

# The Impact of Tort Reform on Employer-Sponsored Health Insurance Premiums

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We evaluate the effect of tort reform on employer-sponsored health insurance premiums by exploiting state-level variation in the timing of reforms. Using a dataset of health plans representing over 10 million Americans annually between 1998 and 2006, we find that the most common set of tort reforms during this period reduces premiums of employer-sponsored self-insured health plans by 2.1%. Of the four individual reforms comprising this set, caps on noneconomic damages and collateral source reforms have the greatest impact. We do not find reductions in premiums for fully insured plans, which in our sample are almost entirely Health Maintenance Organizations (HMOs). Further analysis reveals that self-insured HMOs are also unresponsive to reforms. Taken together, these findings suggest that HMOs reduce “defensive medicine,” even absent reform. The results are the first direct evidence that tort reform reduces healthcare costs in aggregate; prior research has largely focused on particular medical conditions. (*JEL* I1, K3, K13, K20)

For many years, policy makers have been searching for measures that will reduce the growth in healthcare costs. One approach that has gained increasing attention is tort reform, a broad term encompassing various laws that limit the

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tort exposure of healthcare providers. Physician groups, private health insurers, and both Presidents Bush and Obama have argued that tort reform will reduce healthcare costs.<sup>1</sup> After a protracted political debate, tort reform was not included in the Patient Protection and Affordable Care Act (PPACA)—a historic healthcare reform bill signed by President Obama on March 23, 2010. Nevertheless, calls for tort reform as a way to reduce healthcare costs have not ceased.<sup>2</sup>

As a theoretical matter, limiting tort liability could increase or decrease healthcare costs. On the one hand, liability incentivizes health providers to take greater precautions and avoid unnecessary or risky procedures. Thus, reducing the liability of providers could increase costly medical errors and the practice of “inducing” demand, that is, delivering profitable but unnecessary and potentially risky treatments. On the other hand, critics of the tort system argue that courts lack the capability to accurately identify negligent care, and further that providers’ sensitivity to liability leads to excessive care (or “defensive medicine”). The net impact of reform is an empirical question of significant policy interest.

In this study, we exploit variation in state-level timing of reforms to assess the impact of specific types of reform on employer-sponsored health insurance premiums. Most nonelderly Americans receive health benefits through such plans, so premiums are a strong indicator of the impact of reforms on aggregate costs. We identify four specific reforms with sufficient enactment/striking activity to be evaluated during our study period: caps on noneconomic damages, caps on punitive damages, collateral source reform, and joint and several liability reform. We match state-level data on the timing of these reforms to a privately gathered national database of insurance contracts agreed upon by a sample of large, multisite employers between 1998 and 2006. We separate the sample into fully insured and self-insured contracts, as both product characteristics and regulations differ by this distinction.<sup>3</sup> We also examine whether the impact of reform varies by plan *type*, specifically managed versus nonmanaged plans. Kessler and McClellan (2002) find that tort reform and managed care are to some extent substitutes for each other. Just as tort reform reduces the incentive for doctors to undertake defensive medicine, Health Maintenance Organizations (HMOs) manage care to reduce unnecessary procedures.

1. President’s Remarks at the 2004 President’s Dinner. Available at <http://www.whitehouse.gov/news/releases/2004/07/20040721-14.html>. <http://www.whitehouse.gov/news/releases/2004/07/20040721-14.html>. Presidential Debate Transcript (10/15/2008). Available at <http://today.msnbc.msn.com/id/27086587/page/4/>. America’s Health Insurance Plans (AHIP) buys advertisements promoting tort reform, arguing that medical malpractice liability has increased the cost of providing health insurance. AHIP (2006) 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>.

2. PPACA of 2010, Pub. L. No. 111-148, 124 Stat. 119. 11th Congress (2010).

3. In a “self-insured” plan, employers are responsible for their employees’ medical costs, though employers retain a health plan as an administrator and may purchase stop-loss coverage. By contrast, in a “fully insured” plan the insurer is responsible for payment.

We estimate that the aforementioned reforms reduce self-insured premiums by at least 2.1%. The two reforms associated with the most robust effects are caps on noneconomic damages and collateral source reforms. There is also evidence, albeit less conclusive, of similarly sized reductions following reforms to joint and several liability as well as the introduction of caps on punitive damages. However, we do not find evidence that any of these reforms affects fully insured premiums. Further examination reveals that HMOs comprise over 90% of our fully insured sample, which suggests that this plan type may be driving the nonresult. Additional support for this hypothesis is obtained by separating responses among the plan types within the self-insured sample. Most of the observed post-reform cost reductions in this sample are attributable to premium decreases for Preferred Provider Organizations (PPOs) rather than managed care plans (HMOs and point-of-service [POS] plans), corroborating the hypothesis that managed care acts as a substitute for tort reform.

The article proceeds as follows. Section 1 provides background on tort reform and reviews relevant prior work. Section 2 discusses the data, Section 3 the identification strategy, and Section 4 the results. Section 5 concludes.

## 1. Tort Reform and Healthcare Costs

Tort reform can affect healthcare costs through two mechanisms: (1) direct liability costs and (2) costs associated with intensity of treatment. Liability costs are composed of medical malpractice premiums, malpractice damage awards in excess of premiums, and the associated litigation costs.<sup>4</sup> These costs are generally believed to comprise a small share of total healthcare costs, at most 2% (Congressional Budget Office 2004). Therefore, tort reform cannot plausibly reduce healthcare costs much by limiting liability costs. If tort reform is to have a substantial effect on healthcare costs, it must affect treatment intensity, which implies that providers must be sensitive to liability pressures.

As noted, the effect of tort reform on treatment intensity is theoretically ambiguous. Indeed, the potential for tort law to improve or distort incentives to provide optimal care has long been debated in both the legal and economic literature. Ideally, tort law encourages providers to take proper precautions and yields improvements in patient care. Although extra care may mean more spending on treatments, it may reduce costs arising from medical errors. There is certainly strong evidence of high rates of preventable medical errors.<sup>5</sup> In recognition of the depth of this problem, the Centers for Medicare and Medicaid Services recently implemented tougher reimbursement policies for treatment provided to correct medical errors.<sup>6</sup> In addition, tort liability may deter

4. The administrative costs of the courts are largely socialized. Of course, plaintiffs bear a portion of the litigation costs as well; we do not explicitly consider these in our discussion.

5. The Institute on Medicine (1999), relying on several epidemiological studies that reviewed actual patient records, concluded that between 44,000 and 98,000 people die each year due to "preventable medical errors." A brief of the report is now available at <http://www.iom.edu/~media/Files/Report%20Files/1999/To-Err-is-Human/To%20Err%20is%20Human%201999%20report%20brief.pdf>.

6. "Not Paying for Medical Errors," *The New York Times*, August 21, 2007.

doctors from pursuing unnecessarily risky but profitable courses of treatment. There is also ample evidence that physicians “induce demand” when there are financial rewards to doing so (see Gruber et al., 1999 for a review).

However, tort law may distort incentives as well. Tort liability may be only weakly correlated with negligent care, yielding few benefits. (The failure of malpractice insurance companies to systematically experience-rate physicians constitutes suggestive evidence for this view.) Worse still, the threat of liability could persuade physicians to prescribe tests and procedures that are not medically necessary, but which may help the physician avoid litigation. Defensive medicine thereby piles on costs borne not by the physician but by the patient and insurers, and by definition provides little (and potentially negative) value to patients.

### 1.1 Tort Reform and Malpractice Liability

Recent work confirms that tort reform is effective in reducing physicians’ liability exposure.<sup>7</sup> In his survey of the literature, Holtz-Eakin (2004) concludes caps on damages are consistently found to reduce the number of lawsuits and the size of awards. A more recent survey finds that, in addition to caps on noneconomic damages, collateral source reforms and joint and several liability reforms are also associated with changes in awards and (more weakly) with physician supply (Mello 2006). Most recently, Avraham (2007) examines medical malpractice settlements using the tort reform data set employed here. He finds that some tort reforms decrease the number of claims by roughly 5%–13% and total annual payouts by more than 15%.<sup>8</sup> The reduction in the probability of lawsuits may have a greater impact on provider behavior than the reduction in awards themselves. Medical malpractice insurance and the bankruptcy remoteness of many assets (such as homes and retirement savings) generally protect physicians from having to pay large awards. As a result, the reduction in the probability of a lawsuit (with its attendant damage to reputation and psychic and time costs of litigation) may be more relevant to providers’ behavior than the actual financial risk they face.

Another strand of this literature assesses the effect of tort reform on medical malpractice insurance premiums. This literature finds limited evidence that

7. 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 a review of previous literature see Holtz-Eakin 2004).

8. Specifically, Avraham (2007) finds that caps on noneconomic damages and limitation of the doctrine of joint and several liability reduced the number of annual payments, and that caps on noneconomic damages and periodic payment reform reduced average awards. Additionally, caps on noneconomic damages were found to reduce total awards, although this finding was only weakly significant. The other reforms had no statistically significant effect on total annual payments. In their working paper, Currie and MacLeod (2006) also find that reforms reduced malpractice payouts.

rates of growth in malpractice premiums have been slower on average in states that enacted caps on pain and suffering damages than in states with more limited reforms.<sup>9</sup>

## 1.2 Tort Reform, Physician Behavior, and Healthcare Costs

As noted earlier, the direct effects of tort reform (on litigation costs, damage awards, and malpractice premiums) are unlikely to generate sizeable decreases in healthcare costs. For this reason, many researchers focus attention on the reaction of providers to tort reform.

Most empirical work on provider responses to tort reform focuses on a specific condition, namely heart disease or pregnancy. The most frequently cited evidence that tort reforms reduce healthcare costs is from Kessler and McClellan (1996). Using data on all elderly Medicare beneficiaries treated for serious heart disease in the years 1984, 1987, and 1990 and a panel of state reforms, Kessler and McClellan find that “direct” reforms (such as limitations on damages) reduce medical costs by 5%–9% within 3–5 years of adoption without substantially affecting mortality or medical complications.<sup>10</sup> Kessler and McClellan (2002) obtain similar estimates using more recent data for the same population, and they also document smaller impacts in states with greater HMO penetration. However, an updated study (also on Medicare heart patients) by Sloan and Shadle (2009) fails to find significant effects of tort reform on cost or outcomes. The results of all of these studies, however, understate the impact of tort reform on heart patients of all ages. Cases involving older patients typically garner lower awards (due, e.g., to lower lost wages and shorter pain and suffering horizons). In addition, both studies lump reforms together into a single measure, reducing the variation in reform that can identify the coefficients of interest, and potentially pooling effective and ineffective reforms together.<sup>11</sup>

The article by Kessler and McClellan (2002) also suggests that the effectiveness of reforms may vary depending on the type of insurance plans in a given state. They find smaller effects in states with deeper managed care penetration. They conclude that tort reform and managed care are partial substitutes. Tort reform reduces the incentives of doctors to supply defensive medicine, whereas managed care may actively prevent doctors from undertaking defensive medicine. Because our data identify plan types, we can directly test this hypothesis by comparing premium changes across these categories (e.g., HMO and PPO).

9. For surveys, see GAO (2003) and Danzon et al. (2004).

10. Kessler and McClellan (1996: 371–2). The “direct” reforms include: caps on pain and suffering damages, caps on punitive damages, abolition of the collateral source rule and mandatory prejudgment interest. The “indirect” reforms include: contingency fee reforms, periodic payments, joint and several liability, and patient compensation funds.

11. For example, caps on noneconomic damages and punitive damage caps are coded as the same reform, but we find weaker evidence of effects induced by the latter (see Sloan and Shadle pg. 485, tbl. 2).

The evidence from a different medical condition, pregnancy, is mixed. Dubay et al. (1999) find that greater liability pressures are associated with increased use of Cesarean sections, and no improvement in infant Apgar scores. Sloan et al. (1995) also find no systematic improvement in birth outcomes (birth weight and Apgar scores) due to greater liability pressure. Consistent with the conclusions of Kessler and McClellan, these findings suggest that tort reforms reduce costs with no adverse effect on outcomes. Recent work by Currie and MacLeod (2008), however, suggests the reverse for some reforms. Currie and MacLeod find that caps on damages and collateral source reform increase the complication rate for deliveries and the use of more expensive procedures. The authors conclude that limitation of liability makes it easier for physicians to pursue riskier procedures that are more remunerative but offer no offsetting benefits to patients. If this result generalizes across specialties and procedures, certain reforms could increase healthcare costs as physicians induce demand, less encumbered by the threat of liability. By contrast, Currie and MacLeod find that joint and several liability reform improves outcomes because it places more liability on the doctor (as opposed to the hospital and other providers), who may have the most control over care.

There is little work on the effect of tort reform on health insurance premiums or other broad measures of healthcare costs. Avraham and Schanzenbach (2010) find that tort reform increases private health insurance coverage rates for price-sensitive groups and cautiously infer that premiums were reduced by tort reform. In contrast, Morrisey et al. (2008) find that caps on noneconomic damages have no statistically significant impact on premiums reported by employers in the annual Kaiser Family Foundation/Health Retirement Education Trust Surveys of 1999–2004. However, there were only five legal changes in their study period, and consequently the standard errors are large relative to the estimated coefficients on reform. Our point estimates, which have tighter standard errors, fall within their confidence intervals. Finally, there are a few recent articles that link higher malpractice payouts to higher expenditures, implying that tort reform should reduce premiums (e.g., Baicker and Chandra, 2006; Baicker et al., 2007). Notably, Baicker et al. (2007) find state-level changes in medical malpractice payouts increase spending on “defensive” care such as imaging and evaluation.

In sum, research on the effect of tort reform on individual conditions is inconclusive. Moreover, it is difficult to estimate the net effect of tort reforms on healthcare costs by extrapolating from studies on specific conditions. There are few studies that consider the effects of tort reform on broader measures of healthcare costs; hence matching the database of tort reforms to our data on health insurance premiums is of particular import. A key disadvantage of this summary measure, however, is that we are unable to assess how reforms impact health outcomes, which are very difficult to aggregate across a broad swath of conditions.

### 1.3 Tort Reform and Insurance Plans

As previously noted, tort law may have different impacts across insurance plans. If managed care plans effectively monitor utilization through reviews,

gatekeepers, or financial incentives, then tort reform and managed care may be substitutes. Research before the mid-1990s found that HMOs reduced treatment intensity by as much as 15% (see Glied 2000 for a literature survey). Articles using data from the last 10 years are more mixed, suggesting that HMOs and other plan types have converged as even indemnity plans have adopted some utilization review (see Glied 2003 for a literature survey). Most starkly, Altman et al. (2003), using a database of Massachusetts state employees, found that although HMOs were substantially less costly, little if any of the difference could be attributed to reductions in treatment intensity. By contrast, Carlin and Town (2008), using claims data from 2002 to 2005 for a large self-insured Midwestern employer, find HMOs use 16% fewer “relative value units”<sup>12</sup> than other plans (which are POS or consumer-directed health plans), conditional on the same health status of the insured population. In sum, the literature suggests that managed care has potential to reduce treatment intensity, but the magnitude is disputed.

Because we can measure the effect of tort reform on HMO premiums versus other types of coverage, such as PPOs, we can directly estimate the effect of tort reform on premiums for different insurance types. We can also distinguish between the funding arrangements for each plan type (i.e., self insured versus fully insured). The responsiveness of self-insured HMO premiums to tort reform may be further attenuated because federal law exempts self-insured plans from state tort liability for wrongful denial of coverage (though provider liability for negligence remains).

## 2. Data

Our primary source is the Large Employer Health Insurance Dataset (LEHID). LEHID contains information on all of the health plans offered by a large and nonrandom sample of employers between 1998 and 2006, inclusive. LEHID is gathered and maintained by a leading benefits consulting firm, and the employers included in the data set have some past or present affiliation with the firm. The unit of observation is the health plan-year. A health plan is defined as a unique combination of employer, market, insurance carrier, and plan type, for example, Company X’s Chicago-area Aetna HMO. We now discuss each of the components that jointly identify this unit of observation in turn.

The full data set includes observations from 813 *employers*. Most employers are large, multisite, publicly traded firms, such as those included on the *Fortune 1000* list. The leading industries represented include manufacturing (110 employers), finance (101), and consumer products (73), although non-profit and government sectors are also represented (43 in the “government/education” category). Employers may enter or exit the sample at any time.

12. Medicare developed the Resource-Based Relative Value Units scale to measure the resources used to provide care, so this implies the gatekeeper function results in fewer resources consumed by enrollees. This result does not incorporate differences in the prices paid to providers, which may compound the cost advantage of HMOs.

The median number of years an employer is present in the sample is 2. One quarter of employers appear in the sample for four or more years. A small number of employers reappear after exiting. Given the frequency of exit and entry into the sample, it is not feasible to restrict analysis to a balanced sample of firms. We revisit this issue in the empirical analysis below.

Geographic *markets* are defined by the source using three-digit zip codes. The 139 markets reflect the geographic boundaries used by insurance carriers when quoting premiums, and collectively cover all of the continental United States, with the exception of a few rural areas. Large metropolitan areas are separate markets, and nonmetropolitan areas are lumped together within state boundaries, for example, “New Mexico–Albuquerque” and “New Mexico–except Albuquerque.”<sup>13</sup>

Each firm that administers any plan in the data is labeled an “insurance carrier.” During the entire study period, there are 357 carriers that serve at least one employer, of which 195 serve 5 or more. The smaller carriers tend to be local or regional firms, or sometimes “third party administrators” who pay claims and contract with another carrier to “rent” its network of providers and associated discounts.

The plan types, ordered from most to least restrictive in terms of provider choice, are HMO, POS, PPO, and Indemnity. HMOs and POS plans control utilization of care through primary-care physicians (gatekeepers). Only in-network providers are covered by HMOs, whereas POS plans provide some coverage for out-of-network providers (once the gatekeeper has approved the service in question). PPOs engage in less utilization management, and like POS plans, typically cover out-of-network care at a reduced rate. Finally, indemnity plans are traditional fee-for-service arrangements in which benefits do not depend on the network status of the provider.

In addition to the elements that jointly define a plan, we have the following variables: *premium*, *insurance type*, *demographic factor*, *plan design factor*, and *number of enrollees*. Premium is expressed as an average amount per enrollee; it therefore increases with the average family size of enrollees in a given plan. Premium combines employer and employee contributions. The definition of premium depends on insurance type, which refers to whether a plan is self-insured or fully insured. Many large employers choose to self insure, outsourcing benefits management and claims administration but paying realized costs of care. Such employers can spread risk across large pools of enrollees, and often purchase stop-loss insurance to limit their exposure.

Demographic factor is a measure that reflects the family size, age, and gender composition of enrollees in a given plan. Plan design factor captures the generosity of benefits for a particular plan-year, including copays. Both factors are calculated by the source, and the formulae were not disclosed to us. The number of enrollees in LEHID plans averages 4.7 million per year. Given an

13. There is only one market that crosses state boundaries, “Massachusetts—Southern and Rhode Island.” A map of the markets is available in Dafny (2010).



average family size above two, this implies roughly 10 million Americans are represented in the sample in a typical year.

Tables A1 and A2 present descriptive statistics for self-insured and fully insured plans respectively. Notable in Table A2 is the steep decline in the number of fully insured plans from 2004 to 2006. This decline mirrors declines observed in data on similar-sized firms from the Medical Expenditure Panel Survey-Insurance Component, which conducts semiregular annual surveys of employers of sampled individuals.<sup>14</sup> There are several possible reasons as to why fully insured plans have become less attractive for large firms, including the diminishing distinction between non-HMOs and HMOs (sometimes available only as a fully insured product), and the potential that insurance carriers are charging increasing risk premia (perhaps due to market power). This is an important subject for future research. For the present study, our main concern is that selection out of full insurance could be correlated with the timing of reform; we address this in the empirical work that follows and find no such pattern.

We also note that some multisite firms may negotiate deals entailing the same premium for a given plan across multiple locations. Unfortunately, we cannot identify observations belonging to such agreements, nor is it possible to use observed premiums to infer which plans are part of such a contract.<sup>15</sup> However, to the extent such agreements take place, our estimates will be conservative: premiums for such plans cannot exhibit state-specific responses to any reforms.

Before proceeding to the analyses, we evaluate the representativeness of the LEHID data. The best source for nationally representative estimates of employer-sponsored health insurance premiums is the annual Employer Health Benefits Survey, sponsored jointly by the Kaiser Family Foundation (KFF) and the Health Research and Educational Trust (HRET).<sup>16</sup> Using these data, KFF-HRET report the average growth in premiums for a family of four. Although we would not expect premium levels to be similar for this sample and the LEHID sample (both because the selection of firms is nonrandom and because family sizes differ across plans), if growth rates are similar this would suggest the results of our study are applicable to a broader sample of employers because all specifications rely on premium growth over time. Figure A1 graphs the annual growth rate for employee-weighted premiums against that reported by KFF-HRET. The trends in both samples are very similar over time. Dafny (2010) also reports that the ratio of sampled enrollees to total insured lives

14. We are grateful to Kosali Simon for tabulating the MEPS-IC data to investigate this trend.

15. We observe the average premium per plan-year, where a plan is a unique combination of employer, market, insurance carrier, and plan type. Even if Company X's Blue Cross HMO has the same premium, by family size, across all markets, the average premium will take on different values across these markets due to differences in family size at each site.

16. The KFF-HRET survey randomly selects public and private employers to obtain national data about employer-sponsored health insurance; approximately 2000 employers respond each year. The data are not publicly available, nor is the sample designed to provide estimates at the market level.

(available at the county-level from the US Census of 2000) varies little across geographic markets.

### 3. Identification Strategy

We identify the effect of tort reform based on changes to state laws. Throughout our analysis, we carefully consider the risk of legislative endogeneity, that is, the possibility that changes in premiums prompt the passage of laws rather than vice versa. This may be of particular concern because at times tort reforms were limited to medical malpractice instead of applying generally (e.g., to product liability and automobile accidents). We will rely on trends in the data immediately before the passage of reforms to assess the likelihood of this threat to the identification strategy.

We conduct our analysis separately for self-insured and fully insured plans. In a “self-insured” plan, employers are responsible for their employees’ medical costs, though employers retain an insurance carrier as an administrator and may purchase stop-loss coverage. By contrast, in a “fully insured” plan, the insurer is responsible for payment. There are three reasons to consider these plan types separately. First, they are different products and hence have different pricing. Reported self-insured plan “premiums” are actually estimates of employers’ projected healthcare expenditures, rather than final figures that reflect all expenses, including risk premiums charged by insurers.<sup>17</sup> Second, self-insured plans are regulated under the Employee Retirement and Income Security Act at the national level and are exempt from state regulations, including health insurance premium taxes and mandated benefits. The regulatory uniformity of self-insured plans reduces the chances for legislative endogeneity of state reforms in this sample. For example, a state that enacts an expensive mandate for infertility benefits may experience large fully insured premium increases and enact tort reforms to offset them. The benefit mandate would not impact self-insured plans in that state. (Of course, we test for the presence of legislative endogeneity empirically, but a priori this concern is lesser in the self-insured sample.) Finally, in our sample, over 90% of fully insured plans are offered by HMOs, which engage in greater utilization review and employ gatekeeper physicians. HMOs have greater incentive and ability to reduce unnecessary care even absent tort reform; hence, responses in the fully insured sample may be attenuated.

#### 3.1 Timing of Tort Reform

We date tort reforms using the third edition of the Database of State Tort Law Reforms (DSTLR 3rd). This data set, discussed at length in Avraham (2010), was assembled by reviewing the laws and court cases of the 50 states (and Washington DC) from 1980 to 2008 and comparing them with existing tort

17. Stop-loss premiums are included in these estimates, but the amount of coverage is not the same across all employers (and some do not purchase any).

law compilations.<sup>18</sup> The process revealed that commonly used dating schemes suffer from missed reforms, missing or erroneously coded effective dates of reforms, and missing or erroneously coded state supreme court decisions striking down or upholding reforms. The DSTLR 3rd edition is the most comprehensive and accurate data set on tort reform legislation to date. Currie and MacLeod (2008) provide independent corroboration of the tort reform timings used here.

There are seven common tort reforms enacted during the period under study. They include a variety of damage caps, damage payment reforms, and reforms of joint and several liability.<sup>19</sup> However, we have sufficient variation during our study period (1998–2006) to analyze only four in detail: caps on noneconomic damages, caps on punitive damages, reform to the collateral source rule, and limits on joint and several liability. (For robustness, we also estimate models using the simple count of the seven common reforms.) Table A3 lists the states enacting and repealing all seven reforms during our study period.<sup>20</sup> Note that reforms are coded as of July 1 and refer to effective dates as opposed to enactment dates. We now describe the four key reforms in greater detail, emphasizing the number of changes to each that occur between 1998 and 2005. (Because premiums for 2006, our final year of premium data, are set in 2005, only changes in law through 2005 are effectively incorporated in our analysis.)

3.1.1 Caps on Noneconomic Damages. Noneconomic damages encompass pain and suffering, loss of enjoyment of life, and loss of consortium. Such damages can comprise a significant fraction of total damage awards and have often been the focus of tort reform advocates.<sup>21</sup> Caps vary across many dimensions. Some reforms impose a cap of a fixed dollar amount, whereas others use a multiplier of the economic damages. Some are indexed to inflation, and some are not. In addition, the level of caps varies from state to state. By 2007, 26 states had capped noneconomic damages. Between 1998 and

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

19. Reforms related to patient compensation funds are excluded from this list for lack of activity during the study period. A patient compensation fund is a state fund that usually pays medical malpractice victims monies if the damages awarded are above some threshold, such as \$400,000. The rationale is that the fund lowers physicians' premiums and spreads the risk of risky medical specialties across the entire population of physicians and taxpayers. In the mid-1980s, a few states changed their policies regarding contingency fees, capping plaintiffs' lawyers' share in the recovery.

20. Because health insurance premiums are set prospectively, our baseline models lag the reform indicators by one year. For this reason, the table and ensuing discussion refer to changes that take effect during the years 1998–2005, inclusive.

21. Avraham (2006) cites sources claiming that noneconomic damages account for up to 50% of total recoveries in product liability cases.

2005, 12 states enacted or struck down caps on noneconomic damages for medical malpractice.

3.1.2 Caps on Punitive Damages. Punitive damages are those in excess of actual losses suffered and are intended, as the name implies, to punish the perpetrator. By 2007, 30 states had enacted caps on punitive damages. Between 1998 and 2005, nine states enacted or struck down caps on punitive damages, one of which applies only to medical malpractice (Maine). Punitive damages are reputed to be rare in the case of medical malpractice. However, as Polinsky (1997) argues, even the remote possibility of large damage claims may have a significant impact on providers' behavior.

3.1.3 Collateral Source Reforms. Discretionary or mandatory consideration of collateral sources of payment for medical costs is one of the most common tort reforms. The collateral source rule was developed by common law courts in the 19th century when insurance became more common. The 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 may receive more than his full harm in the 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). By 2007, 34 states had reformed the collateral source rule in this way. Between 1998 and 2005, six states enacted or struck down the collateral source rule for medical malpractice claims. In addition, Alabama reformed the collateral source rule for all types of claims.

3.1.4 Joint and Several Liability. Under the common law rule of joint and several liability, the plaintiff can collect the entire damages award from any of the defendants found liable in the lawsuit, regardless of their percentage of liability. This allows plaintiffs to pursue defendants with "deep pockets" and collect the full recovery from them. In medical malpractice cases, the deep pockets are likely hospitals and practice groups. States have limited plaintiffs' access to deep pockets by modifying the joint and several liability rule in various ways, primarily by limiting each defendant's liability to its proportional share of responsibility. This forces patients to collect more from individual doctors, who typically bear the largest share of responsibility for negligence. Thus, joint and several liability reform puts greater liability pressure on doctors and reduces that on other providers. However, doctors are not deep pockets, and recoveries from them are often limited to their insurance coverage policy limits because many personal assets are protected in the case of bankruptcy (see Hyman et al., 2007 for a discussion.) Therefore, joint and several liability reform may limit the amount of recovery a plaintiff can make, not unlike a cap.

Between 1998 and 2005, six states enacted or struck down reforms of the joint and several liability rule, one of which applied solely to medical malpractice (Nevada).

### 3.2 Specifications

Our primary specifications regress premiums at the plan-year level on indicators for insurance reforms, which vary at the state-year level.<sup>22</sup> All models are estimated by weighted least squares, using the average number of enrollees in each plan (over time) as weights. The weights help us to obtain representative estimates of the effect of reform and also act to reduce the influence of smaller markets, in which the smaller sample sizes imply a greater risk of measurement error. We report standard errors clustered by state to allow for correlation in the error terms across plans within the same state.

As noted earlier, we estimate all models separately by insurance type (self-insured and fully insured). The baseline specification is as follows:

$$\begin{aligned} \ln(\text{premium})_{p(m)t} = & \alpha + \gamma \text{TortReform}_{mt-1} + \phi \text{demographics}_{p(m)t} \\ & + \vartheta \text{plan design}_{p(m)t} \\ & + \rho_{p(m)} + \tau_t + \varepsilon_{p(m)t} \end{aligned} \quad (1)$$

The subscript  $p(m)t$  refers to a plan  $p$  (which is defined within a particular market  $m$ ) and year  $t$ .  $\text{TortReform}$  is an indicator variable that takes a value of 1 in the year following the implementation of the reform in question. This lag reflects the timing of insurance contracts, which are typically finalized three or more months before the calendar year in which they take effect. Recall also the reform database codes reforms as present if enacted by July. Thus, a reform implemented in June 2002 can impact premiums for 2003 and beyond. (Subsequent specifications include a more flexible lag structure.) We present estimates including each of the four reforms described above singly as well as altogether. In addition, we report specifications using the sum of the seven most common reforms. This index, which incorporates reforms that were not adopted frequently enough during our study period to permit individual consideration, allows us to test the average impact of introducing an additional liability limitation.

Equation (1) also includes the two time-varying plan-specific measures available in the data: demographics and plan design. Both are associated with higher costs and should have positive coefficient estimates.  $\rho_{p(m)}$  and  $\tau_t$  represent plan and year fixed effects, respectively. The estimate of  $\gamma$  therefore captures the impact of reforms on premium growth for the *same* health plan over time, controlling for average national premium growth. (Note that state fixed

22. Of the 139 geographic markets represented in LEHID, 138 can be matched uniquely to a single state. Plans in the Rhode Island/southern Massachusetts market are matched with reform data for Massachusetts; this choice is immaterial as neither state enacted or repealed any of the seven reforms.

effects are superfluous, as plans are specific to markets and markets are finer geographic areas than states.) Including plan fixed effects reduces the potential for omitted variables bias due to changes in the composition of plans over time. For example, if tort reform reduces premiums and prompts more employers to select more expensive carrier-plan type combinations (e.g., the Blue Cross PPO instead of the Humana PPO or HMO), the effect of tort reform on premiums will be understated in the absence of plan fixed effects.

A consequence of these fixed effects is that only employers who appear in the sample for two or more years and who “straddle” one or more reforms can help to identify the coefficients of interest. Although the median firm is in the sample for 2 years, we still have ample identifying variation. Virtually all employers are active in multiple markets, and therefore are quite likely to be affected by tort reform if they are in the sample for more than 1 year because there are 33 separate legal changes (Table A3). In addition, when we estimate specifications that include leads and lags of reform, firms that straddle any of these dummies also contribute to identification.

We estimate equation (1) separately for each reform and each insurance sample (self and fully insured). We also consider models with all reform indicators entered together, as well as a model that uses the sum of the seven most common reforms as an explanatory variable. After estimating these baseline regressions, we expand the model in two ways. First, we include leads and lags of TortReform. The coefficients on the former will reveal the presence of legislative endogeneity, if any, and on the latter the timing of the response to the legislation. On the one hand, a delayed response is likely given uncertainty around the effect of the law and the lag before changes in damage awards, malpractice premiums, and physician behavior are reflected in healthcare costs. On the other, responses could even precede effective reform dates if insurance carriers and self-insured firms incorporate the expected impact of reform in their premiums. Second, we test whether the effect of reform varies by plan type (indemnity, PPO, POS, and HMO). The results will reveal whether reform has less of an impact in a managed care environment.<sup>23</sup>

## 4. Results

### 4.1 Main Findings

Table 1 presents results from our baseline specification, which reflects a simple before–after estimate of the effect of each reform in the self-insured and fully insured samples. In the self-insured sample, three of the four reforms are

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23. We also evaluated whether the competitiveness of local insurance markets affects responses to tort reform. To the extent a local market is more competitive, we would anticipate a greater pass through of any cost reductions resulting from tort reform. Indeed, Dafny (2010) and Dafny et al. (2010) find a causal relationship between market structure and premiums using the LEHID data. Using the number of carriers as a proxy for competition, we found qualitative support for this argument: the coefficient estimates for the least-competitive markets were never statistically significant from zero. However, most of these coefficient estimates could not be statistically distinguished from estimates pertaining to the most competitive markets.

Table 1. Effect of Reforms on Premiums

	Self-insured sample						Fully insured sample					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Caps punitive reform	-0.008 (0.006)					-0.000 (0.006)	-0.009 (0.008)					-0.028 (0.011)**
Collateral source reform		-0.014 (0.005)***				-0.010 (0.005)**		0.010 (0.010)				0.016 (0.013)
Caps noneconomic damages			-0.013 (0.006)**			-0.011 (0.005)**			0.019 (0.013)			0.024 (0.014)*
Joint and several liability				-0.013 (.005)**		-0.002 (0.004)				0.008 (0.011)		-0.009 (0.014)
Sum of seven common reforms					-0.003 (0.002)*						0.005 (0.004)	
Joint test of reforms ( $p$ value)						0.032						0.162
$R^2$	0.789	0.789	0.789	0.789	0.789	0.789	0.858	0.858	0.858	0.858	0.858	0.858

\* $p < 0.10$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

Standard errors in parentheses are clustered by state. All specifications include plan design, demographic factor, and fixed effects for plans and years. The unit of observation is the plan-year. Observations are weighted by the mean number of enrollees in each plan.  $N = 163,857$  for self-insured;  $N = 76,803$  for fully insured.

associated with a statistically significant decline in premiums on the order of 1.3%–1.4%. The exception is caps on punitive damages, for which the point estimate is negative but imprecisely estimated. Column 5 reports the result obtained using the sum of all seven possible reforms, which represents the intensity of tort reforms in a state-year. The point estimate of  $-0.003$  ( $p < 0.10$ ) implies that moving from no limitations on tort liability to seven reduces premiums by 2.1%. Of course, this specification weights all reforms equally. If some reforms have little effect, aggregating them masks the effect of individually significant reforms. However, because reforms are sometimes implemented at the same time, estimating them separately may overstate their impact. Thus, column 6 includes the four key tort reforms from columns 1 through 4 simultaneously. The collateral source rule and caps on noneconomic damages have clear independent effects, whereas the coefficient on joint and several liability is now small and statistically insignificant. Because there are few cases in which joint and several liability reform takes place separately from other reforms, it is difficult to identify its effect. We reject the null hypothesis that all reforms have no impact with a  $p$  value of 0.03.

In the sample of fully insured plans, the coefficients on reforms are not statistically significant when entered separately (columns 1–4) or as a sum (column 5). However, when the four key reforms are entered simultaneously (column 6), the coefficient on punitive damage caps is negative ( $p < 0.05$ ), whereas that on caps on noneconomic damages is positive ( $p < 0.10$ ). The joint test of reforms is not significant, however, with a  $p$  value of 0.16.

Next, we expand the models in columns 1 through 4 by including leads and lags for each reform. Figures 1 and 2 present estimates from these models for the self-insured and fully insured samples, respectively (corresponding to Tables 2 and 3). The excluded category is 3 years or more before reform. None of the leads for the self-insured sample (Table 2) is statistically significant. Thus, there is no evidence that plans in states with future reforms

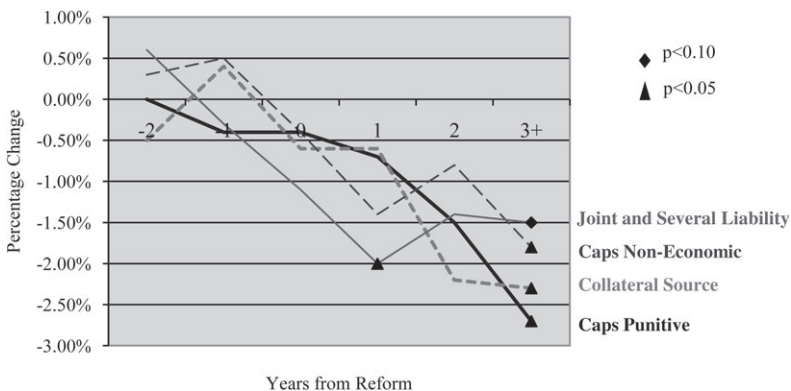


Figure 1. Coefficients on Leads and Lags, Self-Insured Sample.



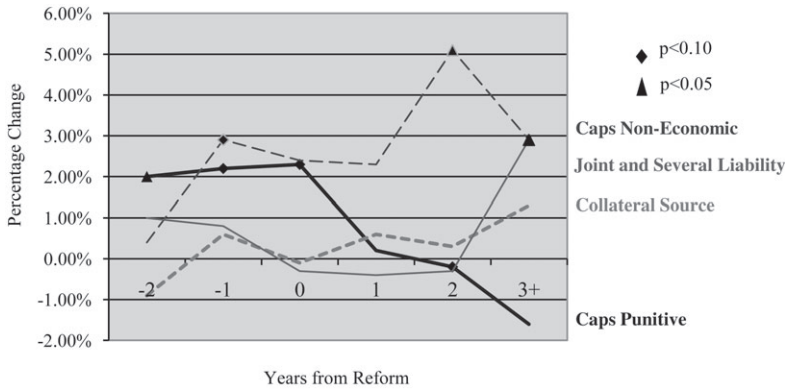


Figure 2. Coefficients on Leads and Lags. Fully Insured Sample.

exhibit different premium growth than plans in states without such reforms. In addition, there is no significant effect of reform in the year of implementation. Based on the point estimates, the premium reduction associated with reforms generally increases slightly over time. By 3 years post-reform, all four of the reforms are associated with significant reductions in premiums. Only reforms to joint and several liability do not appear to have increasing effects over time.

By contrast, in the fully insured sample (results reported in Table 3), we do not find responses to any of the reforms. A pre-reform trend is evident for caps on punitive damages, implying some states may have enacted these reforms in

Table 2. Effect of Reforms on Self-Insured Premiums, by Year

	Caps punitive	Collateral source	Caps noneconomic	Joint and several liability
-2 years reform	-0.000 (0.011)	-0.005 (0.008)	0.003 (0.006)	0.006 (0.010)
-1 year reform	-0.004 (0.016)	0.004 (0.012)	0.005 (0.007)	-0.003 (0.007)
Year of reform	-0.004 (0.009)	-0.006 (0.014)	-0.004 (0.009)	-0.011 (0.008)
+1 year reform	-0.007 (0.012)	-0.006 (0.013)	-0.014 (0.010)	-0.020 (0.006)***
+2 years reform	-0.015 (0.017)	-0.022 (0.015)	-0.008 (0.009)	-0.014 (0.010)
≤+3 years reform	-0.027 (0.013)**	-0.023 (0.009)**	-0.018 (0.009)**	-0.015 (0.007)*
R <sup>2</sup>	0.789	0.789	0.789	0.789

\*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

Standard errors are clustered by state. All specifications include plan design, demographic factor, and fixed effects for plans and years. The unit of observation is the plan-year. Observations are weighted by the mean number of enrollees in each plan. N = 163,857.

Table 3. Effect of Reforms on Fully Insured Premiums, by Year

	Caps punitive	Collateral source	Caps noneconomic	Joint and several liability
-2 years reform	0.020 (0.008)**	-0.009 (0.011)	0.004 (0.012)	0.010 (0.015)
-1 year reform	0.022 (0.012)*	0.006 (0.020)	0.029 (0.016)*	0.008 (0.017)
Year of reform	0.023 (0.012)*	-0.001 (0.028)	0.024 (0.018)	-0.003 (0.024)
+1 year reform	0.002 (0.012)	0.006 (0.026)	0.023 (0.019)	-0.004 (0.014)
+2 years reform	-0.002 (0.014)	0.003 (0.032)	0.051 (0.020)**	-0.003 (0.029)
≤+3 years reform	-0.016 (0.016)	0.013 (0.011)	0.029 (0.022)	0.029 (0.011)***
$R^2$	0.858	0.858	0.858	0.858

\* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Standard errors are clustered by state. All specifications include plan design, demographic factor, and fixed effects for plans and years. The unit of observation is the plan-year. Observations are weighted by the mean number of enrollees in each plan.  $N = 76,803$ .

response to rising fully insured premiums. The pattern of coefficients for the other three reforms do not suggest legislative endogeneity. For caps on non-economic damages, premiums increase 2 years before reform, but remain roughly constant thereafter. Neither collateral source nor joint and several liability reforms show signs of legislative endogeneity, and the latter is associated with a premium *increase* 3 years out. In sum, we find no evidence that tort reform reduces fully insured premiums.

We pause here to note the magnitude and plausibility of some of the estimated coefficients. The point estimates we obtain using the self-insured sample and the model including the four key reforms (Table 1, column 6) sum to -2.3%. These results are smaller than Kessler and McClellan's estimate of a 4%–9% reduction from “direct reforms” for Medicare heart patients. However, the specifications that permit the effect to vary over time reveal deeper reductions three or more years after the passage of reform. We caution that our results are not directly comparable to those of Kessler and McClellan because we examine a slightly different reform mix,<sup>24</sup> Kessler and McClellan measure costs (whereas we measure insurance premiums), and their study pertains to one medical condition and the elderly population. In addition, our results might underestimate the impact of tort reform due to attenuation bias stemming from

24. Kessler and McClellan define “direct reforms” as caps on punitive or noneconomic damages, collateral source reform, or reduction of prejudgment interest. We believe that the abolition of prejudgment interest should have a fairly small impact, as inflation in the years of the study was not high, enabling a fairly straightforward comparison.

Table 4. Effect of Reforms on Self-Insured Premiums, by Plan Type

	Caps punitive	Collateral source	Caps noneconomic	Joint and several liability	Sum of reforms
HMO*Reform	0.000 (0.004)	-0.010 (0.012)	0.032 (0.019)	-0.027 (0.009)***	0.003 (0.006)
POS*Reform	0.021 (0.023)	-0.015 (0.011)	-0.007 (0.007)	-0.002 (0.010)	-0.001 (0.003)
PPO*Reform	-0.032 (0.006)***	-0.018 (0.006)***	-0.033 (0.010)***	-0.020 (0.007)***	-0.008 (0.002)***
Indemnity*Reform	0.017 (0.023)	0.008 (0.015)	-0.008 (0.017)	-0.004 (0.015)	-0.004 (0.006)
$\rho$ Value for H0					
PPO*Reform = HMO*Reform	0.00	0.44	0.01	0.60	0.17
PPO*Reform = POS*Reform	0.04	0.78	0.03	0.19	0.04
$R^2$	0.789	0.789	0.789	0.789	0.789

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

Notes: Standard errors in parentheses are clustered by state. All specifications include *plan design*, *demographic factor*, and fixed effects for plans and years. The unit of observation is the plan-year. Observations are weighted by the mean number of enrollees in each plan. N=163,857.

the inclusion of multisite, single-price contracts in our sample. We discuss this issue in greater detail in the following section.

## 4.2. Extensions and Robustness

4.2.1 Do Effects Vary by Plan Type? Table 4 presents results obtained when interacting the individual reform dummies or the sum of reforms with plan type, using the self-insured sample. (Because over 90% of fully insured plans are HMOs, we lack the power to estimate this model using the fully insured sample.) The coefficients on the interactions with PPO plans are negative and statistically significant in all five specifications. Although the standard errors do not permit strong inferences about *differences* between the categories, hypothesis tests confirm that the PPO interactions are statistically different from the HMO or POS interactions in half of the specifications.

There are no significant reactions among managed care plans (HMO and POS plans), with the exception of joint and several liability reform. We return to this result momentarily. In addition, we do not find significant responses among indemnity plans, although a priori we anticipated a response similar to that of PPOs. However, there was a rapid shift away from indemnity plans over the course of our study period, so these estimates are obtained using a very small sample.

To confirm the robustness of the results by plan type, Table A4 reports the coefficients obtained when estimating this specification on the combined (fully insured + self-insured) sample. The results are strikingly similar to Table 4 and further confirm the hypothesis that HMOs are largely unaffected by reform.

The negative significant effect of joint and several liability reform on HMO premiums may reflect the fact that joint and several liability reform *directly*

reduces liability pressures for HMOs. In recent decades, courts have found HMOs (and not just hospitals) liable under joint and several liability.<sup>25</sup> However, we take the result as suggestive but not conclusive. It is difficult to identify the effect of joint and several liability separately from that of the other reforms (Table 1, column 6). Furthermore, the impact of joint and several liability reform on HMOs is small and imprecisely estimated in the pooled sample in Table A4.

On balance, the results suggest that the premium reductions associated with tort reform are generally concentrated in the PPO category. These findings support the hypothesis that managed care and tort reform are partial substitutes for reducing medical expenditures. In addition, the lack of response by HMO plans in the self-insured sample is consistent with our finding of little effect in the fully insured sample, which is dominated by HMOs. Whether the nonresponse among HMOs is due to utilization controls, recruitment by HMOs of physicians who are less prone to practicing “defensive medicine,” or sorting of less-litigious patients into HMOs cannot, of course, be ascertained from these data.

**Selection Out of the Fully Insured Sample.** Although we have treated the self-insured and fully insured samples separately, among the set of large firms in our sample these two options are substitutes. Thus, high premiums in one sector could spur growth in the other. If movement in and out of these samples is correlated with tort reform, some of our estimated effects could be affected by changes in sample composition. (Given the inclusion of plan fixed effects in all models, such movements are unlikely to affect the results regardless of whether they are correlated with tort reform. It would have to be the case that new self-insured plans spurred by tort reform, for example, have systematically faster or slower growth than pre-existing self-insured plans.) For good measure, however, we examine whether the propensity to fully insure is affected by tort reform. For this analysis, we aggregate the data to the employer-market-year level and estimate models using the proportion of employees enrolled in fully insured plans as the dependent variable. These models include the leads and lags of the individual reforms and employer-market and year fixed effects. Observations are weighted by the average number of enrollees in each employer-market. The results are presented in Table 5. We find no evidence of selection into or out of the fully insured market (or equivalently, out or into the self-insured market) in the years preceding (or following) any of the reforms.

**Changes in Plan Quality.** We control for plan design throughout the analysis, so we can make reliable price comparisons for the same plan over time. One

25. Currie and MacLeod (2008) argue that joint and several liability reform better aligns incentives by placing more liability on the agent doctor, who is better able to prevent harm.

Table 5. Effect of Reforms on Percent of Employees in Fully Insured Plans, By Year

	Caps punitive	Collateral source	Caps noneconomic	Joint and several liability
-2 year reform	-0.004 (0.027)	0.004 (0.020)	-0.002 (0.020)	-0.024 (0.016)
-1 year reform	-0.017 (0.029)	0.026 (0.023)	-0.028 (0.026)	-0.003 (0.011)
Year of reform	-0.054 (0.045)	0.000 (0.021)	-0.019 (0.028)	0.005 (0.014)
+1 year reform	-0.028 (0.046)	-0.011 (0.019)	0.000 (0.042)	0.018 (0.027)
+2 year reform	-0.057 (0.054)	0.004 (0.030)	0.006 (0.043)	0.007 (0.027)
≤+3 years reform	0.010 (0.042)	-0.003 (0.038)	-0.041 (0.034)	-0.031 (0.020)
$R^2$	0.765	0.764	0.764	0.764

$p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$

Standard errors are clustered by state. All specifications include fixed effects for employer-markets and years. The unit of observation is the employer-market-year. Observations are weighted by the mean number of enrollees in each employer-market.  $N = 116,762$ .

concern is that our plan design variable may be an imperfect proxy for plan generosity. For example, one way employers may respond to a decrease (increase) in costs is through more (less) generous benefits. This would lead to downward-biased (i.e., conservative) estimates of the impact of reforms. To gauge whether plan generosity changes over time, Table 6 presents estimates of the baseline specifications using plan design as the dependent variable (rather than a control) and the self-insured sample. Both collateral source and joint and several liability reforms are associated with small but significant increases in plan generosity. To the extent that our measure of plan design is correlated with omitted measures of plan generosity, our estimates of the premium reductions associated with these two reforms will be conservative.

**Influence of Multisite Contracts.** As noted earlier, the presence of multisite contracts spanning multiple states—that is, contracts that name a common price for employees in different states—biases our estimates toward zero. If such contracts are more common among the fully insured, then attenuation bias could explain the nonresponse of fully insured plans rather than the predominance of HMOs in that sample. This is unlikely to be the case. First, our inference regarding the lack of response among HMOs is supported directly in the self-insured sample. Second, the consultancy that maintains LEHID informed us that self-insured plans are actually more likely to be single-price multisite plans. This is unsurprising given one of the key rationales for self-insurance is the ability to offer a uniform benefit package—presumably with uniform prices—across all sites.<sup>26</sup> In addition, self-insured firms are larger, hence more

26. The Self-Insurance Institute of America identifies this as one of the six most common reasons to self-insure (<http://www.siaa.org/i4a/pages/Index.cfm?pageID=4546>, downloaded 3/10/2010).

Table 6. Effect of Reforms on Generosity of Self-Insured Plans

	Dependent variable is "plan design factor"					
	(1)	(2)	(3)	(4)	(5)	(6)
Caps punitive reform	0.002 (0.002)					0.001 (0.001)
Collateral source reform		0.005 (0.001)***				0.003 (0.002)
Caps noneconomic damages			-0.001 (0.002)			-0.003 (0.002)
Joint and Several liability				0.004 (.002)**		0.003 (0.003)
Sum of reforms					0.001 (0.001)	
Joint test of reforms ( <i>p</i> value)						0.010
$R^2$	0.279	0.280	0.279	0.279	0.279	0.280

\* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Standard errors in parentheses are clustered by state. All specifications include demographic factor and fixed effects for plans and years. The unit of observation is the plan-year. Observations are weighted by the mean number of enrollees in each plan.  $N = 163,857$ .

likely to have multiple sites for which premiums could potentially be jointly negotiated.

To supplement the consultancy's assessment, we compared the prevalence of observations that are potentially jointly priced in the fully insured and self-insured samples. Although we cannot directly identify such plans, observations corresponding to carrier-plantype-year-plandesign combinations that appear in more than one state per *employer* are more likely to be jointly negotiated. As an example, consider an Aetna HMO with plandesign of 1.01 that is offered by the same employer in both California and Nevada; we posit that such plans are more likely to share a common price. Although we find a greater prevalence of these plans in the fully insured sample, the difference is not great (56% of enrollees as compared to 45% in the self-insured sample). We emphasize that these are overestimates of the percentage of jointly negotiated premiums, as many of these multistate plans may in fact have different prices.

## 5. Conclusion

Tort reform is among the most popular cost-control initiatives implemented by individual states and was seriously considered by the President and Congress during the policymaking process that culminated in the passage of PPACA. However, most empirical estimates of the impact of reforms are based on particular conditions with heavy malpractice caseloads, namely deliveries and heart disease. These may not be representative of health care at large and have led to wide variations in the estimated impact of reform. For example, in 2002, the Department of Health and Human Services asserted that tort reform could reduce healthcare costs by 5%–9%, whereas the Congressional Budget Office (2004) concluded reforms would have little impact. The Department of Health and Human Services study appears to be an extrapolation from Kessler and McClellan's study of heart patients,<sup>27</sup> whereas the Congressional Budget Office based its analysis on a broader set of health conditions. In a recent letter to the Congress, the Congressional Budget Office (2009) revised its assessment in light of recent studies, including this one.<sup>28</sup> They currently project that savings from a broad package of federal tort reforms would be closer to 0.5% of total national health spending. Because this estimate reflects the fact that many states have passed some or all of the reforms, it is consistent with our ~2

27. US Department of Health and Human Services (2002). *Confronting the New Health Care Crisis: Improving Health Care Quality and Lowering Costs by Fixing Our Medical Liability System* (2002). <http://aspe.hhs.gov/daltcp/reports/litrefm.pdf>, p. 5. Congressional Budget Office. *Limiting Tort Liability for Medical Malpractice*. Washington, D.C.: Congressional Budget Office (2004). Available at <http://www.cbo.gov/ftpdocs/49xx/doc4968/01-08-MedicalMalpractice.pdf>.

28. Letter to Sen. Orrin G. Hatch, October 9, 2009, available at [http://www.cbo.gov/ftpdocs/106xx/doc10641/10-09-Tort\\_Reform.pdf](http://www.cbo.gov/ftpdocs/106xx/doc10641/10-09-Tort_Reform.pdf).

percentage point estimate, particularly given that fewer than half of enrollees in employer-sponsored plans are in nonresponsive fully insured plans.<sup>29</sup>

To assess the aggregate impact of reforms, we make use of a proprietary database of employer-sponsored health plans covering over 10 million non-elderly Americans each year, from 1998 to 2006 inclusive. To the extent private insurers pass through cost changes associated with reforms, premiums will reflect the impact of reforms across the entire spectrum of healthcare expenditures. We find evidence that the four most commonly implemented reforms (in recent years) are associated with decreases in self-insured premiums of 1–2 percentage points each. The effect of reform tends to increase over time. Because reforms are sometimes implemented in tandem, however, there is only sufficient independent variation in two of these to conclusively link them to cost reductions: caps on noneconomic damages and collateral source reforms. The point estimates imply a joint reduction of healthcare costs of at least 2.1%.

The magnitudes of the estimated effects of tort reform far exceed any savings that reform could generate from reducing direct liability costs. For example, suppose caps on noneconomic damages reduce awards and malpractice costs by one-quarter (a high-end estimate). Total direct liability costs (awards, insurance premiums, litigations costs) are estimated to be only two percent of total healthcare costs. If a cap on noneconomic damages acted solely to reduce direct liability costs with no change in physician behavior, the reform would reduce health care costs by 0.5%, which is only a fraction of our estimated impact of imposing these caps (~1.3%). In other words, if the point estimates are correct, treatment intensity must have been affected by reform.

By contrast, we do not find evidence of reductions in fully insured premiums. The absence of a response is consistent with monitoring of medical practices by HMOs, which comprise 90% of fully insured plans. We corroborate this conjecture by finding that cost reductions in our self-insured sample are concentrated outside of managed care plans. Thus, we conclude that tort reform reduces medical costs outside of a managed care environment, implying that providers are most responsive to liability pressures when they have the most control over treatment choices. These results constitute the first evidence that tort reform reduces healthcare expenditures broadly (albeit not in a managed care environment). Importantly, our results suggest the savings generated by tort reform are not offset by increases in induced demand or in short-run remediation of medical errors. To understand the social welfare implications of these reforms, however, additional research on health outcomes and long-run costs is needed.

29. The percent of covered workers in fully insured plans has decreased steadily over the past decade, from 56% in 1999 to 43% in 2009 (Kaiser Family Foundation/Health Research and Educational Trust Survey of Employer-Sponsored Health Benefits, Exhibit 10.1.)



Table A1. Descriptive Statistics: Self-Insured Plans

	1998	1999	2000	2001	2002	2003	2004	2005	2006
Premium (\$)	4277 <i>1134</i>	4306 <i>1230</i>	4580 <i>1319</i>	5012 <i>1357</i>	5644 <i>1437</i>	6460 <i>1582</i>	7000 <i>1701</i>	7460 <i>1799</i>	7892 <i>1948</i>
Number of enrollees	191 <i>726</i>	159 <i>602</i>	151 <i>507</i>	169 <i>576</i>	167 <i>612</i>	175 <i>639</i>	170 <i>530</i>	192 <i>862</i>	179 <i>610</i>
Demographic factor	2.42 <i>0.52</i>	2.29 <i>0.45</i>	2.26 <i>0.45</i>	2.27 <i>0.44</i>	2.28 <i>0.46</i>	2.30 <i>0.42</i>	2.32 <i>0.43</i>	2.31 <i>0.43</i>	1.84 <i>0.40</i>
Plan design	1.02 <i>0.07</i>	1.01 <i>0.07</i>	0.99 <i>0.08</i>	1.02 <i>0.07</i>	1.03 <i>0.07</i>	1.02 <i>0.08</i>	1.01 <i>0.08</i>	0.97 <i>0.08</i>	0.97 <i>0.08</i>
Plan type (%)									
HMO	2.5	4.3	6.9	10.0	14.6	16.7	17.5	18.0	19.7
Indemnity	34.5	31.8	22.1	16.7	14.2	10.5	7.8	5.9	5.9
POS	36.1	27.4	30.5	25.3	20.6	18.0	18.2	16.0	15.9
PPO	26.8	36.5	40.5	48.0	50.6	54.8	56.4	60.0	58.5
Number of employers	180	193	191	233	248	315	238	256	222
Number of markets	139	139	139	139	139	139	139	139	139
Number of observations	12,194	14,117	14,408	18,460	21,303	24,853	20,211	20,794	17,517

All statistics are unweighted. The unit of observation is the plan-year, unless noted otherwise. Sample includes all self-insured plans. Standard deviations are reported in italics. Demographic factor reflects age, gender, and family size for enrollees. Plan design measures the generosity of benefits. Both are constructed by the data source and exact formulae are not available.

Table A2. Descriptive Statistics: Fully Insured Plans

	1998	1999	2000	2001	2002	2003	2004	2005	2006
Premium (\$)	3648 <i>995</i>	3904 <i>919</i>	4188 <i>1010</i>	4621 <i>1131</i>	5339 <i>1378</i>	5995 <i>1465</i>	6689 <i>1818</i>	7181 <i>2053</i>	7608 <i>2251</i>
Number of enrollees	170 <i>487</i>	173 <i>489</i>	164 <i>420</i>	180 <i>489</i>	190 <i>499</i>	190 <i>561</i>	176 <i>504</i>	214 <i>695</i>	236 <i>749</i>
Demographic factor	2.23 <i>0.44</i>	2.22 <i>0.39</i>	2.20 <i>0.39</i>	2.23 <i>0.38</i>	2.26 <i>0.39</i>	2.28 <i>0.39</i>	2.36 <i>0.40</i>	2.33 <i>0.42</i>	1.84 <i>0.38</i>
Plan design	1.12 <i>0.05</i>	1.13 <i>0.04</i>	1.11 <i>0.04</i>	1.13 <i>0.04</i>	1.12 <i>0.04</i>	1.11 <i>0.04</i>	1.09 <i>0.08</i>	1.07 <i>0.06</i>	1.05 <i>0.08</i>
Plan type (%)									
HMO	88.7	90.0	92.6	91.9	90.9	92.7	85.7	90.5	88.8
Indemnity	3.0	0.6	0.4	0.1	1.0	0.0	1.9	0.6	0.4
POS	6.6	6.7	4.0	4.8	3.0	4.2	4.0	4.5	3.9
PPO	1.7	2.4	2.9	3.2	5.0	3.0	8.4	4.4	6.8
Number of employers	181	197	185	226	226	274	194	203	173
Number of markets	139	139	139	139	139	137	138	138	137
Number of observations	9880	11561	9253	10,654	10,236	8839	6364	5679	4337

All statistics are unweighted. The unit of observation is the plan-year, unless noted otherwise. Sample includes all fully insured plans. Standard deviations are reported in italics. Demographic factor reflects age, gender, and family size for enrollees. Plan design measures the generosity of benefits. Both are constructed by the data source and exact formulae are not available.

Table A3. Database of State Tort Law Reforms, Third Edition

Year		1998	1999	2000	2001	2002	2003	2004	2005	Flips*	Total
Cap noneconomic	Enactments			ME			FL, OH, MS	OK, TX	GA, NV, TN	9	12
	<i>Strike downs</i>	<i>IL, OH</i>		<i>OR</i>						3	
Cap punitive	Enactments	AK		AL, ME			AR, MS	ID, MT	OH	8	9
	<i>Strike downs</i>	<i>OH</i>								1	
Collateral source	Enactments				AL	OH, PA	WV	OK		5	6
	<i>Strike downs</i>	<i>OH</i>								1	
Joint and several	Enactments					PA	AR, OH, NV			4	6
	<i>Strike downs</i>	<i>IL, OH</i>								2	
Periodic payment	Enactments					PA	OH	TX	GA	4	5
	<i>Strike downs</i>								AL	1	
Split recovery	Enactments	AK				PA			CA, MT	4	5
	<i>Strike downs</i>	<i>FL</i>									
Punitive evidence	Enactments			FL			AR			2	3
	<i>Strike downs</i>	<i>KY</i>								1	

States in which a reform was struck down are denoted by italics.

\*Because insurance premiums are set the year prior and our insurance data run from 1998 to 2006, the relevant years for identification are changes from 1998 to 2005.

Table A4. Effect of Reforms on Premiums, by Plan Type (self + fully insured samples)

	Caps punitive	Collateral source	Caps noneconomic	Joint and several liability	Sum of reforms
HMO Reform	-0.010 (0.005)*	-0.001 (0.009)	0.023 (0.014)*	-0.006 (0.010)	0.004 (0.004)
POS Reform	0.025 (0.023)	-0.009 (0.011)	-0.003 (0.007)	0.003 (0.010)	0.001 (0.003)
PPO Reform	-0.030 (0.006)***	-0.013 (0.008)*	-0.033 (0.010)***	-0.017 (0.008)**	-0.007 (0.003)***
Indemnity Reform	0.020 (0.022)	0.013 (0.016)	-0.006 (0.017)	0.007 (0.016)	-0.002 (0.006)
$\rho$ Value for H0					
PPO Reform = HMO Reform	0.00	0.15	0.01	0.43	0.06
PPO Reform = POS Reform	0.01	0.71	0.01	0.13	0.02
$R^2$	0.810	0.810	0.811	0.810	0.810

Standard errors in parentheses are clustered by state. All specifications include plan design, demographic factor, and fixed effects for plans and years. The unit of observation is the plan-year. Observations are weighted by the mean number of enrollees in each plan. N=240,660.

\* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

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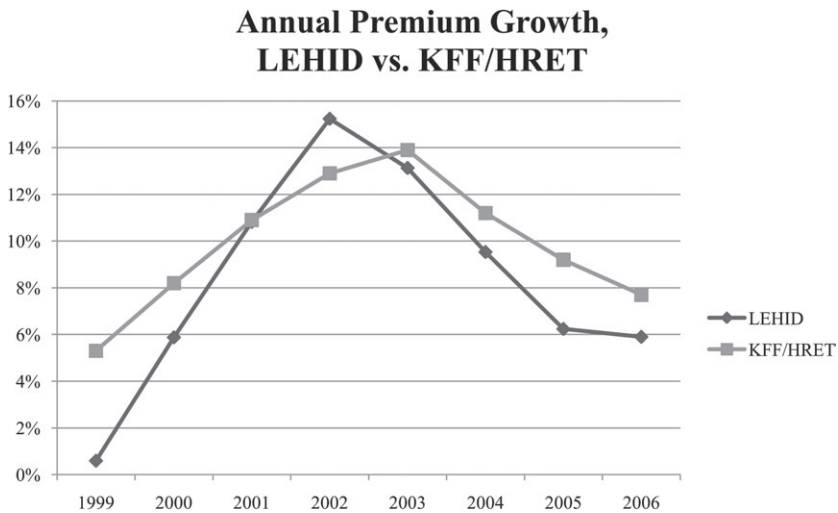


Figure A1. Annual Premium Growth, LEHID versus KFF/HRET. Sources: LEHID sample (all plans), and 2007 Kaiser/HRET Annual Survey of Employer-Sponsored Health Benefits. Annual growth rates for the LEHID sample are calculated using employee-weighted average premiums for each year. Both sources combine fully insured and self-insured plans.

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