# An Empirical Study of the Impact of Tort Reforms on Medical Malpractice Settlement Payments

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**ABSTRACT**

This paper evaluates the impact of six different types of tort reforms on the frequency, size, and number of total annual settlements in medical malpractice cases between 1991 and 1998. Medical malpractice data come from the National Practitioner Data Bank, which contains more than 100,000 malpractice settlement payments in the study time frame. Of the six tort reforms examined, two reforms (caps on pain and suffering damages and limitations on joint and several liability) reduced the number of annual payments, and two reforms (caps on pain and suffering damages and the periodic-payment reform) reduced average awards. Caps on non-economic damages had an effect on total annual payments, although the statistical significance of that effect was weak. The joint effect of enacting all six reforms was statistically significant for reducing the number of cases but not the state-level average award or total payments.

## 1. INTRODUCTION

Legislative alterations to common-law tort doctrines—otherwise known as "tort reform"[1]—have been a hot political issue for at least 3 decades.

1. Some object to the term "tort reform," as it suggests modifications to something that is clearly defective. Whether the tort system is broken is a complicated question that touches upon both philosophical and economic questions; these will not be dealt with in this paper. However, this term will be used throughout this paper as it is a well-entrenched term referring to the host of alternatives to tort law doctrine addressed here.

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In particular, reforms of medical malpractice law have held a central place on many state legislative agendas. Dozens of different reforms have been enacted, struck down, or reenacted in the recent decades (Avraham 2006b). Even at the national level, tort reform has made an appearance. Indeed, no fewer than 16 bills to federalize the various aspects of medical malpractice law (currently governed by state common law) have been debated in the U.S. Congress over the last decade.[2] Most recently, a bill directed at limiting defendants' liability in medical malpractice lawsuits was passed by the Senate on May 6, 2006 (S. 22, 109th Cong., 2d Sess. [2006]).

Medical malpractice law is clearly an issue of great concern not only to the public at large but also to many influential organized political and professional associations. Interest groups such as the American Association of Justice, American Tort Reform Association, America's Health Insurance Plans, American Medical Association, and Pharmaceutical Research and Manufacturers of America, to name a few, spend hundreds of million of dollars each year in the battle over tort reform.[3]

These high stakes make the accurate understanding of the effects of tort reform important. The extent to which certain reforms are or are not effective can shed light on the strategic behavior of health care providers, medical liability insurers, and litigants facing a changed legal

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2. H.R. 3103, 104th Cong., 2d Sess. (1996); H.R. 956, 104th Cong., 2d Sess. (1996); H.R. 1091, 105th Cong., 1st Sess. (1997); H.R. 2242, 106th Cong., 1st Sess. (1999); H.R. 2563, 107th Cong., 1st Sess. (2002); S. 812, 107th Cong., 2d Sess. (2002); H.R. 4600, 107th Cong., 2d Sess. (2002); H.R. 5, 108th Cong., 1st Sess. (2003); S. 2061, 108th Cong., 1st Sess. (2003); S. 11, 108th Cong., 1st Sess. (2003); S. 2207, 108th Cong., 2d Sess. (2004); H.R. 4280, 108th Cong., 2d Sess. (2004); H.R. 534, 109th Cong., 1st Sess. (2005); S. 366, 109th Cong., 1st Sess. (2005); S. 367, 109th Cong., 1st Sess. (2005); S. 354, 109th Cong., 1st Sess. (2005); H.R. 5, 109th Cong., 1st Sess. (2005); S. 22, 109th Cong., 2d Sess. (2006).

3. The data are available at the Center for Public Integrity, Lobbywatch: Top 100 Companies and Organizations (http://www.publicintegrity.org/lobby/top.aspx?act=topcompanies).

regime. Understanding the effects of tort reform is also important for policy makers in their attempts to change the legal, health care, and insurance markets. Moreover, the actual impact of tort reforms is an important component for courts in reviewing the constitutionality of tort reforms. For example, the Supreme Court of Wisconsin recently invalidated a statute placing a cap on noneconomic damages in medical malpractice actions (*Ferdon v. Wisconsin Patients Comp. Fund*, 701 N.W.2d 440 [Wis. 2005]). Applying the rational basis test, the court held that the statute was not rationally related to the legislative objective of lowering malpractice insurance premiums and reducing overall health care costs only after examining various empirical studies on the impact of tort reform.[4] In contrast, the Supreme Court of Utah recently upheld such caps on the basis of the perceived reasonableness of the empirical studies relied on by the Utah legislature (*Judd v. Drezga*, 103 P.3d 135 [Utah 2004]). Thus, careful analysis of the effects of different tort reforms may help determine the constitutionality of these laws.

Given the political (not to mention public health) import of these issues, it is surprising that academic scholarship on the effect of these reforms on litigation outcomes has found such mixed results. In fact, many studies have failed to detect any impact of these hotly debated reforms on either settlement practices or damage awards. However, as this paper demonstrates, previous studies faced significant data limitations and involved misguided methodological assumptions. Some of the methodological problems that may have limited the ability of previous studies to detect effects include failure to properly reflect substantive changes in the law in the data, a focus on litigated as opposed to settled cases, small sample size, and inadequate model specification.

However, the most frequent methodological mistake in past studies was the failure to properly link cases to applicable law. Specifically, cases were coded as subject to reforms when in fact they were not, for several reasons. First, cases were often miscoded as subject to reforms because scholars believed that the relevant date was the filing of the case, whereas in most cases the relevant date is the injury date. Second, cases were

4. The Alabama Supreme Court previously reached the same conclusion after reviewing empirical studies (*Moore v. Mobile Infirmary Ass'n*, 592 So.2d 156 [Ala. 1991]). But even before the growth in empirical work, courts were involved in their own back-of-the-envelope estimates of whether caps (and other reforms) can achieve a legislature's goal of reducing costs. See, for example, *Arneson v. Olson*, 270 N.W.2d 125 (N.D. 1978), and *Carson v. Maurer*, 424 A.2d 825 (N.H. 1980). Both North Dakota and New Hampshire have struck down caps on pain and suffering damages.

often miscoded because of misunderstandings regarding the retroactivity of constitutionally invalidated laws. Striking down a reform means that not only future cases but also pending cases will not be subject to it. The latter point has escaped previous scholars' attention. Yet properly linking cases to applicable law is essential to accurately estimating the effect of reforms on litigation outcomes.

In this study, I examine the effect of various reforms on the frequency, average size, and total annual payments of medical malpractice settlements between 1991 and 1998. Specifically, the study tracks the effect of six types of reform: caps on noneconomic damages, caps on punitive damages, higher evidentiary requirements for punitive damages, limitations on joint and several liability, limitations on the collateral source rule, and periodic payments of awards. These six reforms were chosen because they are the most prevalent reforms that states have enacted in the last decades[5] and because they all appear in the federal bills debated in Congress.[6]

Most previous studies employing state-level data estimated little or no impact of reforms on case outcomes. The only exception to this overall pattern is a reform that caps pain and suffering damages; this reform appears sometimes to decrease the number of positive payments and at other times to decrease the magnitude of payments. The key results of this study show some evidence in support of the conclusions of past research, while other results provide striking new evidence contrary to the conclusions of previous research. The state-level results (number of cases and total payments) are consistent with some past research indicating that a cap on noneconomic damages is the only reform that produces moderate effects on aggregate state-level variables (although I also find some evidence that limitation of joint and several liability and the periodic-payment reforms have some effect). The results

5. See Table 1, which shows the prevalence of six reforms.

6. Nine reforms are found in the latest federal bill (S. 22, 109th Cong., 2d Sess., May 3, 2006 [http://www.govtrack.us/congress/billtext.xpd?bill=s109-22]): limitation of 3 years for filing a lawsuit, cap of $250,000 on noneconomic damages, abolition of joint and several liability, limitation on contingency fees, abolition of the collateral source rule, proof of malicious intent to injure the victim by clear and convincing evidence, cap of $250,000 or 2 times monetary damages (whichever is higher) on punitive damages, periodic payments if future damages exceed $50,000, and immunity from punitive damages for manufacturers of drugs approved by the Food and Drug Administration. I do not study the statute of limitations and contingency fee reforms because of a lack of variance among states in these reforms during the time period studied. Nor do I study the immunity of drug manufacturers, as it is extraneous to my data set (which tracks medical malpractice lawsuits and settlements against physicians and hospitals).

at the case level (average award), however, are stunning. Once the data are adequately coded for the retroactive applicability of striking down reforms, caps on noneconomic damages and the periodic-payment reform each were correlated with a large decrease (up to 55 percent) in the average settlement payment. This effect is significant at the 1 percent confidence level.[7]

Yet the results also suggest that tort reforms may provide incentives to plaintiffs' lawyers to wait until a reform is struck down before settling. In that case, the total economic effect of tort reforms is much smaller, because it applies only to a small fraction of the cases that probably would not have been subject to the reforms.

This study offers valuable contributions to our understanding of the effects of tort reform on case outcomes. First, it incorporates two data sets never before used for this purpose: the National Practitioners Data Bank (NPDB), which is the most comprehensive data set on medical malpractice settlement, and the newly constructed Database of State Tort Law Reforms (DSTLR), which incorporates data from previously available sources (including previous compilations, research papers, and public information) as well as independently researched data tracking reforms (Avraham 2006b). The DSTLR documents dozens of reforms in all 50 states and Washington, D.C., since the 1980s.

Second, this study tracks settlements rather than judgments. As is well known, only a small fraction of cases are litigated. More than 90 percent of the medical malpractice cases are settled (Danzon and Lee 1983). Thus, from a policy-making perspective, documenting the effect of tort reform on settlements is of utmost importance. Moreover, this study does not rely on the representativeness of a sample of settlements to estimate population parameters; instead, at least in theory, the data represent the entire universe of settlements in the United States during the time period studied.[8] Third, the study tracks settlements for injuries that occurred between 1991 and 1998 and were settled by December 31, 2005. Thus, the study is the most recent analysis to date in terms of the years it covers. Fourth, the study tracks the impact of tort reform in all 50 states, whereas previous research studied a subsample of states. Fifth, the study tabulates cases according to injury date and not filing

7. The impact of the periodic-payment reform, however, exists in what I call the separate specifications but not in the joint specification.

8. As I will explain in more detail, there are reasons to believe that the Database of State Tort Law Reforms does not record all settlements. Still, I use about 100,000 observations, while other studies use samples of only a few hundred cases.

date.[9] A review of previous literature suggests that this might not have been done sufficiently in past studies. If so, previous results might be unreliable. Thus, besides the important policy implications of the findings of this analysis, this study also advances the empirical literature on the impact of tort reform by exposing and correcting past methodological flaws.

The structure of this paper is as follows. Section 2 reviews relevant literature. Section 3 describes in detail the two data sets (the NPDB and the DSTLR) used in this study. Section 4 introduces the statistical methodology and econometric models used to test three main empirical questions. Section 5 presents the results, explains various limitations of the estimation strategy, and provides possible explanations for why the impact of tort reforms may not be detected in empirical studies. Section 6 concludes by suggesting that the ongoing tort reform research effort should be shifted to exploring the effects of medical malpractice reform on microeconomic variables such as infant mortality, defensive medicine, and health insurance coverage.

## 2. LITERATURE REVIEW

There is a dearth of reliable empirical or experimental evaluations of medical malpractice tort reform (Diamond, Saks, and Landsman 1998; Babcock and Pogarsky 1999; Robbennolt and Studebaker 1999).[10] In fact, over the last 3 decades, only a dozen or so empirical studies have examined the impact of tort reforms on medical malpractice payments or medical liability insurance premiums.

The first wave of empirical studies was conducted in the 1980s and examined the tort reform revolution of the 1970s. These studies generally used multivariate regression to determine the effect of changes in various tort doctrines on the frequency and severity of malpractice claims using data obtained from insurance companies. But these studies, reviewed by Zuckerman, Koller, and Bovbjerg (1986), drew mixed conclusions. Some

9. See Section 2.4 for a description of how awards and settlements were matched to the reforms in effect at the time.

10. In experimental literature, one finds several studies that employ classroom experiments with students acting as mock jurors under differing damage cap regimes. These studies usually conclude that damage caps introduce cognitive biases such as anchoring and recalibration. While these studies have the advantage of a laboratory setting that can control for outside influences, they raise questions of robustness that can be answered only by further empirical field research.

studies concluded that these reforms were ineffectual in reducing malpractice liability burdens, while a majority concluded that they were effective in this regard. Regardless of their findings, these studies are based on data that are over 2 decades old; they predate recent reform measures taken by states and cannot take into account the longer-term effects of tort reform.

The second wave of studies from the late 1980s and early 1990s is summarized in a 1993 review by the Office of Technology Assessment (OTA) that explores the effect of tort reforms on malpractice costs. Comparing results from six studies (Adams and Zuckerman 1984; Danzon 1986; Zuckerman, Bovbjerg, and Sloan 1990; Sloan, Mergenhagen, and Bovbjerg 1989; Barker 1992; Blackmon and Zeckhauser 1991), the OTA noted that all of the studies "suffer from methodological problems and limitations that make interpretation and comparison of their results difficult" (U.S. Congress 1993, p. 16). The OTA study concluded that capping total damage awards was the only tort reform that consistently seemed to reduce payments per claim and therefore malpractice insurance premiums. However, there were inconsistent results for caps on noneconomic damages, a subset of this tort reform.

A third wave of studies, from the mid-1990s to the early 2000s, is reviewed in a 2004 Congressional Budget Office (CBO) report (U.S. Congress 2004). Reviewing nine studies (Born and Viscusi 1998; Browne, Lee, and Schmit 1994; Browne and Puelz 1999; Kessler and McClellan 1996, 2000, 2002; Thorpe 2004; Viscusi et al. 1993; Yoon 2001), the CBO flagged methodological problems and data limitations similar to those mentioned above. Again, the most consistent finding of the CBO was that caps on damages awards reduced the number of lawsuits filed, the magnitude of the awards, and insurance costs. Yet some of the studies reviewed failed to document any measurable effect of tort reforms, and, more generally, most findings were not independently corroborated by other studies.

In sum, a brief review of prior studies suggests that there is no consensus on the impact of tort reform on case outcomes. Some studies find certain reforms effective, while others find that the same reforms are ineffective. Caps on damages are probably the only reform that keeps surfacing as effective. Overall, the disparate findings should not be surprising given differences in the legal and claims data sets, econometric methods, variables, and time periods. A more recent survey (Mello 2006) reaches the same conclusion.

### 2.1. Data Description

The study draws from two main data sources: (1) a database of medical malpractice payments and (2) a database of tort reforms affecting medical malpractice claims. Each data source is discussed in detail below. After describing both data sets, I explain one of the contributions of this study, which is correctly matching malpractice payments to tort reforms.

### 2.2. Medical Malpractice Payments

I obtained medical malpractice payment information from the NPDB Public Use Data File, dated December 2005 (U.S. Department of Health and Human Services 2005). This file is published quarterly by the U.S. Department of Health and Human Services in accordance with the Health Care Quality Improvement Act of 1986 (42 U.S.C. secs. 11101–11152 [1986]) and its implementing regulations (45 C.F.R. 60). Beginning September 1, 1990, these laws require that (with some exceptions) all medical malpractice payments be reported to the Department of Health and Human Services within 30 days of payment.[11]

While the original database contains more than 240,000 medical malpractice cases—both court awards and settlements—in all 50 states, the District of Columbia, and U.S. territories, payments were made in only about 160,000 between 1991 and 2005. Moreover, because the data set overrepresents cases that settle early, which most likely involve minor injuries with smaller damages, the data for this study were limited to settlements in the 50 states for injuries occurring between 1991 and 1998. The final data set comprised 105,944 cases. Table 1 provides descriptive statistics of these data.

Like any other database, the NPDB is not perfect.[12] I ran analyses in which I corrected in different ways for the identified problems.

11. Self-insured practitioners originally reported their malpractice payments. However, on August 27, 1993, the U.S. Court of Appeals for the D.C. Circuit reversed the December 12, 1991, federal district court ruling in *American Dental Association v. Shalala* (3 F.3d 445 [U.S. App. D.C. 1993]) and held that self-insured individuals were not "entities" under the Health Care Quality Improvement Act and did not have to report payments made from personal funds. All such reports have been removed from the National Practitioner Data Bank (NPDB).

12. Avraham (2006a) contains a detailed analysis of the NPDB's deficiencies and a comparison with other databases that are widely used in the literature.

## 2.3. Tort Reforms

The DSTLR, which supplied state-level variables, is a new data set I compiled with the assistance of a National Science Foundation grant (Avraham 2006b) The database was assembled by cross-referencing my own review of the laws and court cases of the 50 states (and Washington, D.C.) from 1980–2005 to existing compilations.[13] In the process, it was discovered that other available compilations suffer from one or more of the following problems: missing reforms, missing or erroneously coded dates that reforms were in effect (hereafter, effective dates), missing or incorrectly reported Supreme Court cases reviewing the constitutionality of these reforms, and a lack of information regarding whether the law requires that the jury be advised of the applicable reform. In contrast, the DSTLR includes complete variables for reform title, description, effective date, whether the jury was advised or explicitly not advised of the applicable rule, whether the reform was upheld or struck down by the state supreme court, and, in such cases, whether it was amended, repealed, or replaced by another law. In the spring of 2006, the first draft of the data set was posted online. After correcting various errors, the second draft was posted online in the fall of 2006. The second version of the DSTLR was used for this study.[14]

In this study I explore the impact of six tort reforms. These reforms are either enacted by a state's legislature or adopted by its courts.[15] These six reforms not only appear in the medical malpractice bill passed by the House or the Senate but also are the most prevalent tort reforms in the United States.[16]

The most common reform passed in state legislatures in recent decades is the limitation of joint and several liability for malpractice de-

13. The compilations include the American Tort Reform Association, State and Federal Reforms (http://www.atra.org/reforms/); National Conference of State Legislators, Medical Malpractice Tort Reform (http://www.ncsl.org/standcomm/sclaw/medmaloverview.htm); Westlaw, 50 State Surveys, Health Care, Medical Malpractice, Tort Reform; and Cohen (2005).

14. I wish to thank Bentley MacLeod and Dan Carvell for sending me a long list of corrections, almost all of which were found to be accurate.

15. Sometimes courts, rather than state legislatures, adopt tort reforms. Judicial adoptions of higher evidentiary requirements for securing punitive damages are the most common type of nonlegislative reform.

16. I was originally interested in the nine reforms that appear in the deferral bill. However, because of a lack of enough variation during the relevant years for two of them (statute of limitations and contingency fee reforms) and a lack of relevance for one of them (limiting liability for drug manufacturers), I eventually analyzed just six of them.

**Table 1.** Descriptive Statistics

| Variable | Description | Mean | SD |
|---|---|---|---|
| Dependent variables: | | | |
| Average payments | Settlement amount ($) | 249,988 | 90,918 |
| Number of cases | Number of settlements per doctor (per person) | 1620 | 682 |
| Total payments | Total annual payouts per state ($) | 52,100,000 | 68,200,000 |
| Reforms: | | | |
| R_CN | Equals one if state has passed a cap on noneconomic damages | .32 | .47 |
| R_CP | Equals one if state has adopted a cap on punitive damages | .36 | .48 |
| R_CS | Equals one if state has adopted limitations on the collateral source rule | .62 | .48 |
| R_JS | Equals one if state has adopted limitations on joint and several liability | .75 | .43 |
| R_PE | Equals one if state has adopted evidentiary requirements for punitive damages | .54 | .50 |
| R_PP | Equals one if state has allowed periodic payments | .58 | .49 |
| State controls: | | | |
| C_65 | Percentage of population over 65 | 12.66 | 2.04 |
| C_BS | Percentage with bachelor's degrees | 22.18 | 4.33 |
| C_CarDeath | Car fatalities per million people | 173.87 | 56.05 |
| C_HealthSpend | Per capita health care expenditures ($) | 4,323.83 | 558.471 |

| | | | |
|---|---|---|---|
| C_Income | Annual income per capita ($) | 26,866 | 3,911 |
| C_Lawyer | Lawyers per capita | 289.85 | 90.73 |
| C_LifeExp | Life expectancy for newborns (years) | 75.65 | 1.28 |
| C_MedCPI | Consumer price index for medical goods | 213.11 | 21.12 |
| C_Metro | Metropolitan percentage | 67.51 | 20.72 |
| C_NewRes | Percentage of new residents | 3.27 | 1.42 |
| C_Pop | Population (1,000s) | 5,242 | 5,714 |
| C_Unempl | Unemployment rate | 5.58 | 1.55 |
| Individual controls: | | | |
| I_AgeGroup | Age of physician at event (years) | 42.78 | 10.76 |
| I_CaseLength | Case length (years) | 3.45 | 1.54 |
| I_Grad | Year of graduation | 1969.89 | 11.39 |
| I_StFundPay | Equals one if state fund paid | .03 | .18 |
| I_Field | 15 Fields of physicians | — | — |

**Note.** Dependent variables and individual controls are from U.S. Department of Health and Human Services (2005), and reforms are from Avraham (2006b).

fendants. Forty-one states had some variation of this reform in 2004. The common-law doctrine of joint and several liability allows the plaintiff to collect full damages from any of the defendants irrespective of the defendant's proportional fault, should one defendant be insolvent. This means that the plaintiff can go after "deep-pocket" defendants, like hospitals, and collect all damages even if the doctor is the main party at fault. The reforms adopted by states limit this possibility by either imposing liability based on fault or allowing for joint and several liability only if the defendant is responsible for a significant proportion of the harm, usually at least 50 percent.

Discretionary or mandatory consideration of payment for medical costs is another common reform established in 35 states by 2004. The collateral source rule was developed by common-law courts in the nineteenth century when insurance became more popular. The rule says that the defendant's damages will not be offset by the plaintiff's insurance coverage. An implication of this rule is that the plaintiff may receive compensation for more than his or her full harm in case of an accident. The reforms adopted by states either require or allow courts to offset the plaintiff's private and/or public insurance benefits from the awarded damages.

Periodic payment of large future damage awards is now allowed or required in 23 states. The reform allows or requires courts to award future damages that are above some threshold, usually $200,000, in periodic installments. This reform eases the burden on the defendant, who can purchase an annuity for that purpose, and can potentially relieve a defendant of a portion of his or her due damages if the plaintiff dies before the damages are fully awarded.

Some of the more controversial reforms involve caps on damage awards. These caps most commonly apply to noneconomic damages (23 states) or punitive damages (27 states). There are many types of caps. Some reforms impose a cap of a fixed dollar amount, sometimes indexed to inflation, while others use a multiplier of the economic damages. Many states have implemented heightened pleading, evidentiary, or other procedural standards for punitive damages (34 states). For example, many states now require punitive damages to be proven with "clear and convincing evidence" rather than merely the traditional "preponderance of the evidence." Other states require proof that defendant acted with "deliberate disregard" or "willful indifference" toward the plaintiff's potential injury.

### 2.4. Matching Reforms to Payments

After identifying the pertinent data sets, analyzing them together required accurately matching the malpractice payments to tort reforms. There are several important legal substantive and related methodological issues associated with the effective date of the reform that are carefully addressed in this study that enhance the reliability of the results. First, in many cases there is a lag between enactment date and effective date. For example, a reform might be passed on July 1 with an effective date of January 1 the following year. Second, legislative creation of reforms has only (subject to some qualifications presented below) prospective application unless expressly made retroactive, whereas striking down reforms has retrospective applicability, because striking down a law declares it to never have been constitutional. Yet the prospective applicability of the enactment of reforms is not as straightforward as it initially seems. For the purposes of matching a case to the applicable legal regime, the relevant date in most cases is the injury date and not the complaint filing date. Because those malpractice cases with injury dates before the passage of a tort reform were subject to the prereform legal regime, careful coding assured that this was accurately reflected in the data. Previous studies have (mistakenly, in my view) assumed that the relevant date is the filing date.[17] The general common-law rule, however, is that tort reform is not a mere procedural rule but actually affects the substantive rights of medical malpractice plaintiffs and thus may not be applied retroactively to pending cases unless expressly stated by the legislature.[18] Moreover, even if the legislature explicitly dictated that the

17. For example, Danzon (1984) seems to have used the filing date as the relevant date for whether the reform applies to specific cases. Thus it seems that many of the claims used in her data set, those closed between 1975 and 1978, should not have been coded as subject to reforms that were enacted in 1975 and 1976. First, claims closed in these years were most likely for injuries that occurred before 1975. Second, those claims that did occur after 1975–76 and closed before 1978 were likely only the small claims, a fact that creates selection bias in the analysis. The same problem appears in Danzon (1986, p. 80), which argues that "tort reforms are likely to affect the filing and disposition of claims during the calendar years in which the laws are in effect." Sharkey (2005, table 1) uses filing dates instead of injury dates for Ohio and Illinois reforms in her analysis of punitive damages. However, for many of the punitive damages reforms, the effective date is indeed the filing date.

18. The general principle that statutes operate only prospectively unless expressly stated "is familiar to every law student" (*United States v. Security Industrial Bank*, 459 U.S. 70, 79–80, 103 S. Ct. 407, 413, 74 L. Ed. 2d 235 [1982]). The principle has been applied repeatedly to medical malpractice reforms. See *Davis v. Omitowoju*, 883 F.2d 1155 (CA3 V.I. 1989); *Marcel v. Louisiana State Dep't of Public Health*, 492 So. 2d 103 (La. 1986),

tort reform would apply retroactively on the basis of the filing date, courts might strike it down as unconstitutional on due process grounds.[19] The general rule is therefore that tort reform applies only to injuries occurring after the effective date and does not apply to injuries occurring before the effective date even if the complaint is filed after the effective date. The rare exception is when legislatures issue an explicit stipulation that the statute applies to cases filed after the effective date and that stipulation is not struck down as unconstitutional.[20]

The injury date is the relevant date for most, but not all, of the reforms. Some reforms explicitly stipulate that they apply to cases filed after the effective date, and thus essentially apply to pending cases as well.[21] This means that even injuries that occurred before a reform was enacted would have been subject to the postreform legal regime (assuming that they were not resolved after the reform was struck down). Since the NPDB does not provide the filing date, the applicability of these reforms to the appropriate cases could not be adequately coded.

Third, the retroactive applicability of striking down a reform implies that malpractice cases initiated after a reform was enacted, yet pending at the time the reform was struck down, should be treated as subject to the prereform regime. The reason is that striking down a reform as unconstitutional declares it to never have been valid law. I suspect that many of the previous studies, especially those done by nonlawyers, neglected to account for this point. Moreover, striking down reforms in-

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cert. den. (La.) 494 So. 2d 334; *Martino v. Sunrall*, 619 So. 2d 87 (La. 1993); *Graley v. Satayatham*, 343 N.E.2d 832 (Ohio 1976); *Allen v. Fisher,* 118 Ariz. 95, 574 P.2d 1314 (1977); *Bolen v. Woo,* 96 Cal. App. 3d 944, 158 Cal. Rptr. 454 (1979); *Robinson v. Pediatric Affiliates Medical Group,* 98 Cal. App. 3d 907, 159 Cal. Rptr. 791 (1979).

19. See, for example, *Simon v. St. Elizabeth Medical Center*, 355 N.E.2d 903 (Ohio 1976); in *Martin by Scoptur v. Richards*, 531 N.W.2d 70 (Wis. 1992), the Supreme Court of Wisconsin determined that a medical malpractice act that applies to actions "filed on or after June 14, 1986," had retroactive application to cases that occurres before that date and was therefore unconstitutional. See also *Neiman v. American Nat. Property and Cas.,* 236 Wis. 2d 411, 613 N.W. 2d 160 (Wis. 2000). But see *Crowe v. Wigglesworth,* 623 F. Supp. 699 (1985), which applied Kansas law and determined that the collateral source rule is a procedural rule governing the admissibility of evidence and therefore can be applied retroactively.

20. See note 52.

21. For example, Louisiana's caps on punitive damages apply to all actions filed after January 1, 1992 (La. Civ. Code, art. 3546). More often, however, legislators explicitly stipulate that a reform should be construed propectively. See, for example, a Maryland reform states, "[T]his Act shall be construed only prospectively and may not be applied or interpreted to have any effect on or application to any cause of action arising before the effective date of this Act" (Acts 1994, ch. 477, sec. 2). In most cases, however, the legislature is silent, in which case the general rule explained above applies.

troduces the most problems for state-level analyses, as are many of the studies discussed above.[22] To see why, consider two cases that occurred after a tort reform was in effect. One of them was resolved while the reform was in effect, while the other was settled after the reform was reversed. A state-level analysis that lumps together all payments for cases accruing in a given year will use both cases to estimate the effect of tort reform on settlement payments. This is an artifact of aggregating up to construct the variables at the state-year level, as many time-series analyses do when they are looking for the effect of a state-level variable (reform in a state by year) on an aggregated state-level variable (the average settlement of all cases in that state-year). This of course would be an error because only one of them was subject to the tort reform regime. Since the constitutionality of many reforms is usually challenged within a few years of their enactment, many cases that accrue after a reform was enacted are resolved after the reform is struck down and thus are not subject to the reform regime.[23]

To illustrate this point, consider the following example. Illinois passed caps on noneconomic damages that applied to "causes of action accruing on or after" March 9, 1995 (735 Ill. Compiled Stat. Ann., art. 5, sec. 2-1115.1). The reform was struck down December 18, 1997 (*Best v. Taylor Machine Works,* 179 Ill. 2d 367, 689 N.E.2d 1057 [1997]). If one counts the payments for injuries that occurred after the reform's effective date but before it was reversed, the number is about 1,155. Yet if one counts the cases that were actually paid before the reversal date (and were therefore really subject to the reform), the number is only about 80, less then 7 percent of the cases. This becomes critical when using a difference-in-differences approach with state fixed effects because the estimation of the effect of tort reforms is performed only on the basis of changes in the reforms.

Accounting for reversal dates to properly link cases to applicable law is not exceedingly difficult when analyzing individual-level data; doing

22. Important studies that use state-level analysis without reporting any correction for reversal of reforms include Danzon (1984, 1986). Studies that report data corrected for reversal are Zuckerman, Bovbjerg, and Sloan (1990, p. 170) and Sloan, Mergenhagen, and Bovbjerg (1989, p. 670). Yet it is not clear whether they corrected the coding of the reforms at the individual level (accounting for the retroactive applicability of reversal) or at the state level. The former is correct way to do it; the latter is not.

23. The problem gets even worse if plaintiffs' lawyers strategically wait until a reform is reversed to settle a case. One would also need to account for defense lawyers' incentives in order to fully account for parties' strategic behavior in the shadow of the forthcoming possibility of a reform's reversal.

so with state-level data is less straightforward.[24] I suspect that previous research has not done it all.

## 3. STATISTICAL METHODOLOGY

### 3.1. Variables

This study examines the effect of various medical malpractice reforms on two state-level and one case-level dependent variables.

*Natural Log of Payment Amount*. This variable measures the size of individual payments. Payment information was obtained from the NPDB (PAYMENT field). All payments were inflated to December 2005 dollars. This variable uses the payment as the unit of analysis, which results in more than 106,000 observations for regression.

*Natural Log of Sum of Payment Amounts per Doctor*. This is a state-level variable that measures the total malpractice payments for each state in a given year. The value was calculated by summing payments shown in the NPDB for each state in each year of study and dividing by the number of doctors practicing in the state that year. The goal of most, if not all, medical malpractice reform is to limit a state's total annual damage and settlement payments. For medical malpractice insurance companies (which collect premiums from doctors), the total annual damage payment per doctor is the variable of interest. All variables using state-years as the unit of analysis produce 400 observations from 50 states over 8 years.

*Cases per 1,000 Doctors*. This state-level variable measures the relative frequency of malpractice payments. Values for each state-year were calculated by counting the number of payments shown in the NPDB for each state in a given year and dividing by the number of doctors (in 1,000s) practicing in the state that year.

The independent variables used include the six tort reforms coded in the DSTLR. The analysis used a collapsed version of the database, the collapsed legal data set, which lumps together similar reforms. Virtually every reform has several types. In this study, the reforms were collapsed

24. In the interest of full disclosure, it should be mentioned that the NPDB does not allow for a perfect remedy to this problem. Because parties can report the case up to 30 days after the payment was executed, a payment made in December of a given year might be reported in January of the following year. As the public records provide the payment year only (and not the exact date), there could be some glitches in my coding for reforms that were struck down in December.

as follows. First, all punitive damages caps were lumped together regardless of their magnitude. Second, all noneconomic damages caps were lumped together regardless of their magnitude. Third, there was no distinction coded between mandatory or discretionary applications of the periodic-payment and collateral source rule reforms. Fourth, there was no distinction between the various types of restrictions on the joint and several liability rule or between the exact higher evidentiary requirements in the punitive evidence reform. Fifth, I did not distinguish between reforms made by the courts and those enacted by the legislature.

One disadvantage of collapsing the reforms is that I cannot distinguish whether each variant of each reform has a distinct impact. In addition, collapsing multidimensional reforms into dummy variables introduces bias against finding any significant impact even where such an impact may exist because noneffective variants will water down the impact of the effective variants. In any case, there were not sufficient state-year observations to statistically measure the impact of each variant of each reform, and collapsing reforms makes results comparable to previous research that combined reforms along similar lines.[25] Thus, the coefficients of the tort reform variables should be interpreted as indicators of average effects.

In addition to the primary tort reform variables of interest, a number of control variables were included to refine the analysis. For the case-level regressions, case-level control variables (or individual-level control variables) were included. Otherwise, average state-level data were used (state-level control variables). Missing values for control variables were imputed using linear regression. Among the control variables, health maintenance organization (HMO) penetration is of specific interest.[26]

25. Most, if not all, studies collapse the reforms in one way or another. For a summary of studies, see U.S. Congress (1993, p. 63). For problems caused because of such collapse, see Mello (2006).

26. Kessler and McClellan (1996) used data from all elderly Medicare beneficiaries treated for serious heart disease. The authors found that "direct" reforms reduce medical costs by 5–9 percent within 3–5 years of adoption without substantially affecting mortality or medical complications. In the category "direct" reforms, the authors include caps on pain and suffering damages, caps on punitive damages, abolition of the collateral source rule, and mandatory prejudgment interest. Conversely, the authors categorized "indirect" reforms as contingency fee reforms, periodic payments, joint and several liability, and patient compensation funds (see Kessler and McClellan 1996, pp. 371–72). Importantly, in Kessler and McClellan (2002), they controlled for health maintenance organization (HMO) penetration on the same population and found that direct tort reforms reduce medical costs by only about 4 percent. Thus, it is important to control for HMO penetration owing to HMOs' role in providing incentives for optimal care.

Table 1 provides descriptive statistics for these variables, and each variable is discussed in detail in the Appendix.

### 3.2. Regression Model

Several variations on the following basic equations were estimated:

$$\mathrm{LP}_{st} = \beta_0\mathrm{Constant} + \beta_1\boldsymbol{R}_{st} + \beta_2\boldsymbol{C}_{st} + \beta_3\mathrm{MalYear}_t + \beta_4\mathrm{State}_s + \varepsilon_{ist}, \quad (1)$$

where $s$ indexes state and $t$ indexes year. The dependent variable is the log of the average payment in state $s$ in year $t$. The term $\boldsymbol{R}$ is a vector of reforms, $\boldsymbol{C}$ is a vector of state-level control variables, MalYear is the year of the injury, and State is the state of the physician.

$$\begin{aligned}\mathrm{LP}_{ist} = {} & \beta_0\mathrm{Constant} + \beta_1\boldsymbol{R}_{st} + \beta_2\boldsymbol{C}_{st} + \beta_3\mathrm{MalYear}_t \\ & + \beta_4\mathrm{State}_s + \beta_5\boldsymbol{I}_{ist} + \varepsilon_{ist},\end{aligned} \quad (2)$$

where $i$ indexes individual awards, $s$ indexes state, and $t$ indexes year. The dependent variable is the log of individual settlement $\boldsymbol{I}$ in state $s$ in year $t$. The rest is the same except for $\boldsymbol{I}$, which is a vector of individual payment characteristics. As the dependent variable of interest is case level, 85,997 individual-level observations were used.

$$\begin{aligned}\mathrm{LTP}_{st} = {} & \beta_0\mathrm{Constant} + \beta_1\boldsymbol{R}_{st} + \beta_2\boldsymbol{C}_{st} + \beta_3\mathrm{MalYear}_t \\ & + \beta_4\mathrm{State}_s + \varepsilon_{ist}.\end{aligned} \quad (3)$$

The dependent variable is the log of total annual payments per doctor per state.

$$\begin{aligned}\mathrm{NP}_{st} = {} & \beta_0\mathrm{Constant} + \beta_1\boldsymbol{R}_{st} + \beta_2\boldsymbol{C}_{st} + \beta_3\mathrm{MalYear}_t \\ & + \beta_4\mathrm{State}_s + \varepsilon_{ist}.\end{aligned} \quad (4)$$

The dependent variable is the number of annual payments per 1,000 doctors per state.

Two specifications are reported in Tables 2–5. Model OLS1 controls only for state fixed effects, year effects, and reforms; model OLS2 adds the state-level (and, if relevant, individual-level) control variables, including HMO penetration. All models use clustering by state to account for nonindependence between case-level data within states. Bertrand,

**Table 2.** State-Level Regressions: Dependent Variable Is Log of Average Award

| | OLS1 | OLS2 |
|---|---|---|
| Caps on noneconomic damages | −.07 (.09) | −.10 (.08) |
| Joint and several liability | −.21 (.20) | −.22 (.20) |
| Collateral source rule | −.03 (.12) | −.03 (.12) |
| Punitive damages evidence | −.01 (.08) | .03 (.08) |
| Caps on punitive damages | .10 (.06) | .09 (.06) |
| Periodic payment | .03 (.08) | .07 (.06) |
| Joint significance (*p*-value) | .70 | .18 |
| Controls | No | Yes |

**Note.** Standard errors are clustered by state. Not reported are year dummies, state dummies, percentage of the population over age 65, percentage of the population with a bachelor's degree, car fatalities per million people, per capita health care expenditures, income per capita, lawyers per capita, life expectancy for newborns, consumer price index for medical goods, percentage of the population living in a metropolitan area, percentage of new residents, unemployment rate, and health maintenance organization penetration. OLS = ordinary least squares. $N = 400$.

Duflo, and Mullainathan (2004) suggest clustering as a way of dealing with the serial correlation that results from the "stickiness" of the law.[27]

## 4. RESULTS

### 4.1. State-Level Analysis

Figure 1 presents the state-level annual average payment. Between 1991 and 1998, the annual mean settlement payment increased steadily from about $185,000 to about $250,000. This 35 percent increase over the 7-year period reflects an average 4 percent annual increase, above and beyond the annual consumer price index (CPI).[28] Figure 2 shows that the annual mean number of cases per state increased from about 258 in 1991 to about 264 in 1998, an increase of 2.3 percent (less than .5 percent a year). Figure 3 shows that the annual mean total settlement payments per state increased from about $46 million in 1991 to $64 million in 1998, an increase of about 39 percent (about 5 percent a year). Together, Figures 1–3 show a worrisome picture of a steady in-

27. By the "stickiness" of the law, I refer to the fact that once a law is enacted it is likely to be in effect in the following years. This creates problems in estimating the impacts of tort reforms. See Kessler and McClellan (2002).

28. It may reflect the higher annual increase in the medical consumer price index, although it is not clear whether that increase does not reflect the higher annual awards.

**Table 3.** State-Level Regressions: Dependent Variable Is Number of Cases per 1,000 Doctors

| | OLS1 | OLS2 |
|---|---|---|
| Caps on noneconomic damages | −2.04** (.69) | −2.52** (.81) |
| Joint and several liability | −1.78** (.56) | −1.59* (.78) |
| Collateral source rule | .15 (.69) | .77 (.68) |
| Punitive damages evidence | 1.45+ (.73) | 1.15 (.96) |
| Caps on punitive damages | −.90 (1.10) | −.89 (.85) |
| Periodic payment | −1.02* (.44) | −1.39 (.85) |
| Joint significance (*p*-value) | .000 | .006 |
| Controls | No | Yes |

**Note.** Standard errors are clustered by state. Not reported are year dummies, state dummies, percentage of the population over age 65, percentage of the population with a bachelor's degree, car fatalities per million people, per capita health care expenditures, income per capita, lawyers per capita, life expectancy for newborns, consumer price index for medical goods, percentage of the population living in a metropolitan area, percentage of new residents, unemployment rate, and health maintenance organization penetration. OLS = ordinary least squares. $N = 400$.

+ Significant at the 10 percent or less level.

* Significant at the 5 percent or less level.

** Significant at the 1 percent or less level.

crease in annual medical malpractice settlement payments. However, these results are misleading because during those years there was an increase of almost 20 percent in the average number of doctors per state. Figure 4 accounts for this growth in the number of doctors per state. While Figure 2 shows a small increase in the average number of cases per state, there was in fact a decrease of more than 18 percent in the number of cases per 1,000 doctors. Similarly, while Figure 3 shows an increase of about 39 percent in mean total payments per state, Figure 5 shows that the mean total payments per doctor fluctuated during the entire period, eventually showing an increase of only about 10 percent.

The decrease in the volume of cases per doctor is consistent with trends documented widely in the medical malpractice claims literature (Clermont and Eisenberg 2002). To the extent that the results presented in Figures 1–5 reflect the impact of tort reforms, they are consistent with the hypothesis that state reforms decrease the lower end of the distribution of awards without affecting total annual payouts because the number of cases decreased concurrent with an increase in the average award.

I now turn to explore the data more systematically. Table 2 reports two different panel ordinary least squares (OLS) regressions of equation

**Table 4.** State-Level Regressions: Dependent Variable Is Total Annual Payment per Doctor

| | OLS1 | OLS2 |
|---|---|---|
| Caps on noneconomic damages | −.15 (.11) | $-.20^{+}$ (.10) |
| Joint and several liability | −.36 (.24) | −.36 (.22) |
| Collateral source rule | −.05 (.14) | −.03 (.13) |
| Punitive damages evidence | .03 (.08) | .07 (.08) |
| Caps on punitive damages | .03 (.10) | .02 (.07) |
| Periodic payment | −.04 (.09) | −.01 (.06) |
| Joint significance (*p*-value) | .32 | .13 |
| Controls | No | Yes |

**Note.** Standard errors are clustered by state. Not reported are year dummies, state dummies, percentage of the population over age 65, percentage of the population with a bachelor's degree, car fatalities per million people, per capita health care expenditures, income per capita, lawyers per capita, life expectancy for newborns, consumer price index for medical goods, percentage of the population living in a metropolitan area, percentage of new residents, unemployment rate, and health maintenance organization penetration. OLS = ordinary least squares. $N = 400$.
$^{+}$ Significant at the 10 percent or less level.

(1). The dependent variable is the log of the (state-level) average settlement payment. The linear regressions show mixed results. Some reforms increase average state settlement payments, and some reforms decrease them, yet none of the reforms were found to be significant. The joint effect of all six reforms was found to be insignificant. Table 3 is similar to Table 2, but the dependent variable is the number of cases per 1,000 doctors. Table 3 shows that the two reforms that reach statistical significance across both models are noneconomic damage caps and limitations on joint and several liability. Caps on noneconomic damages reduced the number of cases by 2.04 per 1,000 doctors to 2.52 per 1,000 doctors, which translates to a reduction of 10–13 percent, depending on the specification. These results are significant at the 1 percent level. Joint and several liability reform decreased the number of cases by 1.59–1.78 cases per 1,000 doctors, which translates into a reduction of 8–9 percent, depending on the specification. The results are significant at the 1 percent level for model 1 and at the 5 percent level for model 2. Periodic-payment reform decreased the number of cases by 1.02–1.39 cases per 1,000 doctors, which translates to a reduction of 5–7 percent, depending on the specification. Yet while the coefficient was significant at the 5 percent level for model 1, it was only weakly significant ($p \approx .1$) for model 2. The joint effect (which is associated with a decrease in number of cases per 1,000 doctors) is significant for both models ($p <$

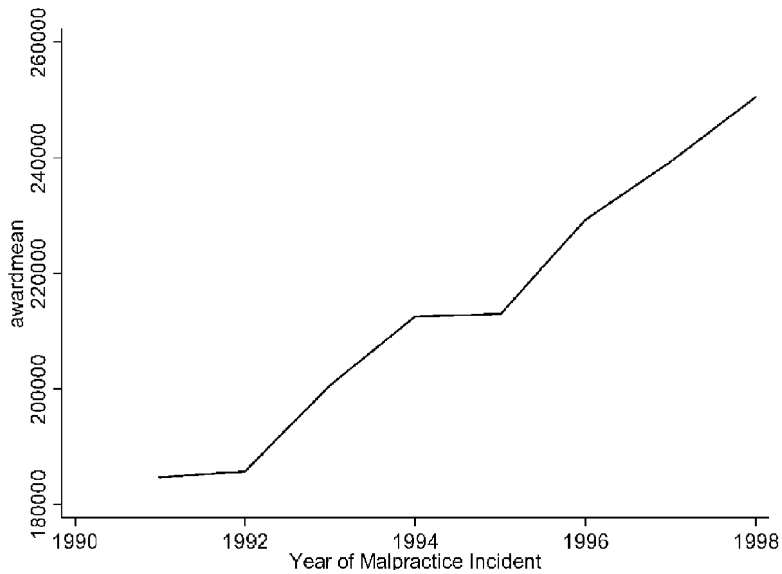


**Figure 1.** State-level annual average payment


**Figure 2.** Average number of cases per state

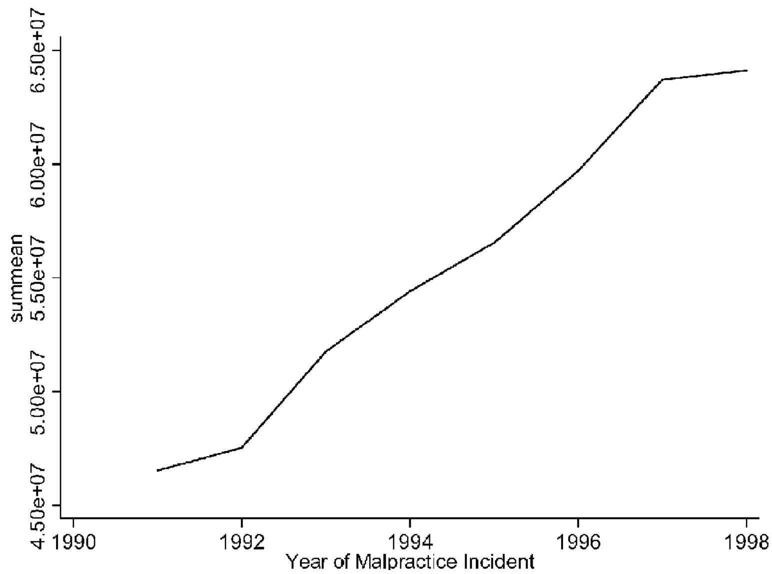


**Figure 3.** Average annual total payments per state

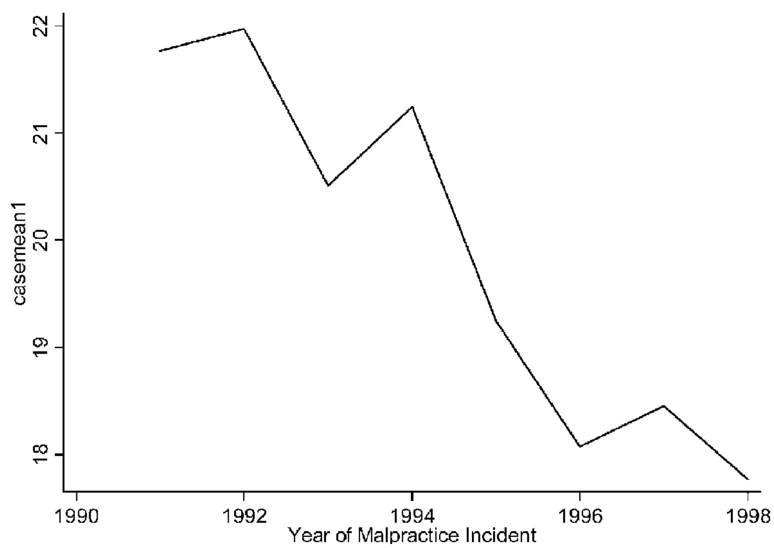


**Figure 4.** Average number of cases per 1,000 doctors


**Figure 5.** Total payments per doctor

.01). Table 4 estimates the effects of tort reforms on the total annual settlement payout per doctor by state-year. Caps on pain and suffering damages reduce the total payments by 15–20 percent, depending on the specification. Yet the coefficient was found to be significant only in model 2 ($p = .52$). Reforming the joint and several liability rule was estimated to decrease total payments by 36 percent, yet the coefficient was not significant at the 10 percent level. All other reforms were identified as not having a significant effect on total annual settlement payouts. Reforms were not found to be jointly significant, although in model 2 they were close to the 10 percent level.

In sum, state-level analysis shows that caps on pain and suffering damages reduce the number of cases by 10–13 percent and potentially reduce the total annual payment per doctor by 15–20 percent. Yet the statistical significance of these estimates is not strong for total annual payments. Reforming joint and several liability decreases the number of cases by 8–9 percent. Periodic-payment reform decreases the number of cases by 5–7 percent, yet this effect was weakly significant in model 2. All other reforms did not show an independent statistically significant effect on the dependant variables. Finally, the joint effect of enacting all

**Table 5.** Individual-Level Regressions: Dependent Variable Is Log of Individual Payment

| | Without Correction | | With Correction | |
|---|---|---|---|---|
| | OLS1 | OLS2 | OLS1 | OLS2 |
| Caps on noneconomic damages | .01 (.05) | −.01 (.04) | −.74** (.24) | −.65** (.18) |
| Joint and several liability | −.08 (.13) | −.10 (.11) | $.37^{+}$ (.19) | .18 (.19) |
| Collateral source rule | .02 (.06) | .04 (.06) | $-.32^{+}$ (.16) | −.17 (.11) |
| Punitive damages evidence | .03 (.05) | .06 (.05) | $-.15^{+}$ (.08) | −.05 (.06) |
| Caps on punitive damages | $.10^{+}$ (.06) | .03 (.04) | .21* (.08) | $.09^{+}$ (.05) |
| Periodic payment | −.07 (.10) | −.03 (.08) | −.54** (.19) | −.38** (.11) |
| Joint significance ($p$-value) | .37 | .44 | .000 | .000 |
| Controls | No | Yes | No | Yes |

**Note.** Standard errors are clustered by state. Not reported are year dummies, state dummies, percentage of the population over age 65, percentage of the population with a bachelor's degree, car fatalities per million people, per capita health care expenditures, income per capita, lawyers per capita, life expectancy for newborns, consumer price index for medical goods, percentage of the population living in a metropolitan area, percentage of new residents, unemployment rate, health maintenance organization penetration, age of physician at event, year of graduation, state fund paid, and physician's field. OLS = ordinary least squares. $N = 105{,}944$.

$^{+}$ Significant at the 10 percent or less level.

* Significant at the 5 percent or less level.

** Significant at the 1 percent or less level.

six reforms was found to be significant for decreasing the number of cases but not significant for decreasing the average payment or the total annual payments per doctor. These results are consistent with the literature that identifies an effect mainly for caps on noneconomic damages.

However, as is shown in the next section, by exploiting the fact that this data set allows me to analyze the effect of medical malpractice reforms not only at the state level but also at the level of the individual case, I reach much more nuanced conclusions about the real impact of medical malpractice reform.

### 4.2. Individual-Level Analysis

The regressions presented in Table 5 estimate the effects of tort reforms on expected settlement payouts (equation [2]) using more than 100,000 observations of case outcomes from 50 states over 8 years. Estimating the effect of state-level reforms on case-level outcomes provides two advantages. First, case-specific characteristics, such as physician specialty, physician age, and so on, can be controlled for, thereby eliminating any potential bias in the estimation of the reform's effect arising from correlation of these variables with both case outcome and reform. Second, and more important, precise matching of individual cases to applicable law is more accurately accomplished using individual-level data for those cases that were resolved after a reform was struck down. Therefore, the effect of a reform on case outcomes can be accurately estimated. This correction is crucial in difference-in-differences models for accurately identifying the impact of a tort reform.

Table 5 presents individual-level results for the case-level dependent variable of logged case settlement amounts. The first two columns show the results without correction for the retroactive applicability to striking down reforms. The results in model 1, which have no controls, should be comparable to those in model 1 in Table 2. The last two columns present the results after correction for the retroactive applicability of striking down reforms. Several reforms have economically and statistically significant effects on average settlement amounts. Caps on noneconomic damages reduce average awards by 65– percent to 74 percent, depending on the specification ($p < .01$ for both specifications). Joint and several liability and caps on punitive damages increase the average award, yet these effects were not strongly significant in model 1 and not significant at all in model 2. The collateral source reform decreases average awards by 17–32 percent, depending on the specification. Yet this effect was not strongly significant in model 1 and not significant at all in model 2. Finally, periodic-payment reform decreases average awards by 38–54 percent, depending on the specifications ($p < .01$ for both specifications).

However, there is a reason to be suspicious of this result because there are only two variations in periodic-payment reform in the relevant years; two states enacted the reform in the relevant years.[29] Identifying the effect of reform from only a few states is generally suspect. All other reforms were not identified as having economically or statistically significant effects. Finally, while the test for joint significance of the state-level regressions was not significant, the test for the individual-level regressions suggests that the joint effect is negative and economically and statistically significant ($p < .01$).

29. Alabama struck down the reform in 2005, but the impact on my study is null. See Table 6.

## 5. DISCUSSION

This study evaluates the effectiveness of tort reforms in accomplishing the purported goals of their proponents and not whether the reforms are efficient.[30]

Effective reforms may not necessarily reduce average payout. Tort reforms can cut off the lower or the upper tail of the distributions of awards. For example, caps on damages may cut off the upper tail of the distributions. Limitations of contingency fees, in contrast, may cut off the lower distributions. While both reforms are expected to decrease number of cases as well as total annual payments, they differ with respect to their influence on average awards. Whereas reforms that cut off the upper tail are expected to decrease average awards, those that cut off the lower tail are expected to increase average awards.[31]

Similarly, tort reforms may increase or decrease the number of lawsuits, first, because of their ambiguous impact on the incentives to settle once an injury occurs and/or, second, because of their ambiguous impact on health care providers' original behavior. With respect to the first reason, if damages become more certain under various tort reforms and the bargaining range narrows, we would expect the number of settlements to increase. On the other hand, if tort reforms bring defendant stakes more in line with plaintiff stakes, then we may see more trials and fewer settlements (Priest and Klein 1984). With respect to the second reason, tort reform may increase the number of settlements because doctors may (rationally) exercise a lower level of care when they know that they are partially insulated from liability (for example, because of caps on damages; Bovbjerg et al. 1996). Similarly, a medical center may (rationally) post fewer warning labels or less frequently train its staff when restrictions on joint and several liability apply, knowing that, for example, in the event of misuse of a medical device, the doctor or nurse will be held primarily liable and the medical center may no longer be responsible for the doctor or nurse's liability.

Since even an effective reform can have an ambiguous effect on the

30. Probably every academic study that explores the impact of tort reforms makes this caveat. See, for example, Danzon (1986, p. 79), Viscusi et al. (1993, p. 175), and Viscusi and Born (2005, p. 41).

31. A second possible reason why effective reforms do not necessarily reduce average payout is that cognitive biases lead jurors toward the maximum possible payout (designated by the cap) simply because this amount is a prominent anchor in their minds. Prior laboratory research predicts just such an effect. Yet in many states the jury does not know about these caps, which may undermine this logic.

annual number of cases and average awards, a relevant question that requires our attention is whether the total annual payout increases or decreases.

The regression results of this study estimate a decrease in average settlement of between 65 and 72 percent with the enactment of caps on noneconomic damages and between 38 and 54 percent with periodic-payment reforms, depending on the specification. (However, the impact of periodic-payment reform might be attributed to the fact that there are only two variations of the reform in the relevant years and to its high correlation with the other two reforms.) In comparison, Danzon (1986) found that noneconomic damage caps reduced average awards by 19 percent in one study and by 16–26 percent in another.

Indeed, it is not surprising that caps on noneconomic damages have a large effect. Various scholars have estimated that pain and suffering awards make up approximately 50 percent of total awards, at least in some types of personal injury cases (Danzon 1984, 1986; Vidmar et al. 1998; Viscusi 1988). Many caps are set by legislatures at $250,000, while more than 30 percent of the cases in the data set include awards larger than that. Indeed, the mean (median) award of cases not subject to caps is $262,000 ($132,000), whereas the mean (median) award of cases subject to caps is only $199,000 ($84,000).

What is perhaps surprising, however, is that periodic-payment reform decreases average settlement payments by more than 40 percent. There are only two variations of the reform in the relevant years, which suggests that the estimation is significantly biased.[32]

### 5.1. Concerns about Effective Reforms

The analysis shows the importance of employing individual-level data and correcting for the retroactive applicability of tort reform. While analysis at the state level suggests that none of the reforms have an effect on the average award, analysis at the individual level suggests that caps on noneconomic damages have a significant impact. At the state level it is hard—if not impossible—to correct for the retroactive applicability of striking down a reform.[33] Thus, previous literature underestimated

32. However, it is still worth thinking about the mechanism by which periodic-payment reform might have an impact. See Avraham (2006a).

33. There are several reasons for this. First, assuming that the data are tabulated by the injury date (which is, as a default, the more reasonable date to use), then for any number of cases that appear in any given reform year, some cases will be settled before the reform is struck down and some cases will be settled after. But in state-level analysis

the effect of tort reforms because it analyzed the data at the state level and did not correct for such retroactive applicability. As Table 5 shows, the large impact of tort reform does not stem from simply employing individual-level analysis but rather from correcting for judicial reversal by matching cases to the applicable law.

It is not surprising that recoding the payments made after a reform was struck down as not subject to the tort reform so drastically changed the estimated effect of the reform. Recall that in the difference-in-differences approach the coefficients are identified from changes in the reform variables. It is therefore important to include a change in tort law when a law is struck down. Indeed, as Table 6 shows, five out of the 10 variations for noneconomic damages are due to reversals.[34]

This might explain why other reforms were found to have no effect. As was shown above, to be able to identify the effect there must be a sufficient number of reforms struck down in the relevant years. For the other reforms in the years in this study, there might be enough reforms struck down to bias the coefficients if the data set is not corrected for reversals yet not enough reforms struck down to adequately identify an effect.[35]

The only other study that seems to conduct an individual-level analysis is Sloan, Mergenhagen, and Bovbjerg (1989). The authors found that caps on noneconomic damages reduced payments by 37 percent and that limitations on the collateral source rule reduced payments by 21 percent, yet the latter was not found to be statistically significant. While it is not clear whether the authors accounted for the retroactive applicability of striking down a reform in their analysis, it seems they did not.

However, despite the similarity of the individual-level results with previous research, I believe that the coefficients identified here are most

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they are indistinguishable. Second, selection problems result if cases that were settled after the reform was struck down are dropped from the data. Third, if the data are tabulated by payment year, then for any number of cases that appear in any given reform year, some cases will be from injuries that occurred before the reform was enacted and other cases from injuries occurring after enactment. But in state-level analysis they are indistinguishable, and dropping any one type of case causes selection problems.

34. See Table 6, note a, for the states that reversed caps on noneconomic damages.

35. Specifically, as Table 6 shows, for joint and several liability there was only a single reversal out of the five variations. For caps on punitive damages, there were only three reversals out of the 12 variations. For higher evidentiary requirements for punitive evidence, there were only two reversals out of the nine variations. For periodic payments, there were three reversals out of only three variations. For collateral source rules, there were six reversals out of the eight variations.

**Table 6.** Enactments and Judicial Reversals in States with Reforms

| | | | | | | | | | Relevant Variations | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1991 | 1992 | 1993 | 1994 | 1995 | 1996 | 1997 | 1998 | State-Level Analysis | Case-Level Analysis |
| Caps on noneconomic damages[a] | 16 | 14 | 14 | 14 | 15 | 18 | 19 | 17 | 9 | 10 |
| Joint and several liability[b] | 33 | 35 | 35 | 35 | 36 | 36 | 37 | 36 | 5 | 5 |
| Collateral source rule[c] | 32 | 32 | 31 | 31 | 31 | 31 | 30 | 29 | 5 | 7 |
| Caps on punitive damages[d] | 18 | 17 | 18 | 18 | 19 | 22 | 24 | 24 | 11 | 12 |
| Punitive damages evidence[e] | 27 | 29 | 29 | 30 | 32 | 34 | 34 | 32 | 9 | 9 |
| Periodic payment[f] | 31 | 31 | 31 | 31 | 29 | 29 | 29 | 29 | 2 | 3 |
| Split recovery[g] | 5 | 6 | 6 | 5 | 4 | 5 | 5 | 5 | 5 | 5 |
| Caps on total damages[h] | 7 | 7 | 7 | 7 | 7 | 6 | 6 | 6 | 1 | 1 |
| Patient compensation fund | 10 | 10 | 10 | 10 | 10 | 10 | 10 | 10 | 0 | 0 |
| Contingency fees fund | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 0 | 0 |

**Note.** If a reform was enacted or reversed on or after July 1, it was coded as enacted or reversed in the following year. The values for state-level analysis include changes not directly reflected in the year columns. For example, in 1995, Kentucky struck down collateral source rule reform and Wisconsin enacted it. While the year columns show no change in the number of states with collateral source rule reform between 1994 and 1995, this is counted as two variations. The values for case-level analysis include reversals that occurred after 1998 and would apply to pending cases. For example, Ohio reversed collateral source rule reform in 2000 and Kentucky in 2002. Therefore, the case-level column has two more variations than the state-level column.

[a] Alabama and Ohio struck down the reform in 1992, Illinois and Ohio in 1998, and Oregon in 2000.

[b] Ohio struck down the reform in 1998.

[c] Kansas struck down the reform in 1993, Ohio in 1998, Kentucky in 1995, Alabama in 1997, Rhode Island in 2002, and Missouri in 2005.

[d] Alabama struck down the reform in 1992, Ohio in 1998, and Idaho in 2003.

[e] Illinois and Kentucky struck down the reform in 1998.

[f] Ohio and Arizona struck down the reform in 1995, and Alabama in 2005.

[g] New York struck down the reform in 1994, Colorado in 1995, and Florida in 1998.

[h] South Dakota struck down the reform in 1996.

likely biased and that the real effect is probably much smaller. There are two, and perhaps three, reasons to believe the coefficients may be biased.

The first is that reforms are often struck down within a very short time after enactment. Immediately after a reform is enacted (and perhaps even immediately before it is enacted), interest groups that object to tort reform look for cases that, upon reaching the states' supreme courts, will serve as a cause to strike down the reform. If successful in finding such a case, lower courts and supreme courts would shortly strike down the reform.[36] Thus, it could be that the cases subject to the reforms—which settle before the reform is reversed—are the "easy" ones that settle within a relatively short time. But if these cases also have low expected monetary damages, then I get a correlation between tort reform and low settlement payments. This correlation cannot be interpreted as evidence that tort reform necessarily causes low settlement payments, however. In such a case, I run into a selection bias problem.

The second reason why the coefficients might be biased is that, in coding cases paid after a reform was reversed as not being subject to tort reform, invalidation of the reform is implicitly assumed to be an exogenous shock not related to lawyers' behavior. But this is a strong assumption if interest groups commence efforts to invalidate a reform immediately after (or before) its enactment. If that is true, then settlements in this time period are conducted in the shadow of these efforts. This uncertainty about the future applicability of the tort reform laws might lead parties to behave strategically in delaying settlement. Moreover, if plaintiffs and defendants have different expectations about whether the reform will be reversed, the strategic behavior will increase and settlement might be further delayed (Spier 2007).

I have shown that less than 7 percent of the "qualified" cases in Illinois (in which injury occurred after caps on noneconomic damages were enacted) were in effect subject to the reform; the rest were resolved after the reform was struck down.[37] The same holds for reforms in other states.[38]

36. For example, in the case of caps on noneconomic damages, three of the five reversals by state supreme courts (in the relevant years) were within 4 years of enactment. Lower courts might have reversed them earlier.

37. See Section 2.4.

38. For example, in Alabama, less than 8 percent of the qualified cases were resolved before the caps on noneconomic damages were struck down; in Arizona, less than 25 percent of the qualified cases were resolved before the periodic-payment reform was struck

To summarize this point, the actions of a state's supreme court may be anticipated, which means that invalidation is not an exogenous shock, and lawyers in fact settle cases with a view to the future action of the supreme court. This endogeneity bias might lead to overestimating the impact of tort reform.

A third reason that the results could be biased is because of yet another endogeneity bias. The problem arises because states are not randomly assigned every year to a "tort reform" condition or "no tort reform" condition as a "perfect" experiment requires. Moreover, an unobserved variable in the error term might be correlated with the tort reform dummies. This might lead to a reverse causality problem: instead of tort reform affecting tort awards, which is the direction of causality that I have assumed so far, tort awards might affect tort reforms. The result of this phenomenon would be that reforms that were determined to be effective are, in fact, not effective.

There are three main reasons to believe that this specific endogeneity problem is not that significant in this study. First, in some states, reforms were adopted by courts rather than by legislatures. Higher evidentiary requirements for punitive damages is an example of such judicial reform. For these reforms, the endogeneity story described above does not neatly apply. Second, many of the reforms enacted in the 1990s are not specific to medical malpractice but apply generally to all types of torts. Thus, even if reforms were enacted in reaction to large awards, they would not (in those more general cases) be a result of large medical malpractice awards. Third, recall that, because I apply a difference-in-differences approach, the effects of the reforms are identified from changes in the laws. But changes in the laws include not only enactments but also reversals. Thus, while the claim that a large award might prompt a state to enact tort reform is reasonable, for a claim of reverse causality to hold, it also has to be that small awards prompt states' supreme courts to strike down reforms. But this is much less reasonable, as it seems that plaintiffs' lawyers attempt to strike down the reforms without any correlation to actual awards.[39] Finally, insofar as the findings suggest a decrease in settlement payments due to tort reform, it is unlikely that

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down. In Ohio, less than 14 percent of the qualified cases were resolved before the collateral source reform was struck down.

39. One may argue, however, that in anticipating small awards lawyers fight to strike down the reform. Yet the important point is that lawyers' efforts to strike down tort reforms seem to be unrelated to the level of actual awards in the data set.

lower settlement payments motivated legislatures to adopt medical malpractice reforms.

To further deal with the problem of endogeneity, one would want to add a state-specific time trend variable. This controls for linear time trends of awards (or number of cases) that may be present in different states. At least in theory, such a specification can help with the problem of endogeneity because it controls for the existence of large prereform awards. In practice, however, there are three main problems with taking this approach in this study. First, it comes at the expense of the statistical power of the regression because it introduces another 50 variables to the equation. This creates problems in the state-level analysis. Second, it controls for a linear time trend only, while the actual trend may be nonlinear; adding nonlinear variables will exacerbate the problem identified above. Third, and most important, many of the reforms were enacted in 1991–92 and struck down in 1997–98, so there were not enough prereform and postreform data points to adequately estimate the impact.

Finally, one can deal with the problem of endogeneity by taking an instrumental variables approach.[40] This approach, however, is problematic in studies like mine in which the independent variable of interest (tort reform) is so sticky. To better see the problem with the stickiness of the law, consider as an instrumental variable a dummy that indicates whether, in a specific state and year, the Republican Party was in control of the state government. The hypothesis is that a Republican-controlled government is more likely to pass a tort reform yet is not correlated directly with the dependent variable (tort awards, number of cases, and total payments). For this instrument to be theoretically valid, a Democrat-controlled government should be less likely to pass a tort reform and more likely to cancel a tort reform. But this has (probably) never happened. Tort reform is sticky once it is on the books; only the state courts can strike it down, and this should be, at least in theory, unrelated to whether there is a Democrat-controlled government. It seems that the stickiness of the law might cause theoretical problems for any other instrument.

Despite these theoretical problems, I ran two-stage least squares regressions. I used various combinations of the following variables as in-

40. Danzon (1986) reported both ordinary least squares and two-stage least squares (which are regressions using an instrumental variables approach) results in her regressions. Zuckerman, Bovbjerg, and Sloan (1990) criticized her approach, making arguments similar to the ones in this paper.

struments (all of which have been used by other researchers): (1) whether Republicans controlled the state government, (2) whether a state had previously enacted some kind of a product liability reform, (3) whether a state had previously enacted some kind of a class action reform, (4) the percent of state population that is Roman Catholic, (5) the percent of state population that is Mormon, and (6) whether state legislators have term limits.[41]

Unfortunately, I ran into most, if not all, of the following problems in all my specifications: (1) the instrumental variable coefficient (or coefficients, if I used more than one instrument) in the first stage had the sign opposite than expected, (2) the coefficient (or coefficients) in the first stage was not significant, (3) the $R^2$-value in the first stage was small, (4) the coefficient in the second stage was totally unreasonable, and (5) the specification failed one or more Stata identification tests.[42]

Two problems were most troubling. First, even if an instrument (or instruments) was valid for one reform, it was not valid for others, without any theoretical rationale to justify the difference. For example, I might find that Republican control yielded a significant positive coefficient (with a nice $R^2$-value) in the first stage for one reform (as predicted by the theory that a Republican-controlled government is more likely to pass tort reform) but a significant negative coefficient for another reform. Second, the coefficient estimates and standard errors (in both stages) changed dramatically depending on the combination of instruments I used, without any theoretical rationale to justify these changes. Without analytical consistency or theoretical justification, I had no choice but to abandon the instrumental variables approach for this study. In sum, I believe that the endogeneity bias is probably not a big problem in the analysis, but one needs still to keep it in mind while looking at the results.

### 5.2. Concerns about Ineffective Reforms

While there was significant evidence of an effect on average settlement payment from caps on noneconomic damages and the periodic-payment reform, evidence of an effect from other reforms was less forthcoming. This does not mean that they do not have an effect, of course. There

41. I thank Catherine Sharkey for sharing the first instrument and Jonathan Klick for sharing the other five. For an explanation for the theoretical rationale behind instruments 2–6, see Klick and Stratmann (2005, pp. 13–14).

42. The fact that the instruments did not work for me does not necessarily imply they could not work in studies that look at more years.

might be several plausible explanations for why an effect is not detected. These explanations—in most cases—apply to past research as well.[43]

The first concern is that there might not have been enough reversals in the years studied for the difference-in-differences model to identify an effect.[44] Second, there is a concern that while I reported joint specifications, the separate specifications should have been used. The literature discusses separate specifications that estimate distinct regressions for each tort reform variable, excluding the other possible reforms, and joint specifications that estimate a regression including all tort reform variables as dummies in the same equation (Rubin and Shepherd 2005; Currie and Macleod 2006). The joint specification potentially suffers from some problems. Here the risk is that multicollinearity between some of the reform variables will result in insignificant coefficients. In the separate regressions there is a risk of some bias if one reform is picking up the effect of a correlated reform. Although most of my reform variables are weakly correlated, some do have stronger correlations.[45] Thus, in regressions not reported here, I also estimate a separate regression in which only one reform variable is included at a time. Interestingly, most of the results remain the same, which suggests that the results are robust to this kind of variation.

Third, it could be that dropping cases that lasted more than 6 years (besides reducing the size and depth of the data set) biased my estimators, as these likely represent the most complicated cases, which might be associated with larger awards.[46] To deal with this concern, I ran the regressions for all cases in which injury occurred after 1991, including those that lasted more than 6 years. This increased the sample size to about 139,000 cases. While the size of the coefficients changes, the main results remain the same.

Fourth, it may be that the models specified do not have enough statistical power because they include too many control variables, such that only very large effects will be identified at the customary level of significance. There are two responses to this concern. First, recall that OLS1 includes no control variables (and therefore provides more degrees of freedom to the model), yet the results are very similar to those of the

43. See Avraham (2006a, p. 42) for explanations related to the deficiencies of the NPDB.

44. See the text around note 34.

45. For example, collateral source and periodic-payment reforms have a correlation coefficient of .368.

46. See Section 2.2.

other specifications. Second, this critique applies more to the state-level models than to the individual-level models. One would expect to find in the individual-level data set an effect for the other reforms, if such an effect exists.

Fifth, it is possible that other relevant reforms were not included in the model. Cohen (2004) estimates that about 33 percent of litigated medical malpractice cases involve the death of a patient (of which about 25 percent have a positive payout). If this percentage carries to settlements as well, then many of the cases (about 30,000) are wrongful death cases, for which many states have special legislation that could not be accounted for in this study.[47] In any case, awards are much larger for cases that involve injuries but not death. Indeed, the tort reform machine, including the pending federal bill, is mainly tuned to injuries and not to deaths.

Sixth, it is possible that that the reforms influence certain types of physicians' behavior. Specifically, it could be that tort reforms have an effect on awards involving obstetricians or gynecologists but not on awards for other types of physicians. However, because I checked the effect of tort reform on all types of physicians combined, I might be unable to identify the effect.

Seventh, coding choices in the tort reform law data set may have caused problems in identifying a significant effect. Effects could have been masked by the collapse of various distinct reforms into one reform category. For example, if a reform that imposes several liability on any codefendant has an effect, whereas a reform that imposes several liability only on codefendants whose liability is less than 50 percent does not, then the joint and several liability reform might not have been identified as having an effect because it was coded in a way that combines both of these (and possibly more) permutations. The same holds for variations that exist in every other type of reform in my data set. However, collapsing reforms is a standard practice in the literature and is intended to add statistical power to the model.

47. Until the middle of the nineteenth century, common-law courts barred tort recovery for wrongful death because they were reluctant to allow compensation to those who could not enjoy it: "[I]n a civil court the death of a human being could not be complained of as an injury" (*Baker v. Bolton,*170 Eng. Rep. 1033 [Eng. 1808]). Then courts started to allow recoveries for wrongful death after observing that it was cheaper to kill the plaintiff than to injure him (Malone 1965). Today every state in the United States has some type of statutory remedy for wrongful death. Many states have enacted reforms that cap total damages in wrongful death claims. These are separate from the reforms I have explored here.

Reforms may not significantly affect the number of cases per doctor, but they may affect the time to settlement. For example, once caps on punitive damages are enacted, parties might find it easier to settle because the bargaining range is smaller. Even if it does not have a significant effect on the number of settlements, the smaller bargaining range may help parties who would settle anyway do so more quickly. Conversely, the uncertainty surrounding whether a reform will eventually be invalidated might cause delays in settlements. Ideally, I could have tested this hypothesis, because the NPDB includes both the injury date and payment date. However, the publicly available data set includes only the years and not more exact dates. Thus, it is highly unlikely that I could measure this effect even if the hypothesis is true.

It is also possible that the behavior of plaintiffs' lawyers changes in response to the tort reform, which would negate its effect on the statistics measured by my dependent variables. For example, lawyers may spend more money to overcome higher evidentiary requirements for punitive damages, which would result in the same damage awards. Similarly, lawyers may disguise demands for pain and suffering damages as justifications for punitive damages when the former, but not the latter, are capped (Avraham 2006c). Since I cannot observe such adaptive efforts on the part of lawyers, I am unable to detect their effect. Lawyers might also deal with reforms by pushing for common-law doctrines that will offset the effect of the reforms. For example, after the reforms of the mid-1980s, lawyers may have escaped the effect of caps on pain and suffering damages by itemizing their pleas for pain and suffering damages and moving these itemized claims under the heading of uncapped economic losses.[48] In particular, the boiling-pot hypothesis, which holds that the vast majority of medical malpractice cases are not being handled by the legal system, together with the inability to observe lawyers' adaptive efforts, might explain the lack of effect of tort reforms on my dependent variables.[49] Indeed, it is estimated that only about 8.4 percent of the

48. Malone (1965, n. 63), suggests that loss of consortium for the death of a child and rehabilitation costs are examples of damages that had been general damages but are now considered economic damages and have been itemized in the last decade in response to caps.

49. Support for the claim that the costs of plaintiffs' lawyers may have increased as a result of tort reform can be found in Black et al. (2008), where it is argued that defense lawyer's costs have been increasing in Texas in recent years. Texas enacted several medical malpractice reforms in the mid-1990s. Thus, to the extent that the costs of plaintiffs' lawyers are positively correlated with those of defendants' lawyers, the former may have also increased in Texas, potentially because of the tort reforms.

instances of severe medical injuries are pursued by lawyers.[50] For this hypothesis to hold, one needs to assume that there are frictions in the market for medical malpractice lawyers that impose barriers for entry. Given the role that expertise and experience play in medical malpractice law, this may well be the case.

Another possibility for the lack of detected effect is that reforms did not really change the economic reality of the prereform regime. For example, limiting joint and several liability may not change the regime because long before the reforms were enacted, defendants (who were held jointly liable for their codefendants' fault) had the right to contribution from codefendants. Finding that limiting joint and several liability is potentially effective might imply that the right of contribution was being exercised. Similarly, caps on punitive damages may be set too high to have affected the average settlement. Or it may be that large awards were never collected because of physicians' strategic or nonstrategic bankruptcy. Or, more likely, plaintiffs' lawyers did not attempt to collect beyond the limits of physicians' insurance policies ("blood money"), so the policy limits were set low enough that caps did not actually restrict awards.[51]

Another possible explanation is that reforms had an effect on the distribution of awards but not on their mean. For example, caps may have reduced large awards but inflated small awards, which would result in an unchanged mean. An increase in the number or value of small awards could occur if health care providers engage in more low-harm negligence owing to the externality created by caps. This could also occur if lawyers anchor their settlement expectations to the caps, driving up awards in low-harm cases.[52]

50. Studdert et al. (2007) estimate that only about 16.7 percent of instances of severe medical injuries due to negligence are pursued by lawyers. One may wonder why plaintiffs' lawyers choose to take the old cases (now subject to tort reform) in the first place. A possible explanation is that these cases (now locked by tort reform) were easier and/or cheaper to litigate. Once tort reforms were enacted, it may have made cases subject to them less profitable for plaintiffs' lawyers, who therefore switched to other types of cases.

51. See Baker (2001) for evidence that plaintiffs' attorneys do not pursue personal assets of the defendants. For the effect of policy limits, see also Hyman et al. (2007) and Silver et al. (2006).

52. Others have documented such anchoring as well. For example, in 1993, the Clinton administration attempted to limit the cash compensation of chief executive officers (CEOs) by enacting section 162(m) of the Internal Revenue Code. This eliminated deductibility for executive cash compensation in excess of $1 million. Paradoxically, as the data suggest, the result was that many companies increased cash compensation to $1 million. Rose and Wolfram (2002) suggest that the $1 million limit may have served as a focal point for

Finally, it could be that reforms are ineffectual for their first years. There are two variations of this argument. First, it takes a few years for a reform to have an impact because there is a learning period during which parties are not sure how the reforms will affect settlement payments. Only after some time passes, so goes the argument, are parties able to account for the reform when settling a medical malpractice case. This seems a bit far-fetched. Lawyers on both sides are sophisticated parties, and it is hard to imagine convincing reasons for why they need a significant period of time to learn the implications of a reform. However, to address this concern I ran another set of (unreported) regressions. I added two dummy variables, one to indicate if the reform is in its first 4 years and one to indicate if it is older than that. The results, by and large, do not change. If at all, the impact of reforms was found to decay over time. The second variation of this argument makes more sense. The medical liability insurance companies argue that until a state supreme court upholds a reform, it has no effect on cases because of the risk that the reform could be struck down. If plaintiffs' lawyers delay settlement until after a reform is struck down, that reform, even if technically in effect for a few years, will have no effect on most settlement payments. To test this hypothesis, one would have to control for whether a reform was litigated after it was enacted and whether it was upheld or struck down. Data for this exercise are not readily available.

## 6. CONCLUSIONS AND FUTURE RESEARCH

This study analyzes more than 100,000 settlement cases to suggest that caps on pain and suffering damages do in fact have an impact on settlement payments. In this respect, this study replicates results found for judgments by previous studies. Specifically, caps on pain and suffering damage were found to decrease average payments, number of cases per 1,000 doctors, and total annual payments (although the statistical significance of the latter effect was weak). Yet it remains difficult to determine what the exact scope of the impact on average payments is. It is most likely a combination of both increasing strategic behavior before a reform is constitutionally tested in the state supreme court and lowering average settlement payments. Unlike previous studies, the analysis suggests that requiring (or allowing for) periodic payments of damages

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compensating CEOs. They document a spike in base salaries at $1 million that did not exist before the new tax rules.

for future harm has a potential impact on average settlement payments and some impact on the number of cases. The analysis also suggests that limitations of the doctrine of joint and several liability also decrease the number of cases per 1,000 doctors. However, the joint effect of all six reforms was statistically significant in reducing the number of cases but not (state-level) average awards or total payments.

Several additional conclusions emerge from this analysis. First, the study highlights the importance of understanding what a variation in the law is. This term encompasses not only enacting a law but striking it down as well. Moreover, striking down a law has retroactive applicability in that it generally applies to all pending cases, regardless of whether the injury date was before or after the enactment of the law. Not accounting for the retroactive applicability of striking down reforms in the analysis might bias the results significantly. Second, even if more tort reforms had an actual impact, there are many reasons why various limitations might prevent me from detecting it. Third, while I do find a statistically significant effect for two reforms, there are good reasons to believe that these do not have a significant economic effect on total payments. One of the main reasons is that lawyers probably adapt their legal strategies to the new legal regime. After all, this is exactly what they are paid to do. Their strategies, whether selecting different types of cases, focusing on different types of claims, delaying settlements, or making efforts to mobilize a reversal of the reform, are probably effective in keeping the bottom line unchanged.

The next step should be to analyze the effect of tort reform on macroeconomic variables. The main reason is that the overall welfare effect of a change in the dependent variables never becomes clear. Is a reduction in the number of cases necessarily good? If only a small fraction of cases are being pursued by plaintiffs' lawyers, as the data suggest, then health care providers might not receive enough deterrence signals from the market. Reducing the number of cases even more will erode optimal deterrence even further. Similarly, is a higher average award necessarily bad? If the cases dropped as a result of tort reform are the nuisance cases (those without merit that are settled by defendants simply to avoid legal costs), then the remaining, legitimate cases will have a higher average award. This is a desirable effect. Thus, it is not clear what the policy recommendation should be, no matter what the empirical findings are. It is, perhaps, more productive to explore the effect of tort reform on macroeconomic variables like infant mortality, defensive medicine, life expectancy, or health insurance coverage. These variables might be much harder to collect and analyze but also more informative to policy makers.

## APPENDIX: INDEPENDENT VARIABLES

*Age of Doctor* (I_AgeGroup). Age of the doctor at the time of the malpractice payment, measured in decades. Older doctors may have different levels of care, experience, expertise in modern medical techniques, or accumulated wealth, compared with younger doctors.

*Practitioner Field* (Field_1–Field_14). Eighty-nine codes in the NPDB (LICNFELD field) distinguish among doctors, nurses, dentists, pharmacists, and other types of health care practitioners. I simplified these codes into 14 consolidated codes to control for different risk levels associated with each practitioner and the differing nature of each profession.

*State Fund Payment*. Whether a state fund paid a portion of the settlement payment. This might be an important factor in determining a party's settlement strategy and amount.

*Medical Consumer Price Index* (C_MedCPI). Consumer price index for medical goods (U.S. Department of Health and Human Services 2007) to control for changes in the cost of medical procedures. The consumer price index was assigned to each payment report on the basis of the year of the malpractice incident.

*Income per Capita* (C_Income). The average income for each state and each year serves as a rough estimate of the income of the injured party. Annual figures are from U.S. Census Bureau, Income (http://factfinder.census.gov/servlet/ACSSAFFPeople?_submenuId = people_7&_sse = on).

*Unemployment Rate* (C_Unempl). Unemployment rates for each state and each year. Annual figures are from Bureau of Labor Statistics, States: Employment Status of the Civilian Noninstitutional Population, 1976 to 2006 Annual Averages (http://stats.bls.gov/lau/staadata.txt). As with income per capita, this variable is relevant for the loss-of-income component of the damage award.

*Lawyers per Capita* (C_Lawyer). Number of lawyers per 100,000 population. This variable controls for potential differences in competition among lawyers and accessibility to lawyers caused by different lawyer population densities. Values were obtained from the American Bar Foundation (1988, 1991), and American Bar Association's *Lawyer Population Survey,* 1998–2003 (on file with the author). Data for all other years were estimated from a linear regression of known annual statistics, as described for doctors per capita.

*Metropolitan Percentage* (C_Metro). The percentage of a state's population living in a metropolitan area for each state and year. This statistic is almost identical to the state level of urbanization used by previous researchers. However, metropolitan percentage was more readily accessible. Data for 1990–99 are from U.S. Census Bureau (2000), and data for 2000 are from U.S. Census Bureau, Urban and Rural Population by State: 1990–2000 (http://www.census.gov/compendia/statab/tables/08s0029.pdf). Data for 1988, 1989, and 2001–2003 were not available and were estimated using a regression of the known annual statistics against the time series.

*New State Residents* (C_NewRes). The percentage of residents who moved into the state within the past year. New residents are more likely to sue their doctor because of weaker feelings of trust and loyalty between the patient and doctor as well as unfamiliarity with the patient's health status on the part of the doctor. Data were obtained from the U.S. Census Bureau's *Geographic Mobility Report* (on file with the author). Data were obtained from the U.S. Census Bureau for the years 1988–94 and 1996–98. However, 1988–91 data excluded intraregional movers. These intraregional movers include residents who lived in the same region of the United States (as defined by the Census Bureau) before and after the move. Thus, I estimated these amounts by regressing total immigration into the state onto extraregional immigration into the state for the years in which I had such data. In addition, I estimated data for 1995 and 1999–2003 by taking the average of the years with known data, rather than employing a time-series regression, because there was no clear time trend for this statistic.

*Life Expectancy* (C_LifeExp). Life expectancy for babies born between 1989 and 1991 is used to control for differential health standards across states. In addition, expected remaining life is often used to calculate damages when disabilities are permanent. Data were obtained from Centers for Disease Control for each state for the time period 1989–91 (U.S. Department of Health and Human Services 1999, table F). The same data were used for all years because the changes over time were miniscule.

*Population over 65* (C_65). The percentage of a state's population over age 65 is used to control for the fact that elderly patients have unique medical needs and shorter life expectancies. Senior citizens usually have little to no income, which leads to smaller damage awards for lost wages. Data from 1988–93 were obtained from the Centers for Medicare and Medicaid Services (on file with the author), and data from 1994–2000 and 2002–2003 were obtained from U.S. Census Bureau's *Statistical Abstract of the United States* (on file with the author). Data for 2001 were estimated from a regression of the known years' statistics against the time series.

*Population with Bachelor's Degree* (C_BS). The percentage of a state's population that had at least a bachelor's degree is used to control for the higher income levels of college graduates (used to calculate lost wages). In addition, patients with more education may have a different propensity to take legal action against a doctor for medical malpractice. Annual percentages were attached to payment data using the year of the malpractice incident. Data were obtained from the U.S. Census Bureau (on file with the author) for each year except 1988 and 2003. Data for these 2 missing years was estimated using a linear regression of the known annual values over time.

*Health Care Expenditures per* Capita (C_HealthSpend). Average health care spending by state and year is used to control for differences in the cost of medical care and in the extent of medical care utilization. I gathered the data from 1988–98 from the Centers for Medicare and Medicaid's Office of the Actuary (on file with the author). Data from 1998–2003 were estimated using a regression of the state data from 1988–98 against total national spending.

*Population* (C_Pop). Accounts for the interstate differences in population over time.

*Car Fatalities per Capita* (C_CarDeath). Car fatalities per million people per state and year controls for the state's traffic conditions that influence the number of automobile accidents in a state and therefore injuries requiring medical care. Annual data from 1990–2002 were obtained from the U.S. Department of Transportation, National Highway Traffic Safety Administration, State Traffic Safety Information (on file with the author). Data for 1988 and 1989 were estimated using a regression of known state data from 1980–2002 onto national death figures. The year 2003 was estimated in a similar manner but using the time series instead of national death figures as the independent variable because neither national nor state data were available for that year.

*Health Maintenance Organization Penetration* (C_Hmo). Health maintenance organizations may influence the care level taken by doctors. Kessler and McClellan (2002) controlled for HMO penetration and found significant differences for the impact of tort reforms compared with Kessler and McClellan (1996), which did not control for it.

*Injury Year* (MalYear1_1991-MalYear1_1998). This dummy variable controls for all time-related influences not captured in the other variables.

*State* (ST_1ST_50). Fifty state dummy variables. In 98 percent of the cases, this state was where the doctor worked (NPDB, WORKSTATE field). If the work state was not reported in the NPDB, then I used the state of residence for the doctor. If the home state was not reported, I used the state of medical licensure.

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