The Impact of Non-Economic Damages Cap on Health Care Delivery in Hospitals*

Anca M. Cotet

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Abstract

By reducing the cost of malpractice, tort reforms affect physicians' incentives and treatment choices. However, the prior literature focused on narrowly defined treatments has reached conflicting conclusions about the association between reforms and treatment intensity. This paper evaluates the impact of non-economic damages caps on broadly defined measures of health care delivery in hospitals. Using county level panel data I find that caps adoption leads to a 3.5 percent decrease in surgeries, a 2.5 percent decrease in admissions, a 4.5 percent decrease in outpatient visits but has no significant effect on emergency care. Although there is some evidence of spillover effects from reforms adopted in bordering states, such spillovers are not a significant source of bias. The reduction in hospital utilization rates is not driven by an improvement in health outcomes and there is evidence of an increase in mortality from complications of medical and surgical care two years after the adoption of non-economic damages caps. (JEL: I11, K13)

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I. Introduction

Health and health care are issues at the top of many policy makers' agendas, and this is for good reason: As health status significantly affects general well being, health care often accounts for one of the largest shares of spending, for both individuals and governments. As a result, health care has become not only a prominent issue for many policy makers, but also a controversial one. While most people agree on the objective--to improve access to health care--there is not much consensus about how best to achieve this goal. Various regulations aim to improve access to health care by lowering either the monetary or the time cost of medical care. Among these, tort reforms, such as the introduction of caps on non-economic damages in medical liability cases, aim to lower both.

Since medical liability insurance is an important component of the operating cost of a medical practice, legislation that reduces malpractice insurance premiums increases the profitability of the medical profession and should induce entry into the medical field. The existing literature, as reviewed below, confirms that states that have adopted non-economic damages caps experienced an increase in number of physicians. The drawback is that by lowering insurance premiums, such reforms reduce the cost of malpractice and, thus, change physicians' incentives. Earlier studies have found that malpractice liability and tort reforms lead to changes in the health care production process. The extent of changes varies across types of medical services, however, because caps are more likely to be binding for certain medical specialties.

Moreover, previous work concentrating on specific procedures has generated conflicting results: In some fields, tort reforms led to more intensive care (Currie and Macleod, 2008), while in other fields to less intensive care (Kessler and McClellan, 1996). Moreover, an added complication is that changes in treatment patterns can affect the demand for medical care. If these changes occur, measuring the impact of caps on the supply of physicians may not be able to capture the impact on medical care actually delivered to a population.

This paper investigates the impact of non-economic damages caps on broadly defined medical care utilization rates: hospital admissions, surgeries, and outpatient visits, providing a more comprehensive picture of the impact of this legislation. On the statistical side, such an investigation faces some challenges. First, there may be spillover responses from neighboring states that have either adopted or repealed caps. Second, in many instances, several types of reforms have been enacted simultaneously, thus making it difficult to disentangle the impact of an individual reform. Third, the adoption of caps may be related to the provision of medical care, raising concerns about identifying the exact causal relationship.

This paper addresses these concerns with a panel of county-year observations for the 1990-2006 period, by using statistical models that include county and year fixed effects and state specific trends, and controls for the existence of caps in bordering states. County-level data make it possible to control for small-area-specific factors, such as variations in a county's culture of litigiousness, which has been shown to be an important determinant of malpractice claims (Hart and Peters, 2008) and, thus, of the cost of medical care. County fixed effects adjust for any such differences in unobserved factors that may influence medical care utilization rates. Year fixed effects control for common shocks affecting the medical care, such as changes in health care policy at the federal level. State-specific trends control for one source of selection that would make a state more likely to adopt caps. In this specification, the impact of tort reform is identified from year-to-year changes in legislation after controlling for state-specific trends and shocks common to all counties, so concerns about omitted variable bias are likely to be limited.

There is also limited concern about bias from reverse causality, because the literature indicates that tort reforms were mainly driven by the relative power of diverse interest groups (Rubin, 2005). Nevertheless, any such concerns are further addressed in several ways. First, while tort reform may be endogenous at the state level, this paper uses disaggregated county-level data to mitigate such concerns. Second, I present evidence that non-economic damages caps are statistically unrelated to past trends in medical care utilization rates, while current and past reforms predict future trends in utilization rates.

This paper complements previous studies that investigate the impact of tort reforms on health. These studies have found that states that adopt non-economic damages caps do have more physicians, but there is no evidence of any resulting gain in health. A number of factors might explain this result of more doctors but no health improvement. First, an increase in doctors does not necessarily mean that more medical services are actually delivered. It could be that the demand for medical care is very inelastic (Manning et al., 1987), so that changes in supply do not produce large effects on the quantity of medical care delivered. Second, an increase in medical care utilization is not always accompanied by improved health (Newhouse, 1993) because medical care is not free of risk. Third, economic models of health production

(Grossman, 1972) predict that changes in relative prices lead to substitutions between medical care and other inputs used in health production. Consequently, even if tort reforms do affect access to medical care, there may not be large impacts on health. Fourth, changes in treatment patterns, and with them, changes in the expected quality of health care, can negatively affect medical care utilization rates. This paper finds that caps have a net negative impact on utilization rates of some types of medical services, which is suggestive of the latter proposed explanation, but does not reject the hypothesis that the other factors also play a role.

Overall, the analysis suggests that non-economic damages caps have a significant negative effect on the number of surgeries and hospital admissions. In addition, caps have no statistically significant effect on emergency visits but a negative statistically significant effect on hospital outpatient visits other than emergency.

The rest of the paper is structured as follows. Section II reviews existing evidence on the impact of non-economic damages caps; section III details data sources; section IV presents the empirical strategy used to investigate the questioned effect; section V presents the results; and section VI concludes.

II. Background on Non-Economic Damages Caps

In order to make medical care more accessible, policy makers have focused on measures with the potential to reduce the price of care. Among these, non-economic damages cap reduces the damages awarded in malpractice cases and with these, the medical malpractice insurance premium, an important component of the cost of medical practice.[1] The justification behind non-economic damages caps is that juries face significant difficulty when assessing the value of non-economic losses. This then generates the claim that awarded compensations for such damages should be bounded in order to offer juries guidance in evaluating non-economic losses.

The success of non-economic damages caps depends on several factors.

2.1. Caps must be binding.

Several studies have found such evidence: Sloan, Mergenhagen, and Bovbjerg (1989) find that non-economic damage caps reduce insurer payouts; and Zuckerman, Bovbjerg, and

[1] Non-economic damages compensate for past or future non-economic losses, such as pain, suffering, emotional distress, mental anguish, disfigurement, physical impairment, loss of consortium, loss of companionship, loss of parental guidance, loss of enjoyment of life, loss of society, humiliation, embarrassment, inconvenience, injury to reputation, and other such losses (Pace et al., 2004).

Sloan's (1990) results indicate that caps decrease the average indemnity per claim.[2] More recently, Currie and MacLeod (2006) conclude that reforms reduce malpractice payouts, and Avraham (2007) finds that non-economic damages caps reduce both the number of annual payments and the average award. Hyman et al. (2009) reports that, in Texas, caps reduced both verdicts and payouts.

In addition, reduced awards also decrease incentives to sue. Analyzing data from the American Medical Association Socioeconomic Monitoring System (AMA SMS) survey, Kessler and McClellan (1997) find that general reforms reduce the probability that physicians will be sued. Browne and Puelz (1999) suggest that non-economic damages caps lead to a significant reduction in the number of court cases filed. More recently, however, Donohue and Ho (2007) report that over the 1991 to 2004 period, there is no statistically significant change in malpractice claims against physicians associated with damages caps adoption. A potential reason is that while reduced awards decrease incentives to sue, a change in treatment patterns associated with a higher incidence of mistakes could lead to an increase in the number of cases filed.

2.2. Insurance companies pass some of the savings from reduced awards onto their customers, the physicians, in the form of lower insurance premiums.

An extensive literature documents the chain that links non-economic damages caps and insurance premiums. Using state-specific National Association of Insurance Commissioners (NAIC) data from 1985 to 2001, Thorpe (2004) finds that premium rates were lower in states that regulated the amount of non-economic damages. Over a similar period of time, 1994 to 2003, Danzon et al. (2004) find significant reductions in premium increases in states that adopted caps on awards for non-economic damages at or below $500,000. Baicker and Chandra (2005a) find that increases in malpractice payments do not result in an increase in premium rates; however, Viscusi and Born's (2005) study reports that in the 1984-1991 period, insurers from states with caps on non-economic damages had 17% lower losses and 6% lower earned premiums. Overall, these studies suggest that caps reduce insurance companies' payments in malpractice cases and that part of their gain is passed to their customers, physicians, in the form of lower premiums.

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[2] The results hold across several data sources: Sloan, Mergenhagen, and Bovbjerg (1989) use National Association of Insurance Commissioners (NAIC), while Zuckerman, Bovbjerg, and Sloan (1990) use per-physician premium data from the Health Care Financing Administration survey of insurers.

If the medical profession becomes more profitable, the number of medical care providers should increase in states that adopt damages caps, thus lowering the transportation and time costs associated with the consumption of medical care (Dranove and Gron, 2005). Several studies suggest that this is indeed the case. Mello and Kelly (2005) find that some physicians avoid certain jurisdictions because of high malpractice premiums. Klick and Stratmann (2005) and Encinosa and Hellinger (2005) report that states with caps have more doctors. Kessler, Sage, and Becker's (2005) study also provides support for the view that tort reforms have increased the supply of physicians. Wolfson (2005) finds that non-economic damages caps improve minority access to medical care. Helland and Showalter (2006) estimate physicians' responsiveness to changes in liability and find that a 10 percent increase in expected liability cost is associated with a 2.85 percent decrease in hours worked. Such changes in supply result mainly from an increase in the number of physicians in high-risk specialties (Klick and Stratmann, 2007) and are more likely to occur in regions previously lacking a provider (Matsa, 2007). In contrast, Yang et al. (2008) find no evidence that tort reform increased the number of obstetrician-gynecologists between 1992 and 2002.

2.3. Physicians whose operating costs have been reduced pass some of these savings along to their consumers.

An increase in supply should decrease the price of medical care. There is evidence that damage caps, collateral source reform, and joint and several liability reforms increase health insurance coverage for the most price-sensitive groups (Avraham and Schanzenbach, 2010).[3]

2.4. Changes in treatment patterns do not offset the effect of the increased number of physicians.

By lowering the cost of malpractice, caps produce changes in physicians' incentives that could lead to changes in the process of health care delivery. The usual indicators of quality alteration are changes in technical aspects of health care delivery and changes in outcomes (Baker and McClellan, 2001), and evidence suggests that fear of malpractice affects treatment patterns. For instance, higher malpractice premiums are associated with an increased use of diagnostic and imaging procedures (Baicker and Chandra, 2005b; Baicker et al, 2007) and C-sections (Dubay et al., 1999, Grant and McInnes, 2004). Overall, as many as 93% of physicians

[3] This is consistent with Roberts and Hoch's (2009) results indicating an association between a different measure of malpractice litigation pressure and Medicare Part B (outpatient) expenditures.

report that the fear of being sued has affected their decisions (Studdert et al., 2005), although such self-reports can be self-serving.

There is concern that, by lowering the cost of malpractice, caps affect physicians' choices of treatment. For instance, Currie and Macleod (2008) find that non-economic damages caps increase the number of C-sections and argue that physicians may be more likely to perform unnecessary procedures when they are less fearful of liability. Kessler and McClellan (1996) present evidence of a reduction in physicians' self-monitoring: After caps adoption, physicians choose cheaper courses of treatment. They do not find changes in health outcomes associated with changes in treatment patterns. The interpretation is that the marginal impact of some procedures used before caps adoption is so small that it does not affect outcomes significantly.

In contrast, Dhankhar, Khan, and Bagga (2007) report that increased medical liability pressure is actually associated with lower resource use and better clinical outcomes for at least some patients with acute myocardial infarction (AMI). Specifically, the effect is observed for patients with less severe medical conditions. The particular sample considered could explain this difference, because Kessler and McClellan concentrate on Medicare spending, thus capturing mainly the effect on the elderly population. Sloan and Shaddle (2009), however, find no evidence that tort reforms have reduced payments for Medicare-covered services. Because of their respective sample definitions, none of these studies is necessarily predictive of the overall patterns of defensive medicine. Results obtained for AMI patients are not necessarily generalizable, and results obtained using Medicare data do not account for changes in the treatment patterns of non-elderly populations for whom the cost of care may be driven by different factors.

Moreover, conflicting results, such as those in Dubay et al. (1999) and Grant and McInnes (2004) versus Currie and Macleod (2008), make it even more difficult to assess the actual impact of tort reform on treatment intensity.

2.5. Any impact of changes in treatment patterns on demand does not offset the effect of the increased number of physicians.

The issue is further complicated by the fact that changes in treatment patterns may affect the demand for medical care. Defensive medicine is meant to offer physicians protection against malpractice suits and reassure patients that their physicians did everything possible to insure a positive outcome. To the extent that patients value the extra reassurance[4] and the demand for medical care depends on the current state of information about its production and effectiveness (McClellan 1995), changes in treatment patterns and/or in information about their effectiveness could negatively affect demand.[5] Undoubtedly historically, the types of procedures offered have lowered the threshold for intervention and increased utilization rates,[6] even in the absence of changes in outcomes associated with each procedure (Cutler and Huckman, 2003). If tort reform has also increased medical errors, a demand response is even more likely.

In the presence of changes in treatment intensity, the relevant measure of the impact of tort reforms to insure better access to medical care may not be whether there are more physicians in adopting states, but whether there are more medical services actually delivered to the population. This paper contributes by investigating the impact of non-economic damages caps on broad measures of medical care delivery. The answer to this question could help explain the inconsistencies between the documented increase in the number of physicians, the increase in insurance coverage, and the lack of positive changes in health outcomes. Only one such instance of improvement in health has been found: Non-economic damages caps adoption is associated with a reduction in black infant mortality rates (Klick and Stratmann, 2005, 2007).

III. Data

State by state *legislative data* are taken from Ronen Avraham's (2010) Database of State Tort Law Reforms (DSTLR). In particular, I use the DSTLR 3rd-clever dataset, which has the advantage of retaining only those legislative changes that were binding, thus reducing measurement error in the independent variable of interest.[7] This is important, because fixed effects estimation is known to exacerbate attenuation bias caused by measurement error by removing a significant portion of the variation in the right-hand-side variables. It is possible that measurement error problems are responsible for previous results indicating tort reform has no effect on health and only a small effect on other measures of medical care. The impact of the law

[4] Physicians appear to be successful in reassuring patients, because only a small percentage of malpractice incidents results in a lawsuit (for every 7.5 patients who incurred a negligent injury, 1 malpractice claim was filed) (Weiler et al., 1993).

[5] There is evidence that new quality information does affect choices, especially for individuals making a choice for the first time (Wedig and Tai-Seale, 2002; Jin and Sorensen, 2006; Bundorf et al., 2009).

[6] One example is laparoscopic surgery (Finlayson et al., 2003).

[7] The results are similar to those obtained to using all legislative changes even if not binding – see footnote 37. As expected under the hypothesis of measurement error, those estimated coefficients are smaller.

is measured by introducing a dummy variable indicating whether the state has a cap on non-economic damages in a given year regardless of the value of the cap. If the effective date of the reform was on or after July 1st, it was coded as belonging to the year after.

This analysis is performed on 1990-2006 data.[8] During the 1990-2006 period, there were 14 instances of binding non-economic cap adoption and 4 of repeal (see Table 1) so there is significant time variation in the data. Although this particular period was chosen primarily because of data availability, this sample has the advantage that during this period there were relatively few changes in other types of tort laws, reducing concerns about confounds. Only one state adopted, Nevada, and no state repealed contingency fees rules,[9] only one state, South Dakota, repealed a cap on total damages and no state adopted such cap, only one state, West Virginia, adopted legislation regarding patients' compensation funds, and no state repealed such laws. There is, however, significant time variation among punitive damages caps (10 reforms), joint and several liability[10] (7 reforms), and collateral source[11] (10 reforms).

Punitive damages caps are not likely to be a significant source of concern because of the specific characteristics of punitive damages. First, punitive damages are not awarded as often as compensatory damages. In 2005 U.S. Department of Justice reported that in 2001 punitive damages were awarded in only 4.9 percent of cases.[12] The reason for this small percentage is that judges will award punitive damages only if the act was so offensive that the court believes it is important to make an example out of the defendant. Specifically, punitive damages are intended for willful and wanton conduct. And second, punitive damages do not seem to be significantly larger than compensatory awards. In fact for the 1963-1993 period, for example, Koenig and Rustad (2005) find that punitive verdicts were largely proportional to compensatory awards, with the median ratio of punitive damages to compensatory damages awarded at trial being 1.21 to 1. Because punitive damages are rarely awarded and not significantly larger than compensatory awards, punitive damages caps have a lower effect on malpractice insurance premiums (Viscusi

[8] Sample size varies across regressions function of data availability.
[9] A contingent fee is a fee charged by an attorney for his or her services only if the lawsuit is successful or is favorably settled out of court. Usually, the contingent fee is calculated as a percentage of the amount the plaintiff recovers from the defendant.
[10] Joint and several liability is a form of liability that is used in civil cases where two or more people are found liable for damages. The winning plaintiff in such a case may collect the entire judgment from any one of the parties or from any and all of the parties in various amounts until the judgment is paid in full.
[11] Collateral source rule permits the admissibility of evidence of collateral source payments.
[12] Bureau of Justice Statistics, U. S. Department of Justice, "Selected Findings, Civil Justice Survey of State Courts, 2001, Punitive Damage Awards in Large Counties, 2001," NCJ 208445, March 2005.

and Born, 2005) and, thus, are less likely than caps on compensatory awards to have a significant impact on utilization rates. Nevertheless, to further check the reliability of the above inference, this paper controls for such a possible confounding factor in some specifications. Collateral source and joint and several liability are also included.

This paper estimates separate models for several types of medical services and constructs a falsification test by investigating whether the measured impact is significant where caps are unlikely to affect utilization rates. Based on the National Physician Survey of Professional Liability, the American Medical Association (AMA) identifies high-risk specialties to be general surgery, neurosurgery, orthopedic surgery, thoracic surgery, obstetrics/gynecology, and emergency medicine. This paper investigates the impact of non-economic damages caps on surgeries, hospital admissions, and outpatient visits. As a falsification test, the analysis also distinguishes between emergency outpatient visits and other outpatient visits[13]. Since it is unlikely that caps affect emergency care visits, they should have no effect in this case.

County-level data on hospital *admissions, inpatient and outpatient surgeries, emergency outpatient visits, other outpatient visits, and inpatient days* come from the U.S. Department of Health and Human Services, Area Resource File (ARF). The universe is the sum of all such medical services provided in short term non-general hospitals,[14] short term general hospitals,[15] and long-term hospitals.[16] All ARF hospital utilization data originate from the American Hospital Association (AHA) Annual Survey of Hospitals. Surgeries data is available on yearly basis starting 1995. All other data is reported every year for the entire 1990-2006 period.

The source of *mortality data* is the Compressed Mortality Files compiled by National Center for Health Statistics (NCHS).[17] These data are comprehensive, for they contain information from all death certificates filed in the 50 states and the District of Columbia.

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[13] Other outpatient visits are defined to include clinic and referred visits and exclude emergency visits and outpatient surgeries.

[14] Short Term Non-General Hospitals are those coded as follows by the AHA: Length of Stay = '1', Short-term; Type of Service not equal '10', General medical and surgical. These hospitals provide specialized care, and the majority of their patients stay for fewer than 30 days.

[15] Short Term General Hospitals are those coded as follows by the AHA: Length of Stay = '1', Short-term; Type of Service = '10', General medical and surgical. These hospitals provide non-specialized care, and the majority of their patients stay for fewer than 30 days.

[16] Long Term Hospitals are those coded as follows by the AHA: Length of Stay = '2', Long-term. These hospitals may provide either non-specialized or specialized care, and the majority of their patients stay for 30 or more days.

[17] NCHS is only responsible for the initial data. NCHS is not responsible for any analyses, interpretations, or conclusions; these belong to the author.

The measure of *distance from border of the county population centroid* comes from Holmes (1998). I use this measure to construct the *BORDER CAP* variable that identifies which counties are located within 100 miles of a border with a state that adopted non-economic damages caps.

The sources of the other variables used in the regression analysis are detailed in the Data Appendix.

IV. Econometric Strategy

The following equation describes the empirical model:

$$Y_{ct} = \theta\, CAP_{st} + \lambda\, BORDER\ CAP_{ct} + \beta\, X_{ct} + \alpha_c + \gamma_t + \omega_s t + \varepsilon_{ct} \quad (1)$$

Y_{ct} measures medical care utilization rates in county *c* year *t*, as measured by log hospital admissions, log surgery, or log hospital outpatient visits per 100 individuals. CAP_{st} is the variable of interest. It is a dummy variable indicating whether the state has a cap in effect on non-economic damages in a given year, regardless of the value of the cap.

In response to non-economic damages caps adoption, physicians could move across borders. In addition, consumers' response to caps adoption could be a longer and wider search for a physician, which may include physicians located in bordering states. As a result, there may be spillovers from caps adoption in neighboring states. When such border effects are important, specifications that fail to control for spillovers lead to biased estimates of the impact of the law. To disentangle the direct impact of non-economic caps adoption from any spillovers from neighboring states, the model specification includes an indicator variable equal to one in counties whose population centroid is located within 100 miles of a border with a state that adopted caps, $BORDER\ CAP_{ct}$.[18] X_{ct} is a vector of observable time-varying county characteristics that affect medical care utilization rates, such as population, age, and race composition of the population, and wages. It is to be expected that higher income and an older population are associated with higher demand for health care. The variable reflecting the population's racial structure controls for possible systematic differences in demands from different segments of the population. ε_{ct} is the stochastic error term.

[18] The results do not vary if the sample of affected counties is limited to counties whose population centroid is located within 50 miles of a border with a state that adopted caps (results not reported but available on request). In addition, the timing of the effect may vary across specifications as explained in the text.

To account for unobserved county-specific time-invariant determinants of medical care utilization rates, this model specification includes county fixed effects, α_c. For instance, differences in the overall level of health of population in a county will not confound the effect of non-economic damages caps. The equation also includes year fixed effects, γ_t, meant to capture time-varying differences in the dependent variable common to all counties, such as changes in federal-level health care policies. State-specific trends in medical care utilization rates could impact a state's likelihood to adopt a non-economic damages cap. To control for such a possibility, the model includes state-specific trends, $\omega_s t$. Controlling for these trends reduces the burden of the assumption of reforms' exogeneity. Conditional on county and year fixed effects and state-specific trends, the θ's are identified from year-by-year changes in legislation, after controlling for shocks common to all counties and state-specific trends in medical care utilization rates.

There are two additional issues about the estimation strategy that should be mentioned. First, the estimates obtained from counties with large populations are more precise than those from smaller counties. To control for this source of heteroskedasticity, this paper reports regressions weighted by the county population in each year. Second, the independent variable of interest varies only at the state level. Moreover, there are only four instances of repeals in the data; thus, it is likely that the error terms are correlated within states over time. Misspecification of the autocorrelation process can lead to downward bias in the standard error estimates (Bertrand et al., 2004). Consequently, robust standard errors clustered at the state level that allow for heteroskedasticity and autocorrelation of unspecified form are calculated and reported throughout the paper.[19]

The key identifying restriction in this paper is that the adoption of non-economic damages caps is exogenous. The literature indicates that the timing of the adoption is mostly the result of vagaries of the political process (Rubin, 2005). It is nevertheless useful to investigate this assumption. One way to do so is to test whether the distribution of observable covariates is balanced across the groups defined by the adoption or non-adoption of caps (Heckman and Hotz, 1989). Panel A of Table 2 reports the mean values of a number of variables in the county-years with no caps, separated by treatment status in the following year: no non-economic cap in column [1] and non-economic cap adoption in column [2]. Column [3] reports the results of t-

[19] The results are robust to clustering at the county level, as shown in the robustness check in Table 8.

tests for the equality of means. Since the identifying strategy specifies that non-economic damages caps are exogenous after accounting for county and time fixed effects and state-specific trends, the results reported in column [3] report the t-test of equality of means, conditional on county and time fixed effects and state-specific trends.[20] The findings suggest that there is no statistically significant difference in the year prior to the treatment between counties that receive the treatment and those that do not on a variety of measures: hospital admissions, surgeries, outpatient visits, mortality, age and racial structure, and wages. There is a significant difference in population. Given that 11 variables were examined, it is not too surprising that one was found to be statistically significant.

Panel B of Table 2 further tests for exogeneity in the adoption of caps on non-economic damages by considering the Panel A variables simultaneously in a regression-based analysis controlling for all fixed effects and time trends mentioned in the base specification[21] When considering the medical care measures along with mortality and demographic characteristics of the counties, their joint p-value ranges from 0.17 to 0.29, indicating that these variables are poor predictors of the adoption of non-economic damages caps. In the following analysis (see Table 3), I show that the inclusion of additional controls or the exclusion of all control variables has little effect on the estimated coefficient of the non-economic damages cap variable. Combined, the relative lack of predictability of caps adoption based on variables influencing the demand for medical care, and the invariance of the results to adding controls that may explain adoption, suggests that the identification assumption is plausible.

In addition, graphical evidence (Figures 1, 2, and 3) shows no indication of a transitory pre-treatment increase in utilization rates, the equivalent of an "Ashenfelter dip" for this case (Ashenfelter, 1978), that would suggest the estimates indicate just mean reversion and thus are falsely attributed to tort reform. A formal test for pre-trends performed by entering leads of the non-economic damages caps in the main specification, reported in Table [7] also finds no

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[20] Specifically, I retain again only the sample of county-years observations with no caps and run a separate regression for each explanatory variable on a variable defined as 1 if there was a cap the following year and 0 if there was no cap the following year while controlling for county and time fixed effects and state specific trends. Because columns 1 and 2 report raw means and column 3 reports t-tests of equality of means conditional on a series of fixed effects and time trends, the t-statistics do not always have the sign matching the sign of the simple difference in the means reported in columns 1 and 2. Note that taking the difference in raw means is not appropriate because the caps were adopted at different points in time.

[21] The sample includes all county-years with no non-economic damages caps. The dependent variable is treatment status in following year.

evidence of a pre-trend. At the same time, however, the graphs are also consistent with an upward trend in utilization rates that may vary by state, highlighting the need to control for state-specific trends. To the extent that adopting states were on a faster increasing trend in utilization rates, as suggested by the raw data depicted in Figures 1 to 3, specifications that fail to control for long-term state-specific trends will understate the impact of the non-economic damages cap.

V. Results

5.1. Base Specification.

Table 3 presents the main results obtained from the estimation of equation (1), with each column representing a separate regression.[22] They indicate that, after controlling for county and time fixed effects and state specific trends, non-economic damages caps adoption is negatively correlated with hospital admissions, surgeries, and outpatient visits. As shown in column [2], the results are robust to the inclusion of demographic controls: population, age and race composition of population, and log wages.

One possible source of confound is the simultaneous adoption of other tort reforms. Specifically, between 1990 and 2006 there was significant legislative activity regarding punitive damages, joint and several liability, and collateral source reform. As explained in the data section, the impact of punitive caps is likely small, but joint and several liability and collateral source reform were found to be positively correlated with health insurance coverage and, as such, are potential confounds. To make sure the estimates of non-economic damages caps are not picking up the impact of these other reforms, column [3] of Table 3 reports the estimates obtained from augmenting the base specification with an indicator variable for each type of reform. As expected, punitive damages caps do not affect utilization rates, and its inclusion does not change the estimates. Joint and several liability reform is negatively correlated with surgeries but positively correlated with outpatient visits. This finding potentially indicates substitution across types of medical services. In contrast, collateral source reforms are positively correlated with surgeries suggesting significant heterogeneity across types of tort reforms. Reassuringly, the

[22] All regressions control for county and time fixed effects and state-specific trends. All regressions are weighted by population in a county-year and report robust standard errors clustered at state level.

inclusion of all these controls does not change the estimated coefficient of non-economic damages caps.

As non-economic damages caps may induce physicians to move across borders, and patients may follow them, there could be spillovers from reforms passed in bordering states. As shown in column [4], there is no evidence of an instantaneous effect from bordering reforms. However, since changes in the characteristics of medical care sometimes have effects on the utilization of medical services that become noticeable only in the future and movements across borders may involve longer adjustment periods, I investigate the possibility of lag effects.[23] Indeed, while the direct impact of caps appears to be concentrated in the year of enactment, there is a one-year lag in the border effect on surgeries (column [5]). I find no statistically significant border effect on admissions or outpatient visits. Overall the results suggest that spillover effects are not a source of significant bias.[24] All remaining regressions results reported in this paper, including column [6] Table 3, control for lag border non-economic damages caps.

Finally, column [6] reports estimates obtained after controlling for an additional series of potential confounds. First, the increase in malpractice liability is blamed for the increase in health care costs and is the main argument in favor of caps adoption. At the same time, utilization rates, measured as the number of patient-days at the hospital level, are lower in areas with higher malpractice liability (Lackdawalla and Seabury, 2009). Thus, it is possible that malpractice liability causes both caps adoption and low utilization rates. To control for this source of confound, the main specification is augmented with a variable that measures the medical malpractice premium for internal medicine in panels A and C, and the premium for general surgery in panel B.[25] The lack of sensitivity to this inclusion is consistent with previous literature indicating no relation between malpractice liability and damages caps (Baiker and Chandra, 2005b).

Second, another potential source of confound is the increase in the price of medical care because of the hospital consolidation that took place in the 1990s. Hospital consolidation leads to

[23] There is sometimes considerable delay between the onset of symptoms and surgery or even the first visit to the physician. Even for diseases heavily covered in the press, such as breast cancer, approximately one third of women with confirmed breast cancer originally delayed seeking a diagnosis for at least 3 months or longer after finding their first symptom (Facione, Miaskowski, Dodd, and Paul, 2002).

[24] The interaction between having caps in a state and bordering a state with caps is not statistically significant (results not reported but available on request).

[25] The data are not available for all states in all years, which is why the sample size drops in this specification.

higher transaction prices[26] and thus increases the number of uninsured (Town et al., 2006). Since the proportion of the insured population is an important determinant of the medical care utilization rates and could also affect the timing of caps adoption, an increase in the uninsured rate triggered by hospital consolidation could confound the impact of non-economic damages caps.[27] The specification in column [6] also includes controls for the uninsured rate.[28] The estimates are robust to this addition, matching Avraham and Schazenbach (2009) results that find no causality from insurance coverage to tort reform.

In addition, this specification includes controls for potentially relevant time-varying state characteristics: education, health maintenance organization (HMO) penetration rate, state health and hospital expenditures (data sources detailed in Data Appendix*)*. Education is known to affect behavior, including investments in health (Grossman, 2000), and may also change voting patterns. HMO penetration may affect the price of health care and, thus, utilization rates. Health and hospital expenditures could be correlated with the state's general interest in health regulation and could influence the demand for medical care. The results are robust to the inclusion of all these controls. Since the estimates of interest are not sensitive to the inclusion of these controls, there is no reason to believe these represent significant sources of confound, and the preferred specification remains the more parsimonious one. These results provide suggestive evidence that the identifying assumption is indeed plausible: Under the hypothesis that after accounting for county and year fixed effects and state-specific trends non-economic damages caps adoption is exogenous, adding controls does not affect the estimated coefficients.

The effect of caps on non-economic damages is not very large in absolute value; the adoption of a non-economic damages cap reduces admissions by about 2.5%, surgeries by 3.5%,

[26] There is an extensive literature investigating the effects of increased hospital consolidation on prices. I will limit myself to mentioning Gaynor and Vogt (2000), Connor and Feldman (1998), and Dranove and Lindrooth (2003) as excellent summaries of this literature, as well as a couple more recent papers, Capps and Dranove (2004) and Dafny (2009).

[27] Another change in the relative price of medical care could come from a change in transportation costs if hospital consolidation decreases the number of hospitals. The data indicate, however, that non-economic damages caps are positively correlated with the number of hospitals, as should be expected when there is entry in medical fields. The results hold and become slightly more negative if a control for hospitals is added to the main specification – results not reported but available on request. Because hospitals could be endogenous, the preferred specification does not control for hospitals.

[28] The proportion of the uninsured population is measured at the state level. County-level data are only available for some years. When possible, the county-level uninsured rate from Small Area Income and Poverty Estimates (SAIPE) was used to check the results. The results obtained are similar (results not reported but available on request).

and outpatient visits by 4.5%[29] the equivalent of approximately 3 fewer admissions, 3 fewer surgeries, and almost 90 outpatient visits per 1,000 individuals. However, these numbers should be put into context. Previous studies have estimated that the adoption of non-economic damages caps will increase the supply of physicians by between 2% (Encinosa and Hellinger, 2005; Klick and Stratman, 2005) and 3.3% (Kessler et al., 2005), with a higher impact on the supply of surgeons at 4% (Klick and Startman, 2007). According to Fuchs (1978), a 4% increase in surgeons is expected to be associated with a 1.2% *increase* in surgeries. This analysis, however, finds a 3.5% *decrease* in surgeries, a clear indication that non-economic damages caps lead to changes in health care that offset the impact of the increase in the number of providers.

One explanation is endogeneity, because low utilization rates resulting from poor access to medical care in states with few providers could motivate legislatures to adopt medical malpractice reforms. There is little evidence, however, that endogeneity is in fact the driving factor. Previous literature summarized by Rubin (2005) finds that the adoption of tort laws is mainly driven by political vagaries. It is also not related to health outcomes (Rubin and Shepherd, 2007) or insurance coverage (Avraham and Schazenbach, 2009). Statistical tests also do not indicate any significant differences among counties that adopt and those that do not adopt caps in the following year, as measured by several observable county characteristics.

Another explanation could be an improvement in overall health that reduces hospital utilization rates. Yet another potential explanation is a reduction in defensive medicine, i.e. changes in treatment patterns. This change in treatment patterns may also affect the demand for medical care in a way that could explain the observed drop in utilization rates. One way to distinguish between competing hypotheses is to look across services where caps are likely to be binding and assess their impact as a function of the provider’s degree of discretion.

5.2. Falsification Tests

First, an increase in the number of physicians should improve access to care and could improve overall health. The hypothesis that the decrease in utilization rates is caused by a decrease in demand triggered by health improvements can be tested. Table 4 results indicate no significant correlation between non-economic damages caps and mortality rate from all causes. There is no significant association between caps and broadly defined disease mortality or injury

[29] Note that as shown in Table 8 row 4 there is some evidence that caps are also associated with a change in slope in the case of outpatient visits. The estimated effect on the mean utilization rates will thus be larger the longer of the period passed since caps adoption.

mortality.[30] Moreover, when investigating the impact of caps on mortality by cause of death I find that caps lead to an increase in mortality from complications of medical and surgical care.[31,32] The largest effect appears with a two-year lag, which is reported in Table 4. These results are not consistent with a positive effect on health. They are consistent with a change in treatment patterns that increases the probability of complications, and we can reject the hypothesis that health improvement is behind the reduction in utilization rates.

Second, previous literature suggests that caps are sometimes associated with a reduction in the intensity of treatment. Inpatient care, rather than outpatient care, is required only if the beneficiary's medical condition, safety, or health would be significantly and directly threatened if care were to be provided in a less intensive setting. By reducing the cost of malpractice, non-economic damages caps could change physicians' incentives to classify some patients as requiring intensive care and thus inpatient care. Consistent with this hypothesis, this analysis found a decrease in admissions. A different test of this hypothesis regards the length of stay in the hospital. Results reported in Table 5, however, do not indicate a reduction in the average length of stay computed as total inpatient days divided by admissions. An increase in medical errors may require a longer hospital stay to repair the damage, which could offset the impact of lower-intensity treatments on average length of stay. Also a reduction in admissions likely targets the least serious medical conditions, leaving only the frailest patients to be admitted for treatment.

Third, a different margin that can be investigated refers to the classification of surgeries. The data are reported separately for inpatient versus outpatient surgeries. Because some types of surgeries can be performed on either an inpatient or an outpatient basis, the provider has some discretion. A decrease in defensive medicine could reduce the intensity of treatment and reduce

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[30] Injury mortality corresponds to V01-X59, Y40-Y88 ICD10 codes or E810-E999 ICD9 codes. All other causes of death are included in "disease mortality".

[31] ICD 10 codes: Y40-Y84, Y88 or ICD 9 codes E870-E879 and E930-E949.

[32] Note that the results obtained from this specification do not contradict Carvell, Currie and MacLeod (2009) which finds that the rule of joint and several liability reduced accidental mortality rate, which includes all causes of accidental injury except vehicle accidents. In fact, in this specification I find that the rule of joint and several liability also reduces mortality from complications due to medical and surgical care. The estimated effect is -0.052(0.025). Also, these results need not contradict Rubin and Shepherd (2007) which finds a negative correlation between non-economic damages caps and accidental death. Because the focus of their paper is moral hazard, Rubin and Shepherd (2007) look only at accidental deaths excluding vehicle accidents. The focus here is to identify whether changes in medical care delivery caused by caps lead to potential changes in outcomes. For instance, more emergency physicians could reduce time to intervention and improve outcomes. As a result, the definition of injury mortality covers all causes of injury, accidental or not, and includes vehicle accidents.

inpatient surgeries. It may also reduce outpatient surgeries if unnecessary surgeries were performed before. Table 5 indicates that both inpatient and outpatient surgeries decrease and the effect is larger in the case of inpatient surgeries.

Fourth, if the admission and surgery results are driven by a decrease in the intensity of treatment, then we should observe an increase in other types of services as patients and physicians substitute across treatment options. For instance, if the drop in admissions and inpatient surgeries is driven by a reduction in defensive medicine, physicians and patients could substitute toward outpatient delivered medical care. The data are reported separately for emergency outpatient visits versus other outpatient visits. Caps are likely not binding for providers of other outpatient visits, so they should not have a direct effect on the supply of these services. Moreover, outpatient visits are largely at the discretion of the patients. I find no evidence of substitution toward outpatient care: non-economic damages caps are associated with lower outpatient care utilization and the effect is statistically significant.

Thus, the impact of non-economic damages caps refers only to marginal health care, the care that could be delivered either inpatient or outpatient and the care that could be treated either by surgery or by other means. The results are consistent with a reduction in defensive medicine. A caveat of this explanation is that it is highly unlikely physicians would not offer any treatment. If admissions decrease and surgeries also decrease, we should observe an increase in other medical services that serve as substitutes. However, I find evidence of a significant decrease in outpatient visits. A potential explanation for these results is that information revealed on the occasion of legislative debates along with changes in treatment patterns that led to an increase in medical complications also produces cautious consumers and thus leads to a decrease in demand. Another explanation is a substitution toward non-hospital care such as primary care, with the caveat that this requires some physicians, for instance surgeons, to forgo patients/customers they could serve. Because of data availability, I defer this question to future research.

As a falsification test, caps are likely binding for emergency care, but since not much discretion seems to exist in the level of utilization, caps are not expected to have a significant effect on emergency care either. I find no effect on emergency outpatient visits, which improves confidence in the overall findings.

A different way to test whether the analysis really identifies the impact of non-economic damages caps stems from previous literature, which found that the supply effect is larger in rural areas (Matsa, 2007). Consequently, we should observe a lower decrease in utilization rates in rural areas. To test this hypothesis, I separate predominantly rural counties from predominantly urban counties[33] and estimate the effect of non-economic damages caps on each subsample. Although the coefficient is less precisely estimated, there is no difference in the effect of caps on admissions by rural status. It appears, however, that caps do have a significantly lower impact on surgeries. This is consistent with a larger increase in the supply of surgeons relative to other specialties, especially in rural areas. I also find no effect on mortality from medical complications in rural areas. Early intervention associated with improved access may be reducing the incidence and/or impact of complications.

5.3. Endogeneity

One way to check for potential signs of endogeneity is to verify whether enactment had a different effect than repeal. While enactments are decided by legislatures, repeals/nullifications are decided by courts, which are presumed to be less sensitive to political pressure (Yoon, 2001). The effect of repeals is thus less likely to be distorted by endogeneity bias. To separate the effect of enactment from that of repeal, I construct two subsamples. First, to estimate the effect of enactment, I retain the sample of states that never adopted a cap or that adopted a cap but never repealed a cap. Second, to estimate the effect of repeal, I retain the sample of states that had a cap throughout the period investigated or that repealed a cap, but never adopted a cap during the same period. Because this sample definition includes only one instance of repeal in the case of surgeries this test is difficult to perform on the original sample. To perform this test, I use a panel of data reported every 5 years, i.e. years 1990, 1995, 2000, and 2005.[34] As reported in Table 7, the estimates of the impact of repeal are noisier and have larger standard errors,[35] because there is less variation in this sample and thus this specification is very demanding on the data.[36] The

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[33] The categories are defined by rural status in the 2003 data from the Economic Research Service, as reported by ARF. Rural counties are defined either as “Completely rural or less than 2,500 urban population, adjacent to a metro area” or as “Completely rural or less than 2,500 urban population, not adjacent to a metro area.”

[34] The estimated effect of non-economic damages caps obtained using this sample are very similar to those obtained using yearly data for the 1995-2006 period: -0.055(0.018) and thus significant at 1% significance level.

[35] I maintain the definition of caps as 1 if the state had a cap and 0 if not across all specifications.

[36] Only 3 states repealed caps in the sample used. The forth instance of repeal is Wisconsin, which both adopted and repealed the caps during this period and as such is not included in either the enactment sample or the repeal sample. The results hold when Wisconsin is added to the sample used to estimate the impact of enactment. The estimated effect on admissions is -0.038(0.009), on surgery -0.057(0.021), on outpatient visits -0.066(0.025). The estimated effect of repeals is also not sensitive to the addition of Wisconsin to the sample used to estimate the impact of repeal. The estimated effect on admissions is -0.010(0.018), on surgery -0.089(0.036), on outpatient visits -

estimated effect of the repeal of caps on surgeries are very similar to those obtained for the effect of enactment of caps, providing further confidence that non-economic damages caps are exogenous to utilization rates. In contrast, repeals have no effect on admissions or outpatient visit. This could be either a sign of endogeneity or simply a sign that the effect is not only cost driven but also information driven and that patients do not easily adjust their expectations in response to a repeal.[37] It could also indicate that repeals were in fact anticipated because of prior activity in a lower court.

To further assess whether endogeneity is an issue, I test whether the identified trend in utilization rates happened before the caps' adoption and thus cannot be attributed to the reform. For this purpose, I add to the main specification a variable defined as 1 if the law was effective in the following year. Coefficients for the 1-year lead non-economic damages caps variable are not statistically significant, as shown in specification [2] of Table 7. I also find that neither a 2-year lead nor a 3-year lead of non-economic damages caps is significantly correlated with the current level of utilization rates. Overall, these results suggest that the observed trend did not begin in the years prior to the caps' adoption and that causality runs from damages caps to utilization rates and not the other way around.

As an additional check on the validity of the estimates, I perform a series of sensitivity checks. Heckman and Smith (1999) suggest that the effects of an "Ashenfelter dip" are mitigated by the use of sufficient pre-treatment data. I test the hypothesis that the results are driven by those legislative changes taking place at the beginning of the period investigated by excluding the states with reforms adopted in the first two years of the sample period. The results hold when excluding early reforms and also when excluding the states in the most recent cluster of reforms (those adopted in the last two years of the sample). This is suggestive evidence that there is no selection of adopting states based on their relative expected impact on utilization rates.

0.002(0.015). Because of the two year lag effect, the effect on mortality is identified from reforms taking place during the 1988-2004 period when no state both adopted and repealed caps.

[37] In general there is evidence that the effect is spread over several years after the reform. The lag impact of a repeal is generally larger and in fact more significant than the instantaneous effect in the case of surgeries. As a side note, when using all instances of non-economic damages caps reforms, even non-binding ones, the lag effect is slightly larger than the instantaneous effect in the case of surgeries. A possible explanation is that even non-binding caps affect perceptions about medical care, thus, the effect is spread over several years. Using this data the results are similar but the coefficients are smaller, as expected when measurement error leads to attenuation bias. For instance, the estimated instantaneous effect on admission is 0.014(0.012) and on outpatient visits is -0.024(0.017); the estimated lag effect on outpatient surgeries is -0.011(0.013), and on inpatient surgeries 0.023(0.009), for all outpatient visits -0.021(0.017). The estimates two-year lag effect on mortality rate is 0.0001(0.002) and on mortality rate from medical care complications 0.034(0.013)

In addition, if there is selection, it would likely be driven by those counties that are able to create pressure to obtain the desired legislation. In particular, the county of the state capital may have a more significant weight in the decisions of the policy makers. To test this hypothesis, specification [7] presented in Table 7 runs the main specification on a sample excluding the counties of the state capitals. The results are robust to this exclusion.

A number of alternative specifications were tested and some of the results are presented in Table 8.

5.4. Robustness Check

5.4.1. Row [1] Table 8 presents the primary results for easier comparability.

5.4.2. Value of Restriction A dummy variable that captures the reform even when adjusted for cases where caps are not binding does not capture the effect of a more restrictive cap. To capture this effect, I use a variable calculated based on the amount of the cap. Under the hypothesis that a 1 million dollars cap is not binding,[38] I measure the restriction's tightness by a variable calculated as the logarithm of 100*(1,000,000-value of cap)/1,000,000. A lower cap represents a tighter restriction and a higher value of this computed variable. So if caps lead to a decrease in utilization rates, a higher value of this variable should also be associated with lower utilization rates. The results reported in row [2] of Table 8 are consistent with this hypothesis.

5.4.3. Dependent variable not log. Log utilization rates may be preferred if changes in utilization rates where utilization rates are low are of more interest than changes in utilization rates where they are already high. However, if the assumption is that equal changes in utilization rates should be treated similarly, the dependent variable should be utilization rates. I find that the results are robust to the choice of functional form.

5.4.4. Trend Break. It is possible for the legislation to trigger changes in the time trends of medical care utilization rates. To investigate this hypothesis, I estimate equation (3) and report the results in the third specification reported in Table 8.

$$Y_{ct} = \theta\ CAP_{st} + \mu\ CAP_{st} *t + \lambda\ BORDER\ CAP_{ct} + \beta\ X_{ct} + \alpha_c + \gamma_t + \omega_s t + \varepsilon_{ct} \qquad (3)$$

where $CAP_{st}*t$ controls for a change in time trends.

I find no significant evidence of a shift in slope using this specification. Likely this is due to the fact that the mean shift captures most of the change in utilization rates.

5.4.5. Regional Variation.

[38] This is consistent with the way Avraham (2010) coded legislation in the DSTLR3 clever dataset.

Hospital consolidation varies regionally: During the 1990-2003 period, it increased the most in the South: 9.4% of hospitals consolidated versus 7% in the East, 7.4% Midwest, 6.4% Southwest, and 5.5% West, according to Vogt and Town (2006). However, as shown in row [5], non-economic damages caps do not have a more negative effect on utilization rates in the South, as would be the case if the non-economic damages caps variable were to pick up the variation in hospital consolidation. These tests provide suggestive evidence that trends in insurance rates do not bias the estimated impact of caps.

5.4.6. State level. Since non-economic damages caps is a state-level law, it is useful to test the robustness of the results using state-level data. There may be common random effects at the state level, and the main specification accounts for their existence by computing standard errors corrected for clustering at the state level. Another solution to this problem is to aggregate the data at the state level. Using state-level data also has the advantage of being less noisy, but it may also have significant disadvantages. A state-level specification not only aggregates over significantly different populations, but any endogeneity problems would be more problematic in this setting. Specification [6] in Table 8 presents results obtained using state-level data.[39] The results are generally similar to previous results, consistent with the idea that the timing of adoption is mainly the result of the political process.

5.4.7. Restricted sample: counties with more than 1,000 individuals. Another way to reduce the impact of noise in the data is to exclude counties with very small populations where there is extremely high variance in utilization rates. To investigate the possibility that the results are driven by noise, specification [7] runs the same regression on the sample of county-year observations with populations larger than 1,000. The results obtained from this specification are similar to those obtained from the entire sample.

5.4.8. Unweighted incidence data. All reported results use population to calculate utilization rates and weigh data. To check whether the results are due to changes in population, specification [8] reports unweighted regressions where the dependent variable is measured as

[39] This specification controls for state and year fixed affects, state-specific trends, and state-level time-varying covariates, including: non-economic damages caps in bordering states, population, age composition, race composition, log wage, punitive damages, joint and several liability, and collateral source reform.

incidence: total admissions, total surgeries, total outpatients visits, and total deaths in a county-year. The results are qualitatively similar.[40]

5.4.9. Add Alaska and Hawaii. The estimates presented in row [9] are obtained on a sample augmented to include Alaska and Hawaii. These two states have particular characteristics that distinguish them from other states. Medical care utilization rates are lower, resulting in more imprecise estimates for these states. Also, since these states do not have land borders with other states, there is less scope for spillovers. Perhaps publicity and debates in other states have less of an impact on perceptions in Alaska and Hawaii and could delay caps' adoption even if all the other conditions for adoption are met. The central findings remain unchanged.

5.4.10. Interaction with Self-Employment. Avraham and Schanzenbach (2009) find that some tort reforms affect insurance coverage of the most price-sensitive categories. I follow their method and investigate whether the effects of caps vary with the proportion of self-employed, one category of a price-sensitive population. The interaction terms are positive for surgeries and outpatient visits, which is consistent with the idea that caps have more positive effects in areas where they are more likely to improve insurance coverage. However, these interaction terms are not statistically significant likely because, as Avraham and Schanzenbach mention, non-economic damages caps are a type of tort reform that has only small effects on insurance coverage and because the decrease in the health insurance premium is very small (1-2% according to Avraham, Dafny, and Schanzenbach, 2009 estimates).

5.4.11. Clustering by County. To better control for time dependence, specification [11] calculates standard errors clustered at the county level. The results indicate that the effect is statistically significant at conventional levels.

VI. Conclusion

Non-economic damages caps are intended to make medical care more affordable by reducing malpractice insurance premiums, an important part of the cost of practicing medicine. If successful, these laws would improve access and increase medical care utilization rates. This paper produced estimates of the effect of non-economic damages caps on broadly defined medical care utilization rates.

[40] Because this specification does not account for the higher variability of data for counties with small populations, the obtained standard errors and therefore inferences based on them are suspect. This does not however cause the coefficients to be biased.

I find that non-economic damages caps have a small but statistically significant negative effect on hospital admissions, surgeries, and outpatient visits other than emergency. The results are not biased by spillovers from bordering states. A possible explanation for these results is offered. Because non-economic damages caps reduce the cost of malpractice, they change physicians' incentives and preferred courses of treatment. A reduction in defensive medicine is consistent with lower utilization rates. Moreover, an increase in medical complications, along with the debate around the adoption of caps that publicize incidences of malpractice, could change people's perceptions and expectations about the quality of medical care and thus the demand for care. If the caps decrease defensive medicine and lower the demand for medical care, an increase in the supply of physicians cannot capture the entire impact of the law.

An endogenous response of legislatures to low utilization rates is also considered. Numerous alternative specifications, including restricted samples, different controls and different functional forms are reported to check the robustness of the findings. The estimates are robust to these tests, and, consistent with previous literature, there is no evidence of endogeneity.

Data Appendix

I. Legislative Data

Data on Non-Economic Damages Caps, Punitive Damages Caps, Total Damages Caps, Contingency Fees, Joint and Several Compensation, and Collateral Source comes from Avraham (2010) and was downloaded from http://www.utexas.edu/law/faculty/ravraham/dstlr.html

II. Medical Care Utilization Rates

Data regarding admissions to community hospitals, surgeries performed in community hospitals, number of outpatient visits, number of emergency visits, and number of inpatient days in community hospitals comes from US Department of Health and Human Services, Area Resource File (ARF).

III. Other Data

1. County level mortality - Compressed Mortality File, National Center for Health Statistics;
2. County level population by race and age –U.S. Census Bureau;
3. County level average annual pay – Bureau of Labor Statistics' (*BLS*) Quarterly Census of Employment and Wages (*QCEW*);
5. Education at state level– U.S. Census Bureau;
6. Health Maintenance Organizations (HMO) penetration rate- US Census Bureau, Statistical Abstract of US, Source Book of Health Insurance Data, 1994, 1995;
7. Medical Malpractice Premium – Medical Liability Monitor 1991-2006;
8. Percent People Not Covered by Health Insurance – U.S. Census Bureau;
9. Public Health and Hospital Expenditures (per capita amounts) – U.S. Census Bureau; State Government Finances;
10. Percent self-employed – Current Population Survey, March Supplement.

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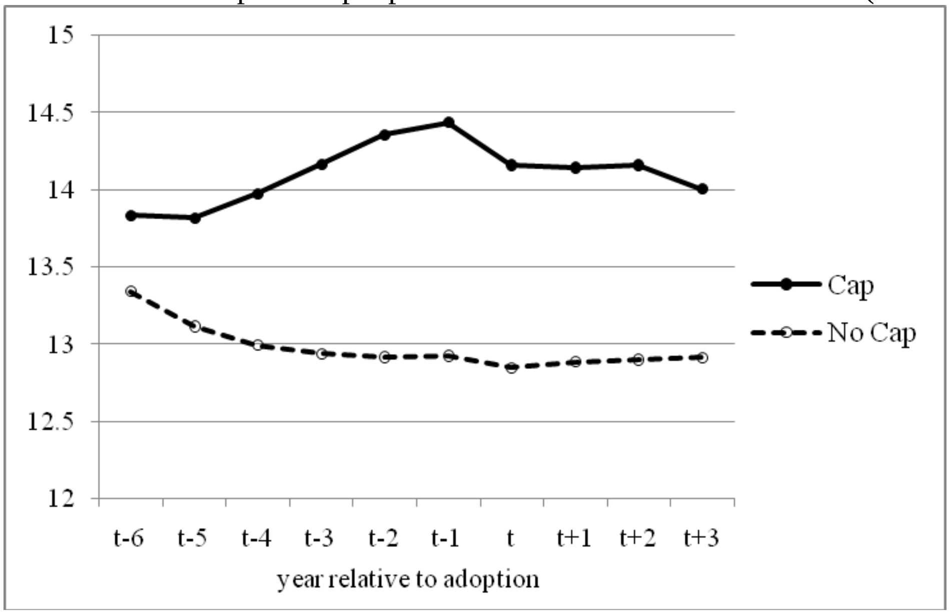
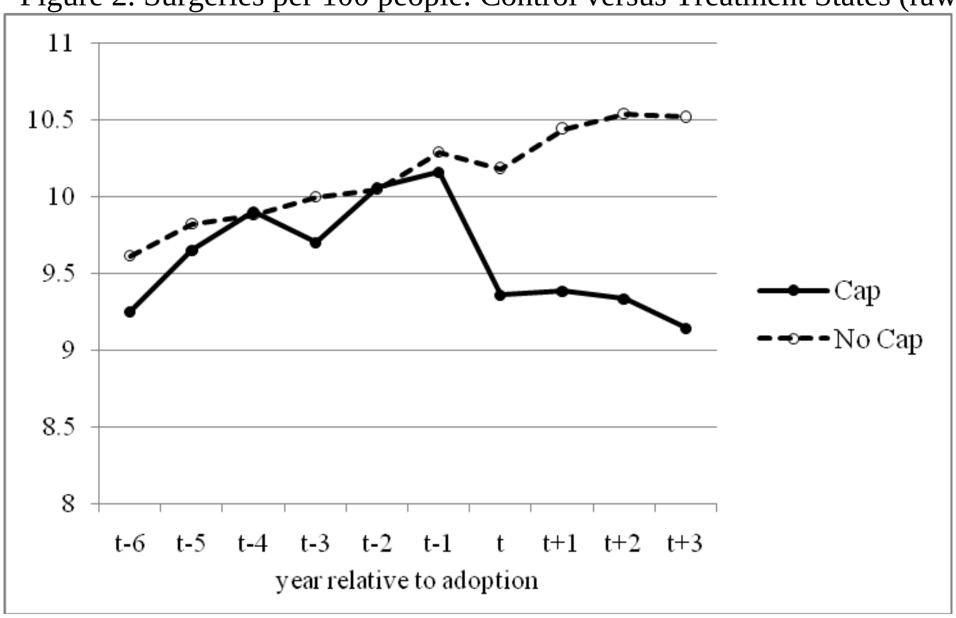
Figure 1. Admissions per 100 people: Control versus Treatment States (raw data)



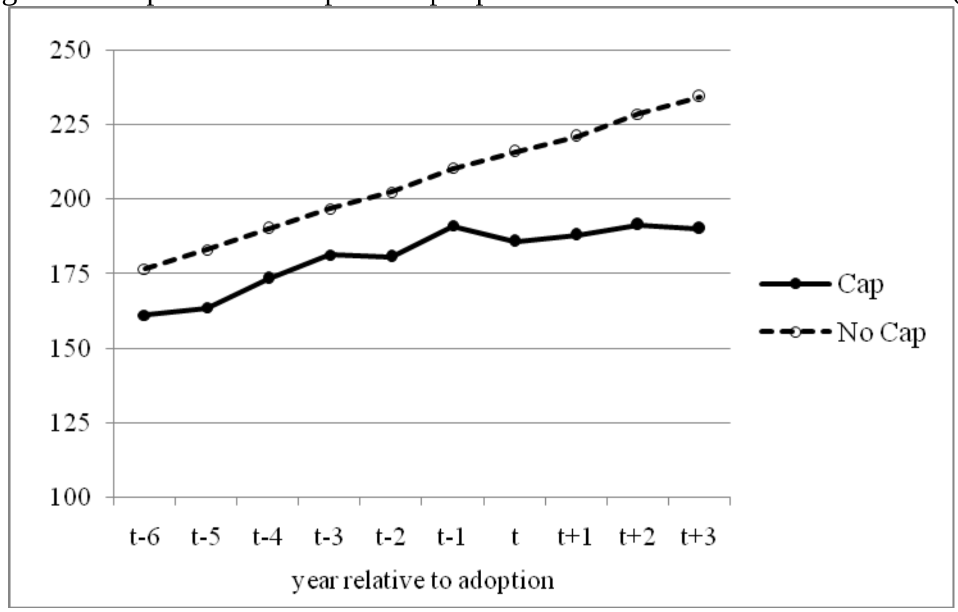
Notes: For the purpose of this figure the treatment group is defined as those states that adopted caps after 1994 but before 2004 so that comparable averages over time could be constructed. The control group is all other states that did not have a cap in the years used in constructing the control group.

Figure 2. Surgeries per 100 people: Control versus Treatment States (raw data)



Notes: For the purpose of this figure the treatment group is defined as those states that adopted caps after 1997 but before 2004 so that comparable averages over time could be constructed. The control group is all other states that did not have a cap in the years used in constructing the control group.

Figure 3. Outpatient Visits per 100 people: Control versus Treatment States (raw data)



Notes: For the purpose of this figure the treatment group is defined as those states that adopted caps after 1994 but before 2004 so that comparable averages over time could be constructed. The control group is all other states that did not have a cap in the years used in constructing the control group.

Table 1. Summary of Non-Economic Damages Caps Legislation: 1990-2006

States with Caps for the Entire Period	CA, CO, HI, KS, MD, MO, UT
States without Caps for the Entire Period	AZ, AR, CT, DE, DC, IL, IN, IA, KY, LA, ME, MA, MI, MN, NE, NH, NJ, NM, , NY, NC, PA, RI, TN, VT, VA, WA, WY
States that Adopted Caps in this Period	AK, FL, GA, ID, MS, MT, NV, ND, OK, SC, SD, TX, WV, WI
States that Repealed Caps during this Period	AL, OH, OR, WI

Source: Avraham (2010) DSTLR3 clever dataset

Table 2. Are States Adopting Non-Economic Damages Caps Similar to Non-adopting States?

Panel A. Pairwise t-tests of sample balance

Variable	No Non-economic Damages Cap	Adopt Non-economic Damages Cap	t-tests
	[1]	[2]	[3]
Log Admissions	1.888	1.797	-1.09
	(1.061)	(1.093)	
Log Outpatient Visits	4.058	3.948	-1.54
	(2.097)	(2.214)	
Log Surgeries	1.620	1.358	0.55
	(1.057)	(1.107)	
Log Mortality	6.895	6.914	0.70
	(0.295)	(0.300)	
Log Mortality - Medical Care Complications	0.213	0.329	-0.97
	(0.539)	(0.685)	
Population	83.926	65.624	3.68***
	(227.578)	(198.401)	
Black	10.084	12.247	-0.63
	(15.197)	(17.071)	
Age 25-44	28.100	26.563	0.69
	(3.277)	(3.180)	
Age 45-64	22.350	23.460	-0.25
	(3.161)	(3.143)	
Age 65	14.814	14.962	0.62
	(3.958)	(4.463)	
Log Wage	2.645	2.612	0.46
	(0.199)	(0.189)	

Notes: Column [1] and [2] report averages of county-year observations with no non-economic damages caps. Column [1] isolates the observations corresponding to states that did not adopt the caps in the following year. Column [2] isolates the observations corresponding to states that adopted the caps in the following year. Standard errors clustered at state level are reported in parentheses. Column [3] reports t-test of equality of means conditional on county and time fixed effects and state specific trends.

* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table 2 Panel B. Multivariate Regressions

	[1]	[2]
Log Admissions	-0.003	
	(0.007)	
Log Outpatient Visits	-0.006	
	(0.005)	
Log Surgeries		0.003
		(0.005)
Log Mortality	0.009	
	(0.013)	
Log Mortality - Medical Care Complications	-0.005	
	(0.005)	
Population	$0.025 \cdot 10^{-3}$	$0.019 \cdot 10^{-3}$
	$(0.019 \cdot 10^{-3})$	$(0.024 \cdot 10^{-3})$
Black	-0.001	-0.000
	(0.001)	(0.001)
Age 25-44	0.003	-0.004
	(0.004)	(0.003)
Age 45-64	0.001	-0.005
	(0.004)	(0.004)
Age 65	0.003	0.001
	(0.003)	(0.003)
Log Wage	0.018	-0.029
	(0.031)	(0.043)
Observations	40210	27275
F (p-value)	0.171	0.294

Notes: All regressions control for county and time fixed effects and state specific trends. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table 3. The Dynamics of the Impact of Noneconomic Damages Cap on Utilization Rates

	[1]	[2]	[3]	[4]	[5]	[6]
Timing of Effect	t	t	t	t	t+1	t
			Panel A: Log Admissions(1990-2006)			
Noneconomic Damages Cap	-0.026**	-0.026**	-0.026**	-0.026**	-0.024*	-0.025**
	(0.012)	(0.012)	(0.011)	(0.011)	(0.013)	(0.010)
Border Cap				0.001	0.004	0.005
				(0.007)	(0.009)	(0.009)
Punitive Damages Cap			0.004	0.004	0.001	0.000
			(0.015)	(0.015)	(0.015)	(0.015)
Joint and Several Liability			-0.008	-0.008	0.001	-0.012
			(0.009)	(0.009)	(0.013)	(0.009)
Collateral Source			0.009	0.009	0.005	0.009
			(0.009)	(0.009)	(0.006)	(0.009)
1st set controls	no	yes	yes	yes	yes	yes
2nd set controls	no	no	no	no	no	yes
Observations	52729	52729	52729	52729	52729	50561
			Panel B: Log Surgeries(1995-2006)			
Noneconomic Damages Cap	-0.036**	-0.035**	-0.034**	-0.036***	-0.030**	-0.038***
	(0.015)	(0.016)	(0.013)	(0.013)	(0.013)	(0.013)
Border Cap				0.018	0.020**	0.024***
				(0.012)	(0.009)	(0.008)
Punitive Damages Cap			0.008	0.008	-0.009	0.007
			(0.013)	(0.013)	(0.009)	(0.012)
Joint and Several Liability			-0.036*	-0.038**	-0.013	-0.043**
			(0.018)	(0.016)	(0.016)	(0.019)
Collateral Source			0.021**	0.020**	0.021**	0.018*
			(0.009)	(0.009)	(0.010)	(0.010)
1st set controls	no	yes	yes	yes	yes	yes
2nd set controls	no	no	no	no	no	yes
Observations	37267	37267	37267	37267	37267	36939
			Panel C: Log Outpatient Visits(1990-2006)			
Noneconomic Damages Cap	-0.043*	-0.041*	-0.045*	-0.044*	-0.038*	-0.040**
	(0.024)	(0.024)	(0.023)	(0.023)	(0.021)	(0.019)
Border Cap				-0.006	0.005	0.005
				(0.010)	(0.013)	(0.013)
Punitive Damages Cap			0.004	0.004	0.003	0.001
			(0.019)	(0.019)	(0.021)	(0.017)
Joint and Several Liability			0.016	0.016	0.039**	0.037***
			(0.021)	(0.021)	(0.013)	(0.013)
Collateral Source			0.011	0.011	0.007	-0.003
			(0.015)	(0.016)	(0.012)	(0.013)
1st set controls	no	yes	yes	yes	yes	yes
2nd set controls	no	no	no	no	no	yes
Observations	52729	52729	52729	52729	52729	50561

Notes: All regressions control for county and time fixed effects and state specific trends and are weighted by population in a county-year. The first set of controls includes county population, age and race structure, log income. The second set of controls adds to it: medical malpractice premium, uninsured rate, education, and state health and hospital expenditures. Robust standard errors clustered at state level are reported in parentheses.

* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table 4. The Impact of Tort Reforms on Mortality 1990-2006

			Injury		
	Total	Disease	All	Medical Care Complications	Other
Noneconomic Damages Cap	-0.001	-0.003	0.027	0.056***	0.024
	(0.004)	(0.004)	(0.021)	(0.014)	(0.021)
Border Cap	-0.000	-0.000	0.001	0.011	0.001
	(0.005)	(0.005)	(0.011)	(0.015)	(0.011)
Observations	52793	52793	52793	52793	52793

Notes: Mortality rates are expressed in logs. Both the direct and the border effects are two-year lags of non-economic damages caps laws. All regressions control for punitive damages caps, joint and several liability, collateral source reform, county population, age and race structure, log income, county and time fixed effects and state specific trends. All regressions are weighted by population in a county-year. Injury mortality corresponds to V01-X59, Y40-Y88 ICD10 codes or E810-E999 ICD9 codes. All other causes of death are included in "disease mortality". Injury mortality from medical care complications corresponds to Y40-Y84 plus Y88 ICD10 codes or E870-E879 and E930-E949 ICD 9 codes. All other causes of injury mortality are included is "other injury mortality". Robust standard errors clustered at state level are reported in parentheses.

* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table 5. The Effect of Noneconomic Damages Cap on Utilization Rates: Falsification Test

	Log Admissions	Average Length of Stay	Log Surgeries			Log Outpatient Visits		
			Total	Outpatient	Inpatient	Total	Emergency	Non-Emergency
Noneconomic Damages Cap, t	-0.027**	-1.600	-0.035**	-0.028**	-0.038***	-0.046**	-0.029	-0.066**
	(0.011)	(1.800)	(0.013)	(0.012)	(0.013)	(0.022)	(0.023)	(0.025)
Border Cap, t-1	0.004	4.676	0.023***	0.031***	0.005	0.005	0.002	-0.002
	(0.009)	(4.552)	(0.008)	(0.009)	(0.007)	(0.013)	(0.012)	(0.016)

Notes: All regressions control for punitive damages caps, joint and several liability, collateral source reform, county population, age and race structure, log income, county and time fixed effects and state specific trends. All regressions are weighted by population in a county-year. Robust standard errors clustered at state level are reported in parentheses. * significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table 6. The Effect of Noneconomic Damages Cap on Utilization Rates: Urban Versus Rural

	Log Admissions	Log Surgeries	Log Outpatient Visits	Log Mortality All Causes	Log Mortality Medical Care Complications
			Panel A: Urban		
Noneconomic Damages Cap, t	-0.027**	-0.036**	-0.047**	-0.001	0.057**
	(0.011)	(0.013)	(0.023)	(0.004)	(0.014)
Border Cap, t-1	0.005	0.023***	0.006	0.000	0.008
	(0.008)	(0.008)	(0.014)	(0.005)	(0.014)
Observations	[41579]	[29397]	[41579]	[41643]	[41643]
			Panel C: Rural		
Noneconomic Damages Cap, t	-0.027	0.001	0.008	-0.003	0.005
	(0.018)	(0.027)	(0.040)	(0.011)	(0.009)
Border Cap, t-1	-0.004	-0.001	0.003	-0.002	-0.003
	(0.019)	(0.019)	(0.029)	(0.005)	(0.008)
Observations	[11150]	[8526]	[11150]	[11150]	[11150]

Notes: Rural counties are defined either as “Completely rural or less than 2,500 urban population, adjacent to a metro area” or as “Completely rural or less than 2,500 urban population, not adjacent to a metro area.”All regressions control for punitive damages caps, joint and several liability, collateral source reform, county population, age and race structure, log income, county and time fixed effects and state specific trends. All regressions are weighted by population in a county-year. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table 7. Test Potential Reverse Causality

	Log Admissions	Log Surgeries	Log Outpatient Visits	Log Mortality All Causes	Log Mortality Medical Care Complications
[1] Enactment versus Repeal					
Enactment	-0.042***	-0.043*	-0.078***	0.003	0.045***
	(0.011)	(0.022)	(0.026)	(0.004)	(0.015)
	[38; 41562]	[38; 9795]	[38; 41562]	[38; 41626]	[38; 41626]
Repeal	0.005	-0.066	-0.011	-0.004	0.072
	(0.021)	(0.057)	(0.017)	0.010	(0.058)
	[9; 9943]	[9; 2340]	[9; 9943]	[9; 9943]	[9; 9943]
[2] Lead Capnoneconomic	-0.008	-0.013	-0.030	0.000	-0.025
	(0.007)	(0.015)	(0.020)	(0.004)	(0.021)
[3] Two Year Lead Capnoneconomic	-0.008	-0.006	-0.027	0.002	-0.040
	(0.008)	(0.016)	(0.017)	(0.003)	(0.024)
[4] Three Year Lead Capnoneconomic	-0.007	-0.010	-0.023	0.004	-0.036
	(0.007)	(0.014)	(0.016)	(0.004)	(0.027)
[5] Exclude Early Reforms	-0.027**	-0.038***	-0.047**	-0.001	0.056***
	(0.011)	(0.012)	(0.022)	(0.004)	(0.015)
[6] Exclude Late Reforms	-0.031**	-0.041***	-0.053**	-0.003	0.050***
	(0.013)	(0.013)	(0.026)	(0.004)	(0.014)
[7] Exclude County of State Capital	-0.027**	-0.043***	-0.044*	-0.001	0.064***
	(0.011)	(0.012)	(0.023)	(0.004)	(0.016)

Notes: To estimate the effect of enactment I retain the states that never adopted a cap and those that adopted a cap but never repealed a cap. To estimate the effect of repeal, I separate the states that had a cap throughout the period investigated and those that repealed a cap, but never adopted a cap during the same period. Note that non-economic damages caps laws are always coded 1 if the state has a cap and 0 if not. Thus, the coefficient always measures how utilization rates compare in states with caps relative to states without caps. In the case of surgeries I use a panel of data reported every 5 years, i.e. 1990, 1995, 2000, and 2005. All regressions control for lag border non-economic damages caps, punitive damages caps, joint and several liability, collateral source reform, county population, age and race structure, log income, county and time fixed effects and state specific trends. All regressions are weighted by population in a county-year. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table 8. Alternative Specifications

	Log Admissions	Log Surgeries	Log Outpatient Visits	Log Mortality All Causes	Log Mortality Medical Care Complications
[1] Main	-0.027**	-0.035**	-0.046**	-0.001	0.056***
	(0.011)	(0.013)	(0.022)	(0.004)	(0.014)
[2] Value of Restriction	-0.006**	-0.008**	-0.010**	-0.000	0.012***
	(0.002)	(0.003)	(0.005)	(0.001)	(0.003)
[3] Not Log	-0.375**	-0.419**	-7.435*	-2.947	0.134***
	(0.163)	(0.157)	(4.217)	(4.218)	(0.042)
[4] Trend	-0.003	-0.003	-0.005*	-0.001	-0.001
	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)
[5] South•Non-Economic Damages Cap	-0.026	-0.030	-0.007	-0.002	-0.045
	(0.024)	(0.026)	(0.047)	(0.006)	(0.028)
[6] State Level	-0.029**	-0.040**	-0.042*	-0.005	0.070***
	(0.013)	(0.015)	(0.024)	(0.004)	(0.024)
[7] Pop>1000	-0.027**	-0.035**	-0.046**	-0.001	0.056***
	(0.011)	(0.013)	(0.022)	(0.004)	(0.014)
[8] Unweighted Incidence Data	-0.238	-0.279*	-5.345	-3.895	0.020
	(0.146)	(0.157)	(4.238)	(6.759)	(0.044)
[9] Add Alaska and Hawaii	-0.026	-0.035***	-0.045**	-0.001	0.056***
	(0.011)	(0.013)	(0.022)	(0.004)	(0.014)
[10] Cap•Self Employment	-0.002	0.001	0.010	0.003	-0.002
	(0.005)	(0.006)	(0.009)	(0.003)	(0.007)
[11] Cluster by County	-0.027***	-0.035***	-0.046***	-0.001	0.056***
	(0.006)	(0.013)	(0.014)	(0.002)	(0.012)

Notes: In row [2] the value of restrictions is calculated as the logarithm of 100*(1,000,000-value of cap)/1,000,000. Row [4] reports the coefficient on the trend break. Rows [5] and [10] report the coefficient of the interaction term. All regressions control for lag border non-economic damages caps, punitive damages caps, joint and several liability, collateral source reform, county population, age and race structure, log income, county and time fixed effects and state specific trends. All regressions are weighted by population in a county-year. Robust standard errors clustered at state level are reported in parentheses.

* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Appendix – not-for-publication

Table A1 – Tort Reforms and Mortality

	Total	Disease	Injury: All	Injury: Medical Care Complications	Injury: Other
1st set of controls					
Noneconomic Damages Cap	0.001	-0.001	0.030	0.013	0.028
	(0.004)	(0.003)	(0.022)	(0.018)	(0.022)
Lag Noneconomic Damages Cap	0.003	0.001	0.032	0.045***	0.030
	(0.003)	(0.003)	(0.023)	(0.016)	(0.023)
2nd Lag Noneconomic Damages Cap	-0.000	-0.002	0.026	0.046***	0.024
	(0.004)	(0.004)	(0.022)	(0.013)	(0.022)
Joint and Several Liability	0.002	0.003	0.008	-0.013	0.008
	(0.004)	(0.004)	(0.016)	(0.018)	(0.016)
Lag Joint and Several Liability	0.006	0.005	0.027	-0.079*	0.032
	(0.004)	(0.005)	(0.027)	(0.042)	(0.026)
2nd Lag Joint and Several Liability	0.009**	0.009**	0.017	-0.084*	0.021
	(0.004)	(0.004)	(0.020)	(0.042)	(0.019)
Collateral Source	-0.000	0.001	-0.003	-0.016	-0.001
	(0.003)	(0.004)	(0.020)	(0.019)	(0.020)
Lag Collateral Source	0.002	0.004	-0.008	-0.059*	-0.005
	(0.004)	(0.004)	(0.016)	(0.034)	(0.015)
2nd Lag Collateral Source	0.003	0.004	-0.003	-0.077**	0.002
	(0.004)	(0.004)	(0.014)	(0.036)	(0.013)
No Controls					
2nd Lag Noneconomic Damages Cap	-0.007	-0.009	0.023	0.034***	0.021
	(0.006)	(0.007)	(0.023)	(0.010)	(0.024)
2nd Lag Joint and Several Liability	0.000	0.000	0.014	-0.090*	0.018
	(0.002)	(0.002)	(0.021)	(0.046)	(0.019)
2nd Lag Collateral Source	-0.002	-0.001	-0.004	-0.087**	0.000
	(0.003)	(0.003)	(0.014)	(0.039)	(0.013)
2nd set of controls					
2nd Lag Noneconomic Damages Cap, t-2	-0.001	-0.003	0.027	0.057***	0.024
	(0.004)	(0.004)	(0.021)	(0.014)	(0.021)
2nd Lag Joint and Several Liability	0.009**	0.009***	0.022	0.052**	0.024
	(0.004)	(0.004)	(0.017)	(0.025)	(0.017)
2nd Lag Collateral Source	-0.001	0.001	-0.015	-0.065	-0.011
	(0.004)	(0.004)	(0.013)	(0.029)	(0.012)
Lead Reform					
Noneconomic Damages Cap	0.001	-0.002	0.027	-0.023	0.028
	(0.004)	(0.003)	(0.018)	(0.021)	(0.018)
Joint and Several Liability	-0.002	-0.002	0.004	0.027	0.002
	(0.002)	(0.003)	(0.020)	(0.024)	(0.021)
Collateral Source	-0.002	-0.002	0.003	0.023*	0.002
	(0.003)	(0.004)	(0.022)	(0.013)	(0.023)

Notes: Mortality rates are expressed in logs. All regressions control for county and time fixed effects and state specific trends. The first set of controls includes county population, age and race structure, and log income. The second set of controls include in addition to the first set of controls: two-year lag of punitive damages caps and the

second lag of the other types of tort reforms considered in this table. All regressions are weighted by population in a county-year. Injury mortality corresponds to V01-X59, Y40-Y88 ICD10 codes or E810-E999 ICD9 codes. All other causes of death are included in "disease mortality". Injury mortality from medical care complications corresponds to Y40-Y84 plus Y88 ICD10 codes or E870-E879 and E930-E949 ICD 9 codes. All other causes of injury mortality are included is "other injury mortality". Robust standard errors clustered at state level are reported in parentheses.
* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.

Table A2: The Effect of Non-Economic Damages Cap: Enactment versus Repeal: An Event Analysis

	Log Admissions	Log Surgeries	Log Outpatient Visits	Log Mortality All Causes	Log Mortality Medical Care Complications
Enactment 1 year impact	-0.004 (0.006)	-0.010 0.008	-0.032** (0.014)	0.005 (0.003)	0.022* (0.011)
Enactment 2 year impact	-0.014* (0.008)	-0.017*** (0.006)	-0.041** (0.016)	0.005 (0.004)	0.039*** (0.013)
Enactment 3 year impact	-0.023** (0.009)	-0.020** (0.008)	-0.054** (0.025)	0.005 (0.004)	0.049*** (0.015)
Repeal 1 year impact	-0.006 (0.010)	0.006 (0.024)	0.027 (0.021)	0.006 (0.004)	0.038 (0.032)
Repeal 2 year impact	-0.011 (0.007)	0.011 (0.020)	0.013 (0.019)	0.002 (0.003)	0.044 (0.036)
Repeal 3 year impact	-0.009 (0.010)	0.014 (0.021)	0.003 (0.017)	0.002 (0.004)	0.051 (0.032)

Notes: To estimate the effect of enactment I retain the states that never adopted a cap and those that adopted a cap during the period investigated. The law is coded as 1 in states with a non-economic damages cap within n years of the cap adoption (n varies across specifications: it is 1 in row 1, 2 in row 2 and 3 in row 3) and zero otherwise.
To estimate the effect of repeal, I separate the states that had a cap throughout the period investigated and those that repealed a cap during the same period. The law is coded as 0 in states without a non-economic damages cap within n years of the cap repeal (n varies across specifications: it is 1 in row 4, 2 in row 5, and 3 in row 6) and 1 otherwise. All regressions control for lag border non-economic damages caps, punitive damages caps, joint and several liability, collateral source reform, county population, age and race structure, log income, county and time fixed effects and state specific trends. All regressions are weighted by population in a county-year. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.