# How Tort Reform Affects Insurance Markets

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March 1, 2010

**ABSTRACT**

A critique of tort reform is that promised reductions in insurance rates do not follow reform enactment. Often, however, a number of reforms are subsequently declared unconstitutional or repealed legislatively. Accordingly, we investigate the duration of tort reforms enacted between 1985 and 2005. We then use the estimated survival probability of tort reform to examine the impact of tort reform on state liability insurance markets. Unlike previous studies which rely upon a binary measure of tort reform—whether a state has a reform or not—we find that tort reforms reduce premiums. Our results suggest that examining the effect of a current law without accounting for its expected future treatment, the norm in the literature, may produce misleading results.

**JEL Classification**: K13, K4, H7, G22
**Keywords**: Tort Reform, State Government, Insurance

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We are grateful for helpful comments and suggestions from Ronen Avraham, Patricia Born, Martin Boyer, Leemore Dafny, Jon Garfinkel, Carol Kane, Jonathan Klick, Lars Powell, Joan Schmit, Martin Zelder, and seminar participants at the 2008 Searle Symposium on Insurance Market and Regulation at Northwestern University School of Law, Florida State University, and the 2007 American Risk and Insurance Association. We also thank Peter Berg, Fanny Liu, and Mark Kloet for research assistance.

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# How Tort Reform Affects Insurance Markets

> Tort reform has failed and will fail again to reduce rates, let alone having terrible consequences for many innocent people. [Joanne Doroshow, President, Center for Justice and Democracy][1]

## 1. Introduction

Tort reform is the subject of a large amount of academic research. This research commenced after the first liability crisis in the 1970s and continued through the liability crises of the 1980s and the early 2000s. Tort reform research focuses on its effect on insurance markets (Viscusi, et al. 1993), the practice of defensive medicine (Currie and MacLeod 2008), accidental deaths (Rubin and Shepherd 2007), the decision to file a claim (Browne and Puelz 1999), productivity and employment (Kessler and McClellan 2002), and damage payments (Avraham 2007b).[2] Studies have not always come to uniform conclusions about how tort reform affects markets. This is likely due, in part, to mundane issues like the time period under analysis or how a particular tort reform is defined. However, in this paper we address a more important issue: the expected duration of a reform.

Previous studies quantify tort reform using a simple dichotomous variable which takes on a value of one for all the years affected by the reform and a value of zero for all non-reform years. The implication is the mere existence of a tort reform law is what matters in determining its impact on markets. However, the impact of a law depends on both its current treatment and expectations about its future status. With tort reform (and many other laws) there is significant uncertainty about its impact due to a potential judicial challenge or a legislative repeal. Ohio, for

[1] Press Release, http://www.commondreams.org/news2002/0618-06.htm.

[2] There are also many additional papers examining tort reform's impact on insurance markets (Viscusi 1990 and 1993; Zuckerman, Bovberg, and Sloan 1990; Blackmon and Zeckhauser 1991; Barker 1992; Born and Viscusi 1994 and 1998; Viscusi and Born 1995a, 1995b, and 2005; Born 2001; Born, Viscusi, and Baker 2009; and Donohue and Ho 2007), the practice of defensive medicine (Kessler and McClellan 1996; Kessler, Sage, and Becker 2005; Klick and Stratmann 2005 and 2007; and Matsa 2007), the decision to file a claim (Lee, Browne, and Schmit 1994; Schmit, 1997; and Browne and Schmit 2008), productivity and employment (Campbell, Kessler, Shepherd and Klevorick 1998); and damage payments (Yoon 2001).

example, has been a leader in the undoing of tort reform as its Supreme Court has struck down fifteen of the eighteen civil justice reforms enacted between 1983 and 2009.[3] Across the United States, 40 of the 148 tort reforms enacted between 1985 and 2005 were struck down or repealed. Given that the risk of the nullification of a reform is potentially significant, it is rational for market participants to use a forward looking approach in their response to the law.

In general, the law governing a tort claim is the law in force at the time the tort occurred. Thus, if agents forecast a new law limiting liability will be enacted, then there is no reason for them to change their behavior until the law actually passes. In contrast, if the reform in effect at the time a tort occurs is later ruled unconstitutional, then agents will not be subject to the reform. For example, if an insurer forecasts that the state's judiciary will declare a law unconstitutional, then it may be reluctant to change its behavior since any cases occurring from torts committed today may be adjudicated when the reform is no longer binding. Not only are future cases not subject to the *ex post* unconstitutional reform but also all pending cases. In short, expectations about the future treatment of the law matters.

One objective of liability reducing tort reforms is to decrease insurance losses with the intent of decreasing the premiums charged for insurance coverage.[4] A major complaint alleged about tort reform, however, is that the evidence on insurance premium declines after reform is weak. In fact, consumer advocates assert that tort reform is an "insidious public relations scam" that enriches insurers at the expense of consumers (Doroshow and Hunter 1999 and 2002).[5] Since the

[3] See American Tort Reform Association (http://www.atra.org/states/index.php?state=OH&display=bychallenged)..

[4] While many other studies of tort reform have a different focus, we direct our research on the insurance market since a substantial prior literature exists and many of the other areas examined (physician supply, defensive medicine, or employment) are arguably second order effects of changes in the supply and demand of liability insurance.

[5] Americans for Insurance Reform (AIR) have a listing of quotes (and their citations) of insurance executives and tort reform proponents suggesting that insurance rates would fall (see for example http://www.insurance-reform.org/pr/Quotes.pdf). AIR then claims either the lack of price reductions or the request for price increases after tort reform.

purpose of liability reforms is to reduce the size and frequency of damage awards, a change in insurance losses should be the first indicators of the reforms' effects. Consistent with this argument, the literature documents an inverse relationship between insurance losses and indicators of the presence of tort reform (Viscusi, et al. 1993; Born and Viscusi 1994 and 1998; Viscusi and Born 1995a and 2005; Brown and Puelz 1999; and Browne and Schmit 2008).[6] A number of these studies also show that liability reforms improve insurer profitability as reflected by lower loss ratios (the ratio of losses to premiums, which is an *ex post* measure of the inverse price of insurance per dollar of losses paid). In contrast, the literature generally documents a weak relationship between premiums and tort reform (Lee, Brown, and Schmidt 1994; Born and Viscusi 1994 and 1998; Viscusi and Born 1995a, 1995b, and 2005). A few studies find that premiums fall, while a majority find reforms do not significantly change premiums.

Our hypothesis is that relative to losses, premiums will not be as responsive to tort reform. While losses may initially be reduced by tort reform, premiums may not fall if insurers do not expect the reform to withstand judicial scrutiny. Insurers set premiums based on expectations regarding future losses and part of appropriately estimating future losses means accounting for the legal environment within a state, including forecasting whether tort reforms will be present. Insurers may not change their pricing, underwriting, or loss reserving practices in the face of a reform which is likely to fail judicial review.

A recent paper documents insurers' uncertainty with tort reform. Born, Viscusi, and Baker (2009) discover that insurers' developed losses, the actual court and settlement outcomes, are below their initially reported incurred losses, which reflect the insurers' perception of how claims will be paid. They conclude that the long-run effects of tort reforms are greater than

[6] An exception is Black *et al*. (2005) which finds that insurance claiming behavior has not changed over time even in the presence of tort reforms.

insurers' initial expectations. Another interpretation of their results is that because of uncertainty about the real impact of the reform due to potential judicial challenge insurers exercise caution when establishing loss reserves and premiums.

Focusing on four of the most common and important tort reforms—caps on noneconomic damages, caps on punitive damages, modifications to joint and several liability (JSL), and collateral source rule reforms (CSR)—we examine the effect of tort reform on insurance markets by explicitly accounting for the likelihood a reform is repealed legislatively or declared unconstitutional. We do so using a two-stage analysis. In the first stage, we estimate the probability that tort reforms enacted between 1985 and 2005 will be repealed or determined unconstitutional. In the second stage, we examine whether tort reform impacts insured liability losses and premiums and insurer profitability using state aggregates of insurer-level data from publicly available, annual accounting reports for 1985 to 2005. We account for tort reform using: (1) the traditional binary tort reform indicators used in the literature; and (2) the estimated survival probability of tort reform from the first stage, which accounts for the future expectations of the reform. The importance of using the survival probability is that it acknowledges that even though a reform is currently in place, it is not necessarily true that it always will be.[7] Our main tests in the second stage use the typical specifications found in the tort reform and insurance literature (Viscusi et al. 1993; Born and Viscusi 1994 and 1998; Viscusi and Born 1995a, 1995b, and 2000). We also investigate the robustness of the results using fixed effects and dynamic panel models.

The first stage analysis reveals that JSL reforms are the reforms most likely to pass judicial review, while noneconomic damage reforms are the least likely. We also find that state income,

[7] Currie and MacLeod (2008) recognize the impact that the repeal of a reform may have on doctors. They examine the leads of tort reforms turning "off" (a change from 1 to 0 for their tort reform indicator) and find that they do have an effect, while leads of tort reforms turning "on" do not.

citizen ideology, and the style of judicial selection significantly influence the likelihood tort reform is determined unconstitutional. Our second stage analysis of insurance markets suggests that investigating the effect of the likelihood of a reform surviving judicial review provides different results than previous analyses which merely test whether a state has a reform. When a tort reform indicator is used, most reforms do not have a significant impact on premiums, which is consistent with the consumer advocates' conjecture and much of the prior literature (Born and Viscusi 1994; Viscusi and Born 1995a, and 2005). In contrast, when the likelihood that a reform will survive judicial review is used as the measure of tort reform, tort reform is significant and inversely related to premiums. This is consistent with our major hypothesis that rational insurers will adjust premiums only to the extent they believe reform will be upheld. In short, we find that expectations about tort reforms matter. A broader implication of our study is the finding that a dichotomous law indicator variable, the norm studies of the impact of a law on some outcome (for example, the deterrent effect of capital punishment), may produce misleading results.

The paper is organized as follows. Section 2 presents the first stage of the analysis, the estimation of the survival probability of tort reform. Section 3 presents the second stage of the analysis, the examination of how the survival probability of reform influences state liability insurance markets. Section 4 concludes.

## 2. Estimating the Survival Probability of Tort Reform

This section discusses the estimation of the probability a tort reform will survive judicial review. We first describe the data and then discuss the estimation of the survival probability of reform.

### *A. Data*

Our main variables of interest are tort reforms. The major tort reforms we examine are changes to the collateral source rule (CSR) and joint and several liability (JSL) and caps on punitive and noneconomic damages. We obtain a listing of all enacted and repealed tort reforms by state in the years 1985 to 2005 from the American Tort Reform Association (ATRA). Since ATRA sometimes misses laws that were overturned, we cross-reference our data with the dataset constructed by Avraham (2007a) and also with the American Association for Justice. Any discrepancies were clarified by the authors with the help of Westlaw and lawyers.[8]

Reforms to the CSR focus on offset provisions for collateral damages. Plaintiffs often receive compensation from a variety of sources. CSR reforms generally try to limit double indemnification through any type of insurance payment (a collateral source) and a tort award. Thus tort damages in states that reform their CSR are generally reduced by the amount of any other insurance proceeds paid to the victim from government insurance programs, workers compensation, life insurance, disability insurance, health insurance, or personal auto insurance.

The modification of JSL has to do with the assignment of responsibilities between two associated parties that may be jointly liable for a tort. An example is the liability of a hospital for the actions of a physician operating at the hospital. The notion is the parties jointly are in control and that they are in the best position to mitigate harm. Under the old rule, a jury could find both parties jointly responsible and, if one party did not have the resources to pay the tort award the other party would still be responsible for the total amount, no matter how small their contribution may be. The modification usually limits the ability of the jury to find a party jointly liable merely because it has a deep pocket. A reform would generally require each party to pay according to its responsibility for the harm, rather than be responsible for the entire amount.

[8] In the interest of full disclosure, the lawyers were not compensated because they are our spouses.

Punitive damages are meant to punish defendants for intentional and malicious conduct and to deter future conduct. The idea behind punitive damage reform is to limit the jury's discretion in awarding punitive damages. Reform is implemented either through a specific numeral cap (say $500,000), restricting the upper limit of punitive damage awards based on economic damages, and/or by limiting the situations under which punitive damages can be awarded.[9]

The last major tort reform we study is the restriction on noneconomic damages. These damages include compensation for nonpecuniary losses, including pain and suffering, loss of consortium, emotional distress, and bereavement. A criticism is that due to their intangibility it is difficult to value these damages and that there can be substantial differences between jurisdictions within a state as well as between juries in the same court. Further there is no science to guide a jury in making decisions and the variation in awards violates due process because it is not a clear standard (Sunstein, 2007). The purpose of noneconomic damage reforms is to establish numerical guidelines, numerical limits, or specific principles for awarding noneconomic damages.

Table 1 displays the total number of reforms enacted by state and type for the years 1985 to 2005. The table also documents the number of reforms that were determined unconstitutional. Of the 148 reforms that were enacted during our sample period, 27 percent did not pass state constitutional muster. Limitations of noneconomic damages are the most likely tort reform to be determined unconstitutional (35 percent), while modifications to JSL are the least likely (18 percent). The most important conclusion from the table is that a large number of reforms are later repealed and a strict dichotomous variable may misestimate a reforms true impact.

[9] In 2003, the U.S. Supreme Court has held that a ratio of less than 10 to one between punitive damages and compensatory damages would not violate due process. See *State Farm Mutual Automobile Insurance Co. v. Campbell*, 538 US 408 (2003).

To estimate the survival probability of tort reform, we investigate the influence of a state's demographic characteristics on whether a reform is determined unconstitutional. Data on state demographic characteristics comes from a variety of sources. First, we collect information on the business activity within the state. Specifically, we obtain from the Bureau of Economic Analysis the per Capita Real Gross Domestic Product by State (GSP).

Second, we acquire information on the number of lawyers per capita (*Lawyers per Capita*). The American Bar Foundation publishes data in their *Lawyer Statistical Report* on the number of lawyers in a state, but not for every year of our study. We extrapolate the values for the years in which the data are not reported.[10] Another limitation of the lawyer data is that not all lawyers are captured and specific information on the number of trial lawyers is not available. The population of each state is collected from annual *U.S. Statistical Abstracts*.

Third, we acquire information on each state's judicial selection mechanism from the American Judicature Society (2007) and Hanssen (2004). We determine whether a state uses a partisan election (*Partisan Election*), a non-partisan election (*Nonpartisan Election*), an appointment system (*Appoint*), or merit appointment system (*Merit*) to place judges. A partisan election is where the candidates are running based on identification with a political party,[11] while in a non-partisan election the candidates do not identify with a party. With an appointment system, the judge is appointed through a political process (by either the governor and/or the legislature). Instead of running for competitive elections or being appointed, the merit plan mandates unopposed "retention elections"; ballots read simply "Should Judge X be retained in office." The state bar generally has a strong degree of influence in a merit system.

[10] We only have 8 years of reported data (1985, 1988, 1991, 1998, 2003, 2004, 2005 and 2006). Similar to Browne and Schmit (2008), we incorporate estimates for the other years, by using the 8 data points in the following regression model: lawyers per capita = $a + b(\text{year}) + \varepsilon$

[11] Helland and Taborrok (2002) find that partisan elected judges cater to their constituents.

Finally, we obtain information about the politics of a state. Specifically, we use the "revised 1960-2006 citizen ideology series" developed by Berry et al. (1998). The *Citizen Ideology Index* is high (maximum is 100) if the state's representatives to Congress are liberal according to various liberal interest groups ratings and it is low (minimum is 0) if the state's representatives are conservative.[12] The metric also considers the political party in power in a state. *Appendix A* contains details on the construction and sources of each of the variables we use in the paper.

Table 2 reports the means and medians of state and insurance market characteristics stratified by whether tort reform passes constitutional muster. Overall, the null hypothesis is that state characteristics should not be related to a tort reforms constitutionality as judges only rule on the legal merits of the legislation. Consistent with this hypothesis, very few state characteristics are related to a tort reforms eventual repeal. In fact, only state income is statistically significant. On average, tort reforms are less likely to be repealed in states with high income (*Per Capital GDP by State*). This difference, however, does not necessarily imply a casual effect. For example, if states with low income are more inclined to pass tort reforms quickly or with less due diligence, then tort reforms in these states will be more likely to be found unconstitutional, regardless of whether state income has any effect.

### *B. Logistic Regression*

A multivariate logistic regression provides a first look at whether the mean differences in the likelihood of unconstitutionality continue after controlling for other characteristics. We investigate the likelihood using two samples. The first sample includes only the year the tort reform is enacted. The dependent variable in this regression is set equal to one if the tort reform is declared unconstitutional or repealed legislatively any time during the sample period and zero

[12] For example, if the state's representatives had high scores from the liberal interest group Americans for Democratic Action, then this would be attributed to the state's citizenry.

otherwise. In the second sample, to allow the likelihood function for each reform to change over time (a time varying model) we reorganize our dataset into a yearly tort reform dataset. A tort reform enters the dataset in the year it is enacted and stays in the dataset each year that it is in force. The dependent variable is set equal to one the year a reform is declared unconstitutional and zero otherwise. The explanatory variables are allowed to vary over the sample period, adding a temporal dimension that is not captured with the first sample.

Table 3 reports the results of these regressions. A positive coefficient implies a variable is associated with a higher probability of repeal, while a negative coefficient indicates a lower likelihood. We also report estimated marginal effects. The marginal effect for a continuous variable is evaluated at the mean. The marginal effect for a discrete variable is the change in the predicted probability due to a change from 0 to 1.

The results reveal that limitations on noneconomic damages are more likely to be determined unconstitutional. The excluded tort reform is JSL. Thus, relative to modifications of JSL the probability that limitations on noneconomic damages are found unconstitutionality is 16.14 percent higher in the year of enactment and 2.37 percent per annum over the life of the reform. This is noteworthy given that caps on noneconomic damages are found to decrease the probability of filing a claim and the size of claims (Browne and Puelz 1999).

The logistic equation also shows that even after controlling for other factors, tort reforms are less likely to be unconstitutional in states with higher incomes. Tort reforms enacted in states with a relatively more liberal citizenry, and therefore meet the standards of the liberal citizenry, are also more likely to pass constitutional muster. Recall that the lowest score for the citizen ideology index is 0 (conservative) and the highest is 100 (liberal). The mean (median) citizen ideology score in the sample of states that enact tort reforms is 46.5 (46.9). Thus, even though

tort reform is, on average, enacted in conservative states, we find that reforms passed by *relatively* more liberal legislatures are more likely to pass constitutional muster than reforms enacted by conservative legislatures.[13]

The form of judicial selection also influences the likelihood of unconstitutionality. Judicial elections, both partisan and non-partisan, significantly increase the likelihood of a reform's unconstitutionality relative to a merit based system (the excluded judicial selection category). This may suggest either that citizens who elect judges do not like tort reforms or that there is an influence on judicial elections not captured by our model, such as campaign contributions or other interest group pressures.

We do not observe lawyers to have a significant influence on whether a state declares a reform unconstitutional. However, one must remember that our variable is the total number of lawyers and not just the number of trial lawyers, who arguably have a bigger stake in the judicial outcome of tort reform legislation. States in the Mid-Atlantic, East North Central, South Atlantic, and Pacific regions are more likely to have tort reforms repealed. The excluded states are those located in the South Central U.S, which is comprised of two geographical divisions—East South Central and West South Central.[14]

The logistic regressions suggest that the type of reform and some state characteristics are related to the probability of a reform is determined unconstitutional. The timing of the reform's enactment may also influence whether it passes state constitutional muster or not. The comparison of means (see Table 2) reveals that reforms enacted earlier in the sample period are more likely to be found unconstitutional. Thus, it is possible that reforms enacted later in the

[13] Without evaluating the specific details of each case it is hard to say exactly why this is. It may be that conservative movements are critical to the passage of a reform, but a more conservative citizenry may also make it more likely that a reform will "overreach" and violate the state constitution.

[14] See Appendix A for the definition of each geographical division (source: U.S. Census Bureau).

sample period may eventually be repealed in the coming years. Standard regression techniques do not account for this censoring and therefore may be biased. In the next section, we account for right-censoring by applying hazard analysis.

### *C. Hazard Model Estimates*

Twenty-seven percent of tort reforms are repealed or declared unconstitutional during the sample period. The average (median) spell length for these reforms is 4.75 (5.00) years compared to 12.38 (12.00) years for all other reforms. To account for the right censoring in the duration of tort reform and to control for the effects of state characteristics we estimate a hazard model. A hazard at time *t* gauges the probability that a spell will terminate in the next moment, conditioned on it surviving from time 0 to *t.* The hazard function for tort reform *i* is specified as $h(t, X_i) = h_0(t) * \exp(X_i\beta)$ where $X_i$ is a vector of explanatory variables and $\beta$ is a vector of coefficients. The term $\exp(X_i\beta)$ shifts the baseline hazard function. A commonly used hazard model is the Weibull, which imposes a baseline hazard of $h_o(t) = \alpha t^{\alpha-1}$.[15] The parameters of the baseline hazard are jointly estimated with the coefficients on the regressors. A positive coefficient signifies that a covariate increases the hazard and reduces the length of the spell.

The preferred hazard model is the Cox proportional model since it requires no assumptions be made about the baseline hazard and thereby avoids any bias which may be created from an arbitrary selection of a functional form for the duration distribution (Cox 1972). A drawback is that calculating expected durations with the Cox model is complex. However, computing survival probabilities when a baseline hazard is specified is straightforward. Even though the Cox model is preferable to imposing an arbitrary baseline hazard, we want to compute the expected survival probability of tort reform so that we can use them to examine the impact of tort

[15] An $\alpha$ less than one implies a decreasing hazard, an $\alpha$ greater than one indicates an increasing hazard, and an $\alpha$ equal to one implies a constant hazard.

reform on insurance markets. We, therefore, present estimates from Cox proportional hazard models and compare them to Weibull estimates. After demonstrating modest differences between the Cox and Weibull estimates, we use the Weibull to compute the survival probability of tort reform. We estimate both models with robust standard errors and clustering by state.

To allow the hazard function for each reform to change over time (a time varying hazard model) we use the same dataset as in the second logistic regression described above. In particular, a tort reform enters the dataset in the year it is enacted and stays in the dataset each year that it is in force. The explanatory variables are allowed to vary over the sample period, adding a temporal dimension.

Table 4 shows the estimates of the Cox proportional and Weibull hazard models with time-varying covariates. Similar to the logistic regression estimates, the hazard results also indicate the likelihood that noneconomic damage reforms are judged unconstitutional is higher than for other types of tort reform (*JSL* reform is the excluded category). The results of the Weibull proportional hazard model also show that states with high income (*Per Capital Real GDP by State*) are associated with a lower tort reform hazard rate.

The results of the Cox proportional hazard model reveal that tort reforms enacted in states with a relatively more liberal citizenry, and therefore meet the standards of the liberal citizenry, are less likely to violate the state constitution. The style of judicial selection also influences the hazard rate. Relative to merit selection (the excluded judicial selection category), judicial elections, both partisan and non-partisan, significantly increase the likelihood of a reform's unconstitutionality. The geographical region of the state also influences the hazard rate. The estimates reveal that tort reforms in states in the East North Central, South Atlantic, and Pacific regions of the United States are more likely to be judged unconstitutional.

Again our goal is to estimate a tort reform's probability of repeal. Table 4 reveals the coefficients of the Weibull to be reasonably similar to the coefficients of the Cox models in terms of magnitude and significance. In addition, the coefficients are identical in sign. The similarity of the Cox and Weibull results indicates that any bias developing from a misspecified baseline hazard or from unobserved heterogeneity is not material for our sample. Therefore we compute repeal (and survival) probabilities with the Weibull model. This procedure provides unique probability estimates for each type of tort reform and for each state and year that the reform is in place.

The mean (median) estimated one-year probability of unconstitutionality is highest for noneconomic damage reforms, 5.1 (3.6) percent, and lowest for JSL reforms, 1.54 (0.95) percent. Thus, the average (median) noneconomic damage reform is 3.3 (3.8) times more likely to fail than JSL reforms. The one-year probability of unconstitutionality for CSR and punitive damage reforms is 3.0 (1.8) and 2.4 (1.3) percent, respectively.

For the tort reforms not determined unconstitutional during our sample period, the mean (median) estimated one-year probability of unconstitutionality is 2.0 (1.16) percent. In contrast, it is 6.0 (5.3) percent for the unconstitutional reforms. Hence, the average (median) probability of unconstitutionality is 3.0 (4.6) times larger for the reforms that fail than for those that do not. Overall, the model appears to adequately distinguish constitutional reforms from unconstitutional reforms.

To assess the robustness of our results, we also use a number of other probability estimates in our second stage analysis of how tort reform impacts insurance markets. For instance, we also use the one-year probabilities from the time-varying logistic regression (Table 3, Model 2). In addition, since it could be argued that tort reforms with early strike downs are reforms whose

demise was likely anticipated, we create a number of variables that account for this possibility. We recode the date of unconstitutionality for reforms that are struck down early, which we define either as reforms struck down within two or within three years. Ten reforms are struck down within two years of enactment and seven more within three years.[16] We then re-estimate both the Weibull and the time-varying logistic regressions using these recoded dates of unconstitutionality. In addition, we also construct indices from the estimated probabilities such that if the estimated probability of unconstitutionality is in the bottom quintile (quintile 1; the reforms with the lowest probability of being struck down) then the index is set equal to 1. If the estimated probability is in the top quintile (quintile 5; the reforms with the highest probability of being struck down) then the index is set equal to 0.2. The index is set equal to 0.8, 0.6, and 0.4 for quintile two, three, and four, respectively. We create similar indices using deciles and quartiles. The indices are reform specific, i.e. there is a separate index for CSR, JSL, noneconomic damage, and punitive damage reforms. All these variables, the probability estimates and the indices, provide relatively similar results to those reported. Thus, we conclude that the major driver of our findings in the second stage is not how the probability of unconstitutionality is estimated or constructed, but rather that it is accounted for in regressions investigating tort reforms' affect on insurance markets.

## 3. Insurance Market Response to Tort Reform

This section presents the examination of how tort reform influences state liability insurance markets. We first describe the data and then we investigate the impact of tort reform on insured liability losses and premiums and insurer profitability.

[16] As mentioned above, of the 148 reforms in our sample period 40 are declared unconstitutional. Thus, 23 reforms are declared unconstitutional more than three years after enactment. The mean (median) time to unconstitutionality is 4.75 (5.0) years. The maximum is 12.

### *A. Data*

Similar to prior studies (e.g., Viscusi 1990; Blackmon and Zeckhauser 1991; Viscusi, et al. 1993), we examine the effect of tort reform on insurance markets at the state level. The data set in this part of the paper includes one observation for each state and year over the 1985 to 2005 period. Publicly available, annual accounting reports, which all insurers must compile with state regulators, provide the data from which aggregated state insured losses and premiums are computed. The insured losses and premiums of each U.S. property-liability insurer are collected for each year (1985 to 2005) from the National Association of Insurance Commissioners (NAIC) Property-Liability Annual Statement Database and summed by state. Unlike prior studies (for example, Viscusi and Born 1995a, 1995b, and 2000) which rely upon the loss ratio, insured losses to premiums, we examine insurance market profitability using the economic loss ratio (Winter 1994). The economic loss ratio, the ratio of the present value of discounted insured losses to premiums, is more meaningful than the traditional loss ratio because premiums reflect the discounting of losses in a competitive market.[17] Thus, it corrects the usual loss ratio to reflect present value concepts in both the numerator and denominator. The present value factors are computed by estimating the loss payout tail for each of the liability lines of insurance and for each year. The payout tail proportions are estimated using the method prescribed by the Internal Revenue Service for computing loss present values for tax purposes (Cummins 1990) and are discounted by the risk-free rate. The risk-free rate is estimated from the U.S. Treasury spot-rate yield curves for each year of the sample period provided in the Federal Reserve Bank of St. Louis' Federal Reserve Economic Data (FRED) database.

[17] The economic loss ratio is typically measured as the ratio of the present value of discounted losses to premiums net of expenses. However, expense data is not available by state for the years 1986 to 1990. In robustness regressions, we use the economic loss ratio net of expenses for the years 1991 to 2005. The results are similar to those reported.

*B. Empirical Estimates*

Our econometric models investigate whether tort reform influences state variations in economic loss ratios, losses, and premiums. To examine this relationship, we follow the prior literature and estimate the following models (Viscusi et al. 1993; Born and Viscusi 1994 and 1998; Viscusi and Born 1995a, 1995b, and 2000):

$$Log\ Econ\ Loss\ Ratio_{it} = a_1 + b_1 Log\ Econ\ Loss\ Ratio_{it\text{-}1} + b_2 TORT_{jit} + e_{it} \quad (1)$$

$$Log\ Losses_{it} = a_1 + b_1 Log\ Losses_{it\text{-}1} + \psi\, Log\ Premiums_{it} + b_2 TORT_{jit} + e_{it} \quad (2)$$

$$Log\ Premiums_{it} = a_1 + b_1 Log\ Premiums_{it\text{-}1} + b_2 TORT_{jit} + e_{it} \quad (3)$$

*TORT* is a vector of either (a) tort reform indicator variables or (b) estimated survival probabilities for reform type *j*, year *t*, and state *i*.[18] The tort reform indicators are collected from the Database of State Tort Law Reforms (Avraham 2007a), which is considered the most current and comprehensive state-level tort reform dataset. Section 2.C details how we estimate the probability of unconstitutionality; the estimated survival probability of a tort reform is one minus the probability of unconstitutionality. All tort reform variables begin at the start of the calendar year in which the reform is first effective.[19]

The use of a logarithmic dependent variable reduces the impact that large outliers may have on the estimation. Since an objective of tort reform is to modify the liability environment, the inclusion of the lagged dependent variable as an independent variable allows us to interpret the

[18] There are many possible variations of the vector *TORT*. We estimate our base model including all four tort reforms together. Since many of the reforms are enacted at the same time, some previous work includes each tort reform separately to avoid multicollinearity concerns. The greatest correlation is observed between CSR reforms and noneconomic damage caps for the indicator variables (0.216) and between CSR and JSL reforms for the survival probabilities (0.291). Correlations of these magnitudes do not warrant concern that our estimates suffer from multicollinearity. Nevertheless, we also estimate the models including each tort reform separately. Our results do not change. Moreover, we do not observe a perceptible drop in the standard errors between the models that include all four reforms together and those that include the reforms separately.

[19] We repeated all the analyses substituting a value of 0.5 for 1.0 for a tort reform indicator if the tort reform began during the year. The results were qualitatively similar to those reported in the paper.

shift in the overall level of the dependent variable as a function of liability reforms.[20] The lagged value also captures the risks associated with writing insurance in a particular state, including its liability environment. We estimate the models with robust standard errors and state clustering. Our tort reform survival probabilities are predicted values, so we also estimate pair-wise bias-corrected bootstrap standard errors with state clusters drawn with replacement (1000 replications) to ameliorate any biases due to a generated regressor problem (Pagan, 1984).

Once a reform has been upheld, its certainty is set. Accordingly, we recode the survival probability as one beginning the year after a reform is upheld. Seventeen reforms meet this criterion. Moreover, in November 2003, Texas voters approved a proposition which put a cap on noneconomic damages into the state's constitution. Even though our estimated survival probability for this reform is quite high (0.999 for 2004 and 2005), we code this reform as one since it could be argued that there is no chance it will not "stick".

Also, in robustness tests, we recode the survival probability of reforms with early "strike downs". The argument is that if reforms are struck down early (which we define both as both within two years or within three years), then they had little time to work. Moreover, their demise was likely widely anticipated. Thus, allowing these reforms to have a survival probability greater than zero is not appropriate. Accordingly, we recode the survival probability of these reforms as zero. Ten reforms are struck down within two years of enactment and seven more within three years. These robustness variables provide qualitatively similar results to those reported.

The results of the ordinary least squares (OLS) regressions are located in Table 5. Panel A displays the results for the tort reform indicators. The results show that noneconomic damage caps reduce aggregate insured liability losses but not aggregate premiums. Noneconomic damage

[20] Equation (2) includes the log of premiums in addition to the lagged value of losses. The rationale is that states with a high premium volume are expected to incur higher losses (Born and Viscusi 1994). Losses are incurred after premiums are written; therefore, there is no problem with simultaneity.

caps also lower the economic loss ratio, which is an ex post measure of the inverse price of insurance per dollar of losses paid. Thus, the results suggest that noneconomic damage reforms increase the per unit price of insurance and thereby the profitability of insurance firms. CSR reforms are also negatively related to the economic loss ratio. In contrast, JSL reforms are positively associated with the economic loss ratio. The mechanism for the higher loss ratio is lower aggregate premiums. With tort reform indicator variables, JSL reforms are the only type of reform observed to significantly impact premiums. Interestingly, JSL reforms are also the tort reform that is most likely to stick, perhaps signifying that expectations about the reform matter.

In general, when we account for tort reform using an indicator variable, we find evidence that substantiates the consumer advocates critique of tort reform – that it does not change aggregate premiums. Even though we examine a longer time period, which makes direct comparisons difficult, our evidence is also fairly consistent with prior studies (Born and Viscusi 1994; Viscusi and Born 1995a, and 2005). Overall, tort reform indicators provide little evidence that reforms lower insurance premiums, especially for reforms with a higher probability of being found unconstitutional. It is important to note that the implicit assumption underlying indicator variables is that the mere existence of a tort reform law is what matters in determining its impact. In reality the impact of a law depends on both its current treatment and expectations about its future treatment. Because of the uncertainty about the real impact of the reform due to potential judicial challenge, it is rational for insurers to exercise caution with respect to setting premiums.

In contrast, the survival probabilities treat reforms idiosyncratically. In place of the indicator variables, Table 6 Panel B uses the survival probability of tort reform to account for the anticipated treatment of the law. The survival probability results for JSL reforms, the reforms which are most likely to stick, are qualitatively similar to those observed using an indicator

variable. JSL reforms lower premiums and increase (decrease) the economic loss ratio (the price of insurance). The results for JSL reforms suggest that indicator variables may be reasonable for reforms that are likely to survive.

The survival probability results for the other types of tort reform, however, reveal that a forward looking approach to the treatment of the reform yields different results for tort reforms that are less likely to survive. Using indicator variables, caps on punitive damages do not impact insurance markets. However, accounting for the survival probability of the reform indicates something different. Punitive damages caps increase losses, a common measure of the quantity of insurance (Cummins and Weiss 2001), without similarly increasing premiums. Punitive damage caps also increase the economic loss ratio (the inverse price of insurance). Thus, punitive damage reforms reduce the per unit price of insurance, suggesting that these reforms have their intended impact. CSR reforms reduce insured liability losses and premiums. A higher survival probability for limitations on noneconomic damages is associated with lower aggregate insured losses and premiums. The magnitude of the shift is greater for losses than for premiums resulting in lower economic loss ratios, greater insurer profitability.

Overall, the results provide persuasive evidence of our main hypothesis that insurers set premiums based on expectations about the legal environment within a state. Using tort reform indicator variables, only JSL reform, the tort reform least likely to be determined unconstitutional, is statistically significantly related to premiums. In contrast, when we account for the expected future treatment of tort reform using the survival probability of a reform, three tort reforms – JSL, CSR, and noneconomic damage reforms – are significantly related to premiums. These findings suggest insurers react to the expected future treatment of tort reform.

Moreover, these results indicate that the prospect that liability reforms will be judicially challenged, and perhaps found unconstitutional, weakens the effect of the reform.

To determine the robustness of our results, we also conduct a number of additional analyses.[21] In particular, we explore whether our results are influenced by unobservable differences across states.[22] For this purpose, we re-estimate equations (1) to (3) using state fixed effects. The combination of fixed effects and a lagged dependent variable, however, can severely bias coefficient estimates (Wooldridge (2002); Baltagi (2005)). This bias decreases with panel length (Nickell (1981)), but can be quite large even for panels with 30 years of data (Judson and Owen (1999)). We address this bias in two ways. First, we analyze the growth in the dependent variable, $\Delta Y_t = \log Y_t - \log Y_{t-1}$. This allows us to examine the shift in the overall level of the dependent variable without the use of a lagged dependent variable. Second, we use dynamic panel models. In particular, we use the "corrected least squares dummy variable" (LSDVC) approach (Bruno (2005)). The approach calculates a consistent estimate of the bias that results from using fixed effects and a lagged dependent variable and explicitly removes it from the estimated coefficients.[23]

[21] In addition to the robustness checks reported in the paper, we also conduct a number of tests for which the results are not reported. Our baseline specifications (equations (1) thru (3)) assume that the impact of tort reform is to modify the overall level of the dependent variable. However, it may be that tort reform changes the structure of the insurance market as well as the level. To examine this possibility, we let the coefficient of the lagged dependent variable vary with tort reform regime by interacting the lagged dependent variable with our reform variables. This lets the lagged dependent variable have a different effect in the post-reform period accounting for the possibility that the past performance of the insurance market may not have the same impact in the future if the state reforms the tort liability system. We also estimate our models with Newey-West Heteroskedastic-Autocorrelation Consistent (HAC) standard errors. Our results are robust to these additional specifications.

[22] We also investigate whether the results are impacted by observable differences between states. To examine this possibility, we re-estimate equations (1) to (3) including the following variables: *Lawyers per Capita*, *Per Capita GDP by State*, insurance gross state product (*Insurance GSP*; the insurance industry's share of private industries contribution to GSP), indicators for judicial selection mechanism (*Appoint*, *Nonpartisan Election*, and *Partisan Election*), and *Citizen Ideology Index*. The reported results are robust to the inclusion of these variables.

[23] To estimate the bias, LSDVC requires an initial matrix be specified. For the starting matrix, we use the Blundell and Bond (1998) GMM estimator. In unreported regressions, we use the Arellano-Bond estimator for the starting matrix. The results are qualitatively similar to those reported. LSDVC corrects the panel length bias up to order $NT^2$.

The results of these fixed effects regressions are shown in Tables 6 and 7. The growth regressions are in Table 6 and the dynamic panel models are in Table 7. Panel A displays the results using tort reform indicators and Panel B the results using the survival probability of tort reform. Overall, the fixed effects results indicate that our results are fairly robust. In general, the tort reform indicators suggest that tort reforms do not impact premiums. The exception is JSL reforms, the reforms that are most likely to stick. This finding suggests that insurers will alter their pricing if they expect the reform to withstand judicial scrutiny. When we account for expectations about the future treatment of tort reforms using our estimates of their survival probability, the fixed effects results generally indicate that tort reform lowers premiums. This is consistent with our major hypothesis that rational insurers will adjust premiums only to the extent they believe a reform will be upheld.

## 4. Conclusion

In previous studies which investigate the impact of tort reform on insurance markets, reforms are recorded using a binary variable. The use of an indicator variable (set equal to 1 for all enacted reforms and zero otherwise), suggests every enacted reform has a 100 percent likelihood of surviving. Numerous tort reforms, however, are later repealed. In contrast to the previous research, we take this fact into account and estimate the likelihood of a tort reform surviving and then assess the impact of the survival probability of tort reform on insurance loss ratios, losses, and premiums. This allows us to more precisely measure the economic value of tort reform. Overall, our framework brings us closer to the true economic impact of tort reform on agent decision making. Our results suggest the prospect that liability reforms will be judicially challenged, and perhaps found unconstitutional, weakens the effect of the reform. We generally find that premiums are inversely related to the likelihood that a reform will survive judicial

review. This result is not found with tort reform indicator variables. Our study demonstrates that the use of tort indicator variables may misestimate the impact of tort reform. A larger implication of our findings is that it is plausible that the use of binary indicators for the presence of other types of laws may also lead to inappropriate estimates.

Controlling for a reform's survival is an important contribution to the tort reform literature. Our study, however, still has a number of limitations. First, our measures of tort reform do not capture the continuous quantitative impact of the specific reforms. Tort reforms differ in "strength". For example, Colorado limits the award of noneconomic damages to $250,000, whereas the state of Washington limits the award of noneconomic damages just for bodily injury to 0.43% times the average annual wage times the plaintiff's life expectancy. These two reforms will obviously have differing impacts on insurer loss costs and therefore premiums. We, however, categorize all reforms only by their type and their likelihood of their survival. The true impact of a reform should also vary depending on its "strictness". Nevertheless, the nature of the reforms does not lend itself to these types of quantitative metrics without making significant subjective decisions that may influence the results. Second, tort reforms often comprise multiple components as each state's legislature crafts its own idiosyncratic reform. We, however, only recognize specific qualities that are identifiable for all states. Third, we do not evaluate the effect of reforms enacted prior to 1985. If a liability reform enacted in response to the 1970's liability crisis has persistent effects on the insurance market, then our study will underestimate the impact of tort reform. Finally, our research does not address the social desirability of tort reform. While we demonstrate some of the benefits of liability reform, a reduction in the cost of liability to insurers and policyholders, we do not evaluate the costs, such as decreased compensation for injured parties or potentially reduced safety incentives.

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**APPENDIX A**

Variable Definitions and Sources (1985 to 2005)

| Variable: | Definition: | Source: |
|---|---|---|
| *Tort Reform:* | | |
| CSR | An indicator variable set equal to 1 if the state enacted a reform to the collateral source rule, and 0 otherwise. | Tort Reform: American Tort Reform Association (www.atra.org) and Avraham (2007a) |
| JSL | An indicator variable set equal to 1 if the state modified its joint and several liability law, and 0 otherwise. | |
| Punitive | An indicator variable set equal to 1 if the state instituted a cap on the size of punitive damage awards, and 0 otherwise. | |
| Nonecon | An indicator variable set equal to 1 if the state enacted a cap on the size of noneconomic damages awards, and 0 otherwise. | |
| *State Characteristics:* | | |
| Per Capital Real GDP by State | Total gross state product (GSP) divided by the total state population. The variable is scaled by 10,000. Gross state product is a measure of value added, calculated from three components: compensation to employees, indirect business tax and nontax liability, and property-type income (including corporate profits, business transfers, rental income, and net interest). | Per Capital Real GDP by State and Insurance GSP: Bureau of Economic Analysis http://www.bea.gov/regional/gsp/ |
| Lawyers per Capita | Ratio of the total number of lawyers in a state to the total population in the state multiplied by 10,000. | Lawyers per Capita: *The Lawyer Statistical Report* , various years |
| Appoint | An indicator variable set equal to 1 if the state's judicial selection mechanism is appointment, and 0 otherwise. | Judicial Selection: Hanssen (2004) and American Judicature Society (http://www.judicialselection.us/) |
| Nonpartisan Election | An indicator variable set equal to 1 if judges in the state are elected in a nonpartisan election, and 0 otherwise. | |
| Partisan Election | An indicator variable set equal to 1 if judges in the state are elected in a partisan election, and 0 otherwise. | |
| Merit | An indicator variable set equal to 1 if the state selects judges based on the merit plan, and 0 otherwise. | |
| Citizen Ideology Index | A measure of the ideology of a state's citizens, 0 is the most conservative and 100 is the most liberal. | Citizen Ideology Index: Berry et al. (1998) |

(*Continued on Next Page* )

**APPENDIX A** - *Continued*
Variable Definitions and Sources (1985 to 2005)

| | | |
|---|---|---|
| *Geographical Divisions* | | |
| Northeast | Maine, New Hampshire, Vermont, Rhode Island, Massachusetts, and Connecticut | Geographcial Divisisions: U.S. Census Bureau |
| Mid-Atlantic | New York, New Jersey, and Pennsylvania | |
| East North Central | Ohio, Indiana, Illinois, Wisconsin, and Michigan | |
| West North Central | Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, and Kansas | |
| South Atlantic | Delaware, Maryland, District of Columbia, Virginia, West Virginia, North Carolina, South Carolina, Georgia, and Florida | |
| East South Central | Kentucky, Tennessee, Alabama, and Mississippi | |
| West South Central | Arkansas, Louisiana, Oklahoma, and Texas | |
| Mountain | Montana, Idaho, Wyoming, Colorado, New Mexico, Arizona, Utah, and Nevada | |
| Pacific | Washington, Oregon, California, Alaska, and Hawaii | |
| *Insurance Market Characteristics:* | | |
| Premiums | Total premiums earned in liability lines of insurance in state *i* . | Premiums and Losses: National Association of Insurance Commissioners (NAIC) Property-Casualty Annual Statements<br><br>Risk-free rates: Estimated from the U.S. Treasury spot-rate yield curves, Federal Reserve Bank of St. Louis |
| Losses | Present value of aggregate losses incurred in liability lines of insurance in state *i* . The present value is the payout tail proportion, i.e., percentage of losses paid in year t (t = 1, 2,…, T), discounted by the risk-free rate. The payout tail proportions were estimated using the method prescribed by the IRS for computing loss present values for tax purposes (Cummins, 1990). | |
| Economic Loss Ratio | *Losses* / *Premiums* | |

**TABLE 1**

Summary of Tort Reforms by State and Type (1985 - 2005)

| | All Reforms | | | CSR | | | JSL | | | Punitive | | | Nonecon | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Total | Unconst. | | Total | Unconst. | | Total | Unconst. | | Total | Unconst. | | Total | Unconst. | |
| STATE | # | # | % | # | # | % | # | # | % | # | # | % | # | # | % |
| AK | 6 | 2 | 33.3 | 1 | 0 | - | 1 | 0 | - | 1 | 1 | 100.0 | 3 | 1 | 33.3 |
| AL | 3 | 2 | 66.7 | 1 | 1 | 100.0 | | | | 2 | 1 | 50.0 | | | |
| AR | 2 | 0 | - | | | | 1 | 0 | - | 1 | 0 | - | | | |
| AZ | 2 | 1 | 50.0 | 1 | 0 | - | 1 | 1 | 100.0 | | | | | | |
| CA | 1 | 1 | 100.0 | | | | 1 | 1 | 100.0 | | | | | | |
| CO | 6 | 1 | 16.7 | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - | 3 | 1 | 33.3 |
| CT | 2 | 1 | 50.0 | 1 | 0 | - | 1 | 1 | 100.0 | | | | | | |
| FL | 8 | 4 | 50.0 | 1 | 1 | 100.0 | 2 | 1 | 50.0 | 2 | 1 | 50.0 | 3 | 1 | 33.3 |
| GA | 3 | 1 | 33.3 | 1 | 1 | 100.0 | 1 | 0 | - | | | | 1 | 0 | - |
| HI | 4 | 0 | - | 1 | 0 | - | 2 | 0 | - | | | | 1 | 0 | - |
| IA | 2 | 0 | - | 1 | 0 | - | 1 | 0 | - | | | | | | |
| ID | 6 | 2 | 33.3 | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - | 3 | 2 | 66.7 |
| IL | 6 | 4 | 66.7 | 1 | 0 | - | 2 | 2 | 100.0 | 1 | 1 | 100.0 | 2 | 1 | 50.0 |
| IN | 3 | 0 | - | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - | | | |
| KS | 4 | 2 | 50.0 | 1 | 1 | 100.0 | | | | 1 | 0 | - | 2 | 1 | 50.0 |
| KY | 3 | 1 | 33.3 | 1 | 1 | 100.0 | 2 | 0 | - | | | | | | |
| LA | 1 | 0 | - | | | | 1 | 0 | - | | | | | | |
| MA | 1 | 0 | - | | | | 1 | 0 | - | | | | | | |
| MD | 2 | 1 | 50.0 | | | | | | | | | | 2 | 1 | 50.0 |
| ME | 1 | 0 | - | 1 | 0 | - | | | | | | | | | |
| MI | 4 | 1 | 25.0 | 1 | 1 | 100.0 | 1 | 0 | - | | | | 2 | 0 | - |
| MN | 4 | 2 | 50.0 | 1 | 1 | 100.0 | 2 | 0 | - | | | | 1 | 1 | 100.0 |
| MO | 5 | 0 | - | 1 | 0 | - | 2 | 0 | - | 1 | 0 | - | 1 | 0 | - |
| MS | 4 | 0 | - | | | | 2 | 0 | - | 1 | 0 | - | 1 | 0 | - |
| MT | 6 | 1 | 16.7 | 1 | 0 | - | 3 | 1 | 33.3 | 1 | 0 | - | 1 | 0 | - |
| NC | 1 | 1 | 100.0 | | | | | | | 1 | 1 | 100.0 | | | |
| ND | 4 | 0 | - | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - |
| NH | 3 | 1 | 33.3 | | | | 1 | 0 | - | 1 | 0 | - | 1 | 1 | 100.0 |
| NJ | 4 | 0 | - | 1 | 0 | - | 2 | 0 | - | 1 | 0 | - | | | |
| NM | 1 | 0 | - | | | | 1 | 0 | - | | | | | | |
| NV | 3 | 0 | - | | | | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - |
| NY | 2 | 0 | - | 1 | 0 | - | 1 | 0 | - | | | | | | |
| OH | 9 | 5 | 55.6 | 3 | 2 | 66.7 | 1 | 1 | 100.0 | 2 | 1 | 50.0 | 3 | 1 | 33.3 |
| OK | 4 | 0 | - | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - | 1 | 0 | - |
| OR | 4 | 1 | 25.0 | 1 | 0 | - | 2 | 0 | - | | | | 1 | 1 | 100.0 |
| PA | 3 | 1 | 33.3 | 1 | 0 | - | 1 | 1 | 100.0 | 1 | 0 | - | | | |
| SC | 2 | 0 | - | | | | 1 | 0 | - | | | | 1 | 0 | - |
| SD | 1 | 0 | - | | | | 1 | 0 | - | | | | | | |
| TN | 1 | 0 | - | | | | 1 | 0 | - | | | | | | |
| TX | 6 | 0 | - | | | | 3 | 0 | - | 2 | 0 | - | 1 | 0 | - |
| UT | 1 | 0 | - | | | | 1 | 0 | - | | | | | | |
| VA | 1 | 1 | 100.0 | | | | | | | 1 | 1 | 100.0 | | | |
| VT | 1 | 0 | - | | | | 1 | 0 | - | | | | | | |
| WA | 2 | 1 | 50.0 | | | | 1 | 0 | - | | | | 1 | 1 | 100.0 |
| WI | 2 | 2 | 100.0 | | | | 1 | 1 | 100.0 | | | | 1 | 1 | 100.0 |
| WV | 2 | 0 | - | | | | 1 | 0 | - | | | | 1 | 0 | - |
| WY | 2 | 0 | - | | | | 2 | 0 | - | | | | | | |
| Total | 148 | 40 | 27.0 | 28 | 9 | 32.1 | 55 | 10 | 18.2 | 26 | 7 | 26.9 | 40 | 14 | 35.0 |

**TABLE 2**

Mean and Median Differences Between Tort Reforms Found Unconstitional and Tort Reforms Not Found Unconstitional

| Variable | Not Unconstitutional (1) | | | Unconstitutional (2) | | | Test of Differences (3) | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Median | Std Dev. | Mean | Median | Std Dev. | t-Test | Wilcoxon z |
| Reform Type: | | | | | | | | |
| Collateral Source Rule (CSR) | 0.176 | 0.000 | 0.383 | 0.225 | 0.000 | 0.423 | -0.673 | -0.675 |
| Joint and Several Liability (JSL) | 0.417 | 0.000 | 0.495 | 0.250 | 0.000 | 0.439 | 1.873 * | 1.857 * |
| Punitive Damages | 0.176 | 0.000 | 0.383 | 0.175 | 0.000 | 0.385 | 0.013 | 0.013 |
| Noneconomic Damages | 0.231 | 0.000 | 0.424 | 0.350 | 0.000 | 0.483 | -1.454 | -1.449 |
| Year | 1994.343 | 1995.000 | 7.351 | 1989.800 | 1987.000 | 4.659 | 3.642 *** | 3.257 *** |
| State Characteristics: | | | | | | | | |
| Per Capital Real GDP by State | 2.584 | 2.515 | 0.841 | 2.177 | 1.879 | 0.680 | 2.740 *** | 2.828 *** |
| Lawyers per Capita | 29.694 | 28.125 | 11.309 | 28.165 | 27.720 | 6.141 | 0.811 | 0.294 |
| Appoint | 0.093 | 0.000 | 0.291 | 0.075 | 0.000 | 0.267 | 0.334 | 0.335 |
| Nonpartisan Election | 0.315 | 0.000 | 0.467 | 0.425 | 0.000 | 0.501 | -1.251 | -1.248 |
| Partisan Election | 0.194 | 0.000 | 0.398 | 0.200 | 0.000 | 0.405 | -0.075 | -0.075 |
| Merit | 0.398 | 0.000 | 0.492 | 0.300 | 0.000 | 0.464 | 1.094 | 1.094 |
| Citizen Ideology Index | 46.919 | 46.889 | 12.810 | 45.221 | 45.829 | 12.113 | 0.726 | 0.695 |
| N | | 108 | | | 40 | | | |

Note.-- See Appendix A for a description of the variables. ***,**, and * denote significance at the 1, 5, and 10 percent level.

**TABLE 3**

Logistic Regressions of Probability of Unconstitutionality During The Sample Period

| Variable: | (1) | | (2) | |
|---|---|---|---|---|
| | Coefficient | Marg. Effect | Coefficient | Marg. Effect |
| State Characteristics: | | | | |
| Per Capital Real GDP by State | -1.436 *** | -0.228 | -0.463 ** | -0.008 |
| | (0.433) | | (0.235) | |
| Lawyers per Capita | 0.012 | 0.002 | -0.007 | 0.000 |
| | (0.019) | | (0.009) | |
| Appoint | 0.012 | 0.002 | 0.442 | 0.009 |
| | (1.254) | | (1.13) | |
| Nonpartisan Election | 0.803 | 0.137 | 0.743 * | 0.014 |
| | (0.657) | | (0.407) | |
| Partisan Election | 1.405 * | 0.276 | 1.252 *** | 0.034 |
| | (0.739) | | (0.438) | |
| Citizen Ideology Index | -0.055 * | -0.009 | -0.021 | 0.000 |
| | (0.029) | | (0.014) | |
| Reform Type: | | | | |
| Collateral Source Rule (CSR) | 0.328 | 0.055 | 0.213 | 0.004 |
| | (0.641) | | (0.521) | |
| Punitive Damage Reform | 0.370 | 0.063 | 0.450 | 0.009 |
| | (0.469) | | (0.402) | |
| Noneconomic Damage Reform | 0.901 * | 0.161 | 0.997 ** | 0.024 |
| | (0.541) | | (0.424) | |
| Geographical Divisions | | | | |
| Northeast | 2.211 | 0.487 | 1.268 | 0.038 |
| | (1.73) | | (1.403) | |
| Mid-Atlantic | 2.134 * | 0.469 | 0.928 | 0.023 |
| | (1.129) | | (1.154) | |
| East North Central | 3.177 *** | 0.651 | 2.088 *** | 0.084 |
| | (1.07) | | (0.761) | |
| West North Central | 1.621 | 0.336 | 1.126 | 0.029 |
| | (1.204) | | (0.844) | |
| South Atlantic | 2.775 *** | 0.588 | 2.331 *** | 0.119 |
| | (0.942) | | (0.736) | |
| Mountain | 0.368 | 0.063 | 0.418 | 0.008 |
| | (0.922) | | (0.737) | |
| Pacific | 2.762 ** | 0.588 | 1.512 * | 0.045 |
| | (1.348) | | (0.843) | |
| Log Likelihood | -66.807 | | -165.605 | |
| Pseudo $R^2$ | 0.226 | | 0.101 | |
| Observations | 148 | | 1490 | |
| Number of Tort Reforms | 148 | | 148 | |
| Number of Unconstitutional Tort Reforms | 40 | | 40 | |

Note.-- The sample in (1) includes enactment year observations. The dependent variable takes the value of one if the tort reform is declared unconstitutional any time during the sample period and zero otherwise. The sample in (2) includes observations for all the years the reform is in place. The dependent variable takes the value of one in the year the tort reform is declared unconstitutional and zero otherwise. For each regression, the table reports the coefficients and estimated marginal effects. The marginal effect for a continuous variable is evaluated at the mean. The marginal effect for a discrete variable is the change in the predicted probability due to a change from 0 to 1. Standard errors adjusted for heteroskedasticity and state clustering are reported in parentheses. The regressions include a constant term which is not reported here to conserve space. All variable definitions are available in Appendix A. ***,**, and * denote significance at the 1, 5, and 10 percent level.

**TABLE 4**

Effects of Explanatory Variables: Proportional Hazard Models

| Variable | Cox Estimates with Time-Varying Covariates (1) | | Weibull Estimates with Time-Varying Covariates (2) | |
|---|---|---|---|---|
| State Characteristics: | | | | |
| Per Capita GDP by State | -0.253 | (0.324) | -0.932 *** | (0.348) |
| Lawyers per Capita | -0.008 | (0.009) | -0.008 | (0.009) |
| Appoint | 0.518 | (1.188) | 0.575 | (1.202) |
| Nonpartisan Election | 0.658 * | (0.373) | 0.800 * | (0.48) |
| Partisan Election | 1.303 *** | (0.434) | 1.468 *** | (0.474) |
| Citizen Ideology Index | -0.023 * | (0.014) | -0.020 | (0.018) |
| Reform Type: | | | | |
| Collateral Source (CSR) | 0.239 | (0.459) | 0.076 | (0.53) |
| Punitive Damages | 0.215 | (0.418) | 0.659 | (0.474) |
| Nonecon. Damages | 0.964 ** | (0.417) | 1.190 ** | (0.474) |
| Geographical Divisions | | | | |
| Northeast | 1.322 | (1.448) | 1.270 | (1.482) |
| Mid-Atlantic | 0.854 | (1.134) | 1.439 | (1.098) |
| East North Central | 2.103 *** | (0.726) | 2.405 *** | (0.764) |
| West North Central | 1.355 | (0.836) | 1.239 | (0.835) |
| South Atlantic | 2.371 *** | (0.718) | 2.551 *** | (0.71) |
| Mountain | 0.550 | (0.707) | 0.487 | (0.756) |
| Pacific | 1.626 ** | (0.791) | 1.658 * | (0.997) |
| Constant | . . . | | -3.735 *** | (1.131) |
| Observations | 1490 | | 1490 | |
| Tort Reforms | 148 | | 148 | |
| Unconstitutional Reforms | 40 | | 40 | |

Note.-- The table reports the coefficients from Cox and Weibull proportional hazard models with time-varying covariates. The sample includes all the years the reform is in place. Standard errors adjusted for heteroskedasticity and state clustering are in parentheses. A positive coefficient indicates that the regressor increases the hazard and reduces the duration of tort reform. All variable definitions are available in Appendix A. ***,**, and * denote significance at the 1, 5, and 10 percent level.

**TABLE 5**

Effect of Tort Reforms on Losses, Premiums, and Profitability (1986-2005)

**Panel A: Tort Reform Indicators**

| | Log Economic Loss Ratio | Log Losses | Log Premiums |
|---|---|---|---|
| | (1) | (2) | (3) |
| Collateral Source Rule (CSR) Reform | -0.051 * | -0.039 | -0.001 |
| | (0.027) | (0.021) | (0.012) |
| | [0.03] | [0.025] | [0.015] |
| Joint and Several Liability (JSL) Reform | 0.060 * | 0.000 | -0.048 *** |
| | (0.026) | (0.02) | (0.011) |
| | [0.031] | [0.025] | [0.015] |
| Punitive Damage Reform | -0.008 | -0.002 | 0.001 |
| | (0.023) | (0.022) | (0.011) |
| | [0.026] | [0.027] | [0.014] |
| Noneconomic Damage Reform | -0.046 * | -0.059 ** | -0.015 |
| | (0.021) | (0.021) | (0.011) |
| | [0.023] | [0.025] | [0.014] |
| $R^2$ | 0.285 | 0.929 | 0.966 |

**Panel B: Survival Probability of Tort Reform**

| | Log Economic Loss Ratio | Log Losses | Log Premiums |
|---|---|---|---|
| | (1) | (2) | (3) |
| Collateral Source Rule (CSR) Reform | -0.036 | -0.029 * | -0.016 * |
| | (0.021) | (0.015) | (0.007) |
| | [0.025] | [0.017] | [0.009] |
| Joint and Several Liability (JSL) Reform | 0.039 ** | -0.003 | -0.035 ** |
| | (0.017) | (0.017) | (0.011) |
| | [0.018] | [0.02] | [0.014] |
| Punitive Damage Reform | 0.049 * | 0.062 ** | 0.019 |
| | (0.022) | (0.02) | (0.016) |
| | [0.026] | [0.025] | [0.02] |
| Noneconomic Damage Reform | -0.094 *** | -0.099 *** | -0.024 ** |
| | (0.028) | (0.029) | (0.012) |
| | [0.03] | [0.032] | [0.011] |
| $R^2$ | 0.287 | 0.929 | 0.967 |

Note.-- This table reports the results from OLS regressions (Viscusi, et al. 1993; Born and Viscusi 1994 and 1998; Viscusi and Born 1995 and 2005). The dependent variable in model (1) is the logarithm of the Economic Loss Ratio (the ratio of the present value of discounted losses to premiums in state *i* and year *t*). The dependent variable in model (2) is the logarithm of Losses (the present value of aggregate liability insurance losses in state *i* and year *t*). The dependent variable in model (3) is the logarithm of Premiums (aggregate liability premiums in state *i* and year *t*). The regressions include a constant term and lagged dependent varaibles which are not reported to conserve space. The number of state-year observations is 1000. Panel A estimates the impact of tort reform on insurance markets using tort reform indicator varaibles (Avraham, 2007a). Panel B uses the estimated survival probability for reform type *j*, year *t*, and state *i*. Standard errors adjusted for heteroskedasticity and state clustering are in parentheses and pair-wise bootstrapped standard errors with state clusters drawn with replacement (1000 replications) are in square brackets. ***,**, and * denote significance at the 1, 5, and 10 percent level based on bootstrapped t-statistics.

**TABLE 6**

Fixed Effects Regressions of the Impact of Tort Reform on Insurance Markets (1986-2005)

**Panel A: Tort Reform Indicator Variables**

| | Economic Loss Ratio | Losses | Premiums |
|---|---|---|---|
| | (1) | (2) | (3) |
| Collateral Source Rule (CSR) Reform | 0.004 | -0.039 | -0.049 |
| | (0.03) | (0.032) | (0.037) |
| | [0.032] | [0.032] | [0.038] |
| Joint and Several Liability (JSL) Reform | 0.083 ** | -0.012 | -0.126 *** |
| | (0.038) | (0.028) | (0.033) |
| | [0.039] | [0.029] | [0.033] |
| Punitive Damage Reform | 0.015 | 0.041 | 0.001 |
| | (0.03) | (0.026) | (0.028) |
| | [0.03] | [0.027] | [0.028] |
| Noneconomic Damage Reform | -0.043 | -0.065 *** | -0.013 |
| | (0.028) | (0.022) | (0.032) |
| | [0.028] | [0.022] | [0.033] |
| $R^2$ | 0.005 | 0.005 | 0.032 |

**Panel B: Survival Probability of Tort Reform**

| | Economic Loss Ratio | Losses | Premiums |
|---|---|---|---|
| | (1) | (2) | (3) |
| Collateral Source Rule (CSR) Reform | 0.033 ** | -0.041 | -0.071 ** |
| | (0.015) | (0.03) | (0.032) |
| | [0.016] | [0.033] | [0.033] |
| Joint and Several Liability (JSL) Reform | 0.055 * | 0.019 | -0.049 ** |
| | (0.028) | (0.023) | (0.023) |
| | [0.029] | [0.024] | [0.024] |
| Punitive Damage Reform | 0.019 | 0.064 ** | 0.033 |
| | (0.033) | (0.029) | (0.02) |
| | [0.033] | [0.029] | [0.021] |
| Noneconomic Damage Reform | -0.088 ** | -0.093 *** | -0.009 * |
| | (0.035) | (0.032) | (0.005) |
| | [0.037] | [0.034] | [0.005] |
| $R^2$ | 0.008 | 0.007 | 0.046 |

Note.-- This table reports the results of fixed effects regressions. The dependent variable in model (1) is the Growth in the Economic Loss Ratio (Log Loss Ratio$_t$ - Log Loss Ratio$_{t-1}$). The dependent variable in model (2) is the Growth in Losses (Log Losses$_t$ - Log Losses$_{t-1}$). The dependent variable in model (3) is the Growth in Premiums (Log Premiums$_t$ - Log Premiums$_{t-1}$). The regressions include a constant term which is not reported to conserve space. The number of state-year observations is 1000. Panel A estimates the impact of tort reform on insurance markets using tort reform indicator varaibles (Avraham, 2007a). Panel B uses the estimated survival probability for reform type $j$, year $t$, and state $i$. Standard errors adjusted for heteroskedasticity and state clustering are in parentheses and pair-wise bootstrapped standard errors with state clusters drawn with replacement (1000 replications) are in square brackets. ***,**, and * denote significance at the 1, 5, and 10 percent level based on bootstrapped t-statistics.

**TABLE 7**

Dynamic Panel Model (LSDVC) of the Effects of Tort Reform on Insurance Markets

**Panel A: Tort Reform Indicator Variables**

| | Log Economic Loss Ratio | Log Losses | Log Premiums |
|---|---|---|---|
| | (1) | (2) | (3) |
| Collateral Source Rule (CSR) Reform | -0.073 | -0.054 | -0.023 |
| | (0.058) | (0.059) | (0.034) |
| Joint and Several Liability (JSL) Reform | 0.212 ** | 0.125 ** | -0.072 ** |
| | (0.084) | (0.055) | (0.033) |
| Punitive Damage Reform | 0.163 * | 0.115 ** | -0.006 |
| | (0.083) | (0.055) | (0.033) |
| Noneconomic Damage Reform | -0.074 * | -0.078 | -0.006 |
| | (0.042) | (0.054) | (0.031) |

**Panel B: Survival Probability of Tort Reform**

| | Log Economic Loss Ratio | Log Losses | Log Premiums |
|---|---|---|---|
| | (1) | (2) | (3) |
| Collateral Source Rule (CSR) Reform | -0.057 | -0.040 | -0.029 * |
| | (0.04) | (0.049) | (0.017) |
| Joint and Several Liability (JSL) Reform | 0.106 *** | 0.065 | -0.027 * |
| | (0.036) | (0.04) | (0.016) |
| Punitive Damage Reform | 0.121 *** | 0.096 * | -0.017 |
| | (0.047) | (0.058) | (0.035) |
| Noneconomic Damage Reform | -0.064 ** | -0.089 * | -0.028 * |
| | (0.032) | (0.053) | (0.016) |

Note.-- This table reports the results from LSDVC dynamic panel models (Bruno, 2005). The dependent variable in model (1) is the logarithm of the Economic Loss Ratio (the ratio of the present value of discounted losses to premiums in state *i* and year *t*). The dependent variable in model (2) is the logarithm of Losses (the present value of aggregate losses in state *i* and year *t*). The dependent variable in model (3) is the logarithm of Premiums (aggregate premiums in state *i* and year *t*). The regressions include lagged dependent variables which are not reported to conserve space. The number of state-year observations is 1000. Panel A estimates the impact of tort reform on insurance markets using tort reform indicator varaibles (Avraham, 2007a). Panel B uses the estimated survival probability for reform type *j*, year *t*, and state *i*. Parametric bootstrapped standard errors are in parentheses. ***,**, and * denote significance at the 1, 5, and 10 percent level.