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

Abstract. This study 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. Previous studies have failed to correctly identify the effective dates of reforms, to account for the retroactive applicability of striking down reforms, or used highly selected samples of jury verdicts or litigated cases. I employ a new legal data set of tort reforms, which carefully evaluates effective dates as well as when certain laws were overturned. Medical malpractice data comes from the National Practitioner Data Bank, which contains more than 100,000 malpractice settlement payments in the study time frame. The data represent the universe of cases in which doctors paid a positive settlement. Thus, the present study has significant advantages over previous work for being the first study to systematically and adequately explore the impact of tort reform on settlements (in contrast to judgments). Of the six tort reforms examined, only one reform (caps on pain-and suffering damages) reduced the number of annual payments, and two reforms (caps on pain-and-suffering damages and limitation on the collateral source rule) reduced average awards. Caps on non-economic damages also 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 average award or total payments.

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I. Introduction

General Background

Legislative alterations to common law tort doctrines—otherwise known as “tort reform”¹—have been a hot political issue for at least three decades now. 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.² Even at the national level tort reform has made an appearance as President Bush named reforms, such as caps on pain-and-suffering and punitive damages, restricting contingency fee agreement between plaintiff lawyers and their clients, to name just a few, high on his agenda. Indeed, no less than sixteen bills to federalize the various aspects of medical malpractice law (currently governed by state common law) have been debated in the Congress over the last decade.³ Most recently, a bill directed at limiting defendants' liability in medical malpractice lawsuits, was passed by the Senate on May 6th 2006.⁴

Medical malpractice law is clearly an issue of great concern to not only the public at large, but also to many influential organized political and professional associations. Interest groups such as American Trial Lawyers Association (ATLA), American Association of Health Plans (AAHP), American Medical Association (AMA), Pharmaceutical Research & Manufacturers of America (PRMA), to name a few, spend hundreds of million of dollars each year in the battle over tort reform.⁵

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 which 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 by this article.

² See Ronen Avraham, Database of State Tort law Reforms, available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=902711

³ 104 H.R. 3103 (1996), 104 H.R. 956 (1996), 105 H.R. 1091 (1997), 106 H.R. 2242 (1999), 107 H.R. 2563, 107 S. 812, 107 H.R. 4600 (2002), 108 H.R. 5 (2003), 108 S. 2061, 108 S. 11 (2003), 108 S. 2207, 108 H.R. 4280 (2004), 109 H.R. 534 (2005), 109 S. 366, 367, 354, 109 H.R. 5, 109 S. 22

⁴ S. 22, 109th Cong. (2006).

⁵ The data is available at: <http://www.publicintegrity.org/lobby/top.aspx?act=topcompanies>

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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 regime. Understanding the effects of tort reform is also important for policy makers in their attempt 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 non-economic damages in medical malpractice actions.⁶ 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.⁷ In contrast, the Supreme Court of Utah recently upheld such caps based on the perceived reasonableness of the empirical studies relied on by the Utah legislature.⁸ Thus, careful analysis of the effect 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.⁹ 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 previous studies' ability 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.

⁶ *Ferdon v. Wisconsin Patients Comp. Fund*, 701 N.W.2d 440, (Wis. 2005).

⁷ *Id.* 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 the legislature's goal of reducing costs. See for example, *Arneson v. Olson*, 270 N.W.2d 125 (N.D. 1978), *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.

⁸ *Judd v. Drezga*, 103 P.3d 135 (Utah 2004).

⁹ See *infra*, text around notes 25, 26, 27, 28, 34, and 35 which discusses previous literature.

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Yet, the most frequent methodological mistake in past studies was failure to properly link cases to applicable law. Specifically, cases were coded as subject to reforms when in fact they were not subject to the reforms for several reasons. First, cases were often miscoded as subject to reforms because scholars believed that the relevant date was the filing of the cases, whereas in most cases the relevant date is the injury date. Second, cases were often miscoded as subject to tort reforms because of misunderstandings regarding the retroactivity of constitutionally invalidated laws. Striking down a reform not only means that future cases will not be subject to the reform, but also that *pending* cases will also not be subject to the reform. The latter point escaped previous scholars' eyes.¹⁰ Yet, properly linking cases to applicable law is essential to accurately estimating the effect of reforms on litigation outcomes.

There are four major methodological improvements this study offers to enhance our knowledge of the effects of medical malpractice on case outcomes. First, it employs a recent dataset of the entire population of malpractice settlements from 1991 to 1998 (more than 100,000 cases). Second, this case-level data is matched to a unique, newly constructed legal data set which accurately identifies reform implementation date, and striking-down date when applicable, for reforms passed in each of the fifty states represented in the data. Third, the analysis accurately identifies the applicable law by linking injury date—as opposed to filing date—to reform passage. Fourth, retroactivity of judicial invalidation of reforms was properly accounted for by coding those cases initiated after a reform was enacted but *settled* after it was struck down as subject to the pre-reform rule.

Most previous studies employing state-level data estimated little or no impact of reforms on cases outcome. The only exception to this overall pattern is a reform which caps pain-and suffering damages; this reform appears to sometimes decrease the number of positive payments and other times the magnitude of payments. Consistent with the previous literature, this study also finds that caps on pain-and-suffering damages reduce the number of cases per (thousand) doctors and the total annual settlement payments.

¹⁰ See *infra*, text around notes 58, 59, and 60, which discusses these points.

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Other reforms had no significant impact. Yet, once the data is examined at the case level and the retroactive applicability of striking down a reform is adequately controlled for, I find that caps on pain-and-suffering damages, limitation of the collateral source rule and potentially the periodic payment reforms decrease average settlement payment. This study provides evidence that cases which are subject to these reforms settle at lower amounts. Further, the difference in settlement amounts are both economically- and statistically-significant. Yet, the results also suggest that tort reforms provide incentives to plaintiff lawyers to wait until a reform is struck down before settling. In that case, the total effect of tort reforms is much smaller, because it lowers payments for only a small fraction of the cases. 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.

Why Study Tort Reform ? Some Obvious and Not So Obvious Remarks

A better understanding of the effects of tort reform on litigation and case outcomes has obvious value for the public at large and policy makers alike. Medical malpractice law is a contentious issue in the United States. On one hand, the frequency and magnitude of malpractice is alarming: some studies claim that up to 98,000 people die each year due to medical error.¹¹ Because only a small fraction of these cases are litigated—and of those litigated about 25% of the victims of medical error receive no compensation whatsoever—health care providers lack the optimal incentive to take care.¹² On the other hand, health providers argue that juries are incompetent in determining medical negligence; at least one recent study has argued that in about 25% of the cases in which plaintiffs receive compensation doctors commit no error.¹³ Critics of

¹¹ Institute of Medicine, *To Err is Human: Building a Safer Health System* 26 (Linda T. Kohn et al. Eds., 2001). *See also* Jennifer Arlen and W. Bentley MacLeod, *Malpractice Liability for Physicians and Managed Care Organizations* 78 *N.Y.U. L. Rev.* 1929, (2003) (summarizing studies documenting the rate of error in medicine).

¹² A recent study estimates that only about 16.7% of severe medical injuries *due to negligence* are being picked up by lawyers, and that about 25% of the victims of medical error do not get any compensation whatsoever. David M. Studdert et al, *Claims, Errors, and Compensation Payments in Medical Malpractice Litigation*, *New Eng. J. Med.* 2006; 354:2024-33.

¹³ *Id.*

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the current medical malpractice regime argue that frivolous lawsuits cause doctors to perform defensive medicine, inflate malpractice insurance premiums (exceeding \$200,000 per year in some areas), and drive many good doctors out of business.¹⁴ These high costs are purportedly passed onto consumers, causing medical costs to skyrocket and patients to forego necessary medical care or health coverage. Clearly, there are weighty public health and fiscal issues at stake in the medical malpractice reform debate.

Unsurprisingly, there are sharp political cleavages between the plaintiffs' bar and physicians and other advocates of tort reform. The plaintiffs' bar often depicts itself as the defender of patients' rights against careless health care providers and profit-hungry insurers. They dismiss tort "reform" as a thinly veiled attempt at limiting the liability of well-connected political actors at the expense of a fair remedy for the majority of citizens. Conversely, tort reform advocates often depict themselves as defenders of patients' health against villainous plaintiffs' lawyers who drive their clients to exploit an overly generous legal regime, thereby forcing doctors to practice defensive medicine, and compromising the quality of medical care.

In legislatures throughout the United States, advocates of tort reform seem to have prevailed on the political debate over medical malpractice reform. States have enacted (and sometimes, after the courts have struck them down, reenacted) dozens of laws over the past three decades to reform the medical tort system in the direction of limiting liability. Discontent with the pace of state reform, President Bush has recently argued that "America needs medical liability reform. No one has ever been healed by a frivolous lawsuit."¹⁵ Indeed, the federal government took up the standard in the mid 1990s with increasing efforts to federalize tort reform. Between September 2002 and May 2006, no less than eight bills were proposed in Congress to reform medical malpractice law.¹⁶

¹⁴ For a recent survey which summarizes and evaluates studies making these claims see Michelle Mello, *Medical Malpractice: Impact of the Crisis and Effect of State Tort Reforms*, Research Synthesis Report, No 10, May 2006, available at: <http://www.rwjf.org/publications/synthesis/index.html>

¹⁵ George W. Bush, Remarks at the President's Dinner, Public Papers of the Presidents (July 26th, 2004)

¹⁶ H.R. 4600, 107th Cong (2002), S. 2793, 107th Cong. (2002) (HEALTH Act of 2002) ; H.R. 5, 108th Cong (2003), S. 607, 108th Cong. (2003) (HEALTH Act of 2003); S. 11, 108th cong. (2003) (Patient First Act of 2003); H.R. 4280, 108th Cong. (2004) (HEALTH Act of 2004); H.R. 5, 109th Cong. (2005) (HEALTH) Act of 2005. As mentioned in the text, on May 8th, 2006, the U.S. Senate voted 48 to 42 to

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These laws seem to have all shared the single goal of lowering defendants' annual payments.

However, policy goals do not necessarily materialize into policy outcomes. Do such reform measures actually affect medical malpractice payments? Are they effective in lowering the amount of money that juries award in medical malpractice lawsuits? Do they substantially change the litigation behavior of patients' lawyers? Past empirical studies of state reforms have, by and large, not established a significant correlation between tort reform legislation and malpractice payments. However, as mentioned above, past studies have generally suffered from defective data, incorrect coding of tort reforms or inadequate sample size. In the hopes of obtaining a more accurate understanding of the effects of tort reform, this study examines the effect of various reforms on the frequency, average size, and total annual payments of medical malpractice settlements between 1991 and 1998, (hopefully) remedying the methodological short falling of previous work. Specifically, the study tracks the effect of six types of reform: caps on non-economic damages, caps on punitive damages, higher evidentiary requirements for punitive damages, limitations on joint-and-several liability, limitations on the collateral source rule, and allowing periodic payments of awards. These six reforms were chosen for two reasons. First, they are the most prevalent reforms that states have enacted in the last decades.¹⁷ Second, they all appear in the federal medical malpractice bills recently debated in Congress.¹⁸

accept S.22, 109th Cong. (2006) (Medical Care Access Protection Act of 2006). This was not enough to prevent filibuster. In addition to these six reforms to medical malpractice law, there were two attempts by the Senate to reform only the practice of ob/gyns: S. 2061, 108th Cong. (2003) (Healthy Mothers and Healthy Babies Access to Care Act of 2003) and S.2207, 108th Cong. (2004) (Pregnancy and Trauma Care Access Protection Act of 2004).

¹⁷ See Table 1, *infra*, which shows the prevalence of ten reforms.

¹⁸ Compare with the nine reforms found in the latest federal bill: limitation of 3 years for filing a lawsuit; cap of \$250,000 on non-economic damages; abolition of joint and several liability; limitation on contingent fees; abolition of the collateral source rule; requiring proof of malicious intent to injure the victim by clear and convincing evidence; cap of \$250,000 or two 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 FDA. Available at <http://thomas.loc.gov/cgi-bin/query/F?c109:3:./temp/~c1091o2b39:e0>:

I do not study the statute of limitations and contingent fee reforms due to lack of variance among the states in these reforms during our time period. Nor do I study the immunity to drug manufacturers, as it is

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For this purpose, and as will be explained in more details below, a new legal dataset was constructed which tracks medical malpractice reforms in great detail.¹⁹ This legal dataset was linked to the National Practitioner Data Bank, a unique federal database encompassing the entire universe of medical malpractice settlements in the United States. Combining the two provides a rich dataset with more than 100,000 settlements over an eight year period. Together, these two datasets provide an opportunity to run the most comprehensive study on the effect of tort reforms to date. These two sources of data provide the empirical framework through which I examine three distinct but related questions.

(1) First, do tort reforms affect the average settlement payment? To answer this question, this study looks at case-level records which contain more than 100,000 cases, by far the largest dataset ever used for that purpose.

(2) Second, do reforms affect the number of annual settlements? The annual volume of tort claims is perhaps the biggest concern to doctors. This is because most doctors carry liability insurance (which is rarely experienced rated) and, in addition, injured plaintiffs rarely go after doctors' personal assets if the judgment is larger than the insurance coverage. Therefore, the principle harm to doctors from litigation is reputational, not financial.²⁰

(3) Third, do reforms affect the total annual payments? The total annual value of settlements is the biggest concern to insurance companies as this potentially affects their profit bottom line. The presumed goal of tort reform for insurance companies is reducing total annual payments.

extraneous to our data set (which tracks medical malpractice lawsuits and settlements against physicians and hospitals).

¹⁹ See Avraham, Database of State Tort law Reforms, *supra* note 2.

²⁰ Tom Baker, Blood Money, New Money and the Moral Economy of Tort Law in Action, 35 Law & Society Review 275 (2001) (bringing evidence that plaintiffs do not go after doctors' personal assets to collect their recoveries); Lawthers, Localio, Lipsitz and Brennan, Physicians' Perceptions of the Risk of Being Sued, Journal of Health Politics, Policy and Law, 17(3) 1992, 463- 482.

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This study offers various valuable contributions to our understanding of the effects of tort reform on case outcomes. First, it incorporates two datasets never before used for this purpose. With the benefit of the generous assistance of the National Science Foundation (NSF), the largest and most comprehensive legal dataset on tort reform was assembled incorporating data from previously available sources (including previous compilations, research papers, and public information), as well as independently researched data tracking reforms.²¹ The dataset documents dozens of reforms in all 50 states and Washington D.C. since the 1980s. The analysis was conducted on both this newly constructed legal data set, as well as on previously used datasets. This revealed significant differences in estimated effects of tort reforms, suggesting that previous research on tort reform may need to be revisited.

Second, this study tracks settlements and not judgments. As is well known, only a small fraction of cases are litigated. More than 90% of the medical malpractice cases are settled.²² Thus, from a policy-making perspective, documenting the effect of tort reform on settlements, as opposed to merely judgments, is of utmost importance. Moreover, this study does not rely on the representativeness of a sample of settlements to estimate population parameters, but rather—at least in theory—the data represents the entire universe of settlements in the United States during the time period studied.²³ Third, the study tracks settlements for injuries that occur between 1991 to 1998 that have been settled up until 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 fifty states whereas previous research had studied a sub-sample of states. Fifth, the study tabulates cases according to injury date and not filing date. As will be explained below, previous research erroneously assumed that tort reforms apply to cases filed after the reform effective date. In reality, however, whether or not a case is subject to a new statute

²¹ See Ronen Avraham, Database of State Tort law Reforms, available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=902711

²² See for example, Patricia Munch Danzon & Lee A. Lillard, Settlement out of Court: The Disposition of Medical Malpractice Claims, 12 J. Legal Stud. 345, 347 (1983) (reporting that less than ten percent of medical malpractice claims studied proceeded to trial and verdict).

²³ As will explain in more detail below (see *infra* page 16) there are reasons to believe that database does not record *all* settlements. Still, I use about 100,000 observations, while other studies use samples of only a few hundred cases.

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depends in most cases on the injury date and not the filing date. Therefore, when analyzing individual records, careful caution was taken to correct for states that have struck down tort reforms, a necessary procedure which must be done to accurately estimate the effect of tort reform. A review of previous literature suggests that this might not have been done sufficiently in past studies. If so, previous results might be unreliable.²⁴

Lastly, in addition to running conventional linear regressions models, this study also introduces quantile regressions models to track the effects of tort reform on different brackets of the settlement distribution. While ordinary least squares linear regression estimates the impact of tort reform on the *average* dependent variable, quantile regression can estimate differential impact of tort reforms on different stratum of the settlement distribution. For example, capping pain-and-suffering damages is a reform which is designed to have a significant impact only on the upper part of the payment distributions. If the impact is not large enough it may not necessarily be reflected in the mean award, making linear regression an ineffectual statistical technique for picking up the effect of the reform. As will be shown in more details below, quantile regressions is an appropriate estimation procedure for data with such characteristics.

The structure of this paper is as follows. The following section reviews relevant literature. Section III describes in detail the two datasets (the NPDB and the DSTLR) used in this study. Section IV introduces the statistical methodology and econometric models used to test the three main empirical questions of this study. Section V presents the results. The key results of this study show some evidence in support of the conclusions of past research, while others 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 caps on non-economic damages is the only reform producing moderate effects on aggregate state-level variables (although I also find some weak evidence that collateral source rule and the periodic payment reforms have some effect). The results at the case-level (average award)

²⁴ See *infra*, page 24, for a description of how awards and settlements were matched to the reforms in effect at the time.

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however are stunning. Once the data is adequately coded for the retroactive applicability of striking down reforms, caps on non-economic damages and modification of the collateral source rule each had a large effect (a decrease of up to 55%) on the average settlement payment. This effect is significant at the 1% level confidence level.²⁵

Section VI discusses these results in detail. As will be explained below, from this data it is impossible to identify the extent to which the reduction in settlement payments is due to the parties' strategic behavior. I therefore estimate that enactment of caps on non-economic damages and modification of the collateral source rule lowers settlement payments (yet to a degree I cannot accurately estimate). In addition, these reforms most likely stimulate strategic behavior among litigants, which might delay settlements because of the uncertainty of whether the reform will be struck down.

The fact these statistical procedures do not estimate a significant effect of other tort reforms on case outcomes should not be taken as evidence that they have none. As noted in the Discussion section, there are multiple possible explanations for why the impact of tort reforms may not be detected in empirical studies in general (and perhaps in this one in particular) even if such an impact does exist.

Section VII 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.

II. Literature Review

There is a dearth of reliable empirical or experimental evaluations of medical malpractice tort reform.²⁶ In fact, over the last three decades, only a dozen or so

²⁵ The impact of the periodic payment reform however exists only in what I call the "separate" specifications, but not in the "joint" specification." More on this below.

²⁶ 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 only be answered by further empirical field research. *See, e.g.,* Michael J. Saks et al., Reducing Variability

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empirical studies have been published examining the impact of tort reforms on medical malpractice payments or medical liability insurance premiums.

The first wave of empirical studies was conducted in the 1980's, examining the tort reform revolution of the 1970's. 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 in a 1986 paper by Zuckerman, Koller and Bovbjerg, drew mixed conclusions.²⁷ Some studies concluded these reforms were ineffectual in reducing malpractice liability burdens, while a majority concluded that they were effective in this regard. Regardless of their findings, all of these studies were based on data that is over two decades old, thus predating recent reform measures taken by states and ignoring the longer-term effects of tort reform.

The second wave of studies from the late 1980s and early 1990s are summarized in a 1993 review by the Office of Technology Assessment (OTA) exploring the effect of tort reforms on malpractice costs.²⁸ Comparing results from six studies, the OTA noted that all of the studies "suffer from methodological problems and limitations that make interpretation and comparison of their results difficult."²⁹ Specific problems noted by the

in Civil Jury Awards, 21 L. & Hum. Behav. 243 (1977); Linda Babcock & Greg Pogarsky, Damage Caps and Settlement: A Behavioral Approach, 28 Journal of Legal Studies 341 (1999); Jennifer K. Robbennolt & Christina A. Studebaker, Anchoring in the Courtroom: The Effects of Caps on Punitive Damages, 23 Law and Human Behavior 353 (1999); Edith Greene, David Coon & Brian Bornstein, The Effects of Limiting Punitive Damage Awards, 25 Law and Human Behavior 217 (2001).

²⁷ Stephen Zuckerman, Christopher F. Koller and Randall R. Bovbjerg, *Information on Malpractice: A Review of Empirical Research on Major Policy Issues*, 49 Law and Contemporary Problems 85, 101-03 (1986).

²⁸ U.S. Congress, Office of the Technology Assessment, *Impact of Legal Reforms on Medical Malpractice Costs*, at 57-75, 105-111 (1993).

²⁹ *Id.* at 62.

The studies are: E. K. Adams, and S. Zuckerman, "Variation in the Growth and Incidence of Medical Malpractice Claims", *Journal of Health Politics, Policy and Law* 9(3):475-488, Fall 1984; P.M. Danzon, "The Frequency and Severity of Medical Malpractice Claims: New Evidence," *Law and Contemporary Problems* 49(2):57-84, Spring 1986; S. Zuckerman, R.R. Bovbjerg, and F. Sloan, "Effects of Tort Reforms and Other Factors on Medical Malpractice Insurance Premiums," *Inquiry* 27(2): 167-182, Summer 1990; F.A. Sloan, P.M. Mergenhagen, and R.R. Bovbjerg, "Effects of Tort Reforms on the Value of Closed Medical Malpractice Claims: A Microanalysis," *Journal of Health Politics, Policy and Law* 14(4):663-689, Winter 1989; D.K. Barker, "The Effects of Tort Reform on Medical Malpractice Insurance Markets: An Empirical Analysis," *Journal of Health Politics, Policy and Law*, 17(1):143-161, Spring 1992; G,

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OTA include: variation in the definition of a “claim” based on different sources, measurement validity issues such as grouping complex reforms into single categories, overstating the effectiveness of ineffective reforms at the expense of more effective reforms.³⁰ 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.³¹ However, there were inconsistent results for caps on non-economic damages, a subsection of this tort reform.³² Limits on attorney fees, mandatory or discretionary periodic payments, and restricting the use of *res ipsa loquitur* showed no significant impact.³³ Restricting the statute of limitations, establishing pretrial screening panels, limiting the doctrine of informed consent, and taxation of costs in frivolous lawsuits, produced spotty or isolated results.³⁴

A third wave of studies, from the mid-1990s to the early-2000s, is reviewed in a 2004 Congressional Budget Office (CBO) report.³⁵ Reviewing nine studies, the CBO flags methodological problems and data limitation 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.³⁶ Yet,

Blackmon, and R Zeckhauser, “State Tort Reform Legislation: Assessing Our Control of Risks,” in *Tort Law and the Public Interest*, Peter H. Schuck (ed.) (New York: W.W. Norton & Co., 1991).

³⁰ *Id.* at 62-63.

³¹ *Id.* at 64-65.

³² *Id.*

³³ *Id.* at 72.

³⁴ *Id.*

³⁵ U.S. Congress, Congressional Budget Office, *The Effects of Torts Reform: Evidence from the States* (2004).

³⁶ The studies are: Born, Patricia H., and W. Kip Viscusi, “The Distribution of the Insurance Market Effects of Tort Liability Reforms,” *Brookings Papers on Economic Activity: Microeconomics* (1998), pp. 55-100; Browne, Mark J., Han-Duck Lee, and Joan T. Schmit, “How Does Joint and Several Tort Reform Affect the Rate of Tort Filings? Evidence from the State Courts,” *Journal of Risk and Insurance*, vol. 61, no. 2 (1994), pp. 295-316; Browne, Mark J., and Robert Puelz, “The Effect of Legal Rules on the Value of Economic and Non-Economic Damages and the Decision to File,” *Journal of Risk and Uncertainty*, vol. 18, no. 2 (1999), pp. 189-213; Kessler, Daniel P., and Mark B. McClellan, “Do Doctors Practice Defensive Medicine?” *Quarterly Journal of Economics*, vol. 111, no. 2 (May 1996), pp. 353-390; Kessler, Daniel P., and Mark B. McClellan, “Malpractice Law and Health Care Reform: Optimal Liability Policy in an Era of Managed Care,” *Journal of Public Economics*, vol. 84, no. 2 (2002), pp. 175-197; Thorpe, Kenneth E., “The Medical Malpractice ‘Crisis’: Recent Trends and the Impact of State Tort Reforms,” *Health Affairs—Web Exclusive* (January 21, 2004), available at

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some of the studies 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 cases outcome. Some studies find certain reforms effective, while others find that the same reforms are ineffective. Caps on damages are probably the only reform which keeps surfacing as effective. Overall, the disparate findings should not be surprising given differences in legal and claims datasets used, econometric methods applied, variables used, and time periods studied.³⁷

III. 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 datasets I explain one of the contributions of this study, which is correctly matching malpractice payments to tort reforms.

Medical Malpractice Payments

I obtained medical malpractice payment information from the National Practitioner Data Bank Public Use Data File, dated December 2005 (the “NPDB Database”).³⁸ 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 1986³⁹

<http://content.healthaffairs.org/cgi/reprint/hlthaff.w4.20v1.pdf>; Viscusi, W. Kip, and others, “The Effect of 1980s Tort Reform Legislation on General Liability and Medical Malpractice Insurance,” *Journal of Risk and Uncertainty*, vol. 6 (1993), pp. 165-186; Yoon, Albert, “Damage Caps and Civil Litigation: An Empirical Study of Medical Malpractice Litigation in the South,” *American Law and Economics Review*, vol. 3, no. 2 (2001), pp. 199-227.

³⁷ A more recent survey by Michelle Mello reaches the same conclusion. Michelle Mello, Medical malpractice: Impact of the crisis and effect of state tort reforms, Research Synthesis Report, No 10, May 2006, available at: <http://www.rwjf.org/publications/synthesis/index.html>

³⁸ National Practitioner Data Bank Public Use File, September 30, 2003, U.S. Dept. of Health and Human Services, Health Resources & Services Administration, Bureau of Health Professions, Division of Practitioner Databanks, available at <http://www.npdb-hipdb.com> (hereinafter “NPDB Database”).

³⁹ 42 U.S.C. §§ 11101-11152 (1986).

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and its implementing regulations at 45 CFR 60, et seq. Beginning on September 1, 1990, these laws require that (with some exceptions) all medical malpractice payments to be reported to the Department of Health within 30 days of payment.⁴⁰

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, only about 160,000 were paid between 1991 and 2005. Moreover, for reasons explained below, the data for this study was limited to just settlements in the 50 states for injuries occurred between 1991 and 1998. The final dataset was comprised of 105,944 cases. Table 1 provides descriptive statistics of this data.

[TABLE 1 HERE]

Like any other database, the NPDB is not perfect. One potential deficiency in the dataset is the possibility of under-reporting in the NPDB Database; there is some criticism in the medical literature of the NPDB Database relating to this deficiency.⁴¹ Although 45 CFR 60.7(c) imposes a \$11,000 penalty for under-reporting, as of November 2000 the Department of Health and Human Services had never exercised this enforcement mechanism.⁴²

⁴⁰ 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, et al., v. Donna E. Shalala*, No. 92-5038, and held that self-insured individuals were not “entities” under the HCQIA and did not have to report payments made from personal funds. All such reports have been removed from the NPDB. See National Practitioner Data Bank 2003 Annual Report, at 9.

⁴¹ See, e.g., U.S. General Accounting Office, *National Practitioner Data Bank: Major Improvements are Needed to Enhance Data Bank’s Reliability* (2000). Most recently, a report by the inspector general of the U.S. Department of Health and Human Services from October 2005 revealed that, from June 1997 to September 2004, the department’s own agencies failed to report 474 cases to the NPDB. See Daniel R. Levinson, *HHS Agencies’ Compliance With the National Practitioner Data Bank Malpractice Reporting Policy*, OEI-12-04-00310, available at <http://oig.hhs.gov/oei/reports/oei-12-04-00310.pdf>.

⁴² *Id.*

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The under-reporting problem may occur due to two reasons. First, cases where the defendants are self-insured entities are not required to report to the NPDB at all. Second, cases where a hospital (and not a physician) is the sole defendant are not required to be reported at all. The latter case may mask suits against doctors due to parties' strategic behavior, known as the "corporate shield" loophole. This happens when a corporate entity like a hospital, a physician's group, etc. is sued along with an individual practitioner and the named individual is dropped from the suit as part of the settlement terms.⁴³ It is not clear by how much this reduces the percentage of cases reported to the NPDB, nor is it clear what the non-reported cases tend to look like.⁴⁴ I expect that the under-reporting due to the corporate-shield loophole will affect only the number of out-of-court settlements but not the number of court judgments.⁴⁵ In any case, the integrity of dependent variables should not be significantly affected if the under-reporting problem is randomly distributed across the data and unrelated to tort reform.⁴⁶

Some studies have also found duplicate payments reported in the file. This may happen whenever there are multiple defendants making payments to the same plaintiff for the same injury. The NPDB directors are aware of this problem: in their annual report

⁴³ "Under current NPDB regulations, if a practitioner is named in the claim but not in the settlement, no report about the practitioner is filed with the NPDB unless the practitioner is excluded from the settlement as a condition of the settlement. The extent of the corporate shield cannot be measured with available data." NPDB Annual report 2003 at 25.

⁴⁴ A recent study of the NPDB Database found "only limited support" for the proposition that this loophole is being abused. Teresa M. Waters and Peter P. Budetti, Research on the Impact of National Practitioner Data Bank Reporting Requirements on the Resolution of Malpractice Claims, *available at* <http://www.rwjf.org/reports/grr/033494.htm> #BACKGROUND. However, a recent estimate by Michelle Mello and her co-authors reveals that results in at least 24% of cases are not being reported. Their estimate is based on comparing a Florida dataset with the NPDB. However, the Florida dataset suffers from duplicate reporting problems, which are hard to correct. While the NPDB also suffers from duplicate reporting problem, it is not clear that the extent of the problem in the two datasets is identical. Thus any comparison between them is not straight-forward. I did our own comparisons of the number and magnitude of claims in the NPDB with several online state resources, including the widely-used Texas data set. Our analysis shows that sometimes NPDB reports are consistent with the state reports, sometimes the NPDB shows downward bias, and sometimes shows upwards bias. However, there is no reason to believe that the state reports are more reliable than the NPDB.

⁴⁵ I believe that once a physician was named as a defendant it would be difficult to erase her name. In future drafts I plan to compare judgments and settlements.

⁴⁶ In their estimation Mello et. al., assumed that the distribution of the omitted cases is identical to the distribution of the existing cases. (See Appendix X for more details).

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they adjust the number of medical malpractice payment reports.⁴⁷ To deal with this issue, I programmed various algorithms to sort out payments that appear to be duplicates. However, I cannot be certain I identified all of the duplicate records and that I did not mistakenly exclude different payments. Below I check the robustness of our results against different algorithms.

Another problem with the NPDB is that it does not accurately report periodic payments before 2003. In many states by law and in some cases by choice a successful plaintiff is paid her damages over a specified payout time period as opposed to in a lump sum, this is called “periodic payments”. Rather than recording the entire damage awards, the NPDB flags that periodic payment is involved but records only the first payment. To deal with this problem I used data from 2003 to 2005 and constructed a state-specific multiplier for those states that had periodic payments. The multiplier represented the ratio for that state between total damages and the first payment. I then inflated our payments that were flagged as involving periodic payment by the state specific multipliers. However there is no way to definitively verify that this technique solved the problem. Below I run a robustness check for this algorithm. Lastly, it should be noted that other datasets suffer from duplicate reporting.⁴⁸

⁴⁷ The NPDB directors seem to believe that the double reporting problem occurs only in nine states, which have or have had state funds that pay damages whenever a total malpractice settlement or award exceeds a maximum set by the State for the practitioner’s primary malpractice carrier. I believe the problem of multiple reporting might show up in other states as well. I therefore compiled three different data sets, in which I used various formulas to merge possible duplicate records together. In one data set I merged records that occurred in the same year, for the same practitioner, in which one of the payments was paid by the state fund. In a second data set I merged all records that occurred in the same year, for the same practitioners. The third dataset is like the second data set, except I united cases against multiple defendants, assuming essentially that there are multiple reports of the same injury. While none of these data sets is perfect, I believe they represent a reasonable spectrum of adjustments: the truth lies somewhere in between.

Another problem with the way the data is coded in the NPDB is that awards structured as periodic payments are recorded showing only the first payment, thus biasing down the total award for an injury. This problem has been fixed in 2003. Starting in 2003, the NPDB includes both the first payment as well as the total payment. To correct for this problem I identified the ratio, for every state, between the first payment and the total award and inflated the awards (if periodic payments were involved) in 1991 to 1998 by the state-specific factor.

⁴⁸ See for example Bernard Black, Charles Silver, David A. Hyman, William M. Sage, *Stability, Not Crisis: Medical Malpractice Claim Outcomes in Texas, 1988–2002*, Journal of Empirical Legal Studies, Volume 2, Issue 2, Page 207-259, Jul 2005 at 214.

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The NPDB records do not satisfactorily include the individual characteristics of each case, presenting another limitation of this dataset. The severity of the injury, the age of the plaintiff, the specialty of the physician, etc. are not reported (or poorly reported) for the period of the study. Some variables are reported are often inaccurate.⁴⁹ Without these variables, individual characteristics cannot be controlled for. I attempted to deal with this problem by creating my indices of physician specialization.

Moreover, the NPDB's publicly available records do not disclose the exact dates of the malpractice and payment, only the year. As a result, in years where specific reforms were passed there is no way to discern whether an individual case accrued before or after the reform was passed, and therefore to accurately link it to the pre or post-reform legal regime. This problem was addressed in two ways. First, reforms with an effective date on or after July 1st were coded as becoming effective the following year. The rationale being that there is a more than 50% chance that a case occurring that year was subject to the pre-reform legal regime. Second, as a robustness check, I lagged the dependent variable, so that in effect, I checked the impact of the tort reform on the payments of the year that followed the reform. Lagging the dependent variable makes sense for two other reasons. First, it provides for a learning period in which the relevant actors adjust their behavior to the new legal regime. Second, it may help with the problem of endogeneity, which is elaborated below.

Another characteristic of the NPDB is that it includes only positive payments; cases with zero payments (cases where the plaintiff did not recover anything) are not recorded. The statistical analysis about the impact of tort reform on the number of cases is thus conditioned on a positive award or settlement. This problem, however, is not unique to the NPDB, and other studies of tort reform which used different datasets face the same problem.⁵⁰

⁴⁹ For example, according to the NPDB Public Use Data File, the variable ALGNATR (which codes the malpractice allegation group) should not be used as substitute for physician specialty. Furthermore, no information for practitioner specialty is available for analysis. *Id.* at 12.

⁵⁰ For example, Black, Silver, Hyman and Sage have recently published a study on medical malpractice claims in Texas using a comprehensive database of closed claims maintained by the Texas Department of

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Lastly, the NPDB contains only closed claims. Thus, relatively fewer cases involving injuries in recent years exist in the dataset compared to earlier years, all else being equal. For example, as of December 2005, there were only a couple thousand cases involving injuries that occurred in 2002, because many of the disputes arising out of injuries occurring in 2002 had not been resolved yet. In a few years I expect to find more 2002 cases recorded in the NPDB. Thus, the current dataset over-represents cases that settle early, which are most likely minor injuries easily corrected by recovery. This introduces selection bias into the dataset.

To deal with this problem, I looked only at cases that occurred between 1991 to 1998 and which lasted six years or less. I chose a time window of six years because in most states and injury years, 85% of the cases were resolved within this time window. The disadvantage to dropping from the dataset cases that lasted more than six years (besides reducing the size and depth of the dataset) is that these likely represent the most complicated cases, which might be associated with higher awards. As a result I might be inaccurate with respect to reforms that target these types of cases, like caps on pain-and-suffering damages. While dropping these cases introduces a bias into the dataset, it is less severe than the bias of an untouched dataset.

Despite these limitations, the merits of NPDB as a data source to answer the empirical question of this paper must be assessed relative to the merits of alternative available datasets that have been used by other scholars. For example, a commonly used dataset is the one which is part of the project of the National Center for State Courts (NCSC) and the Bureau of Justice Statistics.⁵¹ Ted Eisenberg, Kip Viscusi and Catherine Sharkey have each used it in their studies of the tort and medical malpractice systems.⁵²

Insurance. As the authors explain, they lack good data on zero-payout. See, Bernard Black, Charles Silver, David A. Hyman, William M. Sage, *supra* note 47.

⁵¹ Bureau of Justice Statistics, U.S. Dep't of Justice, Civil Justice Survey of State Courts, 2001 (Inter-Univ. Consortium for Pol. & Soc. Research, No. 3957, 2004); Bureau of Justice Statistics, U.S. Dep't of Justice, Civil Justice Survey of State Courts, 1996 (Inter-Univ. Consortium for Pol. & Soc. Research, No. 2883, 2004); Bureau of Justice Statistics, U.S. Dep't of Justice, Civil Justice Survey of State Courts, 1992 (Inter-Univ. Consortium for Pol. & Soc. Research, No. 6587, 2004).

⁵² Theodore Eisenberg et al., *Juries, Judges, and Punitive Damages: An Empirical Study*, 87 *Cornell L. Rev.* 743, 747 (2002); Theodore Eisenberg et al., *The Predictability of Punitive Damages*, 26 *J. Legal Stud.* 623, 632 (1997); Joni Hersch & W. Kip Viscusi, *Punitive Damages: How Judges and Juries Perform*, 33 *J.*

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There are few notable differences between the NCSC data set and the NPDB. First, unlike the NPDB, the data in the NCSC is not continuous. Rather, data were collected at three points in time (for cases disposed of in 1992, 1996, and 2001). Second, while the NPDB contains data from all 50 states and Washington DC, data in the NCSC contain cases from only twenty-two states.. Third, unlike the NPDB which (at least in theory) contains the entire universe of medical malpractice claims, the data in these studies is from state courts in random samples of forty-six of the seventy-five most populous counties in the nation. The data constitute only about thirty-seven percent of the United States population and about half of all civil trials. As a result whereas the NPDB contains in the relevant years more than 160,000 medical malpractice cases with positive payments, the NCSC has only 557 such cases.⁵³ Lastly, the NCSC datasets do not contain information on settlements. As Catherine Sharkey observed: "settlement information is notoriously difficult to obtain....It is important to keep in mind that jury verdicts represent only "the tip of the iceberg" in medical malpractice cases, which obviously tempers the ability to generalize from [the NCSC]".⁵⁴ In contrast the NPDB contains both settlements and judgments.

Thus, as argued above, the NPDB has various notable advantages. I note, however, that in addition to the regular results presented below, I ran analyses of alternative datasets where I, for example, corrected in different ways for the multiple reporting problem, did not correct for the periodic payment problem, or looked at all 160,000 cases in the dataset without dropping those that took more than seven years to settle.

Legal Stud. 1, 2 (2004). Catherine Sharkey, Unintended Consequences of Medical Malpractice Damages Capg, 80 N.Y.U. L. Rev. 391 (2005).

⁵³ See Sharkey, id on page 450.

⁵⁴ Id. See also, Frank A. Sloan & Chee Ruey Hsieh, Variability in Medical Malpractice Payments: Is the Compensation System Fair?, 24 Law & Soc'y Rev. 997, 1013 (1990); Patricia Munch Danzon & Lee A. Lillard, Settlement out of Court: The Disposition of Medical Malpractice Claims, 12 J. Legal Stud. 345, 347 (1983) (reporting that less than ten percent of medical malpractice claims studied proceeded to trial and verdict); Henry S. Farber & Michelle J. White, Medical Malpractice: An Empirical Examination of the Litigation Process, 22 RAND J. Econ. 199, 201 (1991) ("[M]ost medical malpractice cases are either dropped by plaintiffs or settled out of court at some point during discovery").

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Tort Reforms

As mentioned above, the Database of State Tort Law Reforms (DSTLR) which supplied state-level variables is a new dataset compiled by this author with the assistance of an NSF grant.⁵⁵ The database was assembled by cross referencing our own review of the laws and court cases of the 50 states (and Washington, DC) from 1980 to 2005 to existing compilations.⁵⁶ 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 effective dates of reforms, missing or incorrectly reported supreme courts' cases reviewing the constitutionality of these reforms, and lacking information regarding the law's requirement of advising or not advising the jury of the applicable reform. In contrast, the DSTLR includes complete variables on reform title, a description, effective date, whether the jury it is advised or explicitly not advised of the applicable rule, whether it was upheld or struck down by the state supreme court, in such cases whether it was amended, repealed or replaced by another law. The most difficult task was to track down the effective date, especially for older statutes. To that end I often needed to look at microfiche and contact state legislators' libraries. The result is the most comprehensive legal dataset on tort reform to date.

As was mentioned above, in this study I explore the impact of six tort reforms. These reform are either enacted by a state's legislature or adopted by its courts.⁵⁷ These

⁵⁵ The dataset is available for free download at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=902711

⁵⁶ The compilations include: The American Tort Reform Association State and Federal Reforms compilation (available at <http://www.atra.org/reforms/>); A compilation by the National Conference of State Legislators, available at: <http://www.ncsl.org/standcomm/sclaw/medmaloverview.htm>; American Medical Liability Association's Comparison of State Medical Liability Laws; Westlaw's compilation of 50 State Surveys, Medical Malpractice-Tort Reform; CRS Report for Congress; Medical Malpractice Liability Reform: Legal Issues and Fifty-State Survey of Caps on Punitive damages and Noneconomic Damages (February 2005), available at: <http://shelby.senate.gov/legislation/MedicalMalpractice.pdf>.

⁵⁷ 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 non-legislative reform.

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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 US.⁵⁸

[TABLE 2a HERE]

The most common reform passed in state legislatures in recent decades is the limitation of joint and several liability for malpractice defendants (variable name “*Joint and Several*”). 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 plaintiff can go after “deep pocket” defendants, like hospitals, and collect all their 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 by allowing for joint-and-several liability only if the defendant is responsible for a significant proportion of the harm, usually at least 50%.

Discretionary or mandatory consideration of collateral sources of payment for medical costs is another common reform established in thirty-five states by 2004 (“*Collateral Source*”). The collateral source rule was developed by common law courts in the 19th century when insurance became more popular. The rule says that the damages that the defendant needs to pay will *not* be offset by plaintiff's insurance coverage. An implication of this rule is that the plaintiff may get more than his full harm in case of an accident. The reforms adopted by states either requires or allows courts to offset plaintiff's private and or public insurance benefits from the awarded damages.

⁵⁸ I was originally interested in the nine reforms that appear in the deferral bill. However, because of lack of enough variation during the relevant years for two of them (statute of limitations and contingent fee reforms) and lack of relevance for one of them (limiting liability for drug manufacturers) I eventually analyzed just six of them. In addition, as is said in the text, those six reforms are also the most prevalent state tort reforms.

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Periodic payment of large future damage awards is now allowed or required in twenty-three states (*“Periodic Payment”*). The reform allows or requires courts to award future damages that are above some threshold, usually \$200,000, in periodic installments. This reform ease the burden on the defendant who can purchase annuity for that purpose, and can potentially relieve a defendant of a portion of her due damages if the plaintiff dies before the damages are fully awarded.

Some of the more controversial reforms involve ceiling caps on damage awards. These caps most commonly apply to non-economic damages (*“Caps – Noneconomic”*, in twenty-three states) or punitive damages (*“Caps – Punitive”*, in twenty-seven states), but can also apply to total damages (*“Caps – Total”*, in seven states). Caps come in many flavors. Some impose a cap of fix dollar amount, indexed or not to inflation, while others use a multiplier of the economic damages. Many states implemented heightened pleading, evidentiary, or other procedural standards for punitive damages (*“Punitive Evidence”*, in thirty-four 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” towards plaintiff’s potential injury.

In 2004, six states required the plaintiff to share with the state a portion of the punitive damages (*“Split-Recovery”*). The rationale usually provided for such reforms is that plaintiff was already made whole with the compensatory damages, so the punitive damages are a windfall which should therefore be shared with the state.

Fifteen states limited contingency fee agreements between lawyers and their clients, capping plaintiffs’ lawyers’ recoveries (*“Contingency Fee”*). While, historically contracts between lawyers and their clients were left for the market to determine, the reforms impose caps on the percentage of the damages that lawyers can collect.

Lastly, twelve states had compensation funds from which patients can collect their recoveries (*“Patient Compensation Fund”*). The patient compensation fund is a state fund which usually pays the medical malpractice victims monies if the damages awarded are above some threshold, such as \$400,000. The rationale given for such arrangement is that

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it lower physician's premiums and spread the risk of risky medical specialties across the entire population of doctors and tax payer.

Matching Reforms to Payments

After identifying the pertinent datasets, analyzing them together required accurately matching the malpractice payments to tort reforms. There are several important legal substantive and related methodological issues associated to effective date of the reform carefully addressed in this study which enhances 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 in July 1st, with an effective date of January 1st the following year. Second, it is important that every study which explores the impact of tort reform acknowledges the fact that legislative creation of reforms has only (subject to some qualifications presented make below) *prospective* application unless expressly made retroactive, whereas striking down of reforms has *retrospective* applicability, because striking down of 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 is the injury date as opposed to the complaint filing data. As those malpractice cases with injury dates before the passage of the tort reform were subject to the pre-reform legal regime, careful coding assured this was accurately reflected in the data. Previous studies have (mistakenly in my view) assumed that the relevant date is the filing date.⁵⁹ The general common law rule however is that tort reform is not a mere

⁵⁹ For example, Patricia Danzon seems to have used the filing date as the relevant date for whether or not the reform applies to specific cases. See Patricia Danzon, *The Frequency and Severity of Medical Malpractice Claims*, *Journal of Law and Economics*, Vol 27 (1984) 115-158, 139. Thus it seems that many of the claims she used in her dataset, which were closed between 1975 and 1978, should not have been coded as subject to reforms which 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 1975-6 and closed before 1978 were likely only the small claims, a fact which creates selection bias in the analysis. The same problem appears in her 1986 study, where she 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.” See Patricia M. Danzon, *The Frequency and Severity of Medical Malpractice Claims: New Evidence, Law and Contemporary Problems*, Vol 49(2) (1986) 57-84, 80. Catherine Sharkey also seems to use filing dates in her analysis of punitive damages. See Catherine Sharkey, *Unintended Consequences of Medical Malpractice Damages Caps*, 80 *NYU Law Rev* 391 (2005) at Table 1 (using filing dates for Ohio and Illinois reforms instead of using injury dates).

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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 legislator.⁶⁰ Moreover, even if the legislator explicitly dictated that the tort reform would apply retroactively based on the filing date, courts might strike it down as unconstitutional on due process grounds.⁶¹ 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 complaints 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 was not struck down as unconstitutional.⁶²

While the assumption that the effective date applies to the injury date is valid for most of the reforms, it is not correct for all of them. Some reforms explicitly stipulate that they apply to cases *filed* after the effective date, thus essentially applying to pending cases as well.⁶³ This means that even injuries that occurred before the reform was enacted would have been subject to the post-reform legal regime (assuming, as was explained above, they were not resolved after the reform was struck down). Since the NPDB does

⁶⁰ 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. Omitowaju*, 883 F.2d 1155 (CA3 VI 1989); *Marcel v Louisiana State Dep't of Public Health*, 492 So 2d 103 (La. App. 1st Cir. 1986), cert den (La) 494 So 2d 334; *Martino v. Sunrall*, 619 So. 2d 87 (La. Ct. App. 1st Cir. 1993); *Graley v. Satayatham*, 343 NE2d 832 (Oh CP 1976); *Allen v Fisher (1977, App) 118 Ariz 95, 574 P2d 1314*; *Bolen v Woo (1979, 5th Dist) 96 Cal App 3d 944, 158 Cal Rptr 454*; *Robinson v Pediatric Affiliates Medical Group, Inc. (1979, 2nd Dist) 98 Cal App 3d 907, 159 Cal Rptr 791*.

⁶¹ See, for example, *Simon v. St. Elizabeth Medical Center*, 355 NE2d 903 (Oh CP 1976); *Martin by Scopur v Richards*, 531 NW2d 70 (Wis 1992) (Supreme Court of Wisconsin determining a medical malpractice act which applies to actions "filed on or after June 14, 1986" as having retroactive application to cases which occur before that date and is therefore unconstitutional); see also *Neiman v. American Nat. Property and Cas. Co.* 236 Wis.2d 411, 613 N.W.2d 160 (Wis.,2000). But see *Crowe v Wigglesworth* (1985, DC Kan) 623 F Supp 699 (applying Kansas law and determining that the collateral source rule to be a procedural rule governing the admissibility of evidence and therefore can be applied retroactively).

⁶² See *supra* footnote 59.

⁶³ For example, Louisiana punitive damages caps apply to all actions filed after January 1, 1992. See LSA-C.C. Art. 3546. More often, however, legislators explicitly stipulate that the reform should be construed prospectively. See, for example, a Maryland reform that says, "this 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, c. 477, § 2. In most cases though, the legislature is silent in which case the general rule explained above applies.

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not provide the filing date, the effective 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 pre-reform regime. The reason is that striking down a reform as unconstitutional declares it to never have been valid law. I suspect that too many of the previous studies, especially those done by non-lawyers, neglected to account for this point. Moreover, the striking down of reforms introduces the most problem for state-level types of analysis, as were many of the previous studies discussed above.⁶⁴ To see why, consider two cases which occurred after a tort reform was effective. One of them was resolved before the reform was struck down, while the other was settled after the reform was struck down. A state-level analysis which lumps together all payments for cases accruing in a given year will use *both* cases to estimate the effect of *tort reform* on the settlement payments. This is an artifact of aggregating up to construct the variables at the state-year level as many time-series analysis do 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). Yet, 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, most cases which accrue after the reform was enacted are resolved after the reform is struck down, and thus are not subject to the reform regime.⁶⁵

⁶⁴ Important studies which use state-level analysis without reporting any correction for striking down of reforms include: Patricia Danzon, The frequency and Severity of Medical Malpractice Claims, *Journal of Law and Economics*, Vol 27 (1984) 115-158; Patricia M. Danzon, The Frequency and Severity of Medical Malpractice Claims: New Evidence, *Law and Contemporary Problems*, Vol 49(2) (1986) 57-84, 61. Studies which report data corrected for striking down are Zuckerman, Bovbjerg and Sloan, Effects of Tort Reforms and Other Factors on Medical Malpractice, *Inquiry* Vol 27, 167-182 (1990) at 170 and Sloan, Mergenhen and Bovbjerg, Effects of Tort Reforms on The Value of Closed Medical Malpractice Claims: A Microanalysis, *Journal of Health Politics, Policy and Law*, Vol 14(4) 663-689 (1990) at 670. Yet, it is not clear whether they corrected the coding of the reforms on an individual level basis (accounting for the retroactive applicability of striking down) or on a state level basis. The former is correct way to do it, the latter is not.

⁶⁵ The problem gets even worse if plaintiffs' lawyers strategically wait until a reform is struck down 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 being struck down.

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To illustrate this point, consider the following example. Illinois passed caps on non-economic damages which applied to "causes of action accruing on or after" March 9, 1995. The reform was struck down in December 18, 1997.⁶⁶ If one just counts the number of payments for injuries that occurred after the effective date but before the striking down date, the number is about 1155. Yet, if one counts the number of these cases that were actually paid before the strike down date (and were therefore really subject to the reform) the number is only about 80, less than 7% of the cases. This becomes critical when using a difference-in-differences approach that uses state fixed effects, because in difference-in-differences models the estimation of the effect of tort reforms is done based only on *changes* in the reforms.

Put differently, under difference-in-differences approach, only the states that enacted or struck down a reform during the years in question are relevant for the accurate estimation of the impact of the reforms. Other states, which either had or did not have the reforms all the way through the years in question are not as important.⁶⁷ Thus, ignoring the strike-down dates and their impact on individual payments, as shown below, might yield enormous errors. Accounting for strike down dates to properly link cases to applicable law is not exceedingly difficult when analyzing individual level data, doing so with state-level data is less straightforward.⁶⁸ I suspect that previous research has not done it all.

IV. Statistical Methodology

Using the NPDB Database and the Legal Reforms Database, two sets of linear and quantile regressions were estimated to determine the statistical impact of the reforms on medical malpractice payments. I first describe the dependent variables, the reform

⁶⁶ See 735 ILCS 5/2-1115.1 for enactment and *Best v. Taylor Machine Works, Inc.*, 689 N.E.2d 1057 (Ill. 1997) for striking down.

⁶⁷ Jeffrey Wooldridge, *Introductory Econometrics: A modern Approach* (2005)

⁶⁸ In the interest of full disclosure, it should be mentioned that the NPDB does not allow us to perfectly cure this problem. Because parties can report the case up to 30 days after the payment was executed, payment done 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 our coding for reforms that were struck down in December.

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variables, and the control variables of interest. Then I describe the specific regressions I used to estimate the impact of tort reforms.

Dependent Variables

This study examines the effect of various medical malpractice reforms on two state-level and one case-level dependent variable. Each dependent variable is designed to measure a different aspect of malpractice payments. Detailed descriptions of each variable and their purpose follow. The variable codename appears in parentheses after the variable name.

Natural Log of Payment Amount (D_LogPymt). This variable measures the size of individual payments. Payment information was obtained from the NPDB Database.⁶⁹ All payments were inflated to December 2005 dollars, and then transformed into their natural log to accommodate the logarithmic distribution of the payment distribution. This also reduced the variance of the variable, resulting in a closer fit for the regression analysis. This variable utilizes the payment as the unit of analysis, resulting in 85,997 observations for regression.

Natural Log of Sum of Payment Amounts per Doctor (D_LogSumCapita). This is a state-level variable which measures the total malpractice payments for each state in a given year. The value was calculated by summing payments shown in the NPDB Database for each state in each year of study, and divided 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. Thus this variable is of a great interest to those seeking to evaluate the efficaciousness of these reforms. Using state-years as the unit of analysis produces 400 observations from 50 states over eight years.

Cases per Thousand Doctors (D_CountCapita). This state-level variable measures the relative frequency of malpractice payments. Values for each state-year were

⁶⁹ NPDB Database, "PAYMENT" field.

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calculated by counting the number of payments shown in the NPDB Database for each state in a given year, and dividing by the number of doctors (in thousands) practicing in the state that year. As above, 400 observations were generated.

Independent Tort Reform Variables

The independent variables used include the six tort reforms coded in the Tort Reform Database, described in Part III.2 above. The analysis used a collapsed version of the Tort Reform Database, the “collapsed legal data set”, which lumped together similar reforms as follows. First, all punitive damages caps were lumped together regardless of their magnitude. Second, all non-economic damages caps were lumped together regardless of their magnitude. Third, there was no distinction coded between mandatory or discretionary periodic payments and collateral source rule reforms. 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 non effective variants will “water down” the impact of the effective variants.⁷⁰ In any case, there were not sufficient state-year observations to statistically measure the impact of each variant of each reform, and collapsing reforms make results comparable to previous researcher that combined reforms along similar lines. Thus, the coefficients of the tort reforms variables (on just the state-level vars?) should be interpreted as indicators of *average* effects.

Independent Control Variables

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 was used (state-level control variables). Missing values were imputed for control variables using linear regression. Among the control variables HMO

⁷⁰ Most, if not all, studies collapse the reforms in one way or another. For a summary of studies see U.S. Congress, Office of the Technology Assessment, *Impact of Legal Reforms on Medical Malpractice Costs*, at 63 (1993). For problems that are caused because of that see Mello, *supra* note 4.

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penetration is of specific interest.⁷¹ Table 1 above provides descriptive statistics for these variables. Each variable is discussed in detail in the appendix.

Regression Model

Several variations on the following basic equations were estimated:

$$1) LP_{st} = \beta_0 \text{Constant} + \beta_1 R_{st} + \beta_2 C_{st} + \beta_3 \text{MalYear}_t + \beta_4 \text{State}_s + \varepsilon_{ist}$$

Where s indexes state and t indexes year.

The dependent variable is the log of the average payment in a state s in year t . \mathbf{R} is a vector of reforms. \mathbf{C} is a vector of state level control variables, MalYear is the year of the injury, State is the state of the physician. Because I am estimating state level variables, there are only 400 observations.

$$2) LP_{ist} = \beta_0 \text{Constant} + \beta_1 R_{st} + \beta_2 C_{st} + \beta_3 \text{MalYear}_t + \beta_4 \text{State}_s + \beta_5 I_{ist} + \varepsilon_{ist}$$

Where i indexes individual awards, s indexes state and t indexes year.

The dependent variable is the log of individual settlement I , in a state s in year t . The rest is the same except for \mathbf{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.

$$3) LTP_{st} = \beta_0 \text{Constant} + \beta_1 R_{st} + \beta_2 C_{st} + \beta_3 \text{MalYear}_t + \beta_4 \text{State}_s + \varepsilon_{ist}$$

⁷¹ In their 1996 study, Daniel Kessler and Mark McClellan used data from all elderly Medicare beneficiaries treated for serious heart disease. The authors found that "direct" reforms reduce medical costs by 5 to 9 percent within 3 to 5 years of adoption without substantially affecting mortality or medical complications. Kessler, Daniel P., and Mark B. McClellan, "Do Doctors Practice Defensive Medicine?" *Quarterly Journal of Economics*, vol. 111, no. 2 (May 1996), pp. 353-390. By "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 Id at 371-2. Importantly, in their 2002 study on the same population, they controlled for HMO penetration and found that "direct" tort reforms reduce medical costs by about 4 percent only. Kessler, Daniel P., and Mark B. McClellan, "Malpractice Law and Health Care Reform: Optimal Liability Policy in an Era of Managed Care," *Journal of Public Economics*, vol. 84, no. 2 (2002), pp. 175-197. Thus, it is important to control for HMO penetration due to HMOs' role in providing incentives for optimal care.

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The dependent variable is the log of total annual payments per doctor per state. Because I am estimating state level variables, there are only 400 observations.

$$4) NP_{st} = \beta_0 \text{Constant} + \beta_1 R_{st} + \beta_2 C_{st} + \beta_3 \text{MalYear}_t + \beta_4 \text{State}_s + \varepsilon_{ist}$$

The dependent variable is the number of annual payments per thousand doctors per state. The rest is similar to Equation 2.

For all of the equations above, three specifications are reported in Tables 4 to 6. The Model “OLS 1” controls only for state fixed effects, year effects, and reforms; Model “OLS 2” adds the control variables discussed above including HMO penetration. Model “OLS 4” includes state specific trends in addition to the control variables. Adding a state-specific time-trend variable controls for the upward or downward trends each state was experiencing in the levels of the dependent variable. In other words, adding these variables is intended to “detrend” the state-specific trends for award size (or number of cases), thus isolating the effect of the tort reforms themselves from other state-level trends in the dependent variables. As will be explained in greater detail below, this should also help addressing the problem of endogeneity.⁷² All models use clustering by state to account for non-independence between case-level data within states. Clustering was recently suggested by Bertrand and her coauthors as also a way of dealing with the serial correlation that results from the “stickiness” of the law.⁷³

One of the potential contributions of this analysis is that in addition to running conventional linear regression, it also presents quantile regressions on the individual-level data set. While regular OLS identifies the impact of tort reform on the average dependent variable, quantile regressions allow us to better understand the impact of tort reforms by identifying non-linear effects of tort reforms on different parts of the distribution. After all, many times reforms are *designed* to have a distinct impact specifically on the 25th, 50th and 75th percentiles. For example, capping pain-and-

⁷² For the problem of endogeneity see *infra*, page 44.

⁷³ Bertrand, Duflo and Mullainathan, How Much Should I Trust Differences-in-Differences Estimates? Quarterly Journal of Economics 2004, 249, at fn 24. By the “stickiness” of the law I refer to the fact that once a law was enacted it is likely to stay enacted in the following years. This creates problems in estimating the impacts of tort reforms. See *id.*

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suffering damages is a reform which is designed to have a significant impact only on the upper part of the payment distribution. If the impact is not large enough it may not necessarily lower the mean award, and a ordinary linear regression on the entire distribution of the awards will not identify this effect. Linear regression may also be inappropriate to identify the effect of a reform if it simultaneously decreases the upper portion of the award distribution, and at the same time *significantly* increase the lower part of the payment distribution. This could happen if caps not only “cut” the upper tail of the distribution, but also serve as a focal point for settlement of low-awards cases, driving awards in the lower part of the distribution upwards. Because these two effects may largely cancel each other out, I would observe no significant impact on the mean. Thus linear regression on then entire distribution of settlements would again identify no effect of such a reform. While the specification of the quantile regressions account for heteroskedasticity it does not, due to STATA limitations, account for intra-state correlations.⁷⁴ I therefore expect a downward bias in the standard errors. Like in the OLS specifications, here too I apply a Joint specification and a Separate specification.

The next section presents the results.

V. Results

In this section I present the results of the regressions analysis. I first present the state-level analysis, and follow with the individual-level analysis. Before doing so, however, I make a few remarks to clarify those results.

Preliminary Remarks

This study evaluates the effectiveness of tort reforms. A reform is “effective” if it reduces the total annual payout, “counter-effective” if it increases the total annual payout, and “non-effective” if it has no impact on the total annual payout.

Like the vast majority of all other studies this study focuses on the *effectiveness* of tort reforms and not on their *efficiency*. I do not ask whether the reforms are socially

⁷⁴ There is no "cluster" command for quantile regressions.

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beneficial, normatively fair, or efficient, but rather whether they accomplish the purported goals of their proponents.⁷⁵ Efficiency of the health system is determined mainly by whether the 'signal' sent by the medical liability system causes healthcare providers and patients to engage in optimal level of health care activity as well as to take optimal care. While this is perhaps a most relevant question to ask, this data does not allow us to address it.⁷⁶ Instead, I ask whether tort reforms are successful in lowering the dependent variables.

Another thing to observe is that effective reforms may not necessarily reduce *average* payout. Tort reforms can 'cut' the lower or the upper tail of the distributions of awards. For example, caps on damages may cut the upper tail of the distributions of awards. Limitations of contingency fees, in contrast, may cut out the lower distributions of awards. While both reforms are expected to decrease the number of cases as well as total annual payments, they differ with respect to their influence on the average awards. Whereas reforms that cut the upper tail are expected to decrease the average awards, those that cut the lower tail are expected to *increase* the average awards.⁷⁷

Lastly, it is important to observe that tort reform may *increase* the number of law suits because doctors may (rationally) exercise a lower level of care, knowing that they are partially insulated from liability, (for example due to caps on damages).⁷⁸ 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

⁷⁵ Probably every academic study which explores the impact of tort reforms makes this caveat. See, for example, Patricia M. Danzon, *The Frequency and Severity of Medical Malpractice Claims: New Evidence*, *Law and Contemporary Problems*, Vol 49(2), 57-84 (1986) at 79; Viscusi et al, *The Effect of 1980s Tort Reform Legislation on General Liability and Medical Malpractice Insurance*, *J. of Risk and Uncertainty*, Vol 6, 165-186 (1993) at 175; Viscusi and Born, *Damages Caps, Insurability, and the Performance of Medical Malpractice Insurance*, *The Journal of Risk and Insurance*, vol 72(1), 23-43 (2005) at 41.

⁷⁶ An efficient health care system not only provides incentives to all parties to behave optimally, but will also deter non-meritorious lawsuits. For studies that focus on whether lawsuits are meritorious or not, see Studdert et al, *supra* note 1, and studies cited there.

⁷⁷ 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 at all, which may undermine this logic.

⁷⁸ Randall R. Bovbjerg et al., *Defensive Medicine and Tort Reform: New Evidence in an Old Bottle*, 21 *J. Health Pol. Pol'y & L.* 267 (1996).

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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's or the nurse's liability.

Since even an effective reform can have an ambiguous effect on both the annual number of cases and the average awards, a relevant question which requires our attention is whether the *total* annual payout increases or decreases.

The next sections present the results.

State level analysis

Table 3 presents graphs of the state level annual average payment, annual average number of cases per state and per thousand doctors and the annual average total payment per doctor and per state.

[TABLE 3 HERE]

Panel (a) in Table 3 reveals that between 1991 and 1998, the annual mean settlement payment increased steadily from about \$210,000 to about \$280,000. This 33% overall increase over the seven-year period reflects an average 4% annual increase, above and beyond the annual consumer price index (CPI).⁷⁹ Panel (b) in Table 3 shows that the annual mean number of cases per state has increased from about 258 cases a year per state in 1991 to about 264 in 1998, an increase of 2.3% (less than 0.5% a year). Panel (c) shows that the annual mean total settlement payments per state has increased from about \$46 million in 1991 to \$64 million in 1998, an increase of about 39% (about 5% a year). Together, Panels (a) to (c) show a worrisome picture of a steady increase in annual medical malpractice settlement payments. However, these results are misleading because during those years there was almost a 20% increase in the average number of doctors per state. Panel (d) accounts for this growth in the numbers of doctors per state. While panel (b) shows a small increase in the average number of cases per state, there was in fact a decrease of more than 18% in the number of cases per thousand doctors. Similarly, while

⁷⁹ It may reflect the higher annual increase in the medical CPI, although it is not clear whether that increase does not reflect the higher annual awards. (CHECK HOW EXACTLY MEDICAL CPI IS CALCULATED).

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Panel (c) shows an increase of about 39% in mean total payments per state, panel (e) shows that the mean total payments per doctor has fluctuated during the entire period, eventually showing an increase of only about 10%.

The decrease in the volume of cases per doctor is consistent with trends documented widely in the medical malpractice claims literature.⁸⁰ To the extent that the results presented in Table 3 reflect the impact of tort reforms, the results are consistent with the hypothesis that state reforms decrease the lower end of the distribution of awards without impacting the total annual payouts because the number of cases decreased concurrent with an increase in the average award.

The next subsections explore the data more systematically.

A. Linear regression models of tort reforms on log of Average Payment

Moving from descriptive statistics to regression models, Table 4A reports three different panel OLS regressions of Equation 1. The dependent variable analyzed in Table 4A is the log of (state-level) average settlement payment. Four hundred observations are used.

The linear regression regressions in Table 4a show that the only reform that may have a significant effect on mean award levels is periodic payments. Allowing (or requiring) that awards for future damages above some threshold will be paid in periodic payments *increases* average payments. The periodic payment reform is associated with a 4% to 14% increase in average awards, depending on the specification. However, this estimated increase in average awards was statistically significant only in OLS3 (time trend) when the coefficient was 14%. The coefficient changes to 12% and is significant at the 10% level only in the (non-reported) joint specifications. The rest of the reforms and specifications have no significant effect at all. In non-reported regressions I find that the joint effect of all six reforms (which is associated with a decrease in the average award) is

⁸⁰ See for example Kevin Clemont and Theodore Eisenberg, *Litigation Realities*, 88 *Cornell Law Rev.* 119 (2002) at 144.

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significant for OLS 3 (almost at the 1% level of significant) but not for OLS1 and OLS2.

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B. Linear regression models of tort reforms on case volume per thousand doctors

Table 5 is similar to Table 4a above, but the dependent variable is the number of cases per thousand doctors. Table 5 shows that the only reform that reaches statistical significance across all models is that of non-economic damages caps, which reduces the number of cases by 2.11 cases per thousand doctors to 2.55 cases per thousand doctors, which translates to a reduction of 10% to 13%, depending on the specification. Periodic Payments reform decreased the number of cases by 0.8 to 1.15 cases per thousand doctors, which translates to a reduction of 4% to 6%, depending on the specification. Yet, the coefficient was weakly significant ($p < 0.1$) only for model 2. The joint effect (which is associated with a decrease in number of cases per thousand doctors) is significant for OLS1 and OLS2 ($p < 0.01$), but not for OLS3.

C. Linear regression models of tort reforms on total annual settlement payout.

Table 6 estimates the effects of tort reforms on the total annual settlement payout per doctor by state-year. Table 6 estimates that caps on pain and suffering damages reduces the total payments by 17%-23%, depending on the specification. The coefficients were significant at the 5% in OLS2, at the 10% level in OLS3. Reforming the collateral source rule decreased total payments by 13% to 20%, yet the coefficient was weakly significant ($p < 0.1$) only in OLS3. Allowing (or requiring) periodic payments was identified as increasing total payments by 14%, yet the coefficients was significant only in OLS3 ($p < 0.05$) and became weaker in the (non-reported) joint specification. All other reforms were identified as not having significant effect on total annual settlement payout. Reforms were neither found to be jointly significant.

⁸¹ More on the joint specifications under which I can test joint significance see supra text around note 82.

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D. Summary of State Level Analysis.

In sum, the analysis shows that caps on pain-and-suffering damages reduces the number of cases by 10% -13%, and potentially reduces total annual payment per doctor by 17% to 23%. Yet the statistical significance of these estimates is not strong in all specifications. Periodic payments reform potentially increases average awards and total annual payments by 14%, yet this effect appeared only in OLS3 (time trend) and became weaker in the (non-reported) joint specification. In all other models the effect was not statistically significant. All other reforms did not show a statistically significant effect on the dependant variables. Lastly, the joint effect of enacting all six reforms was found to be significant for decreasing the number of cases, yet not significant for decreasing the average payment, or the total annual payments per doctor. These results are consistent with the literature discussed above which identified an effect mainly for caps on non-economic damages.

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

Individual level analysis

The regressions presented in Table 4B estimated 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, etc. 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 importantly, as was explained in detail above, 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

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accurately identifying the impact of a tort reform. Third, with individual-level data, the distribution of awards can be broken down into different percentiles such that non-linear effects of reforms within the distribution can be identified using quantile regressions.

Table 4b presents the results for the case-level dependent variable of logged case settlement amount. Table 4b shows that three reforms have economically- and statistically-significant effects on average settlement amount. First, like in the state level analysis, the various models estimate that caps on non-economic damages reduces average award yet the effect was found to be much larger. Specifically, caps on non-economic damages reduces average awards by 32% to 56%, depending on the specification ($p < 0.01$ for all specifications). Second, and unlike in the state level analysis, the collateral source rule reform decreases average award by 38% to 55%, depending on the specification. These estimated coefficients are significant at the 1% confidence level across the specifications. Third, and also unlike the state level analysis, periodic payment reform decreases average awards by 27% to 34%, depending on the specifications ($p < 0.01$ for all specifications). However, there are three reasons to be suspicious of this result. First, the effect estimated in the individual level analysis (a large decrease in the average award) is in great contrast to the effect estimated in the state level analysis (a large increase in the average award). Because this is the only reform for which such gap exists between the state level and the individual level analyses, one needs to be cautious interpreting the results for the periodic payment reform. Second, there are only three variations in periodic payments reform in the relevant years; two states have enacted and one has struck down the periodic payments reform in the relevant year.⁸² Identifying the effect of reform off of only a few states is generally suspect. Third, while the impact of caps on non-economic damages and the collateral source rule remained the same in the “joint” specification, the effect of periodic payments became smaller (a reduction of 17%-18%) and statistically insignificant in OLS1 and OLS2. (The level of significance in OLS3 was smaller than 0.1, however). This suggests that in the separate regressions, a bias might exist and that one reform (periodic payment) might be picking up the effect of another correlated reform. Indeed, as can be seen in Table 2b, periodic payment is highly

⁸² See Table 2a.

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correlated with both the collateral source rule and caps on non-economic damages (The correlation coefficients are 0.43 and 0.31, respectively). All other reforms were not identified as having an economically- or statistically-significant effects.

Lastly, while the test for joint significance of the state level regressions was not significant, the test in the individual level regressions suggests that the joint effect is negative and economically- and statistically-significant ($p < 0.01$).

The quintile regressions in Table 4b show that the reduction in settlement payments is uniform across the entire distribution for the collateral source reform, but smaller at the upper portion of the distribution for caps on non-economic damages and the distribution for periodic payment reform. The results for caps on non-economic damages (and periodic payments) seem somewhat counterintuitive. After all, caps are thought to be designed to impact the upper portion of the distribution of payments—cases with high recoveries—not the lower portion.⁸³ However, as was discussed above, the choice to drop from the dataset cases that lasted more than six years might be a cause of this result.⁸⁴

To the extent, however that the quintile regressions do reflect reality, and indeed the lower portion of the distribution of settlement payments is the one most significantly impacted by caps on non-economic damages, it is worth mentioning that there are two types of cases with low recoveries that caps on non-economic damages might impact. These cases involve people who were severely injured, yet had a very low loss of income. These cases are usually brought by women, children and minorities, whose income is relatively low. Or, these cases involve people who incurred low medical costs and low loss of income due to the light nature of the injury. While it might seem desirable to cap non-economic damages from the latter group on the theory that the administrative costs they incur are high and the deterrence value is low, it is much harder to defend a cap on non-economic damages for the former group.

⁸³ Similarly, periodic payments affect settlement awards that are above some threshold, so one would expect to see them more effective in the upper portion of the distribution of awards. However, as was explained in the text, it is questionable whether the periodic payments reform is indeed effective.

⁸⁴ See *supra*, text around note 50,

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However, the quintile regressions results should be interpreted with caution for three reasons. First, due to STATA limitations, the only case-level control variable which I could use was the length of the case. Unfortunately I could not control for the age of the physician, the graduation year, or her field. Second, recall that due to STATA limitations the quintile regressions do not cluster by state, (I therefore expect a downward bias in the standard errors). Third, due to STATA limitations, I could not account for state specific time trends, as OLS3 does.

VI. Discussion

In this section I discuss and evaluate the results. I first discuss the robustness of the results to different specifications. I then discuss the reforms which were found to be effective: caps on pain-and-suffering damages, limitations on the collateral source rule and the periodic payments reform. I discuss various reasons (including endogeneity bias) for why the impact of these reforms is probably over-estimated. Next I discuss the reforms which were not found to have a significant impact and discuss various explanations for why this does not imply they have no impact. I conclude that one of the most probable impacts of tort reform is to change parties' incentives such that they find new ways to "game" the system, negating detectable changes in the dependent variables which the reform policy-makers intend to effect.

Evaluation of the Results

The regression results of this study estimated a decrease in average settlement of between 32% to 56% with the enactment of caps on non-economic damages, between 38% to 55% for the collateral source rule, and of 27% to 34% for periodic payments reforms, depending on the specification. (However, the impact of periodic payment reform might be attributed to the fact that there are only three variations of the reform in the relevant years and to its high correlation with the other two reforms.) In comparison, Danzon found that non-economic damage caps reduced average awards by 19% and

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collateral source reforms reduced average awards by 50% in one study and by 16%-26% and 10%-20%, respectively, in another.⁸⁵

Indeed, it is not surprising that caps on non economic damages have a large effect. Various scholars estimated in the past that pain and suffering awards made up approximately fifty percent of total awards, at least in some types of personal injury cases.⁸⁶ Many caps are set by legislatures at \$250,000 while more than 30% of the cases in the dataset include awards larger than that. Indeed, the mean (median) award of cases not subject to caps are \$262,000 (\$132,000), where as the mean (median) award of cases subject to caps are only \$199,000 (\$84,000), respectively.

It is also not surprising that reforming the collateral source rule was found to have a large effect on payments. By forcing (or allowing) courts to deduct from the damages awards income from other collateral sources, legislatures were able to significantly lower amounts plaintiffs recovered. The mean (median) award of cases not subject to the collateral source reform are \$242,000 (\$125,000), where as the mean (median) award of cases subject to the reform are \$243,000 (\$107,000), respectively.

While the reforms directly apply to court awards only, parties settle in the shadow of the legal regime they expect to face down the road in case they fail to settle. Thus, the effect of the reforms carries beyond the courthouse doors.

What is perhaps surprising however is that periodic payment reform decreases average settlement payments by more than 25%. As was mentioned above, there are only three variations of the reform in the relevant years and the effect becomes statistically insignificant at the (non-reported) joint regressions. This suggests that the impact identified in the separate regressions was due to correlation of periodic payment reform with caps on non-economic damages and the collateral source rule reform.

⁸⁵ Patricia Danzon, *The Frequency and Severity of Medical Malpractice Claims*, *Journal of Law and Economics*, Vol 27 (1984) 115-158, 139. Patricia M. Danzon, *The Frequency and Severity of Medical Malpractice Claims: New Evidence*, *Law and Contemporary Problems*, Vol 49(2) (1986) 57-84

⁸⁶ See Neil Vidmar et al., *Jury Awards for Medical Malpractice and Post-Verdict Adjustments of Those Awards*, 48 *DEPAUL L. REV.* 265, 296 (1998); W. Kip Viscusi, *Pain and Suffering in Product Liability Cases: Systematic Compensation or Capricious Awards?*, 8 *INT'L REV. L. & ECON.* 203 (1988)

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However, it is still worth thinking about the mechanism by which periodic payment reform might have an impact. Thirty one states allow or require periodic payment of large future damage awards. As was explained above, 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 her due damages if the plaintiff dies before the damages are fully awarded. The reason that periodic payment reform lowers the settlement payments seems to rest in the tax treatment of the structure settlement. Once awarded in periodic payments, not only is the award not subject to income tax, but also the interest accrued over time on the balance yet to be paid is exempt. Thus the defendant provides the plaintiff with a tax-free income-generating machine. It has been estimated that this tax advantage is worth anywhere between 3% to 80% of the recovery, most likely 10% to 30%.⁸⁷ Parties of course do not need the reform to opt for a structured settlement, they can do it even without the reform. Yet, once a law, the relative bargaining position of the parties changes. Without the reform in place, there might still be a tax advantage but the defendant would have to bribe the plaintiff to forgo the lump sum. The plaintiff would be able to extract some portion of the surplus created by the tax treatment. In contrast, with the reform in place, the defendant might be able to capture a higher, if not the entire, share of the surplus because the plaintiff knows that if she does not settle she might end up with the entire judgment awarded in periodic payments.

The next section discusses possible concerns about the validity of these results.

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 only caps on non-economic damages has an effect on the average award, analysis at the individual level suggests that reforming the collateral source rule and

⁸⁷ See Adam F. Scales, *Against Settlement Factoring? The Market in Tort Claims has Arrived*, 2002 Wis. L. Rev. 859 (2002) at 880-1.

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potentially that allowing (or requiring) future awards to be paid in periodic payments have a large and statistically significant effect along with the caps on non-economic damages. At the state level it is hard—if not impossible—to correct for the retroactive applicability of striking down a reform.⁸⁸ Thus, previous literature underestimated the effect of tort reforms because it analyzed the data at the state level and did not correct for the retroactive applicability of striking down reforms.

Recall that working with individual level dataset has two main advantages: it allows us to control for case-specific characteristics and to correct for striking down in matching cases to applicable law. To isolate these two effects of these two issues I analyze individual level data *without* correcting for the effect of striking down reforms, thus elucidating the effect of controlling for case-specific characteristics when moving from state-level to individual-level analysis. The results of non-reported regressions show that, as in the state-level analysis, the reforms had no effect on the average payments. This means that the sharp differences between the results in Table 4a and Table 4b with respect to the impact of caps on non-economic damages and the collateral source rule reform most likely stem from the fact that I corrected for striking down reforms.

That recoding the payments paid after the reform was struck down as *not subject* to the tort reform so drastically changed the estimated effect of the reform is not surprising. Recall that in the difference-in-differences approach, the coefficients are identified from *changes* in the reform variables. It is therefore important to include changes from a reformed tort law to a non-reformed tort law, once a law is struck down.

⁸⁸ There are several reasons for this. First, assuming one tabulates the data by the injury date (which is, as a default, the more reasonable date to use), then for any number of cases which 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, at the state level analysis, one is unable to distinguish between them. Second one runs into selection problems if one drops the cases that were settled after the reform was struck down from the data. Third, if one tabulates the data by the payment year, then for any number of cases which appear in any given reform year some cases will be from injuries which occur before the reform was enacted and other cases from after. But, at the state level analysis, one is unable to distinguish between them and dropping any one type of cases causes selection problems.

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Indeed, As Table 2a shows, five out of the ten variations for non-economic damages and five out the seven variations for collateral source rule reforms are due to striking downs.⁸⁹

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 studied in this study, there might be enough reforms struck down to bias the coefficients if the dataset is not corrected for striking down, yet not enough striking downs to adequately identify an effect of it.⁹⁰

The only other study which seems to conduct an individual level analysis is by Sloan, Mergenhagen and Bovbjerg. The authors found that caps on non-economic damages lowered payments by 37% and limitation on the collateral source rule by 21%, 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.⁹¹

However, despite the similarity of the results in the individual-level analysis with previous research, I believe that the coefficients identified here are most likely biased and the real effect probably smaller. There are two, and perhaps three, reasons to believe the coefficients may be biased.

The first reason is that many times reforms are struck down within very short time after enactment. Immediately after a reform is enacted, interest groups who 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

⁸⁹ See, *supra*, footnote 1 and footnote 3 for the states which struck down caps on non economic damages and collateral source rule, respectively.

⁹⁰ Specifically, as Table 2a shows, for joint and several liability there was only a single striking down out of the six variations. For caps on punitive damages, there were only three striking downs out of the thirteen variations. For higher evidentiary requirements for punitive evidence there were only two striking downs out of the nine variations. For periodic payments there were two striking downs out of only three variations.

⁹¹ Sloan, Mergenhagen and Bovbjerg, *supra* note 28.

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courts would shortly strike down the reform.⁹² Thus, it could be that the cases subject to the reforms—which settle before the reform is struck down—are the “easy” ones, which settle within a relatively short time. But if these cases also have low expected monetary damages, then we 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 we 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 struck down 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, as discussed above, interest groups commence efforts to invalidate a reform immediately after 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 struck down, the strategic behavior will increase, and settlement might be further delayed.⁹³ Above I showed that less than 7% of the “qualified” cases in Illinois (in which injury happened after caps on non-economic damages were enacted) were in effect subject to the reform, the rest were resolved after the reform was struck down.⁹⁴ The same holds for other reforms in other states.⁹⁵

To summarize this point, the actions of a state's Supreme Court may be anticipated, meaning that invalidation is *not* an exogenous shock, and lawyers in fact

⁹² For example, in the case of caps on non-economic damages, three of the five striking downs by states' supreme courts (in the relevant years) were within four years of enactment. Lower courts might have struck it down earlier.

⁹³ See Kathryn E. Spier. "Litigation," forthcoming in *The Handbook of Law and Economics*, A. Mitchell Polinsky and Steven Shavell, eds., North Holland.

⁹⁴ See supra note

⁹⁵ For example, in Alabama less than 8% of the qualified cases were resolved before the Caps on Con-Economic damages was struck down, in Arizona less than 25% of the qualified cases were resolved before the Periodic Payment reform was struck down. In Ohio less than 14% of the qualified cases were resolved before the Collateral Source reform was struck down.

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settle cases with a view to the future action of the Supreme Court. This might lead to overestimating of the impact of tort reform.

A third and somewhat related, reason for why the results could be biased is because of an 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 impacting tort awards, which is the direction of causality which I have assumed so far, tort awards might impact tort reforms. For example, a spike in annual awards might prompt states to adopt tort reforms in the following year. Yet, if the spike is random, one would expect to see future annual awards regress to their normal level (known as “reversion to the mean”).⁹⁶ But this might be incorrectly identified as if the tort reform caused annual payments to decrease, whereas pure chance is the actual cause. The result of this phenomenon is that reforms which were determined to be effective are, in fact, not effective.

There are three main reasons to believe that this 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 medical malpractice-specific, but rather apply generally to all types of torts. Thus, even if reforms were enacted in reaction to high awards, they would not (in those more general cases) be a result of high *medical malpractice awards*. Third, recall that, as I apply a difference-in-differences approach, the impacts of the reforms are identified from *changes* in the laws. But changes in the laws include not only enactments but also striking downs. Thus, while the claim that a high 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 low awards prompt states'

⁹⁶ Wolfram MathWorld explains that reversion to the mean is “the statistical phenomenon stating that the greater the deviation of a random variable from its mean, the greater the probability that the next measured variable will deviate less far. In other words, an extreme event is likely to be followed by a less extreme event.” See <http://mathworld.wolfram.com/ReversiontotheMean.html>

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supreme courts to strike down reforms. But this is much less reasonable, as it seems that plaintiff lawyers attempt to strike down the reforms without any correlation to actual awards.⁹⁷ Finally, insofar as the findings suggest a decrease in settlement payments due to tort reform, it is unlikely that *lower* settlement payments motivated legislatures to adopt medical malpractice reforms.

To further deal with the problem of endogeneity, and as was discussed above, I specified OLS3, which added 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, OLS3 helps with the problem of endogeneity because it controls for the existence of high pre-reform awards. In practice, however, the extent at which OLS3 helps with the endogeneity problem is not clear.⁹⁸

Lastly, one can deal with the problem of endogeneity by taking an instrumental-variable approach.⁹⁹ This approach, however, is problematic in studies like ours 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 which 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

⁹⁷ One may argue however that anticipating low 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 dataset.

⁹⁸ There are three main problems. First, it comes at the expense of the statistical power of the regression because it introduces another 50 variables to the equation. Second, it controls for *linear* time trend only, while the actual trend may be non-linear. Third, and most importantly, if the reform was enacted in 1991-92, there are not enough pre-reform data points to control for high pre-reform awards.

⁹⁹ An instrumental variable (also known as an IV) approach is a way which has been developed to deal with the problem of endogeneity. The IV is a variable which does not appear in the original regression, and is uncorrelated with its error term. The IV is, however, correlated with the explanatory variable suspected to be endogenous. For example, Republican control is a variable which does not appear in the original regression, and is assumed to be uncorrelated with the error term (or with tort awards), but might be partially correlated with the existence of a reform. See Wooldridge, *supra* note 69 at chapter 15. Danzon, *supra* note 42, reported both OLS and 2SLS (which are regressions using an IV approach) results in her regressions. Zuckerman, Bovbjerg and Sloan criticized her approach making similar arguments to the ones in the text. See Zuckerman, Bovbjerg and Sloan, *Effects of Tort Reforms and Other Factors on Medical Malpractice*, Inquiry Vol 27, 167-182 (1990) at 174.

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valid, a Democrat-controlled government should be more likely not only to not pass a tort reform but also to “unpass” or cancel a tort reform. But this has (probably) never happened. Tort reform is “sticky” once it is on the books; only the states’ courts can strike it down, and this should be unrelated to whether there is a Democrat-controlled government. It seems that the “stickiness” of the law might cause theoretical problems to any other instrument.

Despite these theoretical problems, I ran 2-Stage-Least-Square regressions, which are regressions used for instrumental variables approach. I used various combinations of the following variables as instruments (all of which were used by other researchers): 1) whether or not Republicans controlled the state government; 2) whether a state had enacted some kind of a product liability reform before; 3) whether a state had enacted some kind of a class action reform before; 4) the percent of state population who are Roman Catholics; 5) the percent of state population who are Mormons; and 6) whether or not the state legislators are term-limited.¹⁰⁰

Unfortunately, I ran into most, if not all, the following problems in all our specifications: 1) the instrumental variable coefficient (or coefficients if I used more than one instrument) in the first stage had the opposite sign as expected; 2) the coefficient (or coefficients) in the first stage was not significant; 3) R-Square in the first stage was small; 4) the coefficient in the second stage was totally unreasonable; 5) specification failed one or more of Stata identification tests.¹⁰¹

Two problems were most troubling. First, even if an instrument (or instruments) were valid for one reform they were 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-square) in the first stage for one reform (as

¹⁰⁰ I thank Catherine Sharkey for sharing with us the first instrument, and Jonathan Klick for sharing the other five. For an explanation for the theoretical rationale behind instruments 2 to 6 see Jonathan Klick & Thomas Stratmann, Does Medical Malpractice Reform Help States Retain Physicians and Does It Matter? (working paper, 2005) on pages 13-14.

¹⁰¹ The fact that the instruments did not work for us does not necessarily imply it could not work in other studies which may look at more years. For an interesting paper on abuse of instruments see John J. Donohue and Justin Wolfers, Uses and Abuses of Empirical Evidence in the Death Penalty Debate, 58 Stanford Law Rev. 791 (2006).

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predicted by the theory that 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 the first stage and the second stage) change 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 variable approach for this study.

In sum, for reasons explained above, I believe that the endogeneity bias is probably not a big problem in the analysis. Yet, one needs still to keep it in mind while looking at the results.

In conclusion, this study provides strong evidence that tort reforms impact settlement payments. Among the six reforms examined in this study, caps on non-economic damages and modification of the collateral source rules have significant impacts on the average settlement payment of the cases which end up being subject to the reform. (Implementing the periodic payments reform may also impact average payments.) Yet, it remains hard to determine what the exact scope of the impact is. It is probably a combination of increasing strategic behavior by parties before the reform is constitutionally tested in the state Supreme Court and lowering average settlement payments. Only caps on non-economic damages were identified as having (sometimes) a significant effect on number of cases per thousand doctors and total annual settlement payments.¹⁰² The joint effect of enacting all six reforms was statistically significant in reducing the number of cases but not the average award or total payments.

Concerns About Ineffective Reforms

While there was significant evidence of an effect on average settlement payment from caps on non-economic damages, modifications of the collateral source rule and the periodic payment reform, evidence of an effect from other reforms was less forthcoming.

¹⁰² In any case, it is worth recalling that an effect for caps on non-economic damages was identified despite the fact that I excluded cases that took more than six years to settle. This should have diluted our ability to identify the effect of caps, because these caps are presumably most salient for the most severe injuries, which might take more than six years to litigate.

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This does not mean that they do not have an effect, of course. There might be several plausible explanations for why an effect is not detected, which I will now discuss. These explanations—in most cases—apply to past research as well.

A. Limitations Of The Econometric Models Specified

The first concern is that there might not have enough strike-downs in the years studied for the difference-in-differences model to identify an effect. As that was discussed in detail above I will not repeat it here.¹⁰³

Second, there is a concern that while I reported “separate” specifications, the “joint” specifications are the ones which needed to have been used. The literature discusses “separate” specifications which estimates distinct regressions for each tort reform variable, excluding the other possible reforms, and “joint” specifications which estimates a regression including all tort reform variables as dummies in the same equation.¹⁰⁴ In the separate regressions there is a risk of some bias if one reform is picking up the effect of another correlated reform. Although most of our reform variables are weakly correlated, some do have stronger correlations.¹⁰⁵ Thus, in non-reported regressions I also estimate a joint regression where all of the reforms variables are included. The joint specification, however, potentially suffers from some problems as well. Here, the risk is that multi-collinearity between some of the reform variables will result in insignificant coefficients. Because of this, I run both separate and joint specifications as a robustness check on our estimates. Interestingly, most of the results remain the same in the joint specifications, suggesting that the results are robust to this kind of variation. As was discussed above, however, the only major difference is with respect to periodic payments.

¹⁰³ See, *supra*, text around footnote 87.

¹⁰⁴ Rubin and Shepherd, Tort Reform and Accidental Death (SSRN working paper, 2005). Currie and Macleod, First Do No Harm? Tort Reform and Birth Outcomes, (5/29/2006, working paper on file with author).

¹⁰⁵ For example, Collateral Source and Periodic Payments have a correlation coefficient of 0.368 (see Table 2a). Observe that Table 2a reports the correlation coefficients of reforms existing in the period between 1991 and 2004. A better correlation table will report correlation between enacted (and struck down) reforms in that period. In a non-reported table it is shown that correlation between enacted (and struck down) reforms is much higher for some reforms.

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Third, it could be that my choice to drop cases which lasted more than six years (besides reducing the size and depth of the dataset) biased my estimators as these likely represent the most complicated cases, which might be associated with higher awards.¹⁰⁶ To deal with this concern I ran the regressions for all cases which injury occurred after 1991 (without limiting it to those that ended within 6 years). This increased the sample size to about 139,000 cases. While the size of the coefficients changes the main results of this paper 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 the 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 dataset an effect for the other reforms, if such effect existed.

Fifth, it is possible that other relevant reforms were not included in the model. It is estimated that about 33% of litigated medical malpractice cases involve death of a patient (of which about 25% have a positive payout).¹⁰⁷ 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 was not and could not be accounted for in this study.¹⁰⁸ In any case, the “big” money is in cases that ended with injury and not with

¹⁰⁶ See supra note

¹⁰⁷ This estimate is derived from *Civil Justice Survey of State Courts, Medical Malpractice Trials and Verdicts in Large Counties, 2001* (April 2004, NCJ 203098), available at: <http://www.ojp.usdoj.gov/bjs/pub/pdf/mmtv1c01.pdf>

¹⁰⁸ Up until the middle of the 19th century, common law courts barred tort recovery for wrongful death because they were reluctant to allow compensation to those who cannot enjoy it: “in 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 rather than to injure him. See generally Wex S. Malone, *The Genesis of Wrongful death*, 17 Stan. L. Rev. 1043 (1965) (describing the history of the wrongful death action in the US and England). Today every state in the US has some type of statutory remedy for wrongful death. Many states have enacted reforms which cap total damages in wrongful death claims. These are separate from the reforms I explored here.

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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 the reforms influence specific types of physicians' behavior. Specifically, it could be that tort reforms have an effect on ob/gyns' awards but not on 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 dataset may have also caused problems in identifying a significant effect. Effects could have been masked by collapsing various distinct reforms into one reform category. For example, if a reform which imposes several liability on any co-defendant has an effect, whereas a reform that imposes several liability only on co-defendants whose liability is less than 50% 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 different variations that exist in every other type of reform in our dataset. As was explained above, collapsing reforms is a standard practice in the literature and is intended to gain more statistical power to the model.

B. Limitations of The NPDB.

As was discussed in detail above, it is possible the NPDB is not good enough to identify the effects. Under-reporting and multi-reporting problems of the NPDB may impede our ability to identify an effect, or lacking the exact date (and only having the year available) of the malpractice event and payment creates too much noise to identify an effect.¹⁰⁹ Another possible source of bias would be data management choices regarding duplicate records and incorrect coding of periodic payments.¹¹⁰

¹⁰⁹ Moreover, some reforms have complex effective dates which are hard to take into account. For example, in 1996 South Dakota passed a reform imposing caps on non-economic damages to become effective in 1997 over all cases still pending since 1976.

¹¹⁰ These problems were described in more detail, *supra*, notes 46 and 48.

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To test the significance of all these concerns, I ran the analysis on various permutations of the NPDB, each created by a different algorithm which was intended to identify duplicate records, under-reporting due to incorrect coding of periodic payments and otherwise account for these problems. The results, however, do not change much.¹¹¹

C. The Boiling Pot Hypothesis.

Another possibility for why most reforms seem to have no effect on the number of cases settled is that the medical malpractice market is super-saturated. There may be so many potentially actionable malpractice complaints that even if one is able to reduce the number of certain types of cases, others will immediately fill the vacuum created by that reduction. This is called the boiling pot hypothesis. Indeed, it is estimated that only about 8.4% of severe medical injuries are picked up by lawyers.¹¹² 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.

D. Reforms Had An Impact On Variables That Are Unobservable.

Reforms may not have significantly affected the number of cases per doctor, because reforms may only 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, as was explained above, 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

¹¹¹ Regressions results for these regressions are available upon request.

¹¹² It also estimated that only about 16.7% of severe medical injuries *due to negligence* are being picked up by lawyers. David M. Studdert et al, Disclosure of Medical Injury to Patients: An Improbable Risk Management Strategy (2006) (on file with author). One may wonder why plaintiff lawyers choose to take the old cases (now subject to tort reform) in the first place. A possible explanation is that these cases (that are now locked by the 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.

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date and payment date. However, the *publicly available* dataset includes only the years and not more exact dates. Thus, it is highly unlikely I could measure this effect even if the hypothesis is true.

E. Reforms Changed Lawyers' Incentives.

It is also possible that plaintiffs' lawyers' behavior changed in response to the tort reform, negating the effect of tort reform on the statistics measured by our dependent variables. For example, lawyers may spend more money to overcome higher evidentiary requirements for punitive damages, resulting 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).¹¹³ Since I cannot observe such adaptive efforts on the part of lawyers, I am unable to detect their effect. Another way lawyers might have dealt with reforms is 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 non-capped economic losses.¹¹⁴

Particularly, the boiling pot hypothesis (explained above) 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 our dependent variables.¹¹⁵

¹¹³ See Avraham, Putting a Price on Pain-and Suffering Damages: A Critique of the Current Approaches and Preliminary Proposal for Change, 100 *NU law Rev.* (2006) 87, at 100 (lawyers may ask "the jury to consider plaintiff's suffering in order to send a message to the defendant to never subject anyone to the type of indignity and injustice and intolerable acts to which the plaintiff had been subject").

¹¹⁴ *Id.* at fn 63, suggesting that loss of consortium for child death and rehabilitation costs are examples of types of damages that are now understood as economic damages, yet have been types of general damages in the past, and have been itemized in the last decade in response to caps.

¹¹⁵ A support to the claim that plaintiff lawyers' costs may have been increased as a result of tort reform can be found in a recent study by Black, Silver and Hyman, where they argue that defense lawyer's costs have been increasing in Texas in recent years. Texas has enacted several medical malpractice reforms in the mid-1990s. Thus, to the extent that plaintiff lawyer's costs are positively correlated with defendant's lawyers' costs, plaintiff's lawyers' costs may have also gone up in Texas, potentially due to the tort reforms.

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F. Reforms Did Not Really Change The Economic Reality Of The Previous Regime.

For example, limiting joint-and-several liability may potentially not change the regime because long before the reforms were enacted defendants (who were held jointly liable for other co-defendants' fault) had the right to contribution from co-defendants. 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. Yet, our quantile regressions do not show a significant effect even on the 75th percentile, so this explanation is unlikely in our dataset.¹¹⁶ Or, it may be that high awards were never really collected due to physicians' strategic or non-strategic bankruptcy. Or, more likely, plaintiffs' lawyers do not attempt to collect beyond the limits of physicians' insurance policies ("blood money"), so the policy limits were set low enough that cap did not actually restrict awards.¹¹⁷

G. Reforms Had An Effect On The Distribution Of Awards, But Not On Their Mean.

For example, caps may have lowered high awards but inflated low awards, resulting in an unchanged mean. An increase in the number or value of small awards could happen if health care providers commit more low-harm negligence due to the externality created by caps. This could also happen if lawyers anchor their settlement expectations to the caps, driving up awards in low-harm cases.¹¹⁸ However, our analysis

¹¹⁶ For caps on punitive damages, the likelihood of finding an effect is especially small because these caps are especially rare in medical malpractice cases. See Eisenberg, *supra* note 41.

¹¹⁷ See Baker, *supra* note 20, for evidence that plaintiffs' attorneys do not pursue personal assets of the defendants. For the effect of policy limits see also David Hyman, Bernard Black, Charles Silver, William Sage & Kathryn Zeiler, *Do Defendants Pay What Juries Award?: Post-Verdict Haircuts in Texas Medical Malpractice Cases, 1988-2003* (working paper, March 2006); Charles Silver, Kathryn Zeiler, Bernard Black, David Hyman & William Sage, *Physicians' Insurance Limits and Malpractice Payouts: Evidence from Texas Closed Claims*, (working paper, March 2006).

¹¹⁸ Others have documented such anchoring as well. For example, in 1993, the Clinton administration attempted to limit CEOs' cash compensation 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 suggests, the result was that many companies *increased* cash compensation to \$1 million. Rose and Wolfram suggest that the \$1 million limit may have served as a focal point for compensating CEOs. See

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of the quantile regressions does not support this possibility: there are no cases of a reform which significantly increased the lower part of the distribution and, at the same time, significantly decreased the upper part.

H. Reforms Are Ineffectual For Their First Years.

There are two variations on this argument. First, it take few years for the reform to have an impact because there is a learning period during which parties are still not sure how the reforms would impact 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 variation seems a bit far fetched. Lawyers on both sides are sophisticated parties and it is hard to imagine convincing reasons for why they a significant period of time to learn the implications of the reform. However, to take care of this concern I ran another set of (non-reported) regressions. I added two dummy variables, one which indicates whether the reform is in its first four years and one which indicates whether it is older than that. The results, by and large, do not change.

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 the settlements 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 is not readily available.

VII. Conclusions and Future Research

The study suggests that caps on pain-and-suffering damages, limitations on the collateral source rule and potentially that requiring (or allowing) for periodic payments of

Rose and Wolfram, 2002, "Regulating Executive Pay: Using the Tax Code to Influence Chief Executive Officer Compensation", *Journal of Labor Economics*, V. 20, No. 2: pp. S138-S175 (documenting a "spike" in base salaries at \$1 million that did not exist before the new tax rules).

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damages for future harm have a significant impact on average settlement payments. Caps on pain-and-suffering damages also decreased number of cases per thousand doctors and total annual payments. However, the joint effect of enacting all six reforms was statistically significant in reducing the number of cases but not the average award or total payments.

No less important is the fact that several additional conclusions emerge from the above 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 applies to all pending cases, no matter 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, as was explained in details above, even if more tort reforms had an actual impact, there are many reasons why various limitations might prevent us from being able to detect it. Third, while we do find a statistically significant impact for two reforms, there are good reasons to believe that these tort reforms do not have a significant *economic* effect on total payments. One of the main reasons is that lawyers probably adapt their legal strategy 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 striking-down of the reform, are probably effective in keeping the bottom line unchanged. Fourth, as a result, a reasonable prediction is that passing the pending federal medical malpractice bill might reduce the number of cases per doctor but will not significantly alter the annual average or total awards. However, passing the federal bill might change the distribution of cases being picked up (less low-income plaintiffs, for example), might influence the time for settlements, as well as pinch plaintiff lawyers' pockets.

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 picked by plaintiffs' lawyers, as the data suggests,

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then health providers might not be getting enough deterrence signals from the market. Lowering the number of cases even more will then 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, a more productive research trail 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.

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Appendix A– Tables and Graphs

Table 1- Descriptive Statistics

Variable	Description	Mean	Std Dev.	Source
Dependent Variables				
Avg. Payments	Settlement Amount	249,988	90,918	NPDB
# of Cases	# of Settlements per doc (per person)	1620.378	682.6636	NPDB
Total Payments	Total Annual Payouts per State	52,100,000	68,200,000	NPDB
Reforms				
R_CN	=1 if state has passed a cap on non-economic damages	0.32	0.47	DSTLR
R_CP	=1 if state has adopted a cap on punitive damages	0.36	0.48	DSTLR
R_CS	=1 if state has adopted limitation on the collateral source rule	0.62	0.48	DSTLR
R_JS	=1 if state had adopted limitation on joint and several liability	0.75	0.43	DSTLR
R_PE	=1 if state has adopted evidentiary requirements for punitive damages	0.54	0.50	DSTLR
R_PP	=1 if state has allowed periodic payments	0.58	0.49	DSTLR
State Controls				
C_65	Percentage of Population over 65	12.66	2.04	CMMS, USCB
C_BS	Percentage with Bachelor degrees	22.18	4.33	USCB
C_CarDeath	Car Fatalities Per Million People	173.87	56.05	DOT
C_HealthSpend	Per Capita Healthcare Expenditures	4323.83	558.471	CMMS
C_Income	Income per Capita	26866.9	3911.836	DOC
C_Lawyer	Lawyers per Capita	289.85	90.73	ABF
C_LifeExp	Life expectancy for Newborns	75.65	1.28	CDC
C_MedCPI	Consumer Price Index for medical goods	213.11	21.12	BLS
C_Metro	Metropolitan Percentage	67.51	20.72	USCB
C_NewRes	Percentage of new Residents	3.27	1.42	USCB
C_Pop		5242.665	5714.688	
C_Unempl	Unemployment Rate	5.58	1.55	BLS
Individual Controls				
I_AgeGroup	Age of physician at event	42.78	10.76	NPDB
I_CaseLength	Case length	3.45	1.54	NPDB
I_Grad	Year of graduation	1969.89	11.39	NPDB
I_StFundPay	=1 if State Fund Paid	0.03	0.18	NPDB
I_Field	Fifteen Fields of Physicians			NPDB

ABF- American Bar Foundation, BLS- Bureau of Labor Statistics, CDC- Centers for Disease Control, CMMS- Centers for Medicare and Medicaid, DOC- Department of Commerce, DOT- Department of Transportation, DSTLR- Database of State Tort Law Reforms, NPDB- National Practitioner Data Bank, USCB- U.S. Census Bureau

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Table 2A: Changes in States with Reforms (by adoption and judicial reversal)

	91	92	93	94	95	96	97	98	# of Relevant Variations for State Level Analysis	# of Relevant Variations for Case Level Analysis
CN	16	14	14	14	15	18	19	17	9	10 ¹¹⁹
JS	35	37	37	37	39	39	40	40	5	6 ¹²⁰
CS	32	32	31	30	30	30	31	31	5	7 ¹²¹
CP	15	16	16	17	17	21	23	24	11	13 ¹²²
PE	27	29	29	30	32	34	34	32	9	9 ¹²³
PP	20	20	20	20	18	19	19	19	3	3 ¹²⁴

Table 2a shows the enactment and striking down of: Caps Non-Economic Damages, Joint & Several Liability, Collateral Source Rule, Caps on Punitive Damages, Punitive Evidence, and Periodic Payments. If a reform was enacted or struck down on or after July 1st, it was coded as enacted or struck down at the following year. The column before last presents the number of variations of the reforms from 1991 to 1998. It includes changes that are relevant only for the state-level analysis. It includes changes not directly reflected in the table. For example, in 1995 Kentucky struck down the collateral source reform and Wisconsin enacted it. While the table shows no change in the number of states with collateral source reform between 1994 and 1995, this is counted as two variations. The last column presents the number of variations of the reform which are relevant for the case-level analysis. In addition to the variations counted in previous columns, it includes striking downs which happen after 1998 and would apply to pending cases. For example Ohio struck down collateral source in 2000 and Kentucky in 2002. Therefore, the last column has two more variations than the column before last. The footnotes show the states and years in which each reform was struck down.

¹¹⁹ AL & OH struck down the reform in 1992. IL & OH in 1998. OR in 2000.

¹²⁰ OH struck down the reform in 2000.

¹²¹ KS struck down the reform in 1993. OH in 1994 and 2000. KY in 1995, RI in 2002.

¹²² AL struck down the reform in 1993. OH in 2000, ID in 2003.

¹²³ IL & KY struck down the reform in 1998.

¹²⁴ OH & AZ struck down the reform in 1995.

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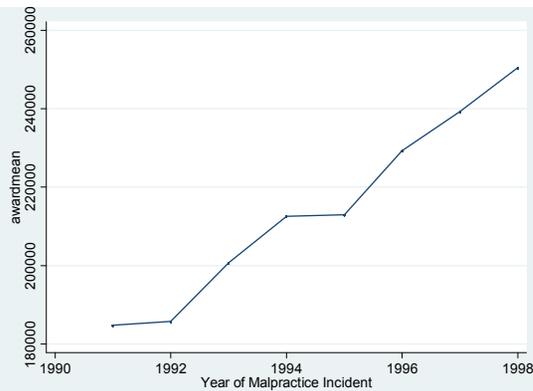
Table 2b: Correlation Between State Tort Reforms 1980-2004

	Contingency Fee	Periodic Payment	Split Recovery	Punitive Evidence	Caps Punitive	Joint & Several	Collateral Source	Caps Total	Caps Non-Econ	Patient Fund
Contingency Fee	1.0000									
Periodic Payment	0.1606	1.0000								
Split Recovery	0.0341	0.1808	1.0000							
Punitive Evidence	-0.0790	0.2326	0.2219	1.0000						
Caps Punitive	-0.0699	0.0767	0.1840	0.2523	1.0000					
Joint and Several	0.0435	0.3017	0.2313	0.3062	0.2566	1.0000				
Collateral Source	0.2894	0.4298	0.2744	0.2893	0.1467	0.2742	1.0000			
Caps Total Damages	-0.2499	0.1219	0.0155	-0.0746	0.2005	-0.0394	-0.0860	1.0000		
Caps Non-Econ	0.0895	0.3110	0.1362	0.2805	0.1235	0.1921	0.2605	-0.1467	1.0000	
Patient Compensation	0.0954	0.0685	0.0284	-0.0920	0.1240	0.0905	-0.1219	0.4314	-0.1467	1.0000

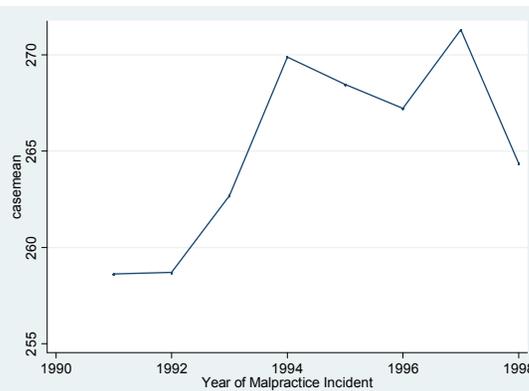
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Table 3- State level aggregated results: general trends. (Medical malpractice settlements; doctors only).

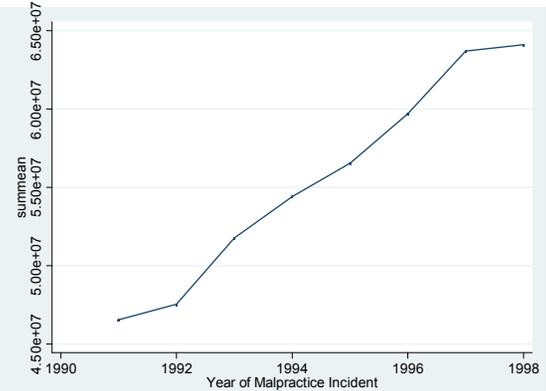
3 (a) – Average Payment



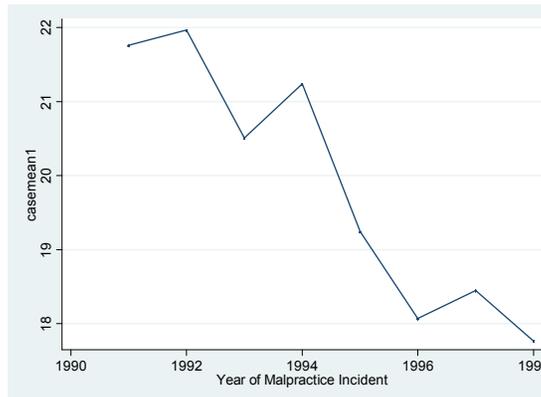
3(b)– Average Number of Cases per State.



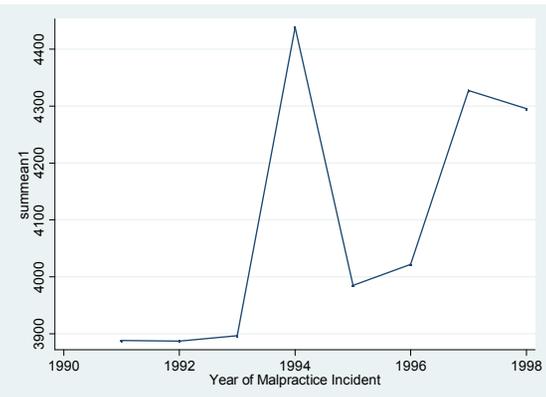
3(c)– Total Payments per State.



3(d)– Average Number of Cases per 1000 Docs



3(e)– Total Payments per Doc



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Table 4a: State Level Regressions (dependent variable=log of Average Award)

Reform	OLS1	OLS2	OLS3
Caps Non-Economic	-0.07 (0.09)	-0.09 (0.08)	-0.12 (0.08)
Joint & Several Liability	-0.15 (0.16)	-0.11 (0.17)	0.04 (0.14)
Collateral Source Rule	-0.11 (0.18)	-0.11 (0.17)	-0.04 (0.09)
Punitive Damages Evidence	-0.05 (0.12)	-0.01 (0.12)	0.04 (0.10)
Caps Punitive Damages	0.06 (0.08)	0.06 (0.07)	-0.03 (0.10)
Periodic Payment	0.04 (0.09)	0.08 (0.07)	0.14*** (0.05)
Controls	No	Yes	Yes
State Time Trends	No	No	Yes

NOTE: N=400. Standard errors are clustered by State. Not reported: Year dummies, State Dummies, Percentage of Population over 65, Percentage with Bachelor degrees, Car Fatalities Per Million People, Per Capita Healthcare Expenditures, Income per Capita, Lawyers per Capita, Life expectancy for Newborns, Consumer Price Index for medical goods, Metropolitan Percentage, Percentage of new Residents, Unemployment Rate, HMO penetration. * significant at 10% or less. ** significant at 5% or less. *** significant at 1% or less. The Time Period is 1991-1998.

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Table 4b: : Individual Level Regressions (dependent variable=log of Individual Payment)						
	OLS Regressions			Quintile Regressions		
	OLS1	OLS2	OLS3	25th	50th	75th
Caps Non-Economic	-0.32 *** (0.10)	-0.32*** (0.10)	-0.56*** (0.14)	-0.33** 0.13	-0.33** 0.16	-0.16** 0.07
Joint & Several Liability	-0.02 (0.08)	-0.04 (0.08)	-0.07 (0.15)	-0.05 0.07	-0.08 0.05	-0.05 0.05
Collateral Source Rule	-0.38*** (0.10)	-0.38*** (0.10)	-0.55*** (0.04)	-0.56*** 0.09	-0.70*** 0.09	-0.60*** 0.10
Punitive Damages Evidence	0.03 (0.04)	0.01 (0.05)	-0.04 (0.05)	-0.04 0.07	-0.00 0.03	-0.01 0.04
Caps Punitive Damages	0.01 (0.03)	0.00 (0.04)	0.00 (0.07)	0.00 0.04	0.04* 0.02	0.09*** 0.02
Periodic Payment	-0.27*** (0.07)	-0.28*** (0.06)	-0.34*** (0.04)	-0.57*** 0.12	-0.69*** 0.10	-0.39*** 0.08
Controls	No	Yes	Yes	Yes	Yes	Yes
State Time Trends	No	No	Yes	No	No	No

NOTE: N=105,944. Standard errors are clustered by State in OLS regressions. Bootstrapping standard errors in brackets in the quintile regressions. Not reported: Year dummies, State Dummies, Percentage of Population over 65, Percentage with Bachelor degrees, Car Fatalities Per Million People, Per Capita Healthcare Expenditures, Income per Capita, Lawyers per Capita, Life expectancy for Newborns, Consumer Price Index for medical goods, Metropolitan Percentage, Percentage of new Residents, Unemployment Rate, HMO penetration, Age of physician at event, Case length, Year of graduation, State Fund Paid, Fields of Physicians. * significant at 10% or less. ** significant at 5% or less. *** significant at 1% or less. The Time Period is 1991-1998.

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Table 5: State Level Regressions (dependent variable=Number of Cases per Thousand Doctors)

Reform	OLS1	OLS2	OLS3
Caps Non-Economic	-2.11*** (0.63)	-2.55*** (0.76)	-2.45* (1.33)
Joint & Several Liability	0.11 (1.25)	0.45 (1.29)	-2.02 (1.57)
Collateral Source Rule	-0.65 (0.82)	-0.39 (1.11)	-1.75 (1.33)
Punitive Damages Evidence	0.57 (0.63)	0.40 (0.76)	-0.32 (0.59)
Caps Punitive Damages	0.09 (1.19)	0.06 (1.09)	-1.40 (1.27)
Periodic Payment	-0.80 (0.51)	-1.15* (0.66)	1.06 (1.57)
Controls	No	Yes	Yes
State Time Trends	No	No	Yes

NOTE: N=400. Standard errors are clustered by State. Not reported: Year dummies, State Dummies, Percentage of Population over 65, Percentage with Bachelor degrees, Car Fatalities Per Million People, Per Capita Healthcare Expenditures, Income per Capita, Lawyers per Capita, Life expectancy for Newborns, Consumer Price Index for medical goods, Metropolitan Percentage, Percentage of new Residents, Unemployment Rate, HMO penetration. * significant at 10% or less. ** significant at 5% or less. *** significant at 1% or less. The Time Period is 1991-1998.

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Table 6: State Level Regressions (dependent variable=Total Annual Payment per Doc)

Reform	OLS1	OLS2	OLS3
Caps Non-Economic	-0.17 (0.10)	-0.21** (0.10)	-0.23* (0.13)
Joint & Several Liability	-0.20 (0.23)	-0.14 (0.23)	-0.07 (0.17)
Collateral Source Rule	-0.20 (0.23)	-0.19 (0.20)	-0.13* (0.08)
Punitive Damages Evidence	-0.07 (0.15)	-0.04 (0.14)	0.01 (0.09)
Caps Punitive Damages	0.05 (0.09)	0.06 (0.08)	-0.10 (0.10)
Periodic Payment Caps Non-Economic	-0.02 (0.10)	0.01 (0.07)	0.14** (0.07)
Controls	No	Yes	Yes
State Time Trends	No	No	Yes

NOTE: N=400. Standard errors are clustered by State. Not reported: Year dummies, State Dummies, Percentage of Population over 65, Percentage with Bachelor degrees, Car Fatalities Per Million People, Per Capita Healthcare Expenditures, Income per Capita, Lawyers per Capita, Life expectancy for Newborns, Consumer Price Index for medical goods, Metropolitan Percentage, Percentage of new Residents, Unemployment Rate, HMO penetration. * significant at 10% or less. ** significant at 5% or less. *** significant at 1% or less. The Time Period is 1991-1998.

Appendix B– Independent Variables.

We first describe the individual-level control variables (I_*) and then state-level ones (C_*).

Age of doctor ($I_AgeGroup$). We used the age of the doctor at the time of the malpractice payment, measured in decades, to control for several possible factors. Older doctors may have different levels of care, experience, expertise in modern medical techniques, or accumulated wealth, compared to younger doctors, thereby influencing the dependent variables. The data was obtained from the NPDB Database.¹²⁵ Missing data was recorded as a null value.

Decade of graduation from medical school (I_Grad). This variable serves a similar function to the age of the doctor and was also obtained from the NPDB database.¹²⁶ Null values were recorded for missing data.

Case Length ($I_CaseLength$). The number of years elapsed between the malpractice incident and the related payment serves as a proxy for the complexity and magnitude of the claim. Longer time periods could indicate more complex cases, greater damage values being at stake, or latent injuries, all of which could influence the dependent variables. This variable is also used to interact with the prejudgment interest reform to adjust for payments which are inflated by prejudgment interest, as described in detail below under Section IV.4., Inflation and the Time Value of Money. Data was obtained from the NPDB database for each payment.¹²⁷ Note that this variable is used in this study as both a control variable and a dependent variable (in which case we, of course, exclude it as a control variable.).

Cumulative Payment Reports ($I_CumulPay$). The NPDB database indicates the number of previous payment reports made against a doctor at the time of the current payment report.¹²⁸ If past performance is indicative of future performance, a higher number of cumulative historical payment reports would lead to more frequent payment reports in the future. Also, doctors with a

¹²⁵ Id. at “AGEGROUP” field.

¹²⁶ Id. at “GRAD” field.

¹²⁷ Id. at “ORIGYEAR” field minus “MALYEAR1” field.

¹²⁸ Id. at “NPMALRPT” field

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high number of reports may practice in a specialty field where malpractice suits are more prevalent. Another possibility is that a doctor with more frequent payments could have a higher propensity to settle for a nominal payment rather than litigate and risk a larger payment, if a jury might somehow learn about her past record. Each of these traits could have an effect on the various dependent variables.

Cumulative Adverse Action Reports (I_CumulAA). In addition to the cumulative number of historical payment reports made against a doctor, the NPDB also reports the cumulative number of historical Adverse Action reports made against a doctor.¹²⁹ We use this data in a similar manner to the Cumulative Payment Reports discussed above.

Number of Defendants (I_NumDfdt). The NPDB Database reports the number of defendants included in a payment.¹³⁰ This information controls for the possibility that payments made on behalf of multiple defendants could have markedly different characteristics than payments made for a single defendant.

Single or multiple payments (I_MultiPymt). The NPDB Database also reports whether the payment is one of a series of payments or a single lump-sum payment. Single payments could have different traits compared to payments made through a series of payments, and we control for this possibility here.

Multiple Errors (I_TwoErr). Up to two malpractice codes, discussed above, can be listed in a NPDB payment report.¹³¹ Approximately 15% of the payments shown in the database reported two error codes. We hypothesize that payments made for two errors will generally be higher than those made for a single error.

Medical CPI (C_MedCPI). We used the annual Consumer Price Index for medical goods to control for changes in the cost of medical procedures. The CPI was obtained from the Bureau of Labor Statistics¹³² and assigned to each payment report based on the year of the malpractice incident. See further discussion of below under Section IV.4., Inflation and the Time Value of Money.

¹²⁹ Id., at “NPLICRPT” + “NPCLPRPT” + “NPPSMRPT” + “NPDEARPT” + “NPEXCRPT”

¹³⁰ NPDB Database, “NUMBPRSN” field.

¹³¹ Id. MALCODE1 and MALCODE2 fields

¹³² U.S Bureau of Labor Statistics, available at <http://www.bls.gov>, Series ID CUUR0000SAM.

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Income per capita (C_Income). Average income per person was obtained for each state and each year. Malpractice damages are often based on the lost wages of the injured party. Because the NPDB database does not include the income of the payee, the state average serves as a rough estimate of the income of the injured party. In addition, this control variable captures interstate differences in the cost of living. Both factors could influence the dependent variables tracked in our study. Annual figures were assigned to the payments based on the date of the malpractice incident.¹³³

Unemployment rate (C_Unempl). Unemployment rates were obtained for each state and each year from the Bureau of Labor Statistics.¹³⁴ As with income per capita, this control variable is relevant for the loss of income component of the total damage award. Annual figures were assigned to payments as of the date of the malpractice incident.

Doctors per capita (C_MD). The number of doctors per 100,000 people in each state was obtained for each year. Higher concentration of doctors per capita leads to more malpractice per capita, even when the rate of malpractice per doctor remains constant. Thus, controlling for the concentration of doctors per capita is essential to convert the dependent variable of payments per capita into payments per doctor. In addition, higher concentrations of doctors could indicate greater competition among doctors, further influencing the dependent variables.¹³⁵ Annual data was assigned to payments based on the year of the malpractice incident.¹³⁶

Lawyers per Capita (C_Lawyer). Like doctors per capita above, this statistic is measured in terms of lawyers per 100,000. It controls for potential differences in competition among

¹³³ Annual figures were obtained from the Bureau of Economic Analysis. Bureau of Economic Analysis, U.S. Department of Commerce, available at <http://www.bea.gov/region/state/local.htm>

¹³⁴ <http://stats.bls.gov/lau/staadata.txt>

¹³⁵ We obtained data on doctor concentration from the American Medical Association. American Medical Association, Physician Characteristics and Distribution.

¹³⁶ Data for 1988, 1991, 1998, 2002 and 2003 was not available. We estimated these values by performing a regression with the year as the independent variable and doctors per capita as the dependent variable for each state, using the years where we had complete data to determine the missing years. The estimated data should have no significant effect on the overall study because the interstate variation in doctor concentration, rather than the inter-year change within a state, is the most significant aspect of this control variable. In addition, there is a clear and consistent growth trend over the period from 1985 to 2001 resulting in an R-squared for the regression of missing years across all 51 states averaging 93%.

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lawyers and accessibility to lawyers caused by different lawyer population densities.¹³⁷ Annual data was assigned to payments based on the year of the malpractice incident.

Metropolitan percentage (C_Metro). The percentage of a state's population living in a metropolitan area, as defined by the U.S. Office of Management and Budget, is used as a control variable for each state and year. This statistic is almost identical to the level of urbanization in a state. We used metropolitan population ratios rather than urban population ratios because annual data for the former was more readily accessible from the U.S. Census Bureau.¹³⁸

New State Residents (C_NewRes). This statistic measures the percent of residents in the state who moved into the state within the past year. We hypothesize that new residents are more likely to sue their doctor because new residents must establish new relationships with doctors in the new state, leading to 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.¹³⁹

Life expectancy (C_LifeExp). We used life expectancy for babies born between 1989 and 1991 to control for differential health standards across states. In addition, expected remaining life is often used to calculate damages where disabilities are permanent. Since the NPDB database doesn't report the age of the victim, this control variable serves as a rough proxy.¹⁴⁰

¹³⁷ Information was obtained from the American Bar Foundation's Lawyer Statistical Report for 1988 and 1991 and from the American Bar Association's Lawyer Population Survey from 1998 to 2003. American Bar Foundation, Lawyer Statistical Report, 1988 and 1991. American Bar Association, Lawyer Population Survey, 1998 to 2003, available at <http://www.abanet.org/barserv/lawyerpopulation98-03.pdf>. Data for all other years was estimated from a linear regression of known annual statistics, as described above for doctors per capita.

¹³⁸ US Census Bureau, 1990 to 1999 available at <http://www.census.gov/population/estimates/metro-city/ma99-03b.txt>, 2000 available at <http://www.census.gov/prod/2004pubs/03statab/pop.pdf>. Data for 1988, 1989, and 2001 to 2003 were not available and were estimated using a regression of the known annual statistics against the time series.

¹³⁹ Data was obtained from the U.S. Census Bureau. Census Bureau, Geographic Mobility Report, Annual, Table 21, available at <http://www.census.gov/population/www/socdemo/migrate.html>. Data was obtained from the U.S. Census Bureau for each year from 1988 to 1994, and 1996 to 1998. However, data from 1988 to 1991 excluded intra-regional movers. These intra-regional movers include residents who lived in the same region of the U.S. (i.e. West, South, Midwest, as defined by the Census Bureau) before and after the move. Thus, we estimated these amounts by regressing total immigration into the state onto extra-regional immigration into the state for the years where we had such data. In addition, we estimated data for 1995 and 1999 to 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.

¹⁴⁰ Data was obtained from Centers for Disease Control for each state for the time period from 1989 to 1991. The same data was used for all years because the changes over time were miniscule. National Center for Health Statistics. Volume 1 U.S. Decennial Life Tables for 1989-91 No. 3, Table F (1999).

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Population over 65 (C_65). We also included the percentage of a state's population over 65 as a control variable because elderly patients have unique medical needs which could affect the dependent variables. This variable also controls for the fact that damages for permanent disabilities are calculated based on expected remaining life. Finally, senior citizens usually have little to no income, leading to smaller damage awards for lost wages.¹⁴¹ The annual figures were attached to the payments based on the year of the malpractice incident.

Population with Bachelors Degree (C_BS). We determined the percentage of a state's population which had a bachelor's degree or higher for each year from 1988 to 2003. This control adjusts for the higher income levels of college graduates, which are used to calculate lost wages in determining damages. In addition, more educated patients 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.¹⁴²

Health care expenditures per capita (C_HealthSpend). We used average health care spending by state and year to control for intrastate differences in the cost of medical care and in the extent of medical care utilization. Differential costs affect payment amounts that are based on the cost of medical procedures needed to remedy the malpractice. Also, higher healthcare utilization may lead to more medical procedures per capita, and thus more malpractice per capita.¹⁴³

Car fatalities per capita (C_CarDeath). The measure of car fatalities per million people per year within a state serves as a cumulative control for the state's traffic safety laws, prevalence of automobile travel, and driving conditions. Each of these factors influences the number of automobile accidents in a state, one of the most significant sources of injury requiring

¹⁴¹ Data from 1988 to 1993 were obtained from the Centers for Medicare and Medicaid Services, and data from 1994-2000 and 2002-2003 were obtained from the Census Bureau. Centers for Medicare and Medicaid Services data available at <http://www.cms.hhs.gov/statistics/nhe/state-estimates-provider/>. Census Bureau, Statistical Abstract of the United States, Section 1, 1994 - 2000, 2002,2003. Data for 2001 was estimated from a regression of the known years' statistics against the time series.

¹⁴² Data was obtained from the U.S. Census Bureau for each year except 1988 and 2003. Census Bureau, <http://www.census.gov/population/www/socdemo/educ-attn.htm>. Data for these two missing years was estimated using a linear regression of the known annual values over time.

¹⁴³ We gathered the data from 1988 to 1998 from the Centers for Medicare and Medicaid. Centers for Medicare & Medicaid, Office of the Actuary, available at <http://www.cms.hhs.gov/statistics/nhe/state-estimates-provider>. Data from 1998 to 2003 was estimated using a regression of the state data from 1988 to 1998 against total national spending.

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medical care. To the extent a state has a high number of traffic fatalities per capita, we expect more malpractice per capita and more malpractice payments per capita.¹⁴⁴

Injury Year (MalYear1_1991-MalYear1_1998). This dummy variable controls for all time-related influences not captured in the other variables discussed above. For example, if malpractice claims had become more prevalent over time, regardless of any tort reforms, this variable would control for this upward trend. Note, however, that this variable does not capture the effect of inflation, as we have separately inflated all payments to 2004 dollars, as discussed below under Section IV.4., Inflation and the Time Value of Money.

State (ST_1 – ST_50). We added 50 dummy variables to indicate the state associated with the doctor who made the payment or on whose behalf the payment was made. In 98% of the cases, this state was where the doctor worked (Work State).¹⁴⁵ If the Work State was not reported in the NPDB Database, then we used the state of residence for the doctor (Home State). If the Home State was not reported we used the state of medical licensure (License State).¹⁴⁶ The State was then used to determine the relevant tort reform environment in which the payment was made. The fifty dummy variables control for interstate differences not captured in the tort reform variables or other state-based control variables discussed above.

Practitioner Field (Field_1 – Field_16). The NPDB Database also includes information on the type of practitioner on whose behalf the payment was made.¹⁴⁷ Eighty-nine codes distinguish between doctors, nurses, dentists, pharmacists and other types of health care practitioners. We simplified these codes into fourteen consolidated codes, and a flag for Residents and Interns, and a flag for Assistants and Technicians, as detailed in the Appendix B. We used these revised fields in some specifications to control for different risk levels associated with each practitioner and the differing nature of each profession.

¹⁴⁴ Annual data from 1990 to 2002 were obtained from the National Highway Traffic Safety Administration of the U.S. Department of Transportation. Available at <http://www.nhtsa.dot.gov/STSI/>. Data covering 1988 and 1989 were estimated using a regression of known state data from 1980 to 2002 onto national death figures. 2003 was estimated in a similar manner, but using the time series as the independent variable instead of the national death figures because neither national nor state data was available for that year.

¹⁴⁵ NPDB Database, “WORKSTATE” field.

¹⁴⁶ This was the recommendation of Dr Oshel from the NPDB, email from **/**/**.

¹⁴⁷ NPDB Database, LICNFELD field

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Practitioner Specialty (Spec_1 – Spec_10). While the NPDB Database does not explicitly indicate the specialty field of the doctor (i.e. – , obstetrician, anesthesiologist, etc.), the malpractice codes and practitioner fields discussed above implicitly relate to certain specialty fields.