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**THE IMPACT OF TORT REFORM  
ON PRIVATE HEALTH  
INSURANCE COVERAGE**

by

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# The Impact of Tort Reform on Private Health Insurance Coverage

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## Abstract

This study evaluates the impact of tort reform on private health insurance coverage using the Current Population Survey's March Demographic Files. Proponents of tort reform argue that reform will reduce medical malpractice insurance costs, damage awards, and costs associated with defensive medicine. If proponents are correct, these cost reductions should lower the price of healthcare and increase health insurance coverage. On the other hand, if the prior tort law was functioning well, reform may increase medical costs by reducing doctors' care-taking or increasing intensity of treatment. In this case, tort reform could actually decrease insurance coverage by raising the price of health care. We evaluate the effect of eight common tort reforms on private health insurance coverage between 1981 and 2004. We find that some reforms increased health insurance coverage for the most price-sensitive groups (the young, single, and the self-employed). Given the multicollinearity of many reforms, it is difficult to gauge the impact of individual reforms. However, the results suggest that periodic payment reform and limitations on non-economic damages do not affect private insurance coverage, while caps on total damages, collateral source reform, joint and several liability reform, and limitations on punitive damages are associated with increased private insurance coverage for price-sensitive groups. Accordingly, we conclude that some tort reforms are effective in reducing healthcare costs. The magnitude of the effect on price sensitive groups suggests that some tort reforms can reduce health care costs by as much as two percent.

Since the early 1980s, state legislatures have enacted dozens of different reforms to tort and medical malpractice law by limiting damages, reforming liability rules, or reforming the structure of payments. Over the same period, state supreme courts have struck down many reforms, some of which have been subsequently reenacted. Indeed, for two decades tort reform has been among the foremost legal item on state legislative agendas. According to the Center for Public Integrity (2007), interest groups regularly spend hundreds of millions of dollars pursuing or fighting against reform. In addition, several bills to federalize aspects of medical malpractice law have been debated in Congress since 1994, the most recent in 2006. Some of these bills came close to passage, although Democratic control of Congress control now makes sweeping reform far less likely. Therefore, understanding the impact of tort reform on the healthcare system is increasingly important.

This paper is the first to directly analyze the impact of tort reform on health insurance coverage.<sup>2</sup> We examine private health insurance coverage for two reasons. First, the effect of tort reform on healthcare costs is ambiguous, but is important in understanding the social welfare implications of tort reform. Tort reform could decrease costs by decreasing the use of defensive medicine, but it could increase costs in other ways. For example, recent work by Curry and MacLeod (2008) suggests that tort reform could increase the use of expensive and unnecessary procedures because after tort reform doctors face less liability for use of such procedures. If such is the case, tort reform could actually increase healthcare costs. Reliable measures of healthcare costs do not exist over time and across states. However, we estimate the effect of reform on private insurance coverage using the Current Population Survey's March Demographic Files (CPS). Combining these estimates with estimates of coverage elasticities, we can back out estimates of the price effects of tort reform.

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<sup>2</sup>Baicker and Chandra (2005) used malpractice award levels by state as an instrument for health insurance premium costs, and find that the two are significantly correlated in the expected direction. The instruments seem to work well in the estimation of health insurance demand.

Second, the effect of tort reform on health insurance coverage rates is interesting by itself and has recently become part of the political debate. President Bush (2004) has linked tort reform and health insurance coverage, saying that insurance coverage could be increased “by limiting costly and abusive litigation that threatens healthcare in America.” Similarly, the Department of Health and Human Services (2002) asserted that “limiting unreasonable awards for non-economic damages could reduce health care costs by 5%-9%....These savings would...permit an additional 2.4-4.3 million Americans to obtain insurance.” More recently, various interest groups have begun to connect tort reform with health insurance coverage. America's Health Insurance Plans (AHIP) (2006), a national association of health insurance providers, has argued that that defensive medicine and litigation costs increase health insurance premiums by nearly nine percent.<sup>3</sup> AHIP has spent millions on a national television campaign against plaintiff's lawyers.<sup>4</sup>

How could tort reform affect private health insurance coverage rates? Proponents of tort reform have argued that reform will reduce medical malpractice insurance costs, damage awards, and costs associated with defensive medicine. If proponents are correct, medical cost reductions should translate into greater private health insurance coverage by lowering the price of healthcare. On the other hand, if prior tort law was functioning well, reform may increase medical costs by reducing doctors' caretaking or increasing the number of unnecessary procedures. In this case, tort reform may actually decrease private insurance coverage by raising healthcare costs.

Our results suggest that some tort reforms increased health insurance coverage for the most price-sensitive groups (the young, the self-employed, the single) relative to older, married non-self employed workers. The joint tests of reform-treatment group interactions are generally strong, and some inferences can be drawn about which reforms have the greatest impact. Accordingly, we conclude that some tort reforms are associated with higher insurance coverage rates and, consequently, decreased medical costs. The

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<sup>3</sup> On AHIP's website the group asserts that “the current litigation system for compensating patients injured by medical negligence is expensive, slow, and does little to benefit the injured patients”. (Available at <http://www.ahip.org/content/default.aspx?bc=39|341|320>).

<sup>4</sup> For their national campaign see <http://www.ahip.org/content/default.aspx?bc=39|8931>.

magnitude of the effects combined with prior estimates of coverage elasticities suggest that some tort reforms can reduce healthcare costs by as much as two percentage points.

The rest of this paper is organized as follows. Section I discusses the theoretical framework, Section II the data, Section III the methodology, Section IV presents the results, and Section V concludes.

## **I. The Impact of Tort Reform on Healthcare Costs**

Medical malpractice liability affects healthcare costs through direct liability costs and the indirect costs created by doctor care and doctor activity levels. Liability costs are comprised of malpractice damage awards and the associated litigation costs.<sup>5</sup> These costs are generally thought to comprise a small share of total healthcare costs, at most two percent (Congressional Budget Office 2004). Therefore, tort reform cannot much reduce healthcare costs directly by limiting the cost of liability. Of course, doctor care and activity levels are a function of doctors' liability. If tort reform is to have a substantial effect on healthcare costs, it must affect doctor care and activity levels, which implies that doctors must be sensitive to liability pressures. There is an emerging consensus that both liability costs and doctor activity levels are affected by tort reform.

Recent work has confirmed that tort reform was effective in reducing doctors' liability exposure.<sup>6</sup> In his survey of the literature, Holtz-Eakin (2004) concludes caps on damages are fairly consistently found to reduce the number of lawsuits, awards, and the loss ratio. Most recently, Avraham (2007) examined medical malpractice settlements applying the comprehensive tort reform database used in this paper, paying special attention to the retroactive applicability of some reforms. He found that some tort

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<sup>5</sup> The administrative costs of the courts are largely socialized, and plaintiffs bear a portion of the litigation costs as well. We ignore these costs in our analysis.

<sup>6</sup> Early studies on tort reform focused mainly on the impact of tort reform on direct medical liability costs such as average award size, frequency of lawsuits, or the price of malpractice insurance. Other studies have explored the effect of tort reform on medical malpractice insurance variables such as premiums, loss ratio, and losses incurred. These studies often found mixed results and were plagued with selection issues (for reviews of previous literature see Zuckerman, Koller, and Bovbjerg [1986]; Holtz-Eakin [2004]).

reforms decreased the number of claims and average payouts.<sup>7</sup> Of course, these studies do not address the welfare implications of tort reform but merely its effectiveness in reducing physician and hospitals' liability costs. Consequently, their normative implications are unclear. Most authors have acknowledged this limitation (see, for example, [Danzon 1986, p. 79]; Viscusi et al [1993, p. 175]; Viscusi and Born [2005, p. 41]).

Another strand of this literature has examined the effects of tort reform on physician location decisions and medical malpractice insurance prices. The literature on the impact of tort reform on physician supply suggests that tort reform may increase the supply of physicians in rural areas but has found otherwise mixed results.<sup>8</sup> Studies of the incidence of malpractice insurance find that costs of malpractice insurance are largely passed on by doctors to consumers in the form of higher prices and switches to higher quantity of services or more profitable procedures (Danzon et al. 1990; Thurston 2001; Pauly et al. 2007). This may explain some of the conflicting findings on whether or not tort reform increases the number of practicing doctors. More importantly for our study, the finding that malpractice costs are passed on suggests that the costs of malpractice insurance are largely passed on in the form of higher medical bills (and therefore can have an impact on health insurance coverage rates). Still, there are no clear normative implications to a reduction in the number of physicians, as this number might have been too high in the first place, especially if the doctors that quit are the less competent ones.

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<sup>7</sup> Specifically, Avraham (2007) found that caps on non-economic damages and limitation of the doctrine of joint and several liability reduced the number of annual payments, and that caps on non-economic damages and periodic payment reform reduced average awards. Additionally, caps on non-economic damages were found to reduce total awards, although the statistical significance of this was weak. The other reforms had no statistically significant effect on the total annual payments. In their working paper, Currie and MacLeod (2006) also found that reforms reduced malpractice payouts.

<sup>8</sup> For example, Baicker and Chandra (2004) found that malpractice premiums do not affect the overall size of the physician workforce, although they may deter marginal entry, increase marginal exit, and reduce the rural physician workforce. On the other hand, Kessler et al (2005) found that the adoption of "direct" malpractice reforms led to 3.3% growth in the overall supply of physicians. Direct reforms had a larger effect on supply through retirements and entries than through the propensity of physicians to move between states. In a recent working paper, David Matsa (2005) used county-level, specialty-specific annual counts of physicians from 1970 to 2000 to estimate the effect of damage caps on physician supply. He found that caps do not affect physician supply for the average resident of states adopting reforms. On the other hand, caps appear to increase the supply of frontier rural, specialist physicians by 10-12 percent. For a survey of older studies see U.S. Government Accountability Office (2003).

An arguably more important line of studies addresses the cost savings and outcomes of patients after tort reform resulting from changes in doctor care and activity levels. While the prevalence of defensive medicine has been established primarily by surveys of doctors, only a few studies attempted to measure its scope in a systematic manner.<sup>9</sup> The most cited studies are by Kessler and McClellan. Using data on all elderly Medicare beneficiaries treated for serious heart disease, Kessler and McClellan (1996) found that "direct" reforms (such as limitations on damages) reduce medical costs by 5 to 9 percent within 3 to 5 years of adoption without substantially affecting mortality or medical complications.<sup>10</sup> In their 2002 study on the same population, Kessler and McClellan (2002) controlled for HMO penetration and found that "direct" tort reforms reduce medical costs for heart patients by about 4 percent. "Indirect" reforms, such as periodic payments or limits on joint and several liability, had little impact.

More recently, Currie and MacLeod (2008) found that joint and several liability reform reduces the use of Caesarean sections and reduces complications of labor and delivery. In contrast, they found that caps on damages increases unnecessary C-sections and the chance of complications in labor and delivery. They conclude that doctors may perform unnecessary Caesarean sections when liability is reduced, increasing complication rates resulting from Caesarean sections. In contrast, other studies have considered Caesarian sections and found them positively correlated with medical malpractice pressure.<sup>11</sup>

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<sup>9</sup> By 1994 there were 47 studies which explored defensive medicine by way of surveys and only two studies from the mid 1980s which did it based on statistical tools using existing datasets (see U.S. Congress 1994). The most recent study based on surveys of physicians is Studdert et al (2005).

<sup>10</sup> Kessler and McClellan (1996, pp. 371-2). 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. By "indirect" reforms the authors include: contingency fee reforms, periodic payments, joint and several liability, patient compensation fund. See Id. at 371-2.

<sup>11</sup> Dubay, Kaestner, and Waidmann (1999) found that greater malpractice pressure leads to a higher probability of Cesarean delivery but no significant improvement in Apgar scores. Sloan et al (1995), using mortality of the child and Apgar scores as indicators of health outcomes, found no systematic improvement in birth outcomes due to increased threat of medical malpractice litigation. Dubay, Kaestner, and Waidmann (2001) found that liability pressure increases defensive medicine, but failed to find evidence that the associated reduced utilization of prenatal care adversely affected infant health as measured by birth weight and Apgar score.

Our work is a contribution to the normative line of studies. Indeed, we attempt to take a broader perspective by focusing on the impact of tort reform on general measure of healthcare costs, private insurance coverage, instead of a class of procedures. The change in health insurance coverage rates after tort reform should reflect the effect of reform on healthcare costs.

In theory, tort reform could decrease or increase private insurance coverage. Tort reform could decrease insurance coverage through a combination of two mechanisms. First, if doctors reduce their care level below the optimal level, resulting increases in medical errors will require more medical expenditures to repair damage. For example, the procedure may have to be performed again or the injured patient may need a longer hospital stay. Depending on the magnitude of the effect, increased medical error could more than offset any savings reductions from reduced defensive medicine and malpractice payouts, decreasing private insurance coverage rates. Second, as Currie and MacLeod suggest, tort reform could increase unnecessary procedures by reducing liability for performing them. Indeed, the example of Caesarean sections is quite telling. Compared to vaginal delivery, Caesarean sections are nearly twice as expensive (see Currie and MacLeod 2008, pg. 806 for a discussion). If tort reform enables more aggressive treatments, then expenditures will rise after tort reform. Therefore, a decrease in insurance coverage rates after tort reform, reflecting higher healthcare costs, would unambiguously suggest a decrease in social welfare.

Tort reform could increase coverage rates by reducing doctor's caretaking. Defensive medicine, by definition, is unjustified caretaking. Doctors may practice defensive medicine for a number of reasons. First, although doctors are insured against medical malpractice liability, the doctors may be sensitive to malpractice claims because of time and reputation costs as well as the psychic costs of liability. Second, doctors may bear little of the cost of defensive medicine, which is paid for by third-party insurers or borne by the hospital and others. Thus, even a weak incentive to practice defensive medicine may lead to significant amounts of defensive medicine.

However, reducing justifiable precautions may also decrease costs. Because reduced justifiable caretaking results in higher costs later as complication rates rise, our analysis recoups the explicit medical costs of reduced caretaking. However, pain-and-suffering and loss of life are important social costs not reflected in this calculus. Even if healthcare costs are reduced by tort reform, patients may bear pain and suffering which will not be reflected in private health insurance premiums and insurance coverage rates. It is therefore important to recall throughout the remaining discussion that higher coverage rates may not necessarily reflect higher social welfare.<sup>12</sup>

Despite the important missing component of pain and suffering damages, an analysis of insurance coverage rates can yield some important conclusions. First, our methodology directly tests for whether the cost savings of tort reform are offset by increased medical complications or increases in treatment intensity. Higher coverage rates would rule out the possibility that tort reform increases unnecessary procedures. Second, increases in insurance coverage and cost savings in medicine are important goals of tort reform, and testing tort reform's effectiveness in reducing them is important to understanding the effects of reform.

## **II. The Data and Identification Strategy**

For the general population, the price elasticity of health insurance coverage is fairly low. Most recently, Chernew, Cutler, and Keenan (2005) estimated a -.08 elasticity of coverage among the entire population. However, well-defined subgroups have significantly higher estimated elasticities of coverage. For example, those without access to a group plans have an elasticity on the order of -.3 to -.5, and this is much higher for

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<sup>12</sup> Although we acknowledge the theoretical possibility that tort reform may generate demand-side effects for private health insurance, we believe that these effects will be small. First, a shift in demand would require that consumers accurately perceive the quality of healthcare before and after tort reform. Though possible, this scenario seems unlikely. Indeed, one reason doctors may be able to prescribe unnecessary procedures is because consumers do not know whether or not various procedures are in their interest. Second, the decision to purchase health insurance may primarily be a decision over whether or not to insure assets against the possibility of catastrophic health costs and therefore may be largely invariant to the quality of care. Consequently, we return to our caveat from above that higher coverage rate may not necessarily reflect higher social welfare. For a discussion of this in the context of Medicare, see Finkelstein and McKnight (2005). The authors find that Medicare failed to improve the quality of care or health outcomes, but did offer protection against large financial losses.

the young, single, and self-employed (for a survey of the research, see Marquis et al. [2004]). Using a tax-code change, Gruber and Poterba (1994) estimate an elasticity of coverage for the self-employed of around -.5, but this was -1.8 for the single self-employed. Thus, the self-employed, young, and single are proper treatment groups, and the remaining population has an almost inelastic demand around small price fluctuations, presenting a legitimate control group for a triple-differences approach.

Since 1982, the Current Population March Demographic Survey has collected data on health insurance coverage in the prior year. We examine the years 1981 through 2004.<sup>13</sup> Thus, we have twenty-four survey years during a period of substantial reform. In addition, we can employ the limited panel nature of the CPS by controlling for prior insurance status, which accounts for a great deal of heterogeneity in insurance coverage.

The CPS's March insurance questions are vague and not terribly detailed, but are consistent enough over time allow for comparable state-level estimates of private health insurance coverage since 1981. The extent of health coverage and health status cannot be ascertained, nor can the employer's contribution. Prior to 1994, we cannot determine which individuals purchased health insurance directly or through an employer. One widely-noted aspect of the survey is that it likely overstates the number of uninsured. The survey is conducted in March, but the health insurance coverage question asks for last year's coverage. Therefore, it is unclear how people would report coverage if they had spells of private coverage combined with other sorts of coverage or periods of no coverage at all. Comparisons to smaller surveys, such as the SIPP (Survey of Income and Program Participation), indicate that the CPS appears to describe how many individuals have or are lacking coverage at a given moment, but significantly overstate the number uninsured for the whole year.<sup>14</sup> Therefore, the CPS cannot be used to determine who is chronically uninsured, and many people are uninsured for short spells, for example when

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<sup>13</sup> CPS survey years 1982-2005. In 1981, the CPS collected information only on group health plans. Our tort reform database goes back only to 1980, so we did not use the 1980 CPS (which contained coverage information for 1979).

<sup>14</sup> For a discussion of how the CPS may overstate the number of uninsured and its other drawbacks, see Congressional Budget Office (2003).

in-between jobs or after graduating from college. Fortunately, these problems should not distort our analysis because the response to tort reform should come from those who are marginally uninsured, and the CPS probably does a good job of capturing them.

In order to make our study comparable to those which estimated elasticities, we limit the sample to adults who reported some employment in the previous year (the year for which health coverage information was collected). In addition, employed adults are highly unlikely to have access to government coverage such as Medicaid and Medicare. In our sample, only 3.2% report coverage from these two programs, suggesting that the options for those attached to the labor force are private coverage or no coverage. Children are excluded for two reasons. First, the determinants of health insurance coverage for children are different than that of adults mainly because, under Medicaid rules and various state programs, children have greater access to government coverage and subsidies. Medicaid programs targeting children were expanded during the later part of the study period. Second, in 1988, the CPS began asking detailed questions about whether household members may have insurance coverage from those outside the household. This largely affected children.<sup>15</sup>

Those 65 and older, of course, have access to Medicare coverage and would generally only have private coverage as a supplement (e.g., Medigap coverage), and are therefore excluded from the analysis.<sup>16</sup> In addition, we wish to compare the response of the self-employed to those who work for others, so we limit the sample to adults with some labor force connection.

Because households are sampled for four months, followed by an eight month break, and then sampled for another four months, two-period panels can be constructed

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<sup>15</sup> The regression results reported below were run with children, taking the self-employment status, age, and marital status of the head of household as the treatment variable. None of the joint tests were significant, and the coefficients were substantially smaller than those reported for adults.

<sup>16</sup> In addition, because of lower damage awards for older individuals, doctors would have less incentive to undertake defensive medicine.

for each March household (except for the 1986 and 1996 surveys).<sup>17</sup> Thus, we can control for an individual's prior insurance status. This undoubtedly will remove a great deal of individual heterogeneity that simple demographic controls do not. For example, if one has health insurance in the prior year, there is a 91% chance that private health insurance is reported in the subsequent year (see Appendix Table 1). For those who report that they were uninsured, only 46% will report private insurance the next year. This also suggests another interesting treatment group: the previously uninsured. Relative to the currently insured, the previously uninsured are made up disproportionately of those who are cycling in and out of insurance and may be more price-sensitive.

The rate of private insurance coverage has fluctuated over time, but has generally declined in the United States since roughly 1987 (for detailed summary statistics see DeNavas-Walt, Proctor, and Lee [2004]). In 1987, the CPS reported that 76% of the U.S. population was covered by private health insurance. By 2004, the percentage with private insurance had declined to roughly 68%. In addition, there is substantial variation between states over time in health insurance coverage (see *id.*). The bulk of the decline in coverage has been attributed to the rising cost of health insurance and the increasing availability of government insurance (see Chernew, Cutler, and Keenan [2005]). Our sample is limited to labor market participants and is therefore different from the population estimates. On average, 82% of the sample is covered by private health insurance, and as of 2004, nearly 79% had private coverage.

### **III. Methodology**

The basic specification takes the following form, which is estimated using a linear probability regression:<sup>18</sup>

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<sup>17</sup> We follow the approach of Madrian and Lefgren (1999) in matching the samples. We included all plausibly matched units. Eliminating observations that failed certain validity tests (e.g., age increased by more than 2 years over the sample period) did not change the results appreciably. The year 1981 cannot be matched because the CPS did not collect full insurance data for 1980. In addition, the survey years 1986 and 1996 cannot be matched to their prior years because household identifiers were changed.

<sup>18</sup> The marginal effects estimated in probit models were similar to those of the linear probability models.

$$\text{Private Insurance}_{ijt} = \alpha \text{Constant} + \lambda \text{Year}_t + \psi \text{State}_j + \tau \text{Demographic}_{ijt} + \rho \text{Insurance}_{ijt-1} + \mu \text{Treatment}_{ijt} + \sigma \text{Tort Reform}_{jt} + \delta \text{Tort Reform}_{jt} \times \text{Treatment}_{ijt} + E_{jt}$$

The dependent variable, *Private Insurance*, equals one if private insurance coverage is reported in the previous year, zero otherwise. *Tort Reform* is a matrix of dummies of eight different reforms (described below). The variable of interest is  $\delta$ , which measures the percentage points increase in private insurance coverage after the adoption of tort reform for the treatment group (self-employed, young, or single) relative to the control group. *Insurance<sub>jt-1</sub>* are dummies for type of insurance coverage in the previous year (private, Medicaid, Medicare, military), with uninsured as the excluded category.<sup>19</sup> *Year* is a matrix of year dummies, *State* is a matrix of state fixed effects. Demographic controls are dummies for high school completion, college (or higher) completion, marital status, and previous year’s employment status (self-employment, uncompensated, or government employment; private employment is the excluded category), Black, Hispanic, and receipt of health insurance from Medicaid, Medicare, and military sources.<sup>20</sup> Continuous variables control for age, age squared, and family income. All regressions include state and year fixed-effects and treatment group main effects. The standard errors reflect clustering by state.

Graph 1 traces the coverage rates over time for our control group (older, married, non-self-employed workers) and the average of our three treatment groups (young, single, and self-employed), labeled “treatment”. We posit that our control group displays little response (at least in the short-run) to the change in insurance prices. The triple-difference specifications rely on the “control” group removing the effect of changes other than price, such as changes in industry composition and macroeconomic shocks that could also affect insurance coverage rates. A potential concern is that the control group is unresponsive in general and uncorrelated with the treatment groups’ coverage rates.

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<sup>19</sup> We also tested the robustness of the results to changes in HMO coverage rates and HMO coverage rates interacted with reforms, and found the made no difference. In any event, our triple-difference specifications should fully account for HMO coverage changes.

<sup>20</sup> Government provided health insurance is not mutually exclusive to private insurance. Spells of Medicaid coverage and a spell of private insurance coverage could both occur in the reference year, or private insurance could be maintained as a supplement.

However, as Graph 1 clearly demonstrates, fluctuations in insurance coverage among the control are highly correlated with fluctuations among the treatment (the year-to-year correlation coefficient between the two was .84). In addition, the difference between the two remains remarkably consistent over time as can be seen by the graph of the difference in coverage rates. In sum, the control and treatment groups appear to be valid.

a. Tort Reforms Dating and Reform Definitions

We date tort reforms using the second edition of Database of State Tort Law Reforms (DSTLR). This is a new dataset compiled by Avraham and discussed at length in Avraham (2006).<sup>21</sup> The database was assembled by reviewing the laws and court cases of the 50 states (and Washington DC) from 1980 to 2005 and comparing them to existing tort law compilations.<sup>22</sup> The process discovered that commonly used dating schemes suffer from missed reforms, missing or erroneously coded effective dates of reforms, and missing or incorrectly coded state supreme court decisions striking down or upholding reforms. We believe the DSTLR to be the most comprehensive and accurate legal dataset on tort reform to date.

There are ten common tort reforms, which include a variety of damage caps, damage payment reforms, and reforms of joint and several liability. However, we have sufficient variation in the years of our study to analyze only eight of them.<sup>23</sup> For a detailed compilation of the number of state adoptions and invalidations of all major

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<sup>21</sup> The dataset is available for free download at:  
[http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=902711](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=902711).

<sup>22</sup> The compilations include: The American Tort Reform Association's compilation (2007); a compilation by the National Conference of State Legislators (2007); American Medical Liability Association's Comparison of State Medical Liability Laws; Westlaw's 50 State Survey (2006); Congressional Research Service (2005).

<sup>23</sup> A few states have compensation funds from which patients can collect their recoveries. 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 it lower physician's premiums and spread the risk of risky medical specialties across the entire population of doctors and taxpayers. In the mid-1980s, a few states changed their policies regarding contingency fees, capping plaintiffs' lawyers' share in the recovery.

reforms, see Appendix Table 3. As can be seen, there is a substantial amount of variation in tort law over the time period studied, ranging from nine state law changes for caps on total damages to thirty-nine state law changes for caps on non-economic damages. Although most of the changes occurred from the mid-80s to the mid-90s, states were still changing tort law even after 2000.

*Reform of Joint and Several Liability.* 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-two states had some variant of this reform by 2004. The common law doctrine of joint-and-several liability allows the plaintiff to collect full damages from any of the defendants (which usually include both the hospital and the doctor) irrespective of the defendant's proportional fault. 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%.

*Periodic Payment.* Periodic payment of large future damage awards is now allowed or required in thirty-one states (“*Periodic Payment*”). The reform allows or requires courts to award future damages that are above some threshold, usually \$200,000, in periodic installments. Structuring payments in this fashion eases the burden on the defendant who can purchase a tax-advantaged annuity for that purpose, and can potentially relieve a defendant of a portion of the damages if the plaintiff dies before the annuity expires.

*Collateral Source.* Discretionary or mandatory consideration of collateral sources of payment for medical costs is another common reform (established in thirty-five states by 2004). The collateral source rule was developed by common law courts in the 19<sup>th</sup> century when insurance became more common. The common law “collateral source rule” generally holds that the plaintiff's personal insurance coverage *cannot* be used to offset the defendant's share of damages. The result is that an insured plaintiff gets more

than his full harm in case of an accident. States coded as having reformed the collateral source rule have abrogated the common law and either require or allow courts to reduce the plaintiff's recovery by the amount of private or public insurance benefits he receives (taking into account the accumulated paid premium).

*Damage Caps.* Some of the more controversial reforms involve ceiling caps on damage awards. These caps most commonly apply to non-economic damages (“*Cap Non-Economic*”, in twenty-three states) or punitive damages (“*Cap Punitive*”, in twenty-six states), but can also apply to total damages (“*Cap Total*”, in seven states). Caps come in many flavors. Some impose a cap of fixed dollar amount, while others use a multiplier of the economic damages. Some are inflation indexed, some are not.

Many states also implemented heightened pleading, evidentiary, or other procedural standards for punitive damages (“*Punitive Evidence*” in 32 states). For example, some 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.

*Multicollinearity of Reforms.* Appendix Table 3 presents reform coverage rates for the sample as a whole and conditional on other reforms being “on.” Numbers are bolded when there is an excess coverage rate of five percentage points or more for conditional coverage rates relative to the general population, and italicized when there is a five percentage points or more deficit. Almost all reforms exhibit a degree of multicollinearity in reform coverage, almost always positive. For example, while 55% of the full sample is covered by *Joint and Several Liability* reform, 70% of those covered by *Collateral Source* reform are also covered by *Joint and Several Liability* reform. The

only reform consistently negatively correlated with others is *Caps Total*. We will discuss the identification issues created by these relationships in greater detail below.

b. Exogeneity of Reforms

A concern with a difference-in-differences methodology is the exogeneity of the reforms studied. In our case, the results could be biased if high medical costs or low insurance coverage helped create the necessary political environment for tort reform. Although a possibility, there are several reasons why endogeneity is probably not an important factor here.

First, our results generally suggest an increase in insurance coverage following tort reform. It is unlikely that better coverage and lower medical costs motivated legislatures to adopt medical malpractice reforms. More likely, lower coverage or rising costs would prompt a legislative response, in which case coverage rates would be trending down around the adoption of a reform. Thus, the bias works against our general finding. Second, given the long time frame of the sample, we are less concerned about spikes or dips in pre-reform coverage rates, followed by a reversion to the mean, biasing our results. Third, the large majority of the reforms enacted in the period of the study are not specific to medical malpractice, but were in instead broad tort reform, applying to accidents and products liability as well. Thus, even if medical malpractice reforms were enacted in reaction to high levels of uninsured Americans, broad tort reforms were most probably not. Graphical evidence presented below (Graphs 2 and 3) suggests that insurance coverage exhibited no noticeable trend in the years prior to tort reform. In addition, in unreported regressions we tested for changes in insurance coverage in the year of reform and the three years prior to reform and find no statistically significant changes in coverage rates within in our control groups.

#### **IV. Results**

We first estimate the effect of reform by simply adding the number of reforms together and then interacting that sum with our treatment groups. This imprecise reform index, varying between zero and eight, generates some surprisingly precise results. Next,

we explore individual reforms through a series of event studies surrounding each reform, and then turn to joint estimation of the effects of all reforms separately entered and interacted with our treatment groups. The joint estimation of the separate effect of reform is important, even if difficult to present. Reforms can have differential impacts. Some may have less effect than others. For example, if punitive damages are not an important factor in medical malpractice, grouping them with other reforms will reduce the estimated impact of tort reform overall. In addition, as discussed above, reforms could have offsetting effects depending on whether they more appropriately align incentives to take optimal care. Given our large dataset and time frame, we can successfully estimate the differential impacts of reform. In addition, we can make an inference about which reforms seem to have the greatest impact and whether there is evidence that reforms offset each other.

*Count Index of Tort Reform Results.* Table 1 presents an estimate of the basic model, except that instead of entering eight reforms individually, the variable *Number of Reforms* simply adds the number of reforms effective in a state in that year. Regression 1 includes no interactions with our treatment groups, and the result is a positive but statistically insignificant number. The coefficient on *Number of Reforms* is easily interpreted. In Regression 1, taking the coefficient as given, the effect of each additional reform increases private insurance coverage by .17 percentage points, and adopting all eight reforms would increase coverage by 1.36 percentage points relative to having no reforms at all. These estimates are quite modest, but the coefficient is insignificant. The next columns incorporate our treatment group interactions. Regression 2 interacts *Number of Reforms* with *Young* and with *Self-Employed*. The coefficient on each interaction is .28 while the main effect, *Number of Reforms*, is small and insignificant. Thus, each additional reform increases private insurance coverage by .28 percentage points for the treatment groups and adopting all eight reforms would increase coverage by 2.26 percentage points relative to having no tort reforms at all.

Regressions 3 and 4 allow for the young and single to be an additional control group and test the robustness of the results to excluding demographic controls. The results are robust to demographic controls (Regression 4), and it is clear that the young

and single are by far the most affected by reform. Indeed, there appears to be no affect for the unmarried old or the married young. This basic result continues to hold throughout the rest of the paper.

We do not give the *Number of Reforms* estimates much weight. There is no theoretical reason to believe that each reform has the same impact: some reforms may be ineffective, and others may be offsetting, reducing the estimated aggregate effect. However, the estimates provide a reasonable approximation of the net effect of enacting a large package of tort reforms. In addition, the estimated results are plausible. Given an elasticity of coverage of around -1.0 for our treatment groups, the results imply that adopting all eight reforms may reduce health care costs by 2.25% to 4.25%. This estimate is out-of-sample as less than 10% of the sample is covered by six or more reforms, but does suggest the results are plausible. Moving from zero reforms to three reforms (the median of our sample) would reduce healthcare costs by .75% to 1.6%.

*Event Studies of Reforms.* We now estimate event studies of the effect of individual reforms on our treatment groups. To ease the presentation of results, we group the three punitive damage reforms into a single category *Punitive Reform*.<sup>24</sup> In addition, we focus on only two treatment groups: the *Young and Single* and the *Self-Employed*. Full results including the young and the unmarried as separate treatment groups yielded results similar to that of Table 1, suggesting that the effects of reform were concentrated within the young and single subgroup.<sup>25</sup>

We first consider a basic graphical treatment of the effect. Graphs 2 and 3 trace the difference in coverage between the average of our treatment groups (self-employed, young and single) and the coverage rate of the control groups (non-self-employed, older and married) before and after reform. Thus, a trend upward implies that the difference in coverage rates between the treatment and control group decreased after the reform, i.e. the treatment group was, relative to the control group, more likely to have insurance after

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<sup>24</sup> Full punitive damage results were originally reported and are available upon request.

<sup>25</sup> Full treatment group results were originally reported and are available upon request.

reform than before. We trace four reforms that were frequently adopted and for which a break was easily detectable: caps on non-economic damages, joint and several liability reform, reform of the collateral source rule, and periodic payment reform. There is in general a discernable increase in coverage rates of one to three percentage points within three years of reform.

To better evaluate the effect of individual reforms, Table 2 examines private insurance coverage within a window of a states' adopting or striking down the reform of interest. Only those states adopting the reform are included in the analysis, and we exclude the year of adoption. Those states in which reforms were immediately struck down are included as an "intent to treat" comparison group.<sup>26</sup> We run two specifications for each reform. The first examines the effect of reform within a six-year window of its adoption (plus or minus three years from adoption). The controls are as before except that with regard to the reform dummies, only the reform of interest and its interactions with the control group are included. The second specification employs the full time period and includes all other reforms and their interactions (results suppressed) and the previous controls as well as state-specific time trends.

For the young & single, caps on total damages, punitive damages reform, joint and several liability reform, and collateral source reform have positive and significant or borderline significant effects for either the six-year window or when all years are used. Also note that the reform main effects are extremely small and statistically insignificant, consistent with the notion that the control group should be largely unaffected by reform.<sup>27</sup>

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<sup>26</sup> This made little difference to most reforms, but was important to identifying the effect of a cap on total damages as few states adopted this reform permanently, but several struck it down quickly.

<sup>27</sup> We ran an additional event study (not reported) on the effect of reform based on previously insurance coverage status. Our hypothesis is that those who previously reported lack of insurance coverage should be more price-sensitive than those who were previously covered. Of CPS respondents who report being uninsured, 45% will report private insurance coverage the next year. Of those who were previously privately insured, only 9% will transition to no coverage. Given the greater amount of churning among the previously uninsured, we would expect the reform to have a greater impact there. We found that reform increased the probability that the previously uninsured transitioned to insurance during the six-year reform window, with no effect on those who previously reported government insurance or private insurance. As the reform window is broadened to all years, the effect largely disappears. This result, however, is expected because private insurance coverage appears to be persistent.

How soon should we expect to see a response? For example, if tort reform reduces defensive medicine, insurance companies must calculate lower costs over time and pass that on to customers, who respond by buying more insurance. This process suggests that the effect of reform should be somewhat delayed, though previous work has found that the coverage rates of the self-employed respond fully within one year of a price change (see Gruber and Poterba 1994, at 716). The event studies show that a response is evident in the three years after the year of passage. Graphical evidence indicates a similar result. The self-employed graphs in particular suggest an effect the year after passage that increases over time, as we would expect. There are a few reasons to suppose the effect is at least a year more delayed than suggested by the graphs or event studies. First, we coded reforms adopted after July 1 as being effective the year after. Second, the CPS March survey retrospectively collects coverage information about the prior year. As mentioned before, there is some evidence that CPS respondents report current insurance status, not that of the prior year. Given this survey quirk and our reform timing, a more accurate presentation of the reform line would be a shift of roughly one to one and a half years to the left. In sum, we believe the response time is plausible.

*Full Estimation of the Effect of Reforms.* Table 3 reports two regressions allowing an independent impact for each reform. Regression 1 uses *Self-Employed* and *Young* as the treatment groups, and Regression 2 uses *Self-Employed* and *Young & Single* as the treatment groups. The results are similar regardless of which treatment group is used. The main effects do not test jointly significant and their point estimates are generally small, indicating that reform had little or no effect outside the treatment groups. This is consistent with the notion that the treatment groups have very low elasticity of coverage. The interaction effects are mostly positive, and, if negative, have very small coefficients. The joint test for significance of the interaction coefficients for the *Young* and *Young & Single* has a p-value of less than .0001 in both regressions, while the joint test of the self-employed is generally weaker, but still significant at the five to six percent level. The

joint test for both treatment groups is quite strong ( $p < .0001$ ). In sum, reforms have a statistically significant impact on insurance coverage rates of the treatment.

In general, the largest impact is found for caps on total damages, punitive damages reform, and collateral source reform. However, the magnitudes are not as consistent as would be hoped across the treatment groups. For example, caps on total damages have a large and statistically significant effect for the *Self-Employed*, but no detectable impact on the *Young* or *Young & Single*. The effect of collateral source rule and punitive damage limitations are more consistent. Some of these inconsistencies may be due to multicollinearity among both reforms and treatment groups. In sum, the coefficients on the treatment group interactions suggest that reform had a positive impact on insurance coverage. The largest estimated coefficients are around 2 percentage points.

Given the strong joint significance of the interaction terms, the inconsistencies in magnitudes and large standard errors are undoubtedly due in part to the multicollinearity of many reforms. To assist with inference, we also test jointly the treatment-reform interactions that the event studies indicated had a large effect: Caps Total, Punitive Damage Reform, Joint & Several Liability Reform, and Collateral Source Reform. With a couple of exceptions, these treatment-reform interaction coefficients were positive in both regressions of Table 3. The separate tests reveal that the reforms indicated as effective by the event studies are highly jointly significant. These reforms also had uniformly positive signs across specifications. We also jointly test the treatment-reform interactions of the reforms that the event studies indicated had a small effect: Periodic Payment Reform and Caps on Non-Economic Damages. The coefficients for these two reforms were often negative. Periodic Payment Reform and Caps on Non-Economic Damages appear to have no jointly statistically significant effect on treatment group coverage rates, with very large p-values in both regressions.

Table 4 reports estimates employing a full set of interactions between the state, treatment, and year dummies.<sup>28</sup> This is particularly important for our self-employed control group because annual changes in the federal tax code may have affected their demand for coverage from year to year. As in Table 3, Regression 1 uses *Self-Employed* and *Young* as the treatment groups, and Regression 2 uses *Self-Employed* and *Young & Single* as the treatment groups. Even under this much more stringent specification, the results of the joint tests are similar to those of Table 3, as are the results of the impact of caps on total damages.

*Plausibility of Estimates.* Our results so far suggest that tort reforms jointly increased private health insurance coverage among the most price-sensitive groups relative to the control groups. Of course, multicollinearity among reforms prevents us from firmly establishing the individual effect of reform. However, we pause here to note the magnitude and plausibility of some of the estimated coefficients. The largest effect of an individual reform on a treatment group was a two percentage-point increase in coverage (the effect of caps on total damages on the self-employed). Given an elasticity of coverage of -1, this suggests that the largest point-estimate for an individual tort reform implies a reduction in health insurance costs of 2%. For the self-employed and the single, the reforms with the largest and most consistent positive effects are the collateral source rule, caps on total damages, and caps on punitive damages. There were no statistically significant negative effects.

Compared to previous estimates of the impact of tort reform on defensive medicine (on the order of 4%-10%), our estimates are substantially smaller. However, this assumes a competitive insurance market. If that assumption is inaccurate, health insurance providers would keep some of the surplus created by lower costs. Hence, our estimates of the price effects are lower bounds.<sup>29</sup>

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<sup>28</sup> The regressions include state-treatment group interactions, year-treatment group interactions, and quartic state trends. We used a fourth-order polynomial state trend because state-year effects were intractable with 22 years of data and 50 states.

<sup>29</sup> Dafny (2008) finds that insurance prices paid by firms are sensitive to profitability in concentrated markets, indicating that some rents are earned by insurers.

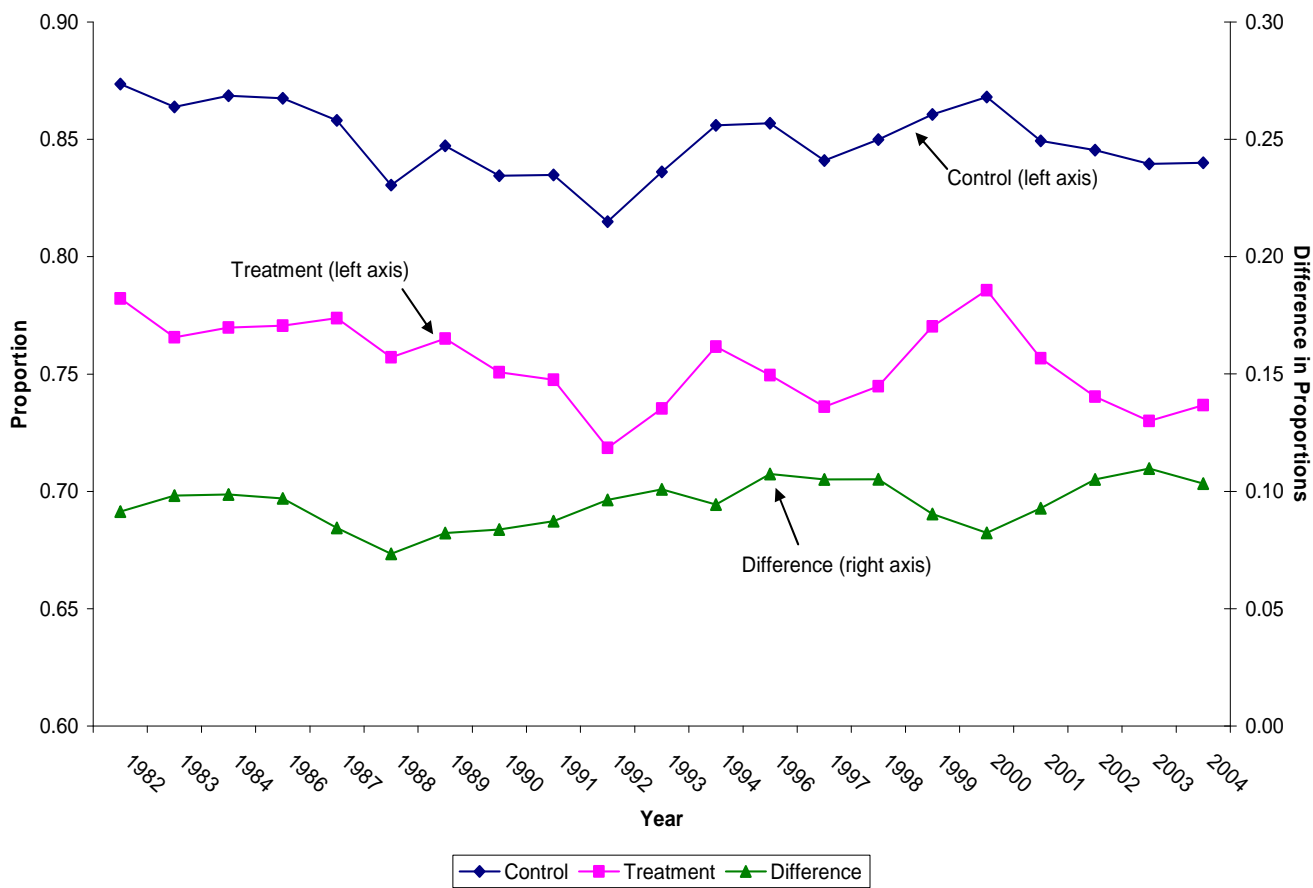
If the results are correct, the reduction in healthcare costs must be generated by changes in doctor behavior as well as savings from liability costs. The implied reduction in healthcare costs far exceeds any savings from reducing the less than 2% of direct liability costs. In other words, if the results are correct, doctors' activity levels must have been affected by reform. For example, assume that reform reduces awards and malpractice costs by one-quarter (a high-end estimate) and had no effect on doctor behavior. This would reduce health care costs by 0.5%, only a fraction of our estimated impact.

## **V. Conclusion**

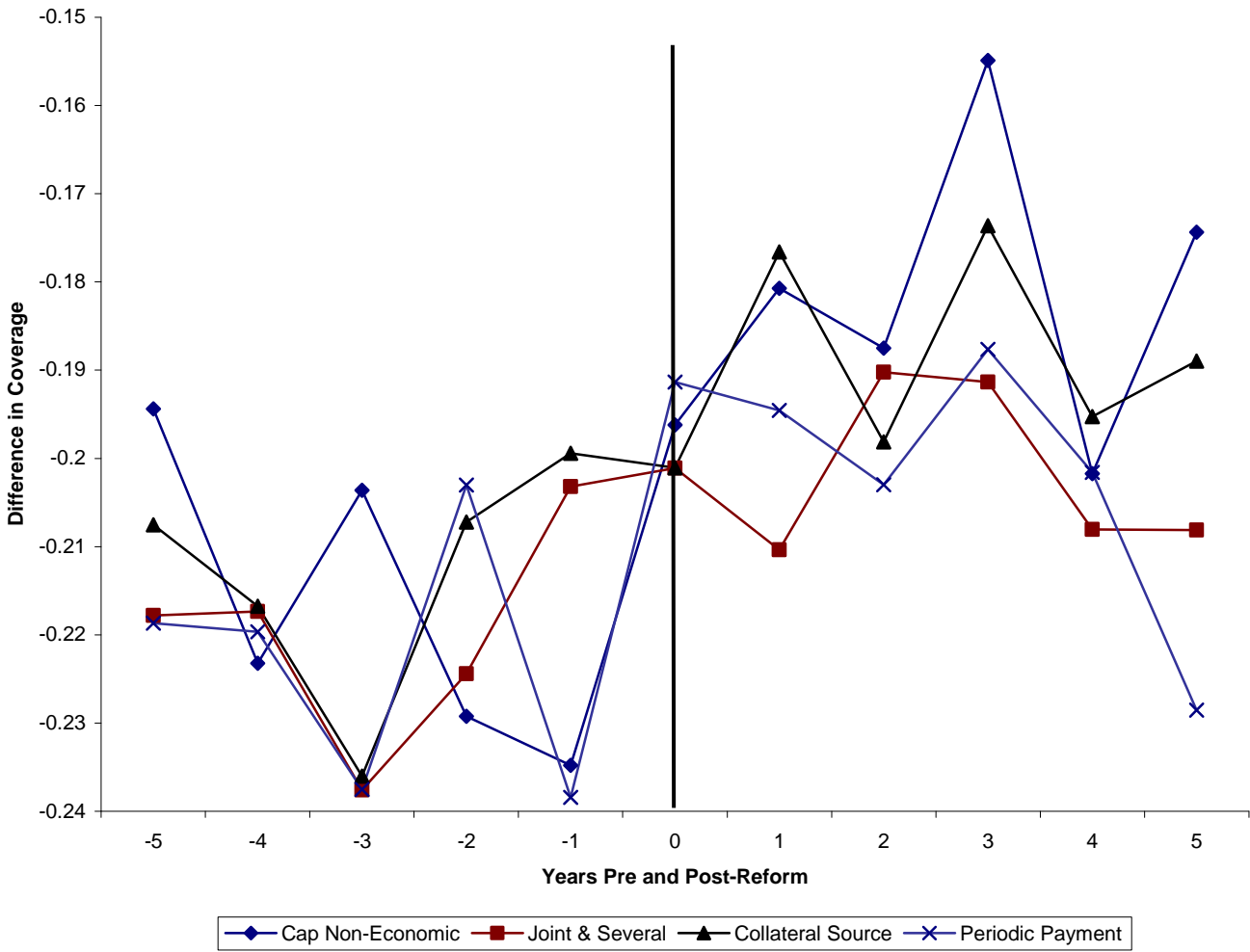
We conclude that tort reform has the potential to increase insurance coverage rates. The magnitude of the increase for the general population appears to be negligible, but for some reforms the coverage rates among the self-employed, the young and the single young increased by .5 to 2 percentage points. Based on the event studies, we also conclude that the effect of reform is both long-lasting and clearly visible a few years after reform. However, even if the magnitude of the increase in private coverage seems small, coverage is relatively inelastic in price, even among our treatment groups. We therefore conclude that tort reform is unlikely to appreciably increase insurance coverage rates for the population as a whole, but may decrease health care costs by an economically significant amount. Although the observed response is mainly from the treatment groups, the underlying cost savings would likely be shared by everyone. Given the substantial cost of medical care, even small reductions in costs translate into significant savings.

Because healthcare costs only recapture a portion of the true costs associated with tort reform, our results do not reflect the full social welfare picture. Pain and suffering and loss of life are not recouped by the insurance coverage analysis. We can conclude, however, that to the extent that treatment intensity or medical complications are increased by tort reform, they do not fully offset the savings from reductions in defensive medicine and liability costs.

**Graph 1: Proportion Covered by Private Health Insurance (Treatment & Control)**



Graph 3: Control Minus Single/Young Coverage Rates



**Table 1: Number of Tort Reforms Effect on Private Insurance**

	Regression 1	Regression 2	Regression 3	Regression 4
Number of Reforms	0.17 (0.13)	0.05 (0.12)	0.22 (0.17)	0.05 (0.12)
Reforms*Self		0.28 (0.17)	0.33+ (0.18)	0.30+ (0.18)
Employed				
Reforms*Young		0.28** (0.08)	-0.33* (0.12)	0.036 (0.079)
Reforms*Single			-0.12 (0.07)	-0.06 (0.10)
Reforms*Single* Young			0.49** (0.14)	0.53** (0.16)
Demographic Controls	Yes	Yes	No	Yes
Joint test of Interactions (p-value)		.0059	.0001	.0006

N=518,490. \*\* sig. at .01; \* sig. at .05; + sig. at .10. Sample is adults aged 18 to 64 who report labor force participation. Dependent variable equals 1 if private insurance coverage is reported, zero otherwise. *Reform*=number of tort reforms adopted (min=0, max=8). All regressions include state and year fixed-effects, state-specific time trends, and treatment main effects. Demographic controls are: high school completion, college (or higher) completion, marital status, self-employment, private employment, or government employment (unemployed are the excluded category), black, Hispanic, and receipt of health insurance from Medicaid, Medicare, and military sources. Continuous variables control for age, age squared, and family income.

**Table 2: Private Insurance Coverage in Reform States—Event Study**

<i>Period Included</i>	Cap Total		Cap Non-Economic		Punitive Damages Reform		Joint & Several		Periodic Payment		Collateral Source	
	±Three Years	All Years	±Three Years	All Years	±Three Years	All Years	±Three Years	All Years	±Three Years	All Years	±Three Years	All Years
Young & Single	-4.24** (0.59)	-3.52* (1.42)	-5.14** (0.91)	-3.52* (1.42)	-5.18** (0.60)	-3.71** (0.63)	-5.34** (1.15)	-5.44** (0.75)	-6.03** (1.90)	-4.73** (0.90)	-5.45** (1.05)	-4.18** (1.05)
Self-Employed	-10.7** (0.80)	-11.9** (1.30)	-10.9** (.90)	-10.7** (1.22)	-10.9** (0.97)	-9.49** (1.10)	-11.5** (1.51)	-11.1** (0.93)	-11.0** (1.52)	-10.2** (1.34)	-10.9** (0.82)	-9.01** (.66)
Reform	1.10 (1.46)	-0.35 (1.30)	0.38 (0.52)	-0.17 (0.67)	-0.011 (0.58)	-1.30+ (0.72)	-0.02 (0.67)	-0.03 (0.62)	-0.003 (1.49)	0.64 (0.54)	-0.44 (0.36)	-2.13* (0.82)
Reform*Self Empl.	3.35 (1.60)	3.93* (1.10)	2.46 (1.60)	-0.34 (1.56)	0.87 (1.02)	0.89 (.78)	1.70 (1.50)	0.29 (1.29)	1.67 (1.38)	1.40 (1.52)	2.04 (1.53)	1.67 (1.15)
Reform*Young & Single	-1.11 (1.37)	0.21 (0.52)	2.00* (0.92)	-0.49 (0.61)	1.84** (0.58)	2.10** (0.65)	1.26 (1.04)	1.78** (0.66)	1.36 (1.20)	0.12 (1.05)	2.44** (0.89)	1.43* (0.71)
Joint Test	.0862	.0046	.0731	.7027	.0113	.0010	.3564	.0217	.2509	.7772	.0368	.1338
Observations	20,907	98,940	90,930	248,469	138,258	393,088	99,701	367,159	59,034	272,489	87,584	204,601

\*\* sig. at .01; \* sig. at .05; + sig. at .10. Dependent variable equals 1 if private insurance coverage is reported, zero otherwise. Sample is adults aged 18 to 64 who report labor force participation. All regressions include the same set of demographic controls, state and year fixed effects, and prior insurance status used in prior tables. The *±Three Years* regressions, however, use only the reform of interest and its interactions, while the *All Years* specifications use the full set of reforms, their treatment group interactions, and state-specific time trends.

**Table 3: Private Insurance Coverage by Reform**

	Regression 1			Regression 2		
	Main Effects	*Self-Employed	*Young	Main Effects	*Self-Employed	*Young & Single
Caps Total	-0.36 (0.99)	2.02* (0.85)	0.10 (0.51)	-0.43 (0.88)	1.84* (0.87)	0.16 (0.52)
Cap Non-Economic	0.32 (0.56)	-0.42 (0.80)	-0.10 (0.66)	0.36 (0.53)	-0.42 (1.10)	0.20 (0.53)
Punitive Reform	-0.42 (0.49)	1.27 (0.89)	1.47** (0.30)	-0.19 (0.47)	1.26 (0.96)	1.67* (0.41)
Joint & Several	-0.61 (0.52)	-0.07 (0.80)	0.16 (0.31)	0.48 (0.51)	0.019 (0.84)	0.59 (0.38)
Periodic Payment	1.08* (0.52)	0.42 (0.85)	-0.14 (0.54)	1.10* (0.52)	0.037 (0.84)	-0.51 (0.51)
Collateral Source	-0.44 (0.60)	0.96 (0.84)	0.57 (0.39)	-.45 (0.58)	1.01 (0.87)	0.95* (0.47)
Joint test (p-value)	.4358	.0537	<.0001	.3745	.0604	.0001
All Interactions		<.0001			.0001	
Cap Non-Econ. & Periodic Payment Interactions		.6739			.7108	
Cap. Tot., Pun. Reform, J&S, & Col. Source Interactions		.0026			.0007	

N=518,490. \*\* sig. at .01; \* sig. at .05; + sig. at .10. Same years as Table 1. Dependent variable equals 1 if private insurance coverage is reported, zero otherwise. Sample is adults aged 18 to 64 who report labor force participation. All regressions include state and year fixed-effects, state fixed-effects, treatment main effects, and Demographic controls. Demographic controls are: dummies for previous insurance status, high school completion, college (or higher) completion, marital status, self-employment, private employment, or government employment (unemployed are the excluded category), black, Hispanic, and receipt of health insurance from Medicaid, Medicare, and military sources. Continuous variables control for age, age squared, and family income.

**Table 4: Private Insurance Coverage by Reform**

	Regression 1			Regression 2		
	Main Effects	*Self-Employed	*Young	Main Effects	*Self-Employed	*Young & Single
Caps Total	-2.73* (1.14)	2.30* (1.32)	0.85+ (0.49)	-1.73 (1.38)	2.93* (1.23)	0.65 (0.51)
Cap Non-Economic	0.49 (0.54)	-0.71 (0.98)	-0.22 (0.33)	0.57 (0.46)	-1.20 (1.10)	0.018 (0.53)
Punitive Reform	0.81 (0.61)	0.79 (1.31)	0.86 (0.48)	-0.18 (0.47)	0.77 (1.32)	0.44 (0.68)
Joint & Several	-0.57 (0.51)	1.82* (1.00)	-0.57 (0.51)	0.48 (0.51)	1.90+ (1.00)	0.20 (0.83)
Periodic Payment	1.83* (0.52)	-1.92 (1.20)	0.58 (0.49)	1.35** (0.44)	-2.09+ (1.23)	-0.03 (0.74)
Collateral Source	-0.75 (0.72)	1.39 (0.91)	0.17 (0.50)	-.076 (0.58)	1.14 (0.04)	0.55 (0.43)
Joint test (p-value)	.4617	.0963	.0440	.0053	.0401	.5825
All Interactions		.0061			.0817	
Cap Non-Econ. & Periodic Payment Interactions		.4127			.2925	
Cap. Tot., Pun. Reform, J&S, & Col. Source Interactions		.0081			.0282	

N=518,490. \*\* sig. at .01; \* sig. at .05; + sig. at .10. Dependent variable equals 1 if private insurance coverage is reported, zero otherwise. Sample is adults aged 18 to 64 who report labor force participation. All regressions include state and year fixed-effects, state fixed-effects, state-treatment group effects, treatment group-year effects, and quartic state trends. Demographic controls are: dummies for previous insurance status, high school completion, college (or higher) completion, marital status, self-employment, private employment, or government employment (unemployed are the excluded category), black, Hispanic, and receipt of health insurance from Medicaid, Medicare, and military sources. Continuous variables control for age, age squared, and family income.

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**Appendix Table 1: Summary Statistics**

	Proportion or Average (S.E.)
<u>Percent Sample Covered by:</u>	
Cap Total	.10
Cap Non-Economic	.29
Cap Punitive	.36
Punitive Evidence	.48
Split Recovery	.08
Joint & Several	.55
Period Payment	.53
Collateral Source	.60
<u>Insurance</u>	
Private Insurance	0.82
Medicare	0.005
Medicaid	0.027
Military	0.031
Uninsured	0.15
Private Insurance cond'l Privately Ins. Last Period	0.91
Private Insurance cond'l Uninsured Last Period	0.46
<u>Demographics</u>	
Privately Employed	0.75
Gov't Employed	0.16
Self-Employed	0.080
College	0.52
High School	0.35
Age	39.8 (12.2)
Young	0.36
Unmarried	0.34
Young & Unmarried	0.19
Black	0.080
Hispanic	0.074
Family Income	53,990 (47,955)



**Appendix Table 3: Changes in Number of States with Reforms (by adoption and judicial reversal)**

Reform \ Year	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	00	01	02	03	04	Total Changes
Cap Non-Economic	3	3	3	3	3	8	17	19	19	17	16	14	14	14	15	18	19	17	17	17	17	17	19	22	39
Joint & Several	4	5	5	6	6	14	22	30	31	33	33	35	35	35	36	36	37	36	36	36	36	37	40	40	38
Collateral Source	15	15	15	15	16	18	25	30	31	33	32	32	31	31	31	31	30	29	29	29	30	31	32	34	31
Cap Punitive	4	4	4	4	6	7	11	15	17	17	17	16	17	17	18	21	23	23	23	25	25	25	27	28	28
Punitive Evidence	5	5	5	6	7	11	16	24	27	27	27	29	29	30	32	34	34	32	32	33	33	33	34	34	33
Periodic Payment	9	9	9	9	11	15	22	28	31	31	31	31	31	31	29	29	29	29	29	29	29	30	31	32	25
Split Recovery	0	0	0	0	0	1	3	4	5	5	5	6	6	5	4	5	5	5	5	5	5	6	6	6	12
Cap Total	6	6	6	6	6	7	9	6	7	7	7	7	7	7	7	6	6	6	6	7	7	7	7	7	9
Contingency Fee	8	8	8	8	10	11	14	14	15	15	15	15	15	15	15	15	15	15	15	15	15	15	15	15	7
Compensation Fund	10	10	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	1

Table 6 presents the enactment and striking down of: Caps Non-Economic Damages, Joint & Several Liability, Collateral Source Rule, Caps Punitive Damages, Punitive Evidence, Periodic Payments, Split Recovery, Caps Total Damages, and Contingency Fee reforms. If a reform was enacted or struck down on or after July 1<sup>st</sup>, it was coded as enacted or struck down at the year after. The last column presents the number of variations of the reform from 1981 to 2004. It includes changes that are not reflected in the table. For example RI struck down collateral source in 2002. But since another state enacted collateral source rule in 2002, the table shows no change. The last column counts this as two changes. The states and years in which reforms were struck down are as follows: Caps on non-economics (SD 1986; FL 1988; WA & MN 1990; NH 1991; AL & OH 1992; IL & OH 1998; OR 2000); Joint & Several (OH 1998); Collateral Source (GA 1991; KS 1993; KY 1995; AL 1997; OH 1998; RI 2002); Caps Punitive (AL 1992; OH, KY, & IL 1998); Periodic Payment (OH & AZ 1995); Split Recovery (NY 1994; CO 1995; FL 1998); Caps Total (FL, TX, & KS 1988; SD 1996).

**Appendix Table 4: Reform Coverage Conditional on Other Reforms**

	Full Sample	Caps Total=1	Caps Non-Econ.=1	Caps Pun.=1	Punitive Evid.=1	Joint & Several=1	Periodic Payment=1	Split Recov.=1	Collateral Source=1
Caps Total	.104	–	<i>.034</i>	<b>.148</b>	.063	.087	.097	<b>.152</b>	.078
Caps Non-Economic	.290	<i>.098</i>	–	.293	<b>.383</b>	<b>.380</b>	<b>.426</b>	.283	<b>.410</b>
Caps Punitive	.358	<b>.508</b>	.363	–	<b>.425</b>	<b>.471</b>	<b>.414</b>	<b>.628</b>	.394
Punitive Evidence	.477	.299	<b>.631</b>	<b>.566</b>	–	<b>.557</b>	<b>.537</b>	<b>.652</b>	<b>.510</b>
Joint & Several	.546	<i>.472</i>	<b>.700</b>	<b>.718</b>	<b>.638</b>	–	<b>.701</b>	<b>.816</b>	<b>.668</b>
Periodic Payment	.540	<i>.507</i>	<b>.781</b>	<b>.612</b>	<b>.597</b>	<b>.680</b>	–	<b>.841</b>	<b>.736</b>
Split Recovery	.079	<b>.120</b>	.078	<b>.140</b>	.109	<b>.119</b>	.123	–	<b>.133</b>
Collateral Source	.600	<i>.462</i>	<b>.851</b>	<b>.660</b>	.642	<b>.734</b>	<b>.835</b>	<b>1.00</b>	–

Values are proportion of sample covered by reform conditional on the other reforms. Bold numbers represent excess coverage of five percentage points or more for conditional sample relative to full sample; italics represent a deficit of coverage of five percentage points or more for conditional sample relative to full.