Tort Reform and the Demand for Medical Care:
Evidence from State-by-State Variation in Non-Economic Damages Caps*

ABSTRACT

Previous literature indicates that non-economic damages caps increase the number of physicians but finds no significant effect on health. A potential explanation is that, by reducing the cost of malpractice, caps affect physicians' incentives to provide high quality care, an important determinant of the demand of medical care. Using county level panel data this paper finds that caps adoption leads to a 4 percents decrease in surgeries, a 2 percent decrease in hospital admissions but has no significant effect on emergency care, outpatient visits, birth rates, or prenatal care conditional on births. There is also evidence of increase use of physicians located across the border. Taken together these results provide suggestive evidence of a decrease in demand leading to a net negative effect on utilization rates.

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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 damages in medical liability cases, aim to lower both.

Since one important component of the operating cost of a medical practice is the medical liability insurance, regulation that reduces insurance premiums increases the profitability of the medical profession and should induce entry into the medical field. The literature to be reviewed below confirms that states that adopted non-economic damages caps experienced an increase in number of physicians. Entry should improve access through two channels. First, more competition may decrease prices. Second, entry may reduce the transportation and time costs associated with medical care consumption (Dranove and Gron, 2005). The drawback is that by lowering the insurance premium, such reforms reduce the cost of malpractice and with it physicians' incentives to provide high-quality care. Previous literature (Kessler and McClellan, 1996; Currie and Macleod, 2008) indicates that tort reform leads to changes in the health care production process. Since demand depends on the current state of information about treatment patterns, such changes are expected to affect demand. When there are changes in the demand for care, measuring the impact of regulation on the supply of physicians cannot capture the entire effect of regulation.

This paper investigates the impact of non-economic damages caps on medical care utilization rates. On the statistical side, the investigation faces some challenges. First, there may be spillover responses from neighboring states that have adopted or repealed caps. Second, many times several types of reforms were enacted simultaneously, thus, making it difficult to disentangle the impact of an individual reform. Third, the adoption of the regulation may be related to the provision of medical care, raising concerns about the identification of the exact causal relationship. And finally, the relationship between non-economic damages caps and medical care varies across types of medical services because caps are more likely to be binding in the case of certain medical specialties.

This paper addresses these concerns in a panel of county-year observations for the period 1990-2005 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 allows the researcher to control for small-area specific factors such as variations in litigiousness culture that were shown to be important determinants 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 demand for medical care such as changes in health care policy at federal level. State specific trends controls for one source of selection that would make a state more likely to adopt the regulation. In this specification the impact of regulation 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 of bias from reverse causality since the literature seems to indicate that tort reforms were mainly driven by relative power of diverse interest groups (Rubin, 2005). Nevertheless, any such concerns are further addressed in several ways. First, while the adoption of regulations is likely endogenous at state level, this paper uses disaggregated county-

level data to mitigate such concerns. Moreover, I use individual level data when available to check the robustness of the results obtained using county level data. It is highly unlikely that any single individual demand for medical care could influence the adoption of the law; consequently, the regulation represents an exogenous change in each individual environment. The consistency of results across different levels of disaggregation provides support for the exogeneity of caps adoption. 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. Third, I show that the estimated effect is robust to using only the variation provided by repeals. Repeals are decided by courts, usually on constitutional bases, and thus more likely to be exogenous.

In addition, this paper is able to solve several other problems that plagued previous research. It concentrates on a period of data, 1990-2005, during which there were relatively few changes in other types of health regulations and, as such, is better able to isolate the impact of an individual regulation, non-economic damages caps. The exception is changes in punitive damages[1] cap type of regulation, and this paper controls for such a possible confounding factor in some specifications.

More importantly, it recognizes that the relevant measure of success of this regulation to insure better access to medical care may not be whether there are more physicians in states adopting the regulation but whether there are more medical services actually delivered to the population. This paper also complements previous studies that investigate the impact of such regulation on health. Previous studies found that states that adopt non-economic damages caps do have more physicians; however, the evidence goes against significant effects on health. A potential explanation is that while there are more doctors there are not necessarily more medical services actually delivered. It could be that the demand for medical care is very inelastic

[1] Punitive damages are damages awarded in addition to compensatory (economic and non-economic) damages to punish a defendant for willful and wanton conduct.

(Manning et al., 1987) so changes in supply do not produce large effects on the quantity of medical care delivered. A second explanation is based on a conceptual problem. Economic models of health production (Grossman, 1972) predict that changes in relative prices lead to substitution between medical care and other inputs used in health production. Consequently, even if the regulation did affect access to medical care, there may not be large impacts on health. Thirdly, changes in the expected quality of health, due to changes in treatment patterns and to the information surfacing on the occasion of legislative debates on the adoption of caps regulation, affect negatively the demand for medical care. This paper finds that caps have a net negative impact on utilization rates of some types of medical services (Figure 1 and 2), suggestive of the later proposed explanation, but does not reject the hypothesis that the other factors also play a role.

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 be binding. Based on the National Physician Survey of Professional Liability, 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, emergency visits, birth rates and prenatal care, all cases where the regulation is likely to be binding. As a falsification test, the analysis is extended to outpatient visits, defined to include clinic and referred visits and exclude emergency visits and outpatient surgeries. Since it is unlikely that caps are binding in this case, the regulation should have no effect.

Overall, the analysis suggests that non-economic damages caps have a significant negative effect on surgeries and hospital admissions. The effect on surgeries is mainly driven by a decrease in outpatient surgeries, with no statistically significant effect on inpatient surgery. In addition, caps have a negative but insignificant effect on emergency visits. As expected, I find no

statistically significant effect of caps on outpatient visits. There is no evidence of an impact on birth rate; however, there is some evidence of a decrease in first-time pregnancies and an increase in births to women who already have children. This result supports earlier findings indicating that new quality information is more likely to affect individuals making a choice for the first time (Wedig and Tai-Seale, 2002; Jin and Sorensen, 2006). There are no significant changes in the timing of initiating prenatal care.

There is also some evidence that the patients in states adopting the cap cross geographical borders in order to search for a physician. Taken together, these results are consistent with an increase in the numbers of doctors that is offset by a decrease in demand.

A caveat to these calculations is that the results may overstate the actual impact if there are mitigating adaptations to the new market conditions. For instance, in long run the new treatment patterns become accepted at the standard of care or physicians and medical organizations may be able to develop internal mechanisms to enforce and signal a certain level of quality, in which case there is no long-term drop in demand.

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

I. Background on Non-Economic Damages Caps

Individuals' demand for health generates the demand for medical care. The actual amount of medical care acquired depends on this input's relative price, which comprises the monetary price of the service, the transportation cost to the provider, and the time cost associated with the consumption of the service (waiting time for instance). In order to make medical care more

accessible, policy makers have focused on measures with potential to reduce this price. 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. 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). The justification behind non-economic damages caps is the difficulty faced by juries in assessing the value of non-economic losses. Thus the awarded compensations for such damages should be bounded in order to offer juries guidance in evaluating non-economic losses.

The success of such regulation depends on two factors. First, it depends on whether insurance companies pass some of the savings from reduced awards onto their customers, the physicians, in the form of lower insurance premiums. Second, it depends on whether the physicians, whose operating costs have been reduced, pass some of these savings onto their consumers. If these two conditions are met, the price of medical care will decrease. In addition, 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 cost associated with the consumption of medical care.

An extensive literature documents the relationship between non-economic damages caps and insurance premiums. First, there is some evidence that non-economic damages caps are binding. Using closed claims data from National Association of Insurance Commissioners (NAIC), Sloan, Mergenhagen, and Bovbjerg (1989) find that non-economic damage caps reduced insurer payouts. In addition, Zuckerman, Bovbjerg, and Sloan (1990) results from per-physician premium data from the Health Care Financing Administration survey of insurers

indicate that caps decreased the average indemnity per claim. However, the evidence of a strong relationship between malpractice awards and insurance premiums is somewhat mixed. Baicker and Chandra (2005a) find that increases in malpractice payments do not result in an increase in premium rates, while Thorpe (2004) using state specific NAIC data from 1985-2001 concludes that premium rates are lower in states that regulated the amount of non-economic damages. Over a similar period of time, 1994-2003, 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. Also Viscusi and Born (2005) study reports that in 1984-1991 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, the physicians, in the form of lower premium rates.

These results provide support to the idea that physicians' operating costs are reduced and we should observe an increase in the number of physicians in states that have adopted non-economic damages caps. 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) find that there are more doctors in states that have a cap. Kessler, Sage, and Becker (2005) study also provides support toward an increase in physicians supply caused by tort reforms. Wolfson (2005) finds that non-economic damages caps improves minority access to medical care. Helland and Showalter (2006) also estimate the responsiveness of physicians to change 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). On the other hand, Yang et al. (2008) find

no evidence of that tort reform increased in the number of obstetrician-gynecologists between 1992 and 2002.

As this regulation affects physicians, it also changes the process of medical care provision. The role of tort law is to discourage negligence and, as such, to promote physicians' behaviors that increase quality and enhance the likelihood of positive outcomes. The result is a change in the characteristics of medical service and thus of the demand of medical care. 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 report that the fear of being sued affected their decisions (Studdert et al., 2005).

There is concern that by lowering the cost of malpractice, caps reduce physicians' incentives to offer high-quality care. Consequently, it is expected that, by reducing the fear of malpractice, caps adoption affect physicians' treatment choices. The literature provides such evidence that caps changed physicians' choice of procedures. Currie and Macleod (2008), for example, 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.

Nevertheless, the purpose of those procedures was likely to reduce involvement in litigations, either by offering the physician extra reassurance against the possibility of a mistake or by offering the patient a guarantee that the physician did everything possible to insure a positive outcome. The strategy appears to have been successful, 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). To the extent that patients value the extra reassurance and the demand for medical care depends on the current state of information about its production and effectiveness (Keeler, 1995; McClellan 1995), changes in treatment patterns will negatively affect demand. Undoubtedly historically, the types of procedures offered have lowered the threshold for intervention and increased utilization rates,[2] even in the absence of changes in outcomes associated with each procedure (Cutler and Huckman, 2003). The question that remains to be answered in this paper is whether the changes in choice of procedures triggered by caps adoption have a significant effect on utilization rates. Such effect would explain the inconsistency between the documented increase in supply 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)

There are several factors that may explain why there is no large impact on health associated with an increase in supply of medical care. First, the quantity of medical care delivered may not have increased much due to an inelastic demand. There is evidence of an inelastic demand for medical care; Manning et al. (1987) using data from the RAND Health Insurance Experiment obtained a -0.2 estimate of the demand for medical care. However, another issue to consider is the responsiveness to changes in the time price of medical care, which is associated with transportation to the provider location. The waiting time for a consultation

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

should drop with an increase in the number of physicians. Even if quantity demanded does not react much to changes in the price charged for medical services, it may react to changes in the time price of medical care. Moreover, early intervention is associated with better outcomes, so it does not necessarily follow that because quantity is insensitive to changes in the monetary price we should not observe changes in health outcomes associated with the adoption of non-economic damages caps.

Second, caps on damages alter physicians' incentives to provide high quality care. An increase in the incidence of medical mistakes may offset the health gains from improved access to medical care. Some indication of this effect may come from changes in trends in the number of malpractice cases filed. A higher incidence of mistakes likely leads to an increase in the number of cases filed by patients trying to cover their losses. However, reduced awards also decrease the 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 a physician 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, Donahue and Ho (2007) find that over the 1991 to 2004 period there is no statistically significant change in malpractice claims against physicians associated with damages caps adoption. As a result trends in the incidence of malpractice cases alone cannot identify changes in malpractice. In any case, these results do not suggest large changes in the incidence of medical mistakes because the effect mentioned above is not large or large enough to offset the decreased incentives to sue due to reduced awards.

Instead, as mentioned above, the literature finds that caps triggered changes in treatment patterns. This suggests a third explanation; these changes along with new information aired on the occasion of the legislative debates about caps adoption affects individuals' perception about the expected quality of service. People react by increasing search for the right physician and

perhaps by reducing their demand for certain services now perceived riskier. Such adaptation would also mitigate the impact of changes in treatment choices on health and are consistent with the findings regarding the number of medical malpractice cases filed. The analysis below finds support for this last hypothesis of a decrease in demand.

II. Conceptual Framework

The market for medical care services is characterized by asymmetric information; physicians know more about their abilities than do their patients and the quality of the product cannot be assessed ex-ante. Individuals may be reluctant to buy a service of uncertain quality, leading to an inefficiently low quantity of medical care consumed (Ackerlof, 1970). Several measures can alleviate the problem.

Governments in all states adopted quality signaling regulation such as licensing requirements based on minimum medical knowledge as proved by tests (United States Medical Licensing Examination, USMLE). Minimum proved expertise offers information about medical care quality but the problem is not completely solved because there is still significant variation among physicians having at least the minimum level of competence.

Part of the problem is solved by reputation: as some physicians prove they deliver high quality care, the demand for their services increases. For such a mechanism to work, physicians should be able to benefit from their commitment to provide high-quality services. The drawback lies in the period needed to build the reputation, period during which the physician does not realize benefits from providing high-quality service. In addition, when the insurance premiums do not perfectly adjust for past experience, (Sloan, 1990), and the cross-subsidization from low-risk to high-risk physicians is substantial (Fournier, 2001) the incentive to increase quality are further diminished.

Another way to convey the information to consumers is to provide a credible guarantee that the service provided has the promised quality (Grossman, 1981). This guarantee has value only if the provider can be constrained to honor his promise in case of failure to deliver. Providers that do not provide the guarantee will find themselves facing a lower demand for their services. Physicians agree to provide standard treatments and use malpractice insurance contracts to guarantee that when failure to provide the standard treatments results in negative outcomes the patient will be fully compensated.

Non-economic damages caps could affect the demand for medical care in two ways. They affect physicians' choice of treatment and with it the characteristics of the service. Caps also affect the guarantee that the patients will be fully compensated in the case of a medical mistake. The result of such changes is an attendant decrease in the demand for medical care.

If the supply increases and the demand decreases, the net effect on the amount of medical services delivered is ambiguous. In principle, this effect should manifest both in the number of visits to the doctor and in the number of services (procedures, tests, etc.) each individual buys. However, given the particularities of health care, it is likely that physicians/suppliers determine the procedures to be performed on the occasion of each visit, while consumers determine only whether and when to initiate an episode of care (McCombs, 1984). In light of the above, it seems appropriate to define a medical service as the care delivered or the bundle of tests, advice, and procedures performed on the occasion of a visit. Consequently, physicians determine the characteristics of medical services through choices of resources to be used per visit. These characteristics affect the demand for medical services as measured by the number of initiated episodes of care/visits.[3] In this framework, there is still significant diversity across medical services, because there is large variance in individuals' reasons for visiting a physician. As a

[3] Empirical evidence supports the idea that consumers react to characteristics of the bundle of services offered on the occasion of a visit. Kenkel (1990) finds that information increases the probability that a consumer uses medical care, but conditional on use, the quantity of care consumed is not related to information.

result, the net impact of damages caps on the delivery of medical services can be captured through observations of the number of visits.

This paper's goal is to obtain an estimate of the impact of non-economic damages caps on health care delivery as measured by number of patients admitted in hospitals, number of surgeries performed, emergency visits, and outpatient visits.[4] In addition, since caps are likely to be binding for obstetrics this paper investigates the impact on births and prenatal care.[5] The next section outlines the econometric model used to investigate the impact of non-economic damages caps on all these variables.

III. 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. It could be the proportion of county population that buys a certain medical service such as impatient treatment, surgery, emergency care, or other outpatient care. It could also be the birth rate, average number of prenatal visits, or the proportion of future mothers that initiated prenatal care in the first, second, third trimester, or that received no prenatal care. CAP_{st} is the variable of interest. It is a dummy variable indicating the state has a cap in effect on non-economic damages in a given year regardless of the value of the cap.

When consumers' response to caps adoption is longer and wider search for a physician, such search may translate into choices of physicians located in bordering states. As a result, there are expected spillovers from regulation adoption in neighboring states. To disentangle the direct impact of non-economic caps adoption from any spillovers from neighboring states, the model

[4] Outpatient visits refer to clinic and referred visits and exclude emergency visits and outpatient surgery.
[5] Finley (2004) finds that in a sample of 28 gynecological malpractice cases in California, 83% of damages awarded by juries were non-economic; Pace et al. (2004) reports that, in California, a $250,000 non-economic damages cap would be binding in 71% of newborn and fetal injuries cases.

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 the caps, $BORDER\ CAP_{ct}$. X_{ct} is a vector of observable time varying county characteristics that affect the demand for medical care such as wages, age and race composition of the population. It is to be expected that higher income and an older population are associated with higher demand for health care. The variable reflecting the racial structure of the population controls for possible systematic differences in demands from different segments of the population. In the regression analysis investigating the impact of caps on prenatal care X_{ct} is a vector of observable county characteristics such as average age, race structure, marital status, and education of mothers in a county-year, and county level average wages. ε_{ct} is the stochastic error term.

In order to account for unobserved county specific time invariant determinants of the demand for medical care this model specification includes county fixed effects, α_c. For instance, differences in the overall level of health of the 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 exogeneity of the regulation adoption. Conditional on county and year fixed effects and state specific trends, the θ's are identified from year-by-year changes in regulation after controlling for shocks common to all counties and state specific trends in medical care utilization rates.

There are two more 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.[6] Second, the independent variable of interest varies only at the state level. Moreover, there are only six 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.[7]

III. Data Sources and Summary Statistics

This analysis is performed on 1990-2005 data.[8] The main advantage to this sample is that during this period there were few changes in other types of health regulation, reducing the concerns about confounds. No state adopted or repealed contingency fees regulations[9], only one state repealed a cap on total damages and no state adopted such cap, only one state repealed regulation regarding patients' compensation funds and no state adopted such regulation. There is however significant time variation in punitive damages caps. Nevertheless, this is 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. [10] The reason is that the judge 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, they are intended for willful and wanton conduct. Given the narrow range of situations where punitive

[6] In the case of prenatal care, regressions are weighted by births, but using county population as weights does not change the results (results available on request).
[7] The results are robust to clustering at county level as shown in the robustness check Table 7.
[8] Sample size varies across regressions function of data availability.
[9] 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] 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.

damages are applicable, any information in media about punitive damages caps is less likely to affect the perception about the quality of the care offered by the average physician. And second, punitive damages do not seem to be significantly larger than compensatory awards. In fact for the period 1963 and 1993, 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 1.21 to 1. Being rarely awarded and not significantly larger than compensatory awards, punitive damages caps are less likely to have a significantly impact. Nevertheless, to further check the reliability of the above inference, this paper controls for such a possible confounding factor in some specifications.

The county level data on hospital *admissions, surgeries, emergency visits, and outpatient visits*[11] comes from US 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[12], short term general hospitals[13], and long term hospitals[14]. All ARF hospital utilization data originates from the AHA Annual Survey of Hospitals. The *birth rate and prenatal care data* comes from the National Center for Health Statistics.

State by state *legislative data* (Table 1) is taken from Ronen Avraham "Database of State Tort Law Reforms," (2006). The impact of the law is measured by introducing a dummy variable indicating 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

[11] Outpatient visits include clinic and referred visits and exclude emergency and surgery.
[12] Short Term Non-General Hospitals are those coded as follows by the American Hospital Association: 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.
[13] Short Term General Hospitals are those coded as follows by the American Hospital Association: 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.
[14] Long Term Hospitals are those coded as follows by the American Hospital Association: 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.

the year after. During the 1990 to 2005 period there were 13 instances of non-economic cap adoption and 6 of repeal so there is significant time variation in the data[15].

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 which identifies which counties are located within 100 miles from 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.

The key identifying restriction in this paper is that the non-economic damages cap regulation adoption is exogenous. The literature indicates that the timing of the adoption is mostly due to the 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 regulation. Panel A of Table 2 reports the mean values of a number of variables in the county-years with no regulation 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 t-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. The findings suggest 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, birth rates, starting date of prenatal care, population, age and racial structure, and wages. Moreover, the results reported in Panel B of Table 2 that considers these variables simultaneously in a regression based analysis

[15] Some states both adopted and repealed caps during this period.

controlling for all fixed effects and time trends mentioned in the base specification support the previous findings. When considering the medical care measures along with demographic characteristics of the counties, their joint p-value ranges from 0.62 to 0.75 indicating that these variables are poor predictors of the adoption of non-economic damages caps. In the robustness analysis (see Table 7), I show that the inclusion of additional controls or the exclusion of all control variables has little effect on the estimated coefficient of non-economic damages cap variable. Combined, the relative lack of predictability of regulation adoption based on variables influencing the demand for medical care, and the invariance of the results to adding controls that may explain regulation adoption, suggests that the identification assumption is plausible.

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.[16] They indicate that non-economic damages caps adoption is negatively correlated with hospital admissions, surgeries, and emergency visits but it is statistically significant only in the case of admissions and surgeries. Since changes in the characteristics of medical care sometimes have effects on the demand for medical services that become noticeable only in the future, I investigate the possibility of lag effects[17]. There is variation in the timing of the effect, with a one-year lag effect on surgeries[18] and some evidence

[16] All regressions control for county and time fixed effects and state specific trends. The utilization rates and births rate regressions control for county age and race structure, log wages, and non-economic damages caps at border, and are weighted by population in a county-year. The "% first child" specifications controls for age, race, education and marital status of mothers in each county, log county wages, and non-economic damages caps at the border, and are weighted by birth count in a county-year.

[17] There is sometimes considerable delay between the onset of symptoms and surgery or even the first visit to the physician. Even in cases 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).

[18] I use 1 year lag of non-economic damages caps even when the analysis is performed on every-5 year data (the case of surgeries).

of a lag in the effect on admissions, but the instantaneous effect remains the most important in the case of admissions.

The effect is not very large in absolute value; the adoption of non-economic damages cap reduces admissions by about 2% and surgeries by 4%, the equivalent of approximately 3 fewer admissions and 4 fewer surgeries per 1,000 individuals. However, these numbers should be put into context. Previous literature estimates the effect of non-economic damages caps on the supply of physicians to be 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 4% decrease in surgeries, a clear indication that the demand decreased. Interestingly, the effect on surgeries is mainly driven by a decrease in outpatient surgeries. There is no statistically significant change in inpatient surgeries,[19] likely because there are fewer alternative treatments for these surgeries and there are significant costs associated with delayed treatment.

In addition, there is some evidence that the adoption of regulations in bordering states leads to an increase in utilization rates in counties within 100 miles of those borders. I find no effect from either in-state or border-state adoption of caps on outpatient visits. Since in this case caps are likely not binding these results improve the confidence in the overall findings.

Since obstetrics and gynecology is one area where non-economic damages caps are binding, I investigate caps' potential effect on the demand for these services by looking for changes in birth rates and the timing of initiation of prenatal care. The last two columns in Table 3 report the estimated effect of caps adoption on birth rates. Because of the 9-months difference between conception and delivery, I use one-year lag of regulation to measure the instantaneous

[19] In the preferred specification when non-economic damages caps is measured with a lag in the surgery regressions, the two standard error bands of the estimated effect on outpatient surgery excludes the estimated effect on inpatient surgery.

effect (the current status of regulation should have no effect on births the same year, as shown in the robustness check). I find no effect on total birth rate, but there is some evidence of selection among women choosing to have a baby. Specifically, the proportion of first-time births decreases. An increase in births to women already having other children is to be expected if access to medical care improved. The decrease in births to first-time mothers is likely explained by demand factors. As mentioned in the introduction, previous literature indicates that new quality information is more likely to affect individuals making a choice for the first time, which is understandable if women who have children had previously found a physician they could trust.

I also investigate the effect of caps adoption on the timing of initiating prenatal care conditional on births. Since NCHS identifies both the state of residence of the mother and the state of the occurrence of birth I can better separate the effect of cap adoption from spillovers from neighboring states by retaining only women that gave birth in their state of residence. Table 4 reports the estimated effect on prenatal care using this sample. However, the results obtained using the entire sample and controlling for caps in bordering states are very similar.[20] Panel A reports county-level estimates of the effect of current status of non-economic damages caps conditional on conception, which amounts to measuring the effect on women already pregnant at the time of the reform.[21] Panel B presents the estimated lagged effect of caps, which can be interpreted as the effect on women that made the choice to have a baby after the adoption of caps.[22] There is no evidence of significant changes in the timing of initiation of prenatal care. There is however, evidence of an increase in prenatal visits. This result may reflect changes in treatment patterns dictated by physicians, because the physician recommends the frequency of

[20] Only 2% of women are reported to give birth in another state than their state of residence.

[21] The dependent variable is the average number of prenatal visits for all mothers giving birth in a particular county-year. This measure is not adjusted for gestation because prenatal visits are not linearly related with gestation. However, using prenatal visits corrected for gestation instead produces no qualitative change of the results (results are available on request). Also there is no evidence of an impact of non-economic damages caps on gestation (results available on request).

[22] Regressions reported in panel A and B control for age, race, education and marital status of mothers in each county, log county income, non-economic damages caps at the border, county fixed effects, time fixed effects and state specific trends, and are weighted by birth count in a county-year.

visits once the woman starts prenatal care. It is also consistent with findings of previous literature suggesting that, upon caps adoption, physicians in this field tend to increase the number of procedures used (Currie and Macleod, 2008). An alternative explanation is that some of the change is explained by selection of women choosing to have a baby.

Additionally, since NCHS collects individual level data on prenatal care I check the robustness of results obtained on county level data using this more disaggregated data. The advantage is that the adoption of regulation is certainly not related to the medical care choices of a single individual and thus, reverse causality is less of a concern in this specification. Thus, there is more confidence in the exogeneity of regulation in this model. The dependent variable is the actual number of months since prenatal care begun. Since the number of months of prenatal care varies with gestation and normalizing by gestation affects the comparability of prenatal care, I restrict the sample to women giving birth after a 40 weeks gestation period.[23]

The results are reported in Table 4 panel C and D separately by race and by order of birth.[24] The separation by race follows previous literature indicating differential impact on infant mortality for blacks. The separation by birth order, first child, and second or subsequent children, accounts for the fact that a woman that has given birth before already found physician and thus there is limited scope for her search for a physician. If there is an impact on how early a woman starts prenatal care it is likely to be larger on the first pregnancy. These estimated effects are entirely consistent with the results obtained from the county-level analysis. The results cannot reject the hypothesis that non-economic damages caps have a zero net impact on prenatal care. I also find that the effect on prenatal visits is likely the result of better prenatal care of black mothers, which is consistent with previous literature indicating decreases in mortality only for black infants (Klick and Stratman, 2005).

[23] A possible solution would be to use a model with the dependent variable indicating the pregnancy month when prenatal care begun. However, this model runs into the problem of coding instances of no prenatal care. Also there is no evidence of an impact on gestation (results available on request).

[24] Regressions reported in panel C and D control for mother's age, race, education, log wage in county of residence, state fixed effects, time fixed effects, and state specific trends.

Taken together, the results provide suggestive evidence that the effect of the shift in the supply of medical care on medical services delivered is offset by other factors. One explanation is endogeneity, another one a change in demand. However, there is little evidence that endogeneity is in fact the driving factor. Previous literature summarized by Rubin (2005) finds that the adoption of tort regulation is mainly driven by political vagaries. It is also not related to health outcomes (Rubin and Shepherd, 2007). Statistical tests also do not indicate any significant differences among counties that adopt and those that do not adopt the regulation in the following year as measured by several observable county characteristics.

5.2. 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.[25] First, to estimate the effect of enactment, I separate the states that never adopted a cap and those that adopted a cap but never repealed a cap. Second, 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. As reported in Table 5 the estimates of the impact of repeal are noisier and have larger standard errors, because there is less variation in this sample and thus this specification is very demanding on the data.[26] Nevertheless, these estimates are very similar to those obtained for the effect of enactment, providing further confidence that non-economic damages caps are exogenous to utilization rates.

[25] Since the results reported in Table 3 indicate heterogeneity in the timing of the effect, I report the 1-year lag effect for surgeries; the instantaneous effect for admissions, outpatient visits, and emergency visits; and the 2-year lag effect for birth rate.

[26] Only 5 states repealed caps in the sample used for columns [1] to [3] and only 2 states repealed caps in the sample used in columns [4] to [6]. I drop the states that both adopted and repealed caps during this period.

Next 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 regulation 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 5. I also find that neither 2-year lead or a 3-year lead of non-economic damages caps are significantly correlated with current level of utilization rates, with the possible exception of the second lead of caps in the case of outpatient surgeries which is positive and significant at 10% significance level. To the extent this is more than a statistical fluke, the estimated effect of caps on outpatient surgeries would be biased downward and would provide a lower bound of the actual effect. 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.

To further check the validity of our estimates, this paper performs a series of sensitivity checks. A number of alternative specifications were tested and some of the results presented in Table 6.

5.3. Robustness Check

5.3.1. Row [1] Table 6 presents the primary results for easier comparability. Since the results reported in Table 3 indicate heterogeneity in the timing of the effect, I report the 1-year lag effect for surgeries; the instantaneous effect for admissions, outpatient visits, and emergency visits; and the 2-year lag effect for birth rate.

5.3.2. 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, dependent variable should be utilization rates. I find that the results are robust to the choice of functional form.

5.3.3. Trend Break. It is possible for the regulation 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 6.

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

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.

5.3.4. Add punitive damages cap. One possible source of confound is the simultaneous adoption of other types of regulation. Specifically, between 1990 and 2005 there was significant legislative activity regarding punitive damages: there were13 instances of punitive damages caps adoption and 2 instances of repeals. As explained in the data section, the impact of such caps is likely small, but to make sure the estimates of the non-economic damages caps are not picking up the impact of punitive damages I control for punitive damages caps adoption. Row [4] of Table 6 reports the estimates obtained from augmenting the base specification with an indicator variable equal to 1 if the state has a punitive damages cap in a particular year, regardless of the level. As expected, the results are not sensitive to the inclusion of this variable.

5.3.5. Malpractice. 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, in row [5] of Table 6, the main specification is augmented with a variable that measures the medical malpractice premium for general surgery in columns [1] to [3], the premium for internal medicine in columns [4] to [6], and the premium for obstetrics-gynecology in columns [7] and [8]. The results are robust to this inclusion, consistent with previous literature

indicating no relation between malpractice liability and damages caps (Baiker and Chandra, 2005b).

5.3.6. Changes in price of medical care. Another potential source of confound is the increase in the price of medical care due to the hospital consolidation that took place in the 1990s. Hospital consolidation leads to higher transaction prices[27] and thus increases the number of uninsured (Town et al., 2006). Since the proportion of insured population is an important determinant of medical care utilization rates and could also affect the timing of caps adoption, an increase in uninsured rate triggered by hospital consolidation could confound the impact of non-economic damages caps.[28] Table 6 row [6] presents results that include controls for the uninsured rate. The results are robust to this inclusion.[29]

Moreover, I further investigate this issue using a falsification test. 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 [7], 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. The interaction terms are significant in the birth rates regressions, a result entirely driven by the case of Alabama.

These tests provide suggestive evidence that trends in insurance rates do not bias the estimated impact of caps.

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[27] 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, and a couple of more recent papers Capps and Dranove (2004) and Dafny (2009).

[28] Another change in relative price of medical care could come from a change in transportation costs if hospital consolidation decreases the number of hospitals. However, the data indicates 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 more negative if a control for hospitals is added to the main specification – results not reported but available on request.

[29] When possible, county level uninsured rate from SAIPE was used to check the results – results not reported but available on request. The results obtained are similar.

5.3.7. No controls. Under the hypothesis that after accounting for county and year fixed effects and state specific trends regulation adoption is exogenous, adding controls should not affect the estimated effect of non-economic damages caps. As noticeable in row [8] of Table 6, the coefficients are not sensitive to the exclusion of all controls, providing suggestive evidence that the identifying assumption is indeed plausible.

5.3.8. Restricted sample: excludes the county of state capital. This model produces consistent estimates under the hypothesis that counties cannot choose to receive or reject treatment. However, it may be the case that some counties are more likely to create pressure to obtain the desired regulation. 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 [9] presented in Table 6 runs the main specification on a sample excluding the counties of the state capitals. The results are robust to this exclusion.

5.3.9. State level. Since non-economic damages caps is a state level regulation, 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 state level. Using the state level data also has the advantage of being less noisy. However, using state level data may have significant disadvantages. State level specification not only aggregates over significantly different populations but is also more likely to suffer from endogeneity problems. Specification [10] Table 6 presents results obtained on state level data. 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, age composition, race composition, wage, no health insurance, education, state health and hospital expenditures. The obtained coefficient estimates are not as precisely estimated, however,

the results are generally similar with previous results. Taken together these estimates are consistent with the idea that the timing of adoption is mainly the result of the political process.

5.3.10. Restricted sample: counties with more than 1000 individuals. Another way to reduce the impact of noise in the data is to exclude counties with very small population where there is extremely high variance in utilization rates. To investigate the possibility that the results are driven by noise, specification [11] runs the same regression on the sample of county-year observations with populations larger than 1000. The results obtained from this specification are similar to those obtained from the entire sample.

5.3.11. Unweighted incidence data. All reported results use population/births data to calculate utilization rates and weigh data. To check whether the results are due to changes in population or births, specification [12] reports unweighted regressions where the dependent variable is measured as incidence: total admissions, total surgeries, outpatients visits, emergency visits, total births, and total first-time births in a county-year. The results are qualitatively similar.

5.3.12. Add Alaska and Hawaii. The estimates presented in row [13] are obtained on a sample augmented to include Alaska and Hawaii. These two states have some particular characteristics that distinguish them from the 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.

5.3.13. Interaction with Self-Employment. Avraham and Schanzenbach (2007) 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 price sensitive population. The interaction terms are negative for surgeries (outpatient) and admission consistent with the idea that caps have more negative effects in areas where the caps are more likely to improve insurance coverage. However, these interaction terms are not statistically significant likely because, as Avraham and Schanzenbach's (2007) mention, non-economic damages caps is a type of tort reform that has only small effects on insurance coverage and because the decrease in health insurance premium is very small (1-2% according to Avraham, Dafny, and Schanzenbach, 2009 estimates).

5.3.14. Sub-period. The results hold when excluding the most recent cluster of reforms (reforms taking place after year 2000) but the estimates shown in row [15] indicate a larger negative effect of early reforms, a possible indication of adaptations to the new standard of care reflected by the new treatment patterns. These results are also consistent with the increased pervasiveness of this regulation, which decrease the scope of searching across the borders.

5.3.15. Clustering by County. To better control for time dependence specification [16] calculates standard errors clustered at the county level. The results indicate the effect is statistically significant at conventional levels.

5.3.16. Added Controls. Specification [17] adds controls for potentially relevant time varying state characteristics: education, 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. Health and hospital expenditures could be correlated with the general interest of the state toward 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 stable 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.

Conclusion

Non-economic damages caps are meant to make medical care more affordable by reducing the malpractice insurance premiums, an important part of the cost of practicing medicine. If successful in improving access, this regulation would increase medical care utilization rates. This paper produced estimates of the effect of non-economic damages caps on medical care utilization rates.

I find that non-economic damages caps have a small but statistically significant negative effect on hospital admissions and surgeries. I also find some evidence of increased physician-searching across borders. Caps adoption does not affect total birth rates but reduces the proportion of births to first-time mothers. Interestingly, conditional on giving birth the timing of initiation of prenatal care is not affected.

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. Changes in treatment patterns could negatively affect the demand for these services. Moreover, the debate around the adoption of caps could publicly air information about incidences of malpractice and change people's perception and expectations about the quality of medical care. If the caps shift the demand for medical care, an increase in the supply of physicians cannot capture the entire effect of regulation.

However, an endogenous response of legislatures to low utilization rates is also a possible explanation. Numerous alternative specifications including restricted sample, different controls, and instrumental variables estimates are reported to check the robustness of the findings. The estimates are robust to these tests and, consistent with previous literature, I find no evidence of endogeneity.

Data Appendix

I. Legislative Data

Data on Non-Economic Damages Caps, Punitive Damages Caps, Total Damages Caps, Contingency Fees, Patient Compensation Funds comes from Avraham (2006) and was downloaded from http://www.law.northwestern.edu/faculty/fulltime/Avraham/avraham.html

II. Medical Care Utilization Rates

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

2. Prenatal Care data source is National Center for Health Statistics**.**

III. Other Data

1. County level population by race and age –U.S. Census Bureau;

2. County level average annual pay – Bureau of Labor Statistics' (*BLS*) Quarterly Census of Employment and Wages (*QCEW*);

3. Race Composition of Population – U.S. Census Bureau;

4. Education at state level– U.S. Census Bureau;

5. Percent People Not Covered by Health Insurance – U.S. Census Bureau;

6. Medical Malpractice Premium – Medical Liability Monitor 1991-2005

7. Public Health and Hospital Expenditures (per capita amounts) – U.S. Census Bureau; State Government Finances;

8. Percent self-employed – Current Population Survey, March Supplement

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Figure 1: Conditional Variation in Hospital Admissions in US 1995-2005

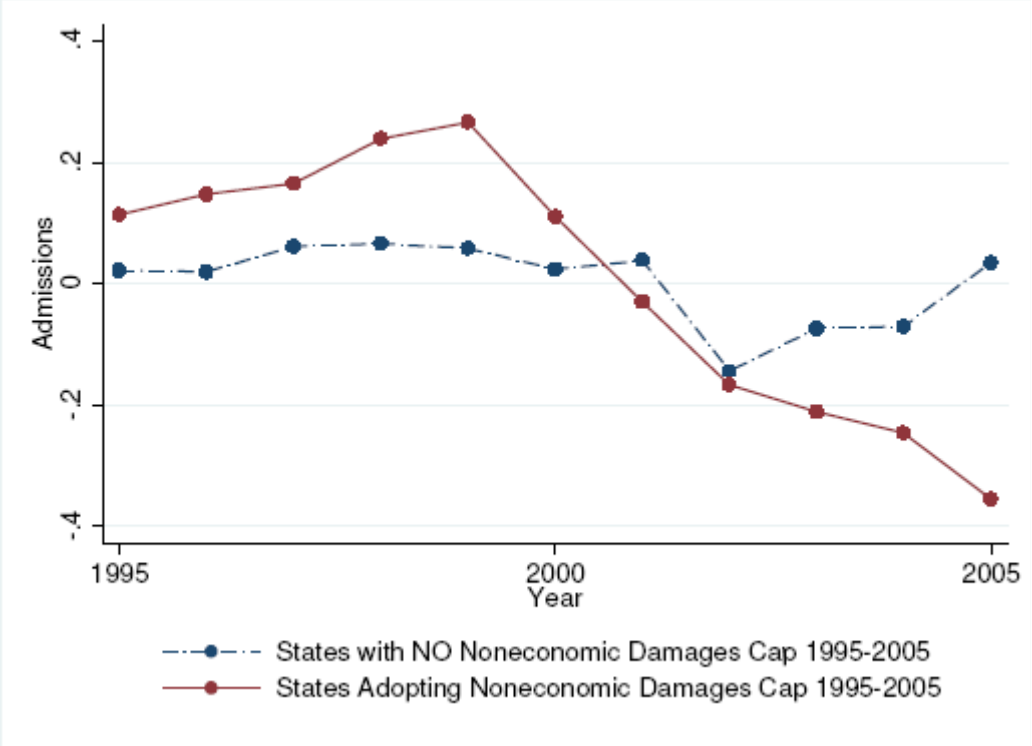

Figure 2: Conditional Variation in Surgeries in US 1990-2005

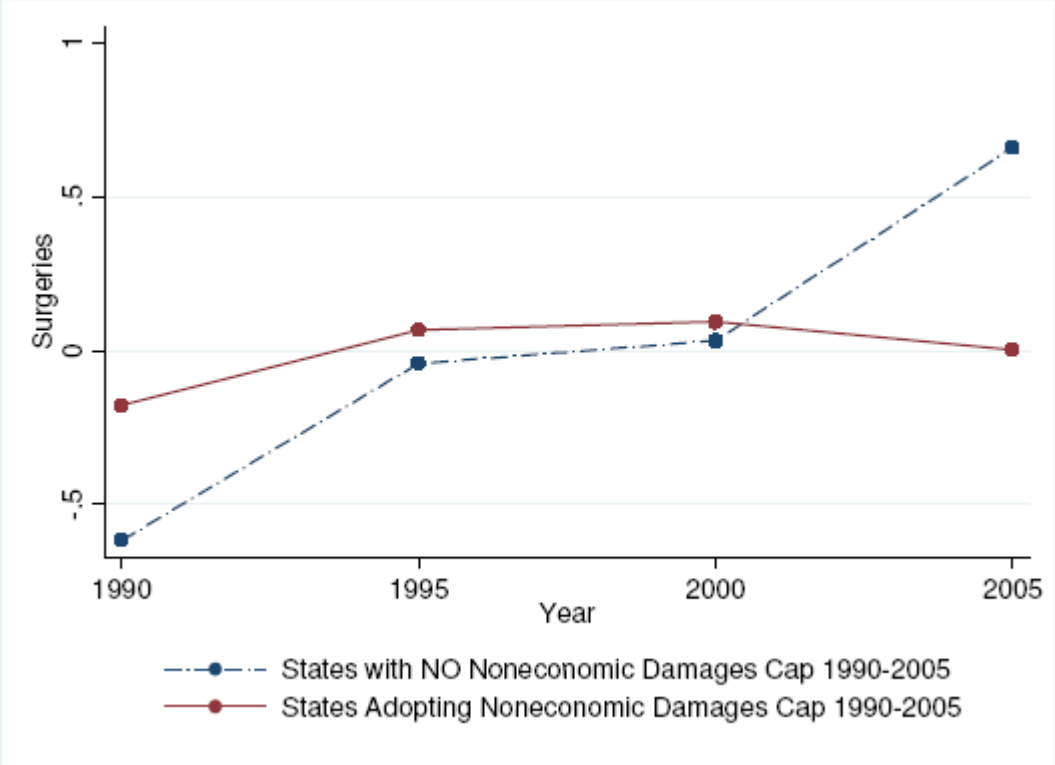

Figure 3: Conditional Variation in Prenatal Care in US 1990-2004

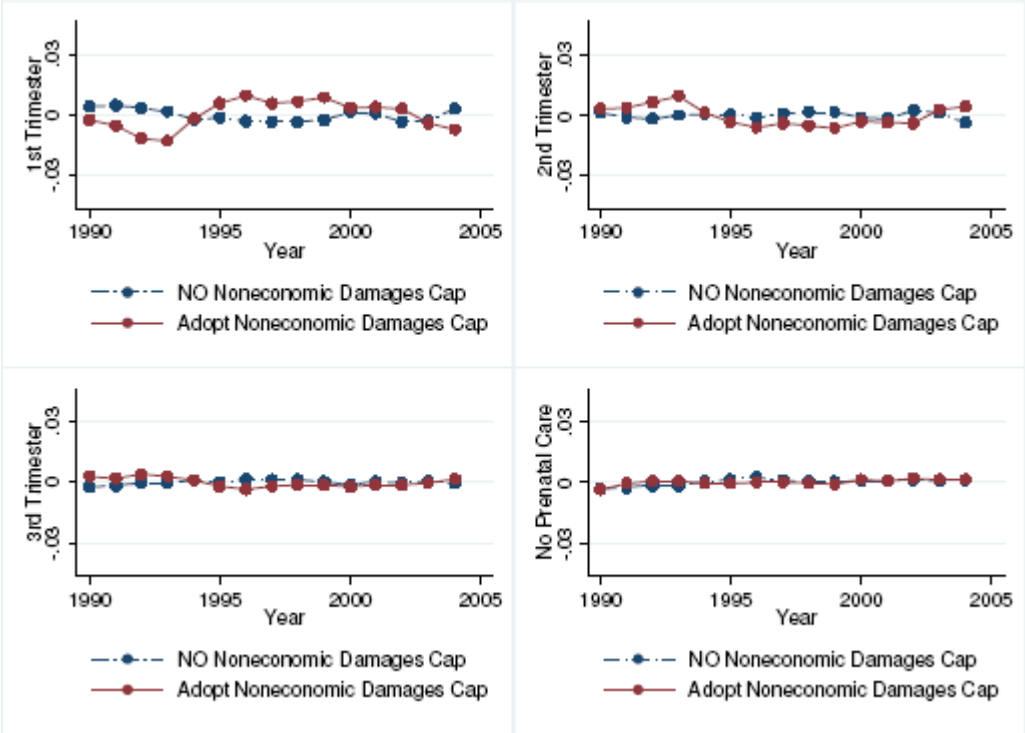

Table 1: Summary of Non-Economic Damages Caps Legislation: 1990-2005

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

Note: The specifications including lag regulation also include the 1990 repeals of caps by Minnesota and Washington. No reforms were effected implemented 1989.

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 [1]	Adopt Non-economic Damages Cap [2]	t-tests [3]
Log Admissions	1.862	1.867	0.38
	(1.077)	(1.036)	
Log Surgeries	1.502	1.397	-1.03
	(1.025)	(1.038)	
Log Birth Rate	2.561	2.532	-0.22
	(0.216)	(0.245)	
Prenatal 1st Trim	79.835	82.112	0.10
	(9.147)	(8.156)	
Prenatal 2nd Trim	15.939	14.387	-0.36
	(6.950)	(6.406)	
Prenatal 3rd Trim	3.165	2.623	-0.24
	(2.690)	(2.430)	
No Prenatal Care	1.062	0.878	-0.34
	(1.347)	(1.219)	
Population	80.899	86.503	1.38
	(221.880)	(276.103)	
Black	5.492	3.974	-1.63
	(7.593)	(6.292)	
Age 25-44	28.329	26.995	-0.23
	(3.241)	(3.098)	
Age 45-64	21.942	23.092	0.28
	(2.948)	(2.849)	
Age 65	14.771	15.199	0.84
	(4.069)	(4.158)	
Log Wage	2.632	2.638	0.56
	(0.196)	(0.196)	

Notes: Column [1] and [2] report averages f or county-years with no non- economic damages caps. Column [1] isolates the county-years in the states that did not adopt the caps in the following year. Column [2] isolates the county-years in the 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]	[3]
Log Admissions, t-1	0.001		
	(0.006)		
Log Surgeries, t-1		0.005	
		(0.004)	
Log Birth Rate, t-1			-0.013
			(0.010)
Prenatal 1st Trim, t-1			-0.001
			(0.002)
Prenatal 2nd Trim, t-1			-0.001
			(0.002)
Prenatal 3rd Trim, t-1			-0.001
			(0.002)
Population, t-1	-1.54×10^{-05}	-3.43×10^{-05}	-3.99×10^{-06}
	(2.04×10^{-05})	(6.74×10^{-05})	(2.09×10^{-05})
Black, t-1	-0.001	-0.004	-0.004
	(0.003)	(0.004)	(0.002)
Age 25-44, t-1	-0.011	-0.002	-0.005
	(0.010)	(0.011)	(0.006)
Age 45-64, t-1	-0.013	0.000	-0.009
	(0.010)	(0.008)	(0.006)
Age 65, t-1	-0.005	-0.006	-0.005
	(0.007)	(0.008)	(0.004)
Log Wage, t-1	-0.037	0.035	-0.054
	(0.051)	(0.065)	(0.043)
Observations	31369	9405	43858
F (p-value)	0.750	0.638	0.6158

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

	Log Surgeries			Log	Log Outpatient	Log		Log Birth	% First
	Total	Outpatient	Inpatient	Admissions	Visits	Emergency		Rate	Child
	[1]	[2]	[3]	[4]	[5]	[6]		[7]	[8]
Panel A: instantaneous impact									
Noneconomic Damages Cap, t	-0.026**	-0.033**	-0.011	-0.023***	0.005	-0.016	Noneconomic Damages Cap, t-1	-0.001	-0.175
	(0.011)	(0.014)	(0.011)	(0.008)	(0.014)	(0.016)		(0.003)	(0.124)
Border Cap, t	0.008	0.022	-0.010	0.008	-0.013	0.013	Border Cap, t-1	-0.005*	-0.067
	(0.015)	(0.021)	(0.007)	(0.005)	(0.018)	(0.010)		(0.003)	(0.235)
Panel B: 1-year lag									
Noneconomic Damages Cap, t	-0.003	-0.007	-0.007	-0.019***	0.006	-0.017	Noneconomic Damages Cap, t-1	-0.001	-0.078
	(0.009)	(0.013)	(0.011)	(0.007)	(0.014)	(0.014)		(0.003)	(0.115)
Noneconomic Damages Cap, t-1	-0.038***	-0.043***	-0.008	-0.013***	-0.007	0.006	Noneconomic Damages Cap, t-2	0.001	-0.218**
	(0.013)	(0.015)	(0.016)	(0.005)	(0.009)	(0.013)		(0.004)	(0.084)
Border Cap, t	-0.013	0.002	-0.022***	0.005	-0.004	0.011	Border Cap, t-1	-0.002	0.008
	(0.016)	(0.022)	(0.007)	(0.005)	(0.018)	(0.009)		(0.002)	(0.170)
Border Cap, t-1	0.030*	0.030	0.018**	0.008*	-0.024	0.006	Border Cap, t-2	-0.005	-0.143
	(0.017)	(0.024)	(0.008)	(0.004)	(0.018)	(0.009)		(0.003)	(0.192)
Observations	12423	12423	12423	34161	34161	34161		45925	45925
Period	1990-2005	1990-2005	1990-2005	1995-2005	1995-2005	1995-2005		1990-2004	1990-2004
Frequency	every 5 years	every 5 years	every 5 years	annual	annual	annual		annual	annual

Notes: All regressions control for county and time fixed effects and state specific trends. Regressions reported in columns [1] to [7] control for county age and race structure, log income, and non-economic damages caps at border, and are weighted by population in a county-year. Regressions reported in column [8] control for age, race, education and marital status of mothers in each county, log county wages, and non-economic damages caps at the border and are weighted by birth count 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 4: The Impact of Non-economic Damages Caps on Prenatal Care Conditional on Pregnancy

	1st Trimester	2nd Trimester	3rd Trimester	None	Visits
Panel A: Instantaneous Effect - county level data					
Noneconomic Damages Cap, t	-1.006	0.691	0.301	0.014	0.034
	(0.868)	(0.607)	(0.185)	(0.170)	(0.075)
Panel B: Lagged Effect - county level data					
Noneconomic Damages Cap, t-1	-0.033	0.135	-0.100	-0.001	0.088
	(0.346)	(0.197)	(0.113)	(0.072)	(0.068)
Noneconomic Damages Cap, t-2	0.169	-0.094	0.024	-0.099	0.069**
	(0.433)	(0.182)	(0.112)	(0.154)	(0.032)

Panel C: Instantaneous Effect - individual level data

	1st child		2nd or any subsequent child	
Months Prenatal Care (0-9)	White	Black	White	Black
Noneconomic Damages Cap, t	-0.016	0.004	-0.001	0.021
	(0.016)	(0.029)	(0.018)	(0.056)
Prenatal Visits				
Noneconomic Damages Cap, t	0.027	0.109	0.046	0.123
	(0.077)	(0.089)	(0.072)	(0.108)

Panel D: Lagged Effect - individual level data

	1st child		2nd or any subsequent child	
Months Prenatal Care (0-9)	White	Black	White	Black
Noneconomic Damages Cap, t-1	0.009	0.000	0.002	0.008
	(0.006)	(0.020)	(0.012)	(0.018)
Noneconomic Damages Cap, t-2	0.002	0.047	0.006	0.034
	(0.032)	(0.070)	(0.021)	(0.079)
Prenatal Visits				
Noneconomic Damages Cap, t-1	0.083	0.074	0.080	0.083*
	(0.057)	(0.072)	(0.070)	(0.046)
Noneconomic Damages Cap, t-2	0.039	0.106	0.046	0.077
	(0.032)	(0.100)	(0.033)	(0.139)

Notes: Regressions reported in panel A and B control for age, race, education and marital status of mothers in each county, log county wages, non-economic damages caps at the border, county fixed effects, time fixed effects and state specific trends, and are weighted by birth count in a county-year. Regressions reported in panel C and D control for mother's age, race, education, log income in county of residence, state fixed effects, 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 5: A Further Investigation of Potential Reverse Causality

	Log Surgeries							
	Total	Outpatient	Inpatient	Log Admissions	Log Outpatient Visits	Log Emergency	Log Birth Rate	% First Child
[1] Enactment versus Repeal								
Total	-0.040***	-0.048***	-0.014	-0.023***	0.005	-0.016	0.0002	-0.259**
	(0.011)	(0.014)	(0.015)	(0.008)	(0.014)	(0.016)	(0.004)	(0.110)
	[12423]	[12423]	[12423]	[34161]	[34161]	[34161]	[45925]	[45925]
Enact	-0.038**	-0.041**	-0.026	-0.038***	-0.013	-0.038*	0.015	-0.145
	(0.019)	(0.020)	(0.019)	(0.009)	(0.024)	(0.019)	(0.012)	(0.267)
	[8059]	[8059]	[8059]	[24395]	[24395]	[24395]	[31499]	[31499]
Repeal	-0.050*	-0.047*	-0.027	-0.018*	-0.018	0.015	-0.002	-0.693*
	(0.024)	(0.025)	(0.021)	(0.010)	(0.038)	(0.021)	(0.020)	(0.352)
	[3604]	[3604]	[3604]	[8798]	[8798]	[8798]	[11576]	[11576]
[2] 1-year lead	-0.005	-0.000	-0.010	-0.000	-0.016	0.001	-0.002	-0.126
	(0.011)	(0.012)	(0.013)	(0.007)	(0.012)	(0.013)	(0.007)	(0.080)
[3] 2-year lead	0.012	0.029*	-0.023	0.007	0.018	0.010	0.005	-0.040
	(0.010)	(0.016)	(0.016)	(0.004)	(0.012)	(0.015)	(0.005)	(0.105)
[4] 3- year lead	-0.023	-0.022	-0.003	0.005	-0.002	-0.007	0.003	-0.304
	(0.017)	(0.033)	(0.036)	(0.008)	(0.022)	(0.010)	(0.003)	(0.195)

Notes: The independent variable of interest is a dummy equal to 1 if a non-economic cap was in effect and zero otherwise. According to results presented in Table 3, I report the 1-year lag effect for surgeries; the instantaneous effect for admissions, outpatient visits, and emergency visits; and the 2-year lag effect for birth rate.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. All regressions control for county and time fixed effects and state specific trends. With the exception of the last column all regressions control for county age and race structure, log wages, and non-economic damages caps at border, and are weighted by population in a county-year. Regressions reported in the last column control for age, race, education and marital status of mothers in each county, log county income, and non-economic damages caps at the border and are weighted by birth count 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: Non-Economic Damages Cap and Medical Care: Alternative Specifications

	Log Surgeries			Log	Log Outpatient	Log	Log Birth	
	Total	Outpatient	Inpatient	Admissions	Visits	Emergency	Rate	% First Child
[1] Main	-0.040***	-0.048***	-0.014	-0.023***	0.005	-0.016	0.0002	-0.259**
	(0.011)	(0.014)	(0.015)	(0.008)	(0.014)	(0.016)	(0.004)	(0.110)
[2] Not Log	-0.417***	-0.344***	-0.074	-0.324**	0.821	-0.762	-0.023	-0.259**
	(0.131)	(0.106)	(0.075)	(0.133)	(2.903)	(0.756)	(0.057)	(0.110)
[3] Trend break	0.003*	0.004	-0.001	-0.001	0.002	-0.010	0.002	0.009
	(0.002)	(0.002)	(0.002)	(0.001)	(0.013)	(0.010)	(0.001)	(0.044)
[4] Punitive Damages Cap	-0.038***	-0.046***	-0.011	-0.024***	-0.000	-0.014	-0.0006	-0.178
	(0.011)	(0.013)	(0.015)	(0.008)	(0.016)	(0.017)	(0.004)	(0.126)
[5] Medical Malpractice Insurance Premium	-0.046***	-0.054***	-0.018	-0.024***	-0.003	-0.022	-0.0002	-0.223*
	(0.011)	(0.014)	(0.015)	(0.007)	(0.013)	(0.013)	(0.004)	(0.113)
[6] Add Control for Uninsured Rate	-0.042***	-0.050***	-0.015	-0.023***	0.005	-0.016	0.0003	-0.257**
	(0.012)	(0.014)	(0.015)	(0.008)	(0.014)	(0.016)	(0.004)	(0.108)
[7] South*Non-Economic Damages Cap	0.010	0.013	-0.022	-0.014	0.004	-0.006	-0.031***	-0.875***
	(0.019)	(0.020)	(0.016)	(0.014)	(0.037)	(0.021)	(0.005)	(0.241)
[8] No Controls	-0.039***	-0.047***	-0.011	-0.023***	0.007	-0.016	0.002	-0.158
	(0.011)	(0.014)	(0.016)	(0.008)	(0.015)	(0.016)	(0.005)	(0.140)
[9] Exclude the County of State Capital	-0.040***	-0.046***	-0.016	-0.023**	-0.006	-0.017	0.0002	-0.249*
	(0.014)	(0.016)	(0.018)	(0.009)	(0.015)	(0.014)	(0.004)	(0.125)
[10] State Level	-0.070	-0.088	-0.050	-0.023**	0.002	-0.023	-0.003	-0.136
	(0.073)	(0.098)	(0.062)	(0.010)	(0.018)	(0.014)	(0.004)	(0.122)
[11] Pop>1000	-0.040***	-0.048***	-0.014	-0.023***	0.005	-0.016	0.0002	-0.259**
	(0.011)	(0.014)	(0.015)	(0.008)	(0.014)	(0.016)	(0.004)	(0.110)

Table 7 continues

[12] Unweighted Incidence Data	-525.097***	-507.217***	-17.880	-195.953*	886.303	-204.391	-10.544	-4.237
	(136.782)	(165.453)	(88.117)	(116.469)	(2548.732)	(691.046)	(7.408)	(3.316)
[13] Add Alaska and Hawaii	-0.036***	-0.045***	-0.011	-0.023***	0.005	-0.016	0.0003	-0.260**
	(0.012)	(0.013)	(0.016)	(0.008)	(0.014)	(0.016)	(0.004)	(0.109)
[14] Cap*Self Employment	-0.001	0.003	-0.003	-0.000	0.004	-0.010	0.001	-0.043
	(0.008)	(0.011)	(0.006)	(0.003)	(0.012)	(0.007)	(0.002)	(0.100)
[15] Exclude Recent Reforms	-0.135***	-0.154***	-0.063*	-0.010***	-0.009	-0.019**	-0.0005	-0.243
	(0.029)	(0.035)	(0.035)	(0.003)	(0.018)	(0.009)	(0.004)	(0.165)
[16] Cluster by County	-0.040***	-0.048***	-0.014	-0.023***	0.005	-0.016	0.0002	-0.259***
	(0.015)	(0.018)	(0.013)	(0.007)	(0.018)	(0.012)	(0.003)	(0.100)
[17] State Controls	-0.042***	-0.051***	-0.014	-0.021***	0.008	-0.014	-0.001	-0.286**
	(0.011)	(0.013)	(0.014)	(0.007)	(0.014)	(0.015)	(0.004)	(0.115)

Notes: According to results presented in Table 3, I report the 1-year lag effect for surgeries; the instantaneous effect for admissions, outpatient visits, and emergency visits; and the 2-year lag effect for birth rate. Unless otherwise specified, all regressions control for county and time fixed effects and state specific trends. With the exception of the last column all regressions control for county age and race structure, log income, and non-economic damages caps at border, and are weighted by population in a county-year. Regressions reported in the last column control for age, race, education and marital status of mothers in each county, log county income, non-economic damages caps at the border and are weighted by birth count in a county-year.
Row [3] reports the coefficient on the trend break. Row [7] reports coefficient on the interaction term. The significant effect on birth rate is entirely driven by Alabama. Regressions reported in row [10] controls for age and race structure of population in state, education, log income, uninsured rate, state health and hospital expenditures, medical malpractice premium, non-economic damages caps at border, state and time fixed effects, and state specific trends, and are weighted by state population. Row [14] reports the coefficient of the interaction term. Row [15] drops all observations on and after 2001. Row [17] adds state level controls: education, state health and hospital expenditures.
Robust standard errors clustered at state level are reported in parentheses for all specifications. * significant at 10% significance level, ** significant at 5% significance level, *** significant at 1% significance level.