforms attract doctors), but also to examine the cost (i.e., whether reforms lead to worsening health outcomes). 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.[10]

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.[11]

Regardless of race, we find that collateral source reform leads to a statistically significant

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

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

increase in infant mortality regardless of whether state specific times trends are included (Tables 9 and 10) or not (Tables 7 and 8). For whites, the increase is estimated to be between 10.3 and 14.6 additional deaths per 100,000 births (Tables 7 and 9, columns 1-3; Tables 8 and 10, columns 1-2). This represents an increase of about 3 percent. For blacks, the collateral source reversal leads to between 47.6 and 72.6 additional deaths per 100,000 births, a percentage increase between 5 and 8 percent (Tables 7 and 9, columns 4-6; Tables 8 and 10, columns 3-4). These results suggest that the level of care provided decreases with the passage of collateral source reform. Unfortunately, from the aggregate data, it is not possible to identify the source of the decline. In principle, it could be the declining care by the obstetrician, the child's pediatrician, or the hospital and its staff in general or some combination of each. Sloan, Entman, Reilly, Glass, Hickson, and Zhang (1997) present results demonstrating that the behavior of obstetricians is not very responsive to variation in liability exposure, perhaps suggesting that our results are driven by some other party.[12]

The other reform measures that have a consistently significant effect on infant mortality are non-economic damage caps. Interestingly, for whites, caps have no statistically significant effect, while for blacks, enacting caps reduces mortality by about 67 deaths per 100,000 births (Tables 7-10). This seven percent decrease is statistically significant. In the specifications

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[12]An additional possibility is that when the standard collateral source rule is in place, insurers use subrogation to obtain compensation from hospitals and physicians. In general, if the rule is reversed, insurers will no longer have this option, leading to an increase in the price of first party insurance. This price increase leads to a decrease in the amount of health insurance purchased, generating a marginal increase in infant mortality. Although we do control for the fraction of a state's residents without health insurance, this variable is likely to be measured with error, keeping us from entirely capturing the insurance effect with our uninsured variable. However, it seems unlikely that this possibility could account for all, or even the majority, of the effect we estimate for collateral source reform.

including state specific trends, while still negative, the coefficient is no longer statistically significant.

Relating these results to the results in the previous section, it is interesting to note that non-economic damage caps consistently and significantly increase the number of physicians practicing in a state. This suggests that blacks might benefit significantly from the increased access and continuity of care occasioned by states attracting and retaining doctors through non-economic damage caps, while both whites and blacks suffer due to a lower standard of care provided as a result of collateral source reform. None of the other reforms generate statistically significant effects on infant mortality for either whites or blacks, regardless of the specification.

The relationships we estimate between reform measures and infant mortality rates appear to be causal. If we use instrumental variables techniques to control for potential simultaneity between reform adoption and infant mortality, through political variables that perform well as instruments as discussed above, Hausman tests indicate that these reforms are not endogenous and that the OLS estimates are unbiased and efficient.

In summary, these results show that collateral source reform leads to increased infant mortality. This change in the quality of care provided affects both white and black patients. However, reforms that increase the number of doctors practicing in a state (i.e., non-economic damages caps) have the potential to improve health outcomes for the blacks.

5. Do Doctors Practice Defensive Medicine?

It is perhaps interesting to relate the results reported in this paper to previous work on the health effects of malpractice reforms, specifically to the work by Kessler and McClellan (1996).

In their seminal article, using micro-level Medicare data, they examine the effect of malpractice reforms on both doctor behavior and health outcomes in the context of heart disease. Their results suggest that while reforms reduce medical expenditures significantly, they have no effect on health outcomes. They suggest that the reduction comes from doctors eliminating defensive practices which provide no health benefits but might provide cover for a physician in a lawsuit. When the threat of lawsuits is diminished through reforms, doctors eliminate the inefficient practices without worsening patient health.

The discrepancy between Kessler and McClellan's (1996) results on health outcomes and ours begs the question of why we should observe a difference. A simple answer is that, perhaps, the behavior of cardiac doctors and obstetricians differs systematically. That is, it is possible that there might be reasons why doctors in different specialties have different reactions to variations in liability exposure. It is possible that our results may suggest a reduction in defensive medicine as well. Though not as clear-cut as Kessler and McClellan's (1996) results, it is theoretically possible that the increase in infant mortality occasioned by reforms represents a reduction in efforts to save very high risk babies who can be saved only at high expense.

Another possible explanation arises from the coding of the reforms. Instead of entering each type of reform into the regressions separately, Kessler and McClellan (1996) code reforms as "direct" (which includes caps and collateral source reform) or indirect (which includes all other reforms), and examine the effect on treatments and health outcomes. If we code reforms in this manner, it is impossible to independently observe the effects of damage caps and collateral source reform. In our regressions, coding reforms in this manner yields results suggesting a positive association between direct reforms and infant mortality and a negative association

between indirect reforms and infant mortality.

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, restricting attorney's fees, 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. For 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.

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	45.74	266.45	AMA
Physicians per 100,000 pop	Physicians/State Population (in 100,000's)	0.87	3.40	AMA
Total Physicians	Number of Physicians in Each State	10,565.78	12,995.61	AMA
Total Physicians per 100,000 pop	Total Physicians/State Population (in 100,000's)	200.06	66.61	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	0.21	0.41	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	0.20	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
Non-economic Damages Cap ($500,000 real)	= 1 if State has Passed a Cap (deflated by CPI, 1982-1984 base) of $500,000 or less on Non-Economic Damages in Medical Malpractice Cases	0.20	0.40	ATRA, BLS
Non-economic Damages Cap ($250,000 real)	= 1 if State has Passed a Cap (deflated by CPI, 1982-1984 base) of $250,000 or less on Non-Economic Damages in Medical Malpractice Cases	0.12	0.32	ATRA, BLS
Medical Malpractice Cap	= 1 if State has Passed a Cap on Total Medical Malpractice Damages	0.09	0.29	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	0.23	0.42	ATRA
Contingency	= 1 if State has Adopted Limitations on Fees Charged by Plaintiff's Attorney	0.41	0.49	ATRA
Periodic	= 1 if State has Mandated Periodic Payments of Judgments	0.29	0.45	ATRA
Victim Fund	= 1 if State has Established a Victim Compensation Fund	0.06	0.24	ATRA
Uninsured	Percent of State Population with no Insurance	0.16	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	0.08	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	14.25	2.66	BEA, BLS
Transfers	Per Capita Governmental Transfer Payments Deflated by CPI	1.77	0.40	BEA, BLS
Medical Transfers	Per Capita Medical Transfer Payments Deflated by CPI	0.26	0.16	BEA, BLS
Alcohol	Alcohol Sales (in gallons of ethanol) per Person aged 14 and older	2.49	0.67	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

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
NIH: National Institutes of Health

Table 2
Malpractice Reforms

Non-Economic Damages Cap	Total Damages Cap	Collateral Source Rule Abolished	Joint and Several Liability Abolished	Fee Restriction	Periodic Payments	Victims' Fund
AK – $400,000 (1997)	AL – $1,000,000 (1987)	AK (1976)	AK (1988)	AZ (1976)	AK (1986)	FL (1989)
AL – $400,000 (1987, **1991**)	CO – $1,000,000 (1997)	AL (1987)	AZ (1987)	CA (1987)	AL (1987)	KS (2000)
CA – $250,000 (1975)	IL (1975, **1979**)	AZ (1984)	CA (1986)	CT (1986)	AZ (1989)	LA (1992)
CO – $250,000 (1997)	IN – $1,250,000 (1999)	CA (1975)	CO (1986)	DE (1976)	CA (1975)	NE (1993)
FL – $350,000 (1988)	LA – $500,000 (1975)	CO (1986)	FL (1986)	FL (1986)	CO (1988)	NM (1996)
HI – $375,000 (1986)	NE – $1,250,000 (1986)	CT (1985)	ID (1987)	HI (1986)	FL (1986)	OR (2003)
ID – $400,000 (1990)	NM – $600,000 (1976)	DE (1976)	IL (1995, **1997**)	ID (1981)	IL (1985)	PA (2002)
IL – $500,000 (1995, **1997**)	OR – $500,000 (1998)	FL (1986)	KY (1988)	IL (1985)	KS (1993)	SC (1989)
KS – $250,000 (1988)	TX – $500,000 (1998)	GA (1987, **1991**)	LA (1996)	KS (1988)	LA (1984)	VA (1987)
MA – $500,000 (1986)	VA – $1,000,000 (1983)	HI (1986)	MI (1986)	MA (1986)	ME (1985)	WI (1995)
MD – $350,000 (1986)		IA (1975)	NE (1991)	MD (1986)	MI (1986)	WY (1997)
ME – $150,000 (1997)		ID (1990)	NV (1987)	ME (1987)	MO (1986)	
MI – $280,000 (1993)		IL (1985)	NM (1987)	MI (1986)	NM (1976)	
MO – $500,000 [adjusts for inflation] (1988)		IN (1975)	ND (1987)	NE (1976)	NY (1985)	
MT – $250,000 (1997)		KS (1992, **1993**)	OH (1987, **1999**)	NH (1986)	OH (1987)	
ND – $500,000 (1987)		KY (1988, **1995**)	OR (1987)	NJ (1976)	OR (1975, **1987**)	

NH – $250,000 (1986, **1997**)

NV – $300,000 (1995)

OR – $500,000 (1998, **1999**)

SD – $500,000 (1997)

UT – $250,000 [adjusts for inflation] (1996)

WA – Formula (1988, **1989**)

WI – $350,000 (1997)

WV – $1,000,000 (1986)

MA (1986)

MD (1989)

ME (1990)

MI (1986)

MN (1986)

MO (1987)

MT (1997)

ND (1987)

NE (1993)

NH (1977, **1980**)

NJ (1987)

NV (1996)

NY (1986, **1992**)

OK (1975)

OR (1987)

PA (2002)

RI (1997)

SD (1977)

TN (1980)

UT (1996)

WA (1992)

WI (1995)

SD (1987)

TX (1987)

UT (1986)

VT (1985)

WA (1986)

WY (1986)

NY (1985)

OK (1953)

OR (1975)

PA (1975)

TN (1975)

UT (1985)

WA (1976)

WI (1986)

WY (1977)

RI (1987)

SD (1988)

UT (1986)

WA (1986)

WI (1975)

Years in bold indicate year in which law was repealed or held unconstitutional by the state's high court.

Table 3
Effect of Malpractice Reform on Number of Physicians per 100,000 Population by State and Specialty
(Standard Errors in Parentheses)

Non-Economic Damages Cap	0.027 (0.006)	–	–	0.027 (0.009)	–	–
Non-Economic Damages Cap ($500,000 nominal)	–	0.028 (0.006)	–	–	0.028 (0.010)	–
Non-Economic Damages Cap ($250,000 nominal)	–	–	0.021 (0.018)	–	–	0.021 (0.018)
Medical Malpractice Cap	-0.013 (0.012)	-0.013 (0.012)	-0.015 (0.012)	-0.013 (0.011)	-0.013 (0.011)	-0.015 (0.009)
Collateral Source	0.002 (0.005)	0.002 (0.005)	0.006 (0.005)	0.002 (0.008)	0.002 (0.009)	0.006 (0.008)
Joint and Several	-0.010 (0.007)	-0.010 (0.007)	-0.005 (0.007)	-0.010 (0.009)	-0.010 (0.009)	-0.005 (0.009)
Contingency	-0.020 (0.007)	-0.019 (0.007)	-0.017 (0.007)	-0.020 (0.007)	-0.019 (0.007)	-0.017 (0.008)
Periodic	0.005 (0.006)	0.004 (0.006)	0.006 (0.006)	0.005 (0.007)	0.004 (0.007)	0.006 (0.008)
Victim Fund	-0.030 (0.008)	-0.030 (0.008)	-0.023 (0.008)	-0.030 (0.011)	-0.030 (0.011)	-0.023 (0.010)
Uninsured	0.067 (0.080)	0.064 (0.080)	0.079 (0.080)	0.067 (0.079)	0.064 (0.079)	0.079 (0.080)
Per 65+	2.546 (0.455)	2.597 (0.454)	2.715 (0.454)	2.546 (1.252)	2.597 (1.238)	2.715 (1.241)
Secondary Education	1.006 (0.133)	1.006 (0.133)	1.067 (0.134)	1.006 (0.217)	1.006 (0.216)	1.067 (0.245)
Income	0.025 (0.004)	0.025 (0.004)	0.026 (0.004)	0.025 (0.007)	0.025 (0.007)	0.026 (0.007)
Transfers	0.069 (0.030)	0.070 (0.030)	0.064 (0.030)	0.069 (0.052)	0.070 (0.052)	0.064 (0.056)
Medical Transfers	0.068 (0.059)	0.072 (0.059)	0.070 (0.059)	0.068 (0.103)	0.072 (0.103)	0.070 (0.102)

Alcohol	-0.045 (0.016)	-0.044 (0.016)	-0.047 (0.016)	-0.045 (0.033)	-0.044 (0.033)	-0.047 (0.033)
Per Black	3.441 (0.488)	3.407 (0.488)	3.482 (0.488)	3.441 (0.947)	3.407 (0.945)	3.482 (0.987)
State-Spec. Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	None	None	None	State	State	State
Adjusted R^2	0.977	0.977	0.977	0.977	0.977	0.977

Note: All specifications 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 and Specialty
(Standard Errors in Parentheses)

Non-Economic Damages Cap ($500,000 real)	0.028 (0.006)	–	0.028 (0.010)	–
Non-Economic Damages Cap ($250,000 real)	–	0.003 (0.007)	–	0.003 (0.014)
Medical Malpractice Cap	-0.013 (0.012)	-0.013 (0.012)	-0.013 (0.011)	-0.013 (0.009)
Collateral Source	0.002 (0.005)	0.006 (0.005)	0.002 (0.008)	0.006 (0.008)
Joint and Several	-0.010 (0.007)	-0.007 (0.007)	-0.010 (0.009)	-0.007 (0.009)
Contingency	-0.019 (0.007)	-0.016 (0.007)	-0.019 (0.007)	-0.016 (0.009)
Periodic	0.004 (0.006)	0.006 (0.006)	0.004 (0.007)	0.006 (0.009)
Victim Fund	-0.030 (0.008)	-0.024 (0.008)	-0.030 (0.011)	-0.024 (0.010)
State-Spec. Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Clustering	None	None	State	State
Adjusted R^2	0.977	0.977	0.977	0.977

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, magnitude, 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 Number of Physicians per 100,000 Population by State
(Standard Errors in Parentheses)

Non-Economic Damages Cap	6.265 (0.979)	–	–	1.268 (0.592)	–	–
Non-Economic Damages Cap ($500,000 nominal)	–	6.525 (0.999)	–	–	1.494 (0.603)	–
Non-Economic Damages Cap ($250,000 nominal)	–	–	4.794 (2.863)	–	–	-3.464 (1.689)
Medical Malpractice Cap	-3.061 (1.866)	-3.027 (1.863)	-3.511 (1.933)	-1.384 (1.404)	-1.564 (1.406)	0.351 (1.382)
Collateral Source	0.370 (0.827)	0.367 (0.826)	1.323 (0.836)	0.131 (0.446)	0.124 (0.446)	0.203 (0.446)
Joint and Several	-2.280 (1.041)	-2.351 (1.041)	-1.216 (1.072)	1.056 (0.577)	1.024 (0.576)	1.123 (0.575)
Contingency	-4.516 (1.082)	-4.469 (1.079)	-3.812 (1.120)	-1.547 (0.649)	-1.552 (0.648)	-1.315 (0.658)
Periodic	1.126 (0.982)	1.029 (0.982)	1.300 (1.009)	-1.470 (0.643)	-1.465 (0.642)	-1.771 (0.651)
Victim Fund	-6.865 (1.317)	-6.948 (1.316)	-5.354 (1.335)	-3.171 (0.956)	-3.225 (0.955)	-2.751 (0.943)
State Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Trends	No	No	No	Yes	Yes	Yes
Adjusted R^2	0.987	0.987	0.987	0.998	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 3. All regressions are estimated with population weights. The time period is 1980-1998.

Table 6
Effect of Malpractice Reform on Number of Physicians per 100,000 Population by State
(Standard Errors in Parentheses)

Non-Economic Damages Cap ($500,000 real)	6.525 (0.999)	–	1.494 (0.603)	–
Non-Economic Damages Cap ($250,000 real)	–	0.792 (1.133)	–	3.541 (0.619)
Medical Malpractice Cap	-3.027 (1.863)	-3.046 (1.923)	-1.564 (1.406)	-1.312 (1.310)
Collateral Source	0.367 (0.826)	1.339 (0.837)	0.124 (0.446)	0.281 (0.436)
Joint and Several	-2.351 (1.041)	-1.589 (1.081)	1.024 (0.576)	0.934 (0.563)
Contingency	-4.469 (1.079)	-3.623 (1.127)	-1.552 (0.648)	-1.264 (0.637)
Periodic	1.029 (0.982)	1.390 (1.018)	-1.465 (0.642)	-0.927 (0.638)
Victims' Fund	-6.948 (1.316)	-5.617 (1.359)	-3.225 (0.955)	-3.322 (0.926)
State Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
State Specific Trends	No	No	Yes	Yes
Adjusted R^2	0.987	0.986	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 3. All regressions are estimated with population weights. The time period is 1980-1998.

Table 7
Effect of Malpractice Reform on 6-Day Infant Mortality Rate per 100,000 Births
(Standard Errors in Parentheses)

	White Infant Mortality Rate			Black Infant Mortality Rate		
Non-Economic Damages Cap	-1.885	–	–	-68.483	–	–
	(5.747)			(19.857)		
Non-Economic Damages Cap ($500,000 nominal)	–	-0.964	–	–	-72.053	–
		(5.800)			(19.797)	
Non-Economic Damages Cap ($250,000 nominal)	–	–	13.909	–	–	148.491
			(14.112)			(95.223)
Medical Malpractice Cap	1.119	1.069	-0.411	16.460	17.100	14.454
	(9.141)	(9.144)	(9.239)	(29.527)	(29.498)	(29.786)
Collateral Source	11.192	11.013	10.789	55.987	55.955	47.634
	(4.869)	(4.863)	(4.747)	(17.132)	(17.094)	(17.041)
Joint and Several	3.420	3.336	3.954	6.498	8.054	-5.827
	(5.974)	(5.981)	(5.988))	(22.802)	(22.820)	(22.599)
Contingency	-3.038	-3.187	-4.459	-0.191	-0.053	-9.312
	(6.163)	(6.155)	(6.208)	(26.650)	(26.625)	(27.108)
Periodic	-1.606	-1.617	-1.740	-36.604	-36.022	-43.375
	(5.626)	(5.629)	(5.621)	(23.358)	(23.341)	(23.419)
Victims' Fund	-12.429	-12.629	-12.479	-21.015	-20.306	-38.800
	(8.017)	(8.022)	(7.920)	(23.914)	(23.875)	(23.474)
Uninsured	33.714	33.637	34.311	-284.841	-263.678	-340.566
	(72.685)	(72.738)	(72.627)	(273.140)	(273.277)	(274.319)
Per 15-19	265.511	257.032	198.032	8,092.627	8,012.062	7,916.354
	(564.997)	(564.393)	(565.652)	(2,699.936)	(2,697.864)	(2,725.799))
Secondary Education	-357.185	-359.005	-350.648	696.020	714.694	741.069
	(128.423)	(128.402)	(128.294)	(512.178)	(511.802)	(517.576)
Income	-4.741	-4.748	-4.720	14.533	14.942	13.268
	(3.665)	(3.668)	(3.661)	(13.940))	(13.930)	(14.025)
Transfers	-47.694	-47.472	-46.285	58.868	58.555	100.119
	(27.547)	(27.563)	(27.498)	(98.919)	(98.783)	(99.087)

Medical Transfers	-55.166 (53.394)	-55.674 (53.370)	-57.676 (53.334)	-119.790 (185.001)	-124.206 (184.767)	-150.944 (186.049)
Alcohol	29.817 (16.427)	29.848 (16.432)	28.346 (16.492)	-23.872 (62.442)	-25.339 (62.388)	-31.414 (63.083)
Abortion Rate	-0.116 (0.668)	-0.124 (0.668)	-0.116 (0.667)	-1.776 (2.432)	-1.722 (2.430)	-2.231 (2.446)
State Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Trends	No	No	No	No	No	No
Adjusted R^2	0.902	0.902	0.903	0.741	0.741	0.737

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 8
Effect of Malpractice Reform on 6-Day Infant Mortality Rate per 100,000 Births
(Standard Errors in Parentheses)

	White Infant Mortality Rate		Black Infant Mortality Rate	
Non-Economic Damages Cap ($500,000 real)	-1.608 (5.856)	–	-67.053 (19.962)	–
Non-Economic Damages Cap ($250,000 real)	–	5.953 (6.308)	–	-27.746 (23.044)
Medical Malpractice Cap	1.097 (9.140)	1.333 (9.135)	16.416 (29.539)	13.254 (29.957)
Collateral Source	11.136 (4.870)	10.647 (4.751)	55.739 (17.139)	50.272 (17.292)
Joint and Several	3.400 (5.979)	2.464 (5.998)	6.271 (22.824)	-7.903 (22.523)
Contingency	-3.094 (6.158)	-4.581 (6.245)	-0.397 (26.660)	1.208 (27.063)
Periodic	-1.591 (5.629)	-1.267 (5.636)	-36.511 (23.375)	-46.384 (23.535)
Victims' Fund	-12.484 (8.024)	-14.129 (8.028)	-21.361 (23.932)	-33.005 (24.061)
State Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
State Specific Trends	No	No	No	No
Adjusted R^2	0.902	0.903	0.740	0.737

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. Coefficients for these covariates are similar in sign, magnitude, and statistical significance to the results presented in Table 7. All regressions are estimated with the appropriate population weights. The time period is 1980-1998.

Table 9
Effect of Malpractice Reform on 6-Day Infant Mortality Rate per 100,000 Births
(Standard Errors in Parentheses)

	White Infant Mortality Rate			Black Infant Mortality Rate		
Non-Economic Damages Cap	7.153	–	–	-22.447	–	–
	(7.079)			(24.167)		
Non-Economic Damages Cap ($500,000 nominal)	–	6.409	–	–	-25.927	–
		(6.977)			(23.569)	
Non-Economic Damages Cap ($250,000 nominal)	–	–	-0.293	–	–	99.923
			(17.341)			(134.925)
Medical Malpractice Cap	7.516	7.750	10.585	-38.064	-36.190	-54.918
	(11.912)	(11.925)	(11.851)	(43.078)	(42.990)	(41.756)
Collateral Source	13.929	13.943	14.318	71.409	71.282	72.262
	(5.613)	(5.615)	(5.606)	(20.653)	(20.644)	(20.627)
Joint and Several	2.742	2.792	3.651	43.264	44.238	38.366
	(7.200)	(7.206)	(7.158)	(26.203)	(26.173)	(25.471)
Contingency	-3.332	-3.249	-3.239	-10.052	-10.972	-12.359
	(7.903)	(7.904)	(8.006)	(32.518)	(32.547)	(32.996)
Periodic	1.123	0.891	0.518	-30.196	-29.316	-27.017
	(8.014)	(8.004)	(8.099)	(33.520)	(33.538)	(33.989)
Victims' Fund	-10.330	-10.102	-8.412	-24.199	-23.505	-30.716
	(12.600)	(12.593)	(12.467)	(38.994)	(38.930)	(38.502)
State Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.914	0.914	0.914	0.771	0.771	0.771

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. Coefficients for these covariates are similar in sign, magnitude, and statistical significance to the results presented in Table 7. All regressions are estimated with the appropriate population weights. The time period is 1980-1998.

Table 10
Effect of Malpractice Reform on 6-Day Infant Mortality Rate per 100,000 Births
(Standard Errors in Parentheses)

	White Infant Mortality Rate		Black Infant Mortality Rate	
Non-Economic Damages Cap ($500,000 real)	7.785 (7.199)	–	-20.951 (24.244)	–
Non-Economic Damages Cap ($250,000 real)	–	13.213 (7.876)	–	-1.973 (30.184)
Medical Malpractice Cap	7.257 (11.922)	8.543 (11.576)	-38.848 (43.093)	-49.698 (41.246)
Collateral Source	13.887 (5.613)	13.873 (5.598)	71.488 (20.654)	72.625 (20.740)
Joint and Several	2.592 (7.210)	3.133 (7.141)	42.902 (26.218)	37.368 (25.453)
Contingency	-3.350 (7.902)	-2.863 (7.896)	-9.928 (32.522)	-8.019 (32.505)
Periodic	1.109 (8.010)	3.010 (8.118)	-30.213 (33.525)	-31.573 (33.927)
Victims' Fund	-10.510 (12.606)	-10.117 (12.482)	-24.585 (39.002)	-29.891 (38.585)
State Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
State Specific Trends	Yes	Yes	Yes	Yes
Adjusted R^2	0.914	0.914	0.771	0.770

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. Coefficients for these covariates are similar in sign, magnitude, and statistical significance to the results presented in Table 7. All regressions are estimated with the appropriate population weights. The time period is 1980-1998.