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Using data on physician behavior from the 1979–2005 National Hospital Discharge Surveys (NHDS), I estimate the relationship between malpractice pressure, as identified by the adoption of noneconomic damage caps and related tort reforms, and certain decisions faced by obstetricians during the delivery of a child. The NHDS data, supplemented with restricted geographic identifiers, provides inpatient discharge records from a broad enough span of states and covering a long enough period of time to allow for a defensive medicine analysis that draws on an extensive set of variations in relevant tort laws. Contrary to the conventional wisdom, I find no evidence to support the claim that malpractice pressure induces physicians to perform a substantially greater number of cesarean sections. Extending this analysis to certain additional measures, however, I do find some evidence consistent with positive defensive behavior among obstetricians. For instance, I estimate that the adoption of a noneconomic damage cap is associated with a reduction in the utilization of episiotomies during vaginal deliveries, without a corresponding change in observed neonatal outcomes.

I. Introduction

A number of empirical studies to date have attempted to explore the relationship between physician behavior and measures of malpractice pressure. Of course, the existence of any such relationship need not signify a problem with the malpractice system itself. It may simply indicate that malpractice liability is working in the intended manner—that is, by deterring physicians from acting in an otherwise undesirable manner. The intent of these empirical investigations, on the other hand, is generally to identify situations in which an imperfectly designed system of malpractice liability causes physicians to produce a sub- or supra-optimal level of care, often referred to as "defensive medicine."

Defensive medicine discussions often focus on the contribution that malpractice pressure makes to aggregate health-care costs. In this regard, the concern is largely over so-called positive defensive medicine, in which physicians perform additional procedures

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and order extraneous tests in order to reduce their potential malpractice exposure. However, malpractice forces need not operate in this direction on all occasions. The threat of malpractice liability may also induce physicians to avoid performing high-risk procedures or accept high-risk patients, leading to a reduction in aggregate expenditures. In the context of certain procedures, positive and negative forces may even offset each other, leaving no resulting impact on physician behavior.

In this article, I explore the role that malpractice pressure plays in shaping certain obstetric practices. Acknowledging that the threat of malpractice liability may have distinct effects on different components of obstetric care, I estimate the relationship between malpractice pressure and each of the following individual utilization measures: (1) episiotomy utilization during vaginal delivery, (2) hospital lengths of stay during delivery admissions, and (3) cesarean section utilization. Certain of these utilization measures are more likely than others to implicate positive defensive medicine concerns given the nature of the medical circumstances in which they arise (e.g., the risks involved in their implementation, the conditions they are intended to treat, etc.). For instance, while some risk is involved in the performance of an episiotomy, such risks arguably pale in comparison to the more general risks involved in the vaginal delivery itself. As such, malpractice fears may induce an obstetrician to perform an otherwise unnecessary episiotomy during a vaginal delivery under the belief that this action will alleviate potential liability from failing to properly deliver the child.

Consistent with much of the malpractice literature, I identify variations in prevailing malpractice pressure using adoptions of various tort reforms, the effect of which is largely to reduce the probability that malpractice suits are filed. While physicians are generally insured against losses in malpractice cases (and typically pay premiums that are not experience rated), they may nonetheless be quite sensitive to the threat of a potential malpractice suit in light of the significant reputational and nonpecuniary costs that are associated with malpractice liability (Currie & MacLeod 2008). The adoption of malpractice laws and other tort reforms may operate to reduce the expected levels of damages imposed in the event that a physician has been found liable. This reduction in expected liability may leave plaintiffs less inclined to bring suit, thereby lessening the level of pressure placed on otherwise insured physicians. The reforms that I emphasize in this analysis, and that have received the most attention by the malpractice literature to date, are caps on noneconomic damage awards—that is, caps on pain and suffering awards.

A large body of related literature has explored the relationship between tort reforms and various outcomes of the malpractice marketplace: claims frequency, claims severity, insurance premiums, and physician location.[1] These studies suggest that noneconomic damage caps are perhaps the most relevant and most influential tort reform measures (Mello 2006). Twenty-eight states currently have noneconomic damage cap provisions in place, most of which were adopted during the malpractice crisis of the mid-1980s. Accordingly, those studies relying on post-1980s data to evaluate the association between noneconomic damage caps and physician behavior (e.g., Currie & MacLeod 2008) fail to draw on

[1]See Mello (2006) for an extensive review of this literature.

the most relevant sources of variation in malpractice law. In this study, I explore questions regarding defensive practices using data on physician behavior from the 1979–2005 National Hospital Discharge Surveys (NHDS). The NHDS data, supplemented with geographic identifier codes, provides inpatient discharge records from a broad enough span of states and covering a long enough period of time to allow for a defensive medicine analysis that draws on an extensive set of legislative variations.

Consistent with a reasonable interpretation of the relevant medical circumstances, I find evidence that a reduction in malpractice pressure, as identified by adoptions of various tort reforms, is associated with decreased utilization of episiotomies during vaginal deliveries and with the number of days spent in the hospital in connection with the delivery of a child (suggesting that greater malpractice pressure is associated with increased utilization of the indicated measures). At the same time, however, such pressure is not associated with any improvements in neonatal outcomes, as proxied by infant Apgar scores, suggesting (at least in the episiotomy case) that the malpractice-induced changes in utilization may likely be defensive in nature. On the other hand, confirming the findings of Currie and MacLeod (2008), I find no evidence to support the conventional wisdom that malpractice pressure induces obstetricians to perform unnecessary cesarean sections.

This article proceeds as follows. Section II provides a review of the related literature concerning defensive physician practices. Section III offers a simple framework by which to evaluate a physician's response to malpractice pressure. Section IV describes the data and empirical methodology, while Section V presents results from the relevant regression analyses. Finally, Section VI concludes.

II. Literature Review

Malpractice scholars and analysts are often inconsistent in their definition of "defensive medicine," but the type of behavior that I hope to identify as being "defensive" in nature is that in which malpractice liability causes physicians to take sub- or supra-optimal levels of care, where optimality is determined according to an appropriate weighing of the costs and benefits of care. This behavior is, of course, inherently difficult to identify. In the alternative, I follow Kessler and McClellan (1996) and related studies and classify observed behavior as being of a positive defensive nature when malpractice pressure induces physicians to provide extra levels of care without leading to corresponding health benefits. Negative defensive behavior, on the other hand, is identified by situations in which malpractice pressure induces physicians to avoid otherwise beneficial care.

Most of the literature exploring the effects of malpractice/tort reforms has focused on first-stage litigation- and insurance-related outcomes (e.g., claims frequency and malpractice premiums); a smaller, yet significant, literature has explored the second-stage effects of tort reform on physician behavior. In perhaps the seminal study on the defensive nature of physician practices, Kessler and McClellan (1996) found that malpractice reforms that directly reduce malpractice pressure ("direct" reforms) are associated with a 5–9 percent decrease in total hospital expenditures incurred for patients in the one-year period following an acute myocardial infarction or new ischemic heart disease, without substantial

reductions in mortality rates or complications. Direct reforms include caps on damages awards (noneconomic, punitive, and total) and reforms of the collateral source rule.

Currie and MacLeod (2008) consider an empirical approach similar to that taken by Kessler and McClellan (1996) but focus on the case of cesarean deliveries, a treatment that is often implicated in popular discussions of defensive medicine and that is a common target of scholarly defensive medicine investigations. Using data from the Vital Statistics Natality files from 1989–2001, Currie and MacLeod test the conventional wisdom that malpractice fears over improperly performed vaginal deliveries induce obstetricians to perform excessive numbers of cesarean sections. Contrary to these expectations, they actually find that the adoption of a noneconomic damage cap (representing a reduction in malpractice pressure) leads to an increase in cesarean utilization.

Currie and MacLeod (2008) contend that this finding is consistent with a model of physician behavior that provides for variations in patient conditions. Significant risks do occur during the commission of a vaginal delivery, which would otherwise induce physicians to opt for cesarean delivery. However, if the marginal patient receiving a cesarean delivery has a favorable case mix and is relatively inappropriate for that delivery, then the risks to performing a cesarean section on this marginal patient may outweigh the general risks of delivering her baby vaginally. Two recent studies (Frakes forthcoming; Baicker et al. 2006) have documented evidence of triage in regional cesarean utilization, where physicians begin to perform cesareans on mothers with fewer and fewer indications for cesarean delivery as cesarean rates increase within regions. In light of this evidence and considering the possibility that financial incentives may operate to push regional cesarean rates to elevated levels, it may be reasonable to assume that the marginal mother is inappropriate, in an absolute sense, for cesarean delivery. Accordingly, it may be reasonable to assume that a physician treating this marginal mother is sensitive to the risk of improperly performing a cesarean delivery and that malpractice pressure thus pushes the cesarean rate downward (not upward) on the margin.

Consistent with the findings of Currie and MacLeod (2008), it follows then that the reduction in malpractice risks associated with the adoption of a noneconomic damage cap may alleviate these downward pressures and possibly lead to an increase in the equilibrium cesarean rate. This increase may occur in a situation where prereform liability pressures (based on fears over improperly performing cesarean sections) had kept physicians from practicing at an otherwise desired higher cesarean rate—for example, in a situation where prereform liability fears discouraged physicians from "inducing" demand among their patients in response to prevailing financial incentives.[2] Avraham and Schanzenbach (2009) describe this possibility as one where tort reform may increase the ability of physicians to practice "offensive medicine."

The findings of Currie and MacLeod stand in contrast to certain other studies that have explored the relationship between malpractice pressure and cesarean utilization, though from different methodological frameworks. For instance, using 1990–1992 Natality

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[2]Of course, it is possible that the resulting cesarean rate will increase following reform where physicians otherwise face fewer liability constraints to increase cesarean utilization for other, nonfinancial reasons.

data, Dubay et al. (1999) estimate a county fixed-effects specification and find a positive association between cesarean utilization and malpractice insurance premiums. Finding no corresponding evidence of a positive association between premiums and health outcomes (as indicated by the incidence of a low Apgar score), they conclude that the observed behavior is defensive in nature. Similarly, using discharge data from acute care hospitals in New York State in 1984, Localio et al. (1993) find a positive association between cesarean utilization rates and malpractice premiums and claims frequency (controlling for patient severity and other factors). Other studies, however, confirm the results of Currie and MacLeod to the extent that they document no evidence of positive defensive medicine in cesarean utilization. Baldwin et al. (1995), for instance, find no association between cesarean utilization and physicians' claims exposure, as measured by both personal physician claims experience and the prevailing practice environment (i.e., county claims per physician).

Using variations in certain characteristics of the malpractice marketplace (e.g., claims frequency) to identify defensive behavior, these additional studies implicate general concerns over unobserved heterogeneity that may be correlated with both the outcome of interest and the relevant malpractice characteristics. The approach taken by Currie and MacLeod (2008), however, draws on within-state variation of an arguably exogenous nature: the adoption of tort laws that are, for the most part, applicable to torts generally and that were, in many instances, adopted in response to broader crises in commercial casualty insurance—that is, not solely in response to more specific crises in medical malpractice lines (Matsa 2007).

In the analysis below, I also identify defensive behavior using the adoption of various tort reforms. However, I build on Currie and MacLeod (2008) by exploring physician behavior over a longer time horizon, including the entirety of the 1980s, a decade during which the heart of the noneconomic damage cap adoptions occurred. By focusing on 1989–2001 data, Currie and MacLeod only consider the adoption of noneconomic damage caps by four states, two of which invalidated the relevant statute within several years. They also consider variation in noneconomic damage caps for four other states whose courts invalidated caps previously adopted in the pre-1989 period. However, there may be good reason to exclude from the specification those states that invalidate damage caps over the sample period. Drawing on variations in the invalidate direction may lead to less precise estimates given the possibility that physicians may respond weakly to a law that has a high probability of being stricken down (Matsa 2007).[3] As discussed further below, the possibility of anticipated changes in physician behavior prior to the invalidated damage caps may also limit the ability to perform important falsification exercises that test for differential trends in utilization rates between treatment and control states in the prereform period. By limiting the specifications to exclude states that invalidated reforms, however, there are only two states from which to identify defensive behavior during the

[3]For a related discussion of the incentives posed by uncertainty over the constitutional validity of damage cap reforms, see Avraham and Bustos (2010). Avraham and Bustos's analysis, however, focuses on incentives within a dispute resolution context, as opposed to a first-order clinical decision-making context.

1989–2001 period, implicating concerns over the consistency of the estimated results (Conley & Taber 2011).

Drawing on data over this longer timeframe, the analysis below includes nearly 20 states with pure noneconomic damage cap adoptions (i.e., where such states did not also invalidate or repeal the relevant statutes). With a greater number of treatment states, it is more likely that spurious state-year shocks that are uncorrelated with damage cap laws will average each other out, leaving consistent estimates of the effect of such reforms (Conley & Taber 2011). Building on the above studies, I use this rich set of legislative variation to test for evidence of defensive behavior in cesarean utilization. However, I also explore for defensive behavior in two related obstetric practices/measures: episiotomy utilization and the number of days that mothers spend in the hospital in connection with the delivery of their children.

III. Malpractice Pressure and Obstetric Practices

Defensive medicine discussions often focus on the role that malpractice pressure plays in driving up total expenditures; however, malpractice fears are likely to impact physician behavior in a highly context-specific manner. In many situations, this fear may indeed be expected to induce physicians to perform additional procedures and order extra tests. In other situations, however, this fear may result in the avoidance of particular behaviors. To understand how liability can lead to such opposite results, it helps to begin with a consideration of the context in which a medical procedure is performed.

In a given medical context, I treat malpractice liability fears as generally arising from two fundamental directions: (1) fear over improperly treating (or diagnosing) an underlying disease or medical condition and/or (2) fear over improperly performing a treatment meant to resolve or alleviate that underlying disease or condition. These risks inherently find themselves in tension with each other, in that a physician risks exposing himself or herself to liability from this first direction by capitulating to the fears arising from the second direction. That is, if a physician avoids performing a particular high-risk treatment over fear of committing an error in the process, he or she is exposed to potential liability from failing to properly treat the condition/disease itself. Thus, the relationship between the underlying level or extent of malpractice pressure and the utilization rate for the treatment in question will depend on which of these two risks dominates under the circumstances.

This simple framework follows from Currie and MacLeod's (2008) model of physician behavior in the face of potential malpractice liability. A fundamental implication of their model is that (1) a physician's choice between performing a procedure and not performing a procedure depends on the relative malpractice (and other) risks of each such choice and (2) accordingly, a legal-induced increase in expected malpractice liability *decreases* procedure utilization if and only if the prevailing risks associated with the procedure itself exceed those that prevail in the absence of its utilization. A key feature of their model is that these calculations vary depending on the condition or health status of the patients. Consider a situation in which the marginal patient receiving the procedure

(i.e., the patient just indifferent between receiving and not receiving the treatment) is in relatively little need of the procedure. The no-procedure option for this patient may pose little risk relative to the risk of improperly performing the treatment itself, leading to the possibility of negative defensive behavior.

Ultimately, the effect of a reduction in potential malpractice liability (e.g., that resulting from tort reform) on the utilization of a given procedure is an empirical question that depends on the risks ensuing from the execution of the procedure versus the risks relating to the failure to treat the underlying condition, evaluated with respect to the marginal patient. With these considerations in mind, I consider the effect of various tort reforms on the utilization rate of several procedures (or medical decisions) that are likely to present different risk-risk tradeoffs.

A. Episiotomy Utilization

An episiotomy is a surgical incision made in the tissue between the vagina and the perineum during a vaginal delivery. Extremely common procedures, episiotomies were performed in nearly 40 percent of the vaginal delivery sample considered below, though their rates have declined considerably over time. Although some of the traditional justifications for episiotomy utilization are no longer widely held (e.g., the belief that controlled incisions heal better than natural tears), episiotomies are still indicated in the event of certain complications of birth, such as abnormal presentation or fetal distress, or in instances in which an expedited delivery is necessary.[4]

The circumstances surrounding episiotomy utilization are supportive of a possible *positive* defensive response to malpractice pressure. Though there are some morbidity risks to the performance of an episiotomy, including an increased risk of fecal incontinence, the expected harm from such risks arguably pales in comparison to the potential mortality and morbidity risks that are generally faced during the vaginal delivery of a child. Moreover, even if the benefits of episiotomy are minimal in most instances, as long as there is some legitimate belief that episiotomies are indicated in certain high-risk situations, this imbalance of risks may lead some risk-averse physicians to utilize episiotomies on a relatively common basis. If anything, physicians may be inclined to perform arguably unnecessary episiotomies for purely "optical" reasons—that is, to enhance the appearance that they took every step possible to prevent harm to the mother and child. Moreover, for much of the sample period, many of the risks associated with the performance of an episiotomy were not well documented[5] and it was not until 2006 that the American College of Obstetricians-Gynecologists issued guidelines recommending restricted, as opposed to routine, use of episiotomies.

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[4]For a recent discussion of the risks and benefits of episiotomy utilization, see Hartmann et al. (2005) and American College of Obstetricians-Gynecologists (2006).

[5]Beginning in the mid-1980s and continuing to the present, a number of clinical trials have analyzed the risks of routine versus restricted use of episiotomies (Hartmann et al. 2005).

B. Delivery Bed Days

During the course of an in-patient delivery admission, physicians are also faced with the decision of how long the mother and child should stay in the hospital (aside from the separate but related decision of whether to perform a cesarean section). An additional day in the hospital itself poses little inherent risk to the mother and child (aside from the greater risk of a hospital-acquired infection). However, the additional monitoring provided by an extra in-patient day may alleviate general risks faced during the neonatal period. The tradeoff in these risks suggests that physicians may respond in a positive defensive manner to the threat of malpractice liability. Moreover, even if an additional bed day does not significantly reduce neonatal risks, physicians may be inclined to keep certain high-risk mothers longer purely to bolster appearances in potential malpractice suits.

Cesarean-adjusted length-of-stay measures may reflect express physician decisions regarding neonatal monitoring durations, but they may also, in part, proxy for various noncesarean procedures or treatments performed in the neonatal period. To the extent that any such treatments also pose few risks in their executions, relative to the general risks involved in delivering a child, physicians may further respond in a positive defensive manner to the associated malpractice forces.

C. Cesarean Utilization

Considering the evidence of triage in cesarean utilization within regions found in Frakes (forthcoming) and Baicker et al. (2006), and in light of the high cesarean rates prevailing in regions, it may be reasonable to expect that the marginal cesarean delivery is not, in an absolute sense, truly in need of cesarean delivery. Thus, with respect to the marginal cesarean patient, the risks posed by cesarean delivery may actually be high enough relative to those posed by a standard vaginal delivery that defensive cesarean behavior, though popularly expected, fails to hold in practice (Currie & MacLeod 2008).

D. Tort Reform Type

In the analysis presented below, I focus on estimating the impact of reforms that place caps on the amount of noneconomic damages (i.e., damages for pain and suffering) that plaintiffs can be awarded. Noneconomic damages represent a significant portion of the typical malpractice damages award. Using a data set of 326 closed claims in Texas for the 1988–2004 period (each with at least a $25,000 payout), Hyman et al. (2009) document an average noneconomic damages award of $681,000 (in 1988 dollars, compared with $542,000 for economic damages), occurring in 272 (or 83 percent) of the closed claims included in the sample. Noneconomic damage caps represent the tort reform measure that has been most commonly associated with an observed change in certain malpractice outcomes: claims severity, physician supply, and malpractice premiums.[6] Twenty-eight states

[6]See Mello (2006) for a comprehensive review of relevant studies.

Table 1: Variations in Noneconomic Damage Caps (1979–2005)

State	*Year Adopted*	*Year Dropped*	*State*	*Year Adopted*	*Year Dropped*
Alaska	1986		Mississippi	2003	
Alabama	1987	1992	Montana	1996	
Colorado	1987		North Dakota	1996	
Florida	2004		New Hampshire	1987 (2)	1981 (1); 1991 (2)
Hawaii	1987		Ohio	2003 (2)	1992 (1)
Idaho	1988		Oklahoma	2004	
Illinois	1995	1998	Oregon	1988	2000
Kansas	1987		Texas	2004 (2)	1988 (1)
Massachusetts	1987		Utah	1988	
Maryland	1987		Washington	1986	1990
Michigan	1987		Wisconsin	1986	
Minnesota	1986	1990	West Virginia	1986	
Missouri	1986				

NOTES: Years of adoption and invalidation/repeal (if applicable) of laws imposing caps on noneconomic damage awards in malpractice cases (or tort cases generally) are indicated above. States are included only if their relevant malpractice laws varied over the 1979–2005 period. Legislative variation is excluded from this table if it represents a situation in which an adoption and invalidation/repeal occurred during the same year.

SOURCE: Database of State Tort Law Reforms (2nd).

currently have in place laws that cap noneconomic damage awards.[7] Seventeen states adopted such laws during the mid-1980s (five of which subsequently invalidated or repealed the relevant provisions). Table 1 lists those states that modified their noneconomic damage cap laws over the sample period considered in the empirical analysis below.

In most of the specifications estimated below, I also explore the association between physician behavior and certain additional types of malpractice/tort reforms, including caps on punitive damages awards, reforms of the collateral source rule, and other "indirect" tort reforms. Punitive damages are awarded on a much rarer basis in malpractice actions than are noneconomic damages awards (without a correspondingly large increase in average awards/payouts).[8] Thus, relative to noneconomic damages, it is less likely that the threat of liability for punitive damages will have a considerable impact on physician behavior.

Similar to caps on noneconomic and punitive damages, amendments to the traditional collateral source rule represent malpractice reforms that operate to directly reduce

[7]The vast majority of these states have laws that are specific to noneconomic damage awards. However, for the purposes of this analysis, I also classify states as having noneconomic damages provisions if they have laws that place caps on total damages awards, where such laws necessarily cap noneconomic damages as well. Considering the imposition of state fixed effects, this decision has relevance only in the context of one state (Texas) that experienced variation in the presence of a total damages cap at a time when it did not have a specific noneconomic damage cap in place. Only one additional state (Colorado) experienced variation in the incidence of a total damages cap over the sample period (two years following the adoption of a noneconomic damages cap). With such little within-state variation in mind and considering the fundamental overlap between a total damages caps and a noneconomic damages cap, I do not separately control for the incidence of a cap on total damages. However, the estimation results for the remaining coefficients remain essentially unchanged when I do include this additional covariate.

[8]For evidence of this claim, see Cohen (2005) and Hyman et al. (2009).

Table 2: Variations in Collateral Source Rule Reforms (1979–2005)

State	*Year Adopted*	*Year Dropped*	*State*	*Year Adopted*	*Year Dropped*
Alabama	1987 (1); 2001 (2)	1997 (1)	Montana	1988	
Colorado	1987		North Dakota	1988	
Connecticut	1986		New Hampshire		1981
Georgia	1988	1991	New Jersey	1988	
Hawaii	1987		New York	1985	
Idaho	1990		Ohio	2002 (2)	1998 (1)
Indiana	1987		Oklahoma	2004	
Kansas		1993	Oregon	1988	
Kentucky	1989	1995	Pennsylvania	2002 (2)	1981 (1)
Massachusetts	1987		Rhode Island		2002
Maine	1990		Utah	1987	
Michigan	1987		Wisconsin	1995	
Minnesota	1986		West Virginia	2003	

NOTES: Years of adoption and invalidation/repeal (if applicable) of laws reforming traditional collateral source rules are indicated above. States are only included if their relevant malpractice laws varied over the 1979–2005 period. Legislative variation is excluded from this table if it represents a situation in which an adoption and invalidation/repeal occurred during the same year.

SOURCE: Database of State Tort Law Reforms (2nd).

the expected level of damages awarded in malpractice actions (Kessler & McClellan 1996). Traditional collateral source rules generally prohibited defendants from introducing evidence of compensatory payments made to plaintiffs from outside sources (e.g., payments from insurance companies). Thirty-three states currently have laws in place that eliminate this traditional rule, effectively reducing the compensatory damage awards that plaintiffs can obtain by amounts received from such outside sources. Many of these amendments also occurred during the mid-1980s, often contemporaneously with the adoption of noneconomic damage cap laws. Nonetheless, there are a substantial amount of independent reforms of each type to allow for identification of the effect of these separate provisions. Table 2 lists those states that adopted collateral source rule reforms over the sample period considered below.

Finally, following the classification of malpractice reforms introduced by Kessler and McClellan (1996), I estimate the general impact associated with a set of reforms that more indirectly (relative to damage caps and collateral source rule reforms) reduce the expected level of damages imposed in malpractice actions. Included in this set of "indirect" reforms are contingency fee limitations, requirements of periodic payment of future damages, joint and several liability reforms, and provisions for a patients' compensation fund.[9] In their seminal study on defensive medicine, Kessler and McClellan (1996) document a small positive effect of "indirect" reforms on malpractice intensity, as proxied by the total

[9]While the emphasis of this article is on damage cap reforms and collateral source rule reforms, I include indirect reforms to control for residual liability initiatives by states. The results presented below for the damage caps and collateral source rule reform coefficients are entirely robust to inclusion of a richer set of controls for each of the individual components of Kessler and McClellan's indirect reform category.

expenditures associated with the care provided to a patient in the one-year period following the new incidence of a serious heart condition.

Each of these malpractice reforms operates in some fashion to reduce expected liability levels; however, each reform may have a unique impact on physician behavior depending on the precise medical circumstances involved. For instance, where the harm caused by a particular type of medical error is more likely to be associated with economic, as opposed to noneconomic, damages and where the relevant harm can likely be treated by subsequent remedial treatments (which may be reimbursed from third-party insurers), a reform of the collateral source rule may be the provision more associated with a change in physician behavior. On the other hand, where the potential damage caused by a given course of action is of a more serious and irreversible nature, the effects of noneconomic damage caps may be more significant in magnitude.

IV. Data and Empirical Methodology

Data on the history of each state's tort laws comes from the Database of State Tort Law Reforms (2nd Draft), compiled by Ronen Avraham.[10] Likewise, data on physician behavior are from the National Hospital Discharge Survey (NHDS), a nationally representative survey of in-patient records from short-stay, nonfederal hospitals conducted annually by the National Center for Health Statistics (NCHS). For roughly 260,000 in-patient records per year, the NHDS contains information on the primary/secondary diagnosis and procedure codes associated with the discharge and on various characteristics of the relevant patient and hospital. I supplement the public NHDS files with geographic identifiers received pursuant to an agreement with the Research Data Center at the NCHS (all empirical work was conducted onsite at the NCHS headquarters). The sample employed in this utilization analysis covers the years 1979 to 2005.

Using the diagnosis codes and other information provided in the NHDS records, I determine whether an episiotomy or cesarean was performed in connection with each individual discharge, in addition to determining the length of the stay associated with each discharge. I then evaluate these utilization measures over the proper subsample of individual discharges, as follows.

1. *Cesarean Utilization and Number of Bed Days.* To evaluate cesarean behavior and delivery lengths of stay, I consider the subsample of all deliveries contained in the NHDS files, determined using the appropriate diagnosis codes. This subsample is well protected against sample selection concerns considering that virtually all deliveries occur in in-patient settings—that is, the malpractice variables of interest should not impact the denominator used in the utilization rate analysis.
2. *Episiotomy Utilization.* To evaluate episiotomy utilization, I consider the subsample of vaginal deliveries, which differs from the above subsample, by excluding those deliveries performed via cesarean section. This subsample, however, is more

[10]The regression results presented below remain virtually unchanged when using the fourth edition of the Database of State Tort Law Reforms.

prone to sample selection issues given that malpractice forces may impact the number of cesareans performed in the relevant states and potentially impact the composition of the resulting vaginal sample. I partially alleviate these concerns by including controls for the case mix of the individual vaginal delivery, which I parameterize using the delivery's predicted probability of cesarean delivery (i.e., a measure of the appropriateness for cesarean delivery), calculated as set forth below.[11]

Descriptive statistics for the key utilization, legal, and outcome variables discussed in the analysis below are provided in Table 3. Episiotomies are performed on roughly 40 percent of the vaginal delivery sample, while cesareans are performed on nearly 24 percent of the total delivery sample. The average mother spends 2.8 days at the hospital during each delivery stay.

With respect to each of the above utilization measures, I first test for evidence of defensive behavior by estimating the following basic difference-in-difference specification:

$$U_{i,s,t} = \alpha + \gamma_s + \lambda_t + \beta_1 CAP_{s,t} + \varepsilon_{i,s,t}, \tag{1}$$

where s indexes state, t indexes year, and i indexes an individual discharge from the appropriate subsample; $CAP_{s,t}$ represents an indicator variable for the presence of a cap on noneconomic damages in state s and year t; and state fixed effects, γ_s, and year fixed effects, λ_t, control for fixed differences across states and across years, respectively. In the episiotomy and cesarean specifications, $U_{i,s,t}$ is an indicator variable for the incidence of the relevant utilization measure. In the delivery length-of-stay specification, $U_{i,s,t}$ equals the log of the number of days that the mother spends in the hospital during the delivery admission. The coefficient of interest in each specification is β_1, representing the relationship between the relevant utilization measure and the adoption of noneconomic damage caps.

I then test the robustness of the above findings to the inclusion of a range of individual and state-year factors by estimating the following specification:

$$U_{i,s,t} = \alpha + \gamma_s + \lambda_t + \varphi_{s,t} + \beta_1 CAP_{s,t} + \beta_2 X_{i,s,t} + \beta_3 Z_{s,t} + \beta_4 O_{s,t} + \varepsilon_{i,s,t}, \tag{2}$$

where $X_{i,s,t}$ represents certain characteristics of the individual discharge: mother's age (15–19, 20–24, 25–29, 30–34, 35–39, and 40+ years old); mother's race (white, black, and other); mother's insurance status (private, government, no insurance, and other); hospital bed size (0–100, 100–200, 200–300, 300–500, and 500+ beds); and hospital ownership type (proprietary, nonprofit, and government).[12] $Z_{s,t}$ represents certain state-year characteristics

[11]Sample selection concerns are further mitigated in the episiotomy specifications by the fact that I document virtually no relationship between the considered tort reforms and the rate at which physicians deliver via cesarean section (as opposed to a vaginal delivery).

[12]For the cesarean and episiotomy specifications (which use a binary dependent variable), I follow Currie and MacLeod (2008) in estimating a linear probability model (LPM). While LPMs have the benefit of offering more

Table 3: Means and Standard Deviations of Selected Variables

	Mean (SD)
Panel A: Utilization Rates (NHDS)	
Episiotomy (from sample of vaginal deliveries)	41.1
	(49.2)
Delivery bed days	2.8
	(2.7)
Cesarean section	23.4
	(42.3)
Panel B: Outcome Measures (Natality Data)	
Apgar scores (5-minute)	8.97
	(0.86)
"Good" Apgar score (>= 7)	98.4
	(12.7)
Panel C: Tort Variables (in NHDS Sample)	
Noneconomic damage caps	39.9
	(49.0)
Noneconomic damage caps: adoption-only states	44.2
	(49.7)
Collateral source rule reform	58.7
	(49.2)
Punitive damage cap	40.5
	(49.1)
"Indirect" tort reform	75.7
	(42.9)
N (full delivery sample)	737,193
N (vaginal delivery sample)	564,683
N (Natality data)	10,431,241

NOTES: All statistics are multiplied by 100, with the exception of those presented for five-minute Apgar scores. Reported statistics for Panels A and C are from an individual sample of deliveries from the 1979–2005 National Hospital Discharge Survey records. Episiotomy utilization rates are presented from the subsample of vaginal deliveries. Statistics for the tort variables are presented out of the full delivery sample in the NHDS records. The statistics for the tort variables are nearly identical (not shown) using the subsample of vaginal deliveries. Statistics reported in Panel B are from a 10 percent random sample of the 1978–2004 Natality Detail files.

(HMO penetration rate and its square, OB/GYN concentration rate, fertility rate, and median household income). $O_{s,t}$ is a matrix representing a set of indicator variables for the incidence of the following tort/malpractice provisions: (1) collateral source rule reforms, (2) caps on punitive damages, and (3) "indirect" tort provisions. I include state-specific

readily interpretable coefficients, they raise various estimation concerns, including the nonnormality of the error terms and the fact that the predicted values of the dependent variable are not bounded to the unit interval. The estimates presented below, however, are robust to the alternative use of probit and logit models. With respect to the length-of-stay specifications, I log-transform the bed days variable, consistent with much of the relevant length-of-stay literature (see, e.g., Austin et al. 2002). It is believed that this logarithmic transformation will better achieve normalization of the length-of-stay error distribution. This log-transformed ordinary least squares approach, however, may generate biased estimates in the face of heteroskedastic error terms on the log scale (Manning & Mullahy 2001).

linear time trends, $\varphi_{s,t}$, to control for slowly moving correlations between the relevant utilization rates in a state and the adoption of tort reforms by that state.[13]

I also include controls in $X_{i,s,t}$ to account for the case mix of the relevant delivery. I parameterize the delivery case mix using the predicted probability of cesarean section (PPC) for the relevant delivery, calculated according to Frakes (forthcoming).[14] Each delivery's PPC value is simply a single parameterization of a much richer set of risk factors and delivery complications. The results are generally robust to the inclusion of a set of individual indicator variables for each risk factor and to the exclusion of these risk-factor controls entirely. In the delivery bed-days specification, I also include controls for the incidence of cesarean delivery and episiotomy utilization (each of which generally lengthens a delivery stay) in order to target the investigation on the length-of-stay decision itself or at least to create a better proxy for any other procedures (i.e., other than cesarean sections and episiotomies) performed during the mother's stay.

A key identification concern in difference-in-difference models of the above nature is posed by the possibility of underlying trends—for example, differential utilization trends between treatment and control states that predate the adoption of the tort reforms in question. The primary results presented above partially account for this concern by including state-specific linear time tends. As a falsification test, I also check for the presence of underlying trends by modifying the above specifications to include "leads" of the relevant tort variables—that is, indicator variables that switch from 0 to 1 in the year(s) prior to the actual adoption of the reforms. Under the assumption that there are no such differential trends that predate the relevant reforms, the coefficients of the lead indicator variables should not differ significantly from zero.

Most of the variation in damage cap laws throughout the sample derives from the initial adoption of the relevant provision, but a number of states also invalidated or repealed previously adopted damage caps (eight states) throughout the sample. Following Matsa (2007), I also estimate specifications that drop these adopt-then-invalidate states. This more restricted approach accounts for the possibility that physicians will be less responsive to the relevant sources of malpractice pressure in those states that face a high probability of dropping damage caps (which I identify by the actual act of invalidation/

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[13]Frakes (forthcoming) documents a relationship between the adoption of laws requiring physicians to follow national (as opposed to local) standards and a resulting convergence in physician practices across regions. In light of the fact that two of the damage cap treatment states used in the defensive medicine analysis below (Hawaii and Texas) were dropped from the specifications estimated in Frakes (forthcoming) (due to an inability to classify the full history of their standard of care laws), I exclude controls for national standard laws in the specifications estimated below and focus instead on damage caps and related provisions. However, the results presented below are robust to the inclusion of controls for national standard laws (not shown).

[14]In specifying a mother's appropriateness/need for cesarean delivery, I calculate the relevant mother's predicted probability of receiving a cesarean section using fitted values of a logit model (estimated annually) of the incidence of cesarean delivery on a set of individual risk factors and complications. Such factors include maternal age, breech presentation, multiple deliveries (e.g., twins), previous cesarean delivery, placenta previa, placenta abruption, dysfunctional labor, cephalopelvic disproportion, fetal distress, precipitous labor, postpartum hemorrhage, prolonged labor, premature rupture of the membranes, cord prolapse, maternal hypertension, maternal diabetes, and maternal anemia.

repeal).[15] Moreover, even in situations where these invalidated sources of variation are more strongly associated with physician behavior, there may still be a concern that physicians anticipate the upcoming change in law and alter their behavior ahead of time (understanding its likely retroactive effect). Any such anticipation effects will confound the ability to perform the falsification exercises discussed above and accordingly test for differential trends in utilization rates between treatment and control states that may otherwise be reflective of omitted factors.

Finally, to complete the defensive medicine analysis, I estimate the above specifications using five-minute Apgar scores as the relevant dependent variable. Data on individual Apgar scores are from the 1978–2004 Natality files, compiled as part of the National Vital Statistics System of the National Center for Health Statistics. The Natality files provide demographic and health data for a 100 percent sample of all births occurring in the post-1985 period and either a 50 percent or a 100 percent sample, depending on the state, of all births occurring in the pre-1985 period. Included in the delivery-related data available throughout the entire sample are five-minute Apgar scores. Given immediately after birth, Apgar tests are designed to assess the health of a newborn infant and to determine the need for resuscitative efforts. Scores are given on a scale from 0 to 10 and assess a newborn's activity, pulse, reflex irritation, appearance, and respiration. While Apgar scores arguably remain inappropriate as predictors of certain long-term outcomes, five-minute scores nonetheless remain valid predictors of neonatal mortality (Casey et al. 2001).

V. Results

Tables 4–6 present the primary estimation results for this defensive medicine analysis, demonstrating the relationship between various tort reform adoptions and (1) episiotomy utilization, (2) delivery length-of-stay decisions, and (3) cesarean utilization, respectively. Table 7 likewise presents estimation results for the effect of the relevant tort reforms on neonatal health outcomes. All regression coefficients and standard errors presented in Tables 4–7 are multiplied by 100. Moreover, standard errors are clustered at the state level to allow for arbitrary within-state correlations of the error structure (Bertrand et al. 2004).

A. Episiotomy Utilization

I begin with an exploration into whether malpractice pressure induces physicians to perform additional episiotomies during vaginal deliveries, consistent with a positive defensive medicine story. Specifically, I identify any such effect by determining whether the adoption of malpractice reforms that reduce expected liability amounts (primarily, caps on noneconomic damages) lead to a reduction in episiotomy utilization rates. Column 1 of Table 4 presents estimates of a basic difference-in-difference (DD) specification that excludes the set of control variables and state-specific linear time trends. Consistent with

[15] In such states, physicians may indeed alter their behavior with this constitutional uncertainty in mind, considering that any such repeal will likely have retroactive effect (Currie & MacLeod 2008).

Table 4: Episiotomy Utilization: Difference-in-Difference Estimates (Among Sample of Vaginal Deliveries)

	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Noneconomic Damage Cap					
Contemporaneous dummy	–4.61*	–5.90**	–3.75	–3.15	–8.01**
	(2.60)	(2.37)	(2.38)	(1.96)	(3.61)
2-year lead dummy	—	—	–1.32	–3.49**	2.96
			(1.11)	(1.74)	(2.17)
Collateral Source Rule Reform					
Contemporaneous dummy	—	3.08	—	2.37	4.60
		(3.02)		(2.94)	(3.61)
2-year lead dummy	—	—	—	2.01	—
				(1.73)	
Punitive Damage Cap					
Contemporaneous dummy	—	0.37	—	–0.88	0.16
		(2.50)		(1.94)	(2.51)
2-year lead dummy	—	—	—	2.54	—
				(2.38)	
"Indirect" Tort Law					
Contemporaneous dummy	—	–2.45	—	–2.63	–8.28***
		(3.49)		(2.62)	(3.06)
2-year lead dummy	—	—	—	–1.74	—
				(2.28)	
Exclude states that invalidate damage caps?	No	No	No	No	Yes
Control variables?	No	Yes	No	Yes	Yes
State-specific linear trends?	No	Yes	No	Yes	Yes
N	564,683	430,960	564,683	430,960	355,091

*Significant at 10 percent; **significant at 5 percent; ***significant at 1 percent.

NOTES: All coefficients and standard deviations are multiplied by 100. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions, using a sample of vaginal deliveries, of the incidence of episiotomy utilization on the adoption of noneconomic damage cap laws. The specifications estimated in Columns 2, 4, and 5 also include additional state-year tort provisions along with state-specific linear time trends and other state-year controls. Columns 3–5 also include two-year lead indicator variables for the damage cap adoption dummies (which switch from 0 to 1 two years prior to the adoption of damage cap laws). Column 5 drops any state that varied its damage cap laws over the sample period by invalidating or repealing a previously enacted damage cap provision. Data on episiotomy utilization are from the 1979–2005 National Hospital Discharge Survey records.

the informal expectations indicated in Section III, I find that the adoption of a law capping noneconomic damage awards by a state is associated with a 4.6 percentage point reduction in the episiotomy utilization rate of that state, representing an 11 percent reduction in the average state episiotomy rate over the sample period.

In Columns 2–4 of Table 4, I demonstrate the sensitivity of these basic results to the inclusion of various control variables and state-specific linear time trends (Columns 2 and 4), along with the addition of lead dummy variables for the noneconomic damage cap laws (that switch two years prior to an amendment in noneconomic damage cap provisions), which allow for a test of trends in episiotomy rates that predate the adoption of noneconomic damage caps (Columns 3 and 4). As demonstrated by Column 2, the estimated relationship between noneconomic damage caps and episiotomy utilization does not

change substantially in magnitude with the addition of both state-specific linear time trends and a set of controls for various patient, hospital, and state-year factors (from a 4.6 percentage point reduction to a 5.9 percentage point reduction). However, while the *p* value from the basic DD specification is only 0.08, the estimated relationship does become statistically significant at the 5 percent level with the inclusion of these additional factors.

A concern arises, however, from the pattern of results presented in the dynamic specifications estimated in Columns 3 and 4. The coefficients of the two-year lead indicator variables are negative in both specifications and large enough in magnitude to suggest that the differential in episiotomy rates between treatment and control states may have materialized in the period prior to the adoption of noneconomic damage cap laws. However, this finding may be due to the fact that a substantial portion of the variation in noneconomic damage caps over the sample period arises from the invalidation or repeal of previously adopted caps (affecting eight states), most often as a result of a finding of unconstitutionality by the relevant state's highest court. Because a physician's behavior in the preinvalidation period may be judged according to the postinvalidation law in such situations, physicians in states that face a high probability of an unconstitutionality ruling may alter their behavior in anticipation of such a ruling (Currie & MacLeod 2008; Matsa 2007).

In their investigation of cesarean practices, Currie and MacLeod (2008) find evidence consistent with an anticipation story and estimate a statistically significant coefficient for the 12-month lead indicator for damage caps that turn "off" (at a magnitude close to that of the contemporaneous law change coefficient); however, they estimate no such lead effect for damage caps that turn "on." With these concerns in mind, Matsa (2007) excludes those states that invalidated or repealed previously adopted noneconomic damage caps in his investigation of the relationship between tort reforms and physician location.

In Column 5 of Table 4, I follow the approach taken by Matsa (2007) and estimate a dynamic specification with lead indicator variables that excludes those states that have invalidated or repealed noneconomic damage caps at some point over the sample period. Focusing only on the initial adoption of damage cap provisions, I continue to estimate a negative relationship between damage caps and episiotomy utilization rates, though now at a slightly higher magnitude (8 percentage point reduction). However, I now estimate a positive, statistically insignificant coefficient for the two-year lead indicator (with a magnitude of 3), strengthening any argument of a causal connection between damage cap adoptions and reduced episiotomy utilization.[16]

The results presented in Table 4 also suggest a negative relationship between episiotomy utilization rates and the incidence of an "indirect" tort reform, though these estimates are statistically insignificant in the general full-state specifications and may have

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[16]In unreported regressions, I also include lagged coefficients of the relevant tort variables and find evidence suggesting that the estimated impact of noneconomic damage cap adoptions materialized in the immediate aftermath of the reforms and intensified over time (i.e., roughly half the estimated postreform impact occurred in the immediate two-year period following the reform and the other half in the subsequent years).

materialized in the preadoption period.[17] While I find evidence that noneconomic damage cap adoptions lead to a reduction in episiotomy utilization, I estimate an opposite-signed coefficient (of similar, but smaller, magnitude) for the collateral source rule reform dummy; however, this estimate is not significantly different from zero and the lead coefficients likewise suggest that this positive relationship may simply be reflective of a preexisting differential trend.

B. Delivery Length of Stay

In Table 5, I test for evidence of positive defensive behavior in a physician's decision concerning the number of days that a mother should spend at the hospital during her delivery stay. I again identify any such effect by observing the physician response to tort reforms that reduce expected liability amounts. In Column 1, I begin by estimating a simple DD specification, without controls or state-specific linear time trends, identifying the relationship between noneconomic damage cap adoptions and the log of the number of bed days associated with the mother's hospitalization. Consistent with an expectation of positive defensive behavior in the length-of-stay decision, I find that the adoption of a law capping noneconomic damage awards by a state is associated with an approximately 3.9 percent reduction in the average maternal length of stay in that state (with a p value of 0.09).[18]

In Columns 2–4 of Table 5, I again demonstrate the sensitivity of these basic results to the inclusion of various control variables (e.g., HMO penetration rates at the state-year level) and state-specific linear time trends (Columns 2 and 4), along with the addition of lead dummy variables for the noneconomic damage cap laws (Columns 3 and 4). As demonstrated by Column 2, the estimated relationship between noneconomic damage caps and delivery bed days remains similar in magnitude with the addition of both control variables and state-specific linear time trends. However, as with the case of episiotomy utilization, the inclusion of these additional factors reduces the estimated standard error of the damage cap coefficient and thereby reduces the associated p value (from a test of no relationship) to 0.04.

I estimate negative coefficients for the two-year lead indicator variables in each of the specifications estimated in Columns 3–5 of Table 5, the last of which drops those states that invalidated or repealed previously adopted damage caps over the sample period. However, the lead coefficients in each instance are statistically insignificant and they are of relatively

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[17]One of the components of the indirect reform category is a reform of the joint and several liability rule. In alternate specifications that break out the individual components of the indirect category, I estimate that the adoption of a joint and several liability reform is associated with a roughly 4.9 percentage point reduction (significant at 10 percent) in the rate of episiotomy utilization.

[18]This estimate suggests an average reduction of roughly 2.6 hours per delivery. While perhaps modest in relative terms, a 4 percent reduction in the length of stay may represent a large absolute reduction in the cost of deliveries, considering (1) that, on average, each delivery costs $3,500 (or roughly $1,300 per delivery bed day) (Russo et al. 2006), and (2) the significant number of deliveries performed in U.S. hospitals each year (deliveries represent the most common medical event in the NHDS records).

Table 5: Delivery Bed Days: Difference-in-Difference Estimates (Among Sample of All Deliveries)

	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Noneconomic Damage Cap					
Contemporaneous dummy	−3.86*	−3.61**	−3.14	−2.79**	−4.57
	(2.28)	(1.74)	(2.32)	(1.31)	(2.74)
2-year lead dummy	—	—	−1.12	−1.47	−0.71
			(1.36)	(1.13)	(1.69)
Collateral Source Rule Reform					
Contemporaneous dummy	—	2.85	—	2.09	4.72**
		(1.76)		(1.44)	(1.81)
2-year lead dummy	—	—	—	1.46	—
				(1.00)	
Punitive Damage Cap					
Contemporaneous dummy	—	2.07	—	2.73	0.03
		(1.42)		(1.98)	(2.14)
2-year lead dummy	—	—	—	−1.32	—
				(2.54)	
"Indirect" Tort Law					
Contemporaneous dummy	—	−3.42*	—	−4.10**	−4.99**
		(1.73)		(1.60)	(2.44)
2-year lead dummy	—	—	—	1.16	—
				(1.03)	
Exclude states that invalidate damage caps?	No	No	No	No	Yes
Control variables?	No	Yes	No	Yes	Yes
State-specific linear trends?	No	Yes	No	Yes	Yes
N	737,193	565,201	737,193	565,201	465,153

*Significant at 10 percent; **significant at 5 percent; ***significant at 1 percent.

NOTES: All coefficients and standard deviations are multiplied by 100. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions, using a sample of vaginal and cesarean deliveries, of the number of bed days associated with an individual delivery stay (logged) on the adoption of noneconomic damage cap laws. The specifications estimated in Columns 2, 4, and 5 also include additional state-year tort provisions along with state-specific linear time trends and other state-year controls. Columns 3–5 also include two-year lead indicator variables for the damage cap adoption dummies (which switch from 0 to 1 two years prior to the adoption of damage cap laws). Column 5 drops any state that varied its damage cap laws over the sample period by invalidating or repealing a previously enacted damage cap provision. Data on delivery lengths of stay are from the 1979–2005 National Hospital Discharge Survey records.

modest magnitude in the repeal specification (Column 5), providing only weak evidence to suggest that the negative differential in delivery lengths of stay between treatment and control states may have begun in the period prior to the adoption of noneconomic damage cap laws.[19] As in the case of episiotomy utilization, I estimate a negative association between the duration of a mother's hospital stay and the adoption of an "indirect" tort reform,

[19]In unreported regressions, I again include lagged coefficients of the relevant tort variables and find evidence suggesting that the estimated impact of noneconomic damage cap adoptions on delivery lengths of stay materialized in the immediate aftermath of the reforms and intensified over time (as with the episiotomy specification, I find that roughly half the estimated postreform impact occurred in the immediate two-year period following the reform and the other half in the subsequent years).

providing further evidence of a positive defensive response in a physician's length-of-stay decision to the threat of possible malpractice liability.[20]

Also consistent with the findings of the episiotomy specifications, I estimate that a reform of the collateral source rule is associated with an increase in the number of delivery bed days. Again, however, this estimate is noisy in the full-state specifications and may also be reflective of an underlying differential trend between treatment and control states (Column 4 of Table 5). In any event, regardless of whether it is reasonable to expect an opposite-signed result between the damages cap coefficient and the collateral source rule reform coefficient, it may be reasonable to expect that each such reform will be associated with a different impact on the equilibrium length-of-stay measure, particularly considering the way the type of damages implicated by this specific medical context (e.g., noneconomic vs. economic) interacts with the type of liability risk associated with this medical decision (e.g., liability risk for improperly treating a condition vs. liability risk for executing the procedure/medical decision itself). For instance, under a traditional collateral source rule, the decision to keep a mother an extra day in the hospital may partially expose physicians to greater liability simply because that extra day itself may be part of the economic damages they are expected to pay in the event that some other aspect of the care leads to malpractice liability.[21] A collateral source rule reform may eliminate the requirement to pay for that cost if it is otherwise covered by a third party (a force that contributes to a positive relationship between such reforms and the resulting length-of-stay measure). Considerations of this specific nature, however, would not be a part of the noneconomic damages calculation.

The middle of the 1990s experienced a wave of reforms mandating lengths of coverage for hospital maternity visits, culminating with the passage of a federal law. To the extent that the differential adoption of such reforms may have been related to a state's prevailing malpractice rules, a concern arises regarding the consistency of the coefficients estimated in Table 5. To alleviate this concern, I estimate specifications that simply exclude the 1995 to 1997 period from the sample (during which all of the relevant rules were passed). The estimated pattern of coefficients remains essentially unchanged under this restricted sample.

C. Cesarean Utilization

In Table 6, I test for evidence of defensive behavior in a physician's decision to perform a cesarean delivery. Table 6 follows the same structure as Tables 4 and 5 discussed above. In each specification, I estimate small and statistically insignificant coefficients for each of the malpractice provisions explored, including caps on noneconomic damages. These results generally provide no evidence in support of the popular perception that malpractice pressures induce physicians to perform a substantially greater number of cesarean

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[20]In alternate specifications that break out the individual components of the indirect category, I estimate that the adoption of a joint and several liability reform is associated with a roughly 6 percent increase in the average delivery length of stay.

[21]Of course, the decision to keep a mother an extra day may be associated with countervailing reductions in expected economic damage awards in light of other risk considerations.

Table 6: Cesarean Section Utilization: Difference-in-Difference Estimates (Among Sample of All Deliveries)

	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Noneconomic Damage Cap					
Contemporaneous dummy	0.07	–0.10	0.13	0.25	0.47
	(0.63)	(0.57)	(0.55)	(0.53)	(1.09)
2-year lead dummy	—	—	–0.09	–0.47	0.31
			(0.50)	(0.35)	(0.62)
Collateral Source Rule Reform					
Contemporaneous dummy	—	0.36	—	0.49	0.43
		(0.58)		(0.44)	(0.69)
2-year lead dummy	—	—	—	–0.30	—
				(0.49)	
Punitive Damage Cap					
Contemporaneous dummy	—	–0.33	—	–0.33	–0.71
		(0.44)		(0.45)	(0.70)
2-year lead dummy	—	—	—	–0.01	—
				(0.46)	
"Indirect" Tort Law					
Contemporaneous dummy	—	0.00	—	–0.09	–0.73
		(0.67)		(0.62)	(1.09)
2-year lead dummy	—	—	—	0.06	—
				(0.46)	
Exclude states that invalidate damage caps?	No	No	No	No	Yes
Control variables?	No	Yes	No	Yes	Yes
State-specific linear trends?	No	Yes	No	Yes	Yes
N	737,193	565,201	737,193	565,201	465,153

*Significant at 10 percent; **significant at 5 percent; ***significant at 1 percent.

NOTES: All coefficients and standard deviations are multiplied by 100. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions, using a sample of all deliveries, of the incidence of cesarean section utilization on the adoption of noneconomic damage cap laws. The specifications estimated in Columns 2, 4, and 5 also include additional state-year tort provisions along with state-specific linear time trends and other state-year controls. Columns 3–5 also include two-year lead indicator variables for the damage cap adoption dummies (which switch from 0 to 1 two years prior to the adoption of damage cap laws). Column 5 drops any state that varied its damage cap laws over the sample period by invalidating or repealing a previously enacted damage cap provision. Data on cesarean utilization are from the 1979–2005 National Hospital Discharge Survey records.

deliveries. In the specification that includes both state-specific linear time trends and a set of control variables, as presented in Column 2, I find that the adoption of a noneconomic damage cap is associated with a statistically insignificant 0.1 percentage point reduction in a state's cesarean rate. Even assuming that the true effect is at the lower end of the estimated 95 percent confidence interval for this coefficient, the adoption of a noneconomic damage cap would only be associated with a –1.2 percentage point reduction in prevailing cesarean rates, representing a relatively modest 5 percent reduction in the prevailing cesarean rate.[22]

[22]In alternate specifications that break out the individual components of the indirect category, I estimate that the adoption of a joint and several liability reform is associated with a roughly 0.5 percentage point increase in cesarean utilization (though statistically indistinguishable from 0, with a 95 percent confidence interval from –0.4 to 1.4). This

Table 7: Five-Minute Apgar Scores: Difference-in-Difference Estimates (Among Sample of All Deliveries)

	(1)	*(2)*
	Log(Apgar Score Level)	*"Good" Apgar Score (0/1)*
Noneconomic damage cap dummy	0.01	–0.00
	(0.09)	(0.03)
Collateral source rule reform dummy	0.12	0.01
	(0.09)	(0.03)
Punitive damage cap dummy	–0.10	–0.05
	(0.12)	0.03
"Indirect" tort law dummy	0.10	0.11**
	(0.08)	(0.03)
Sample	All states	All states
Control variables and state-specific trends?	Yes	Yes
N	7,450,600	7,450,600

*Significant at 5 percent; **significant at 1 percent.

NOTES: All coefficients are multiplied by 100. Robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Reported coefficients are from difference-in-difference regressions, using a sample of cesarean and vaginal deliveries, of the log of each individual Apgar score associated with the delivery (Column 1), or the individual incidence of a "good" Apgar score (Column 2), on the adoption of noneconomic damage cap laws and other tort provisions. Apgar scores of 0 are set to 0.1 prior to the log transformation in Columns 1 and 2. Each specification also includes a set of individual demographic and other state-year controls, in addition to state-specific linear time trends. Data on neonatal outcomes is from a 10 percent sample of all deliveries in the 1978–2004 Natality Detail files.

These estimates confirm the findings presented in Currie and MacLeod (2008) to the extent that they too challenge the conventional wisdom that malpractice pressure is responsible for much of the excess in cesarean utilization. However, the estimates do suggest that if the relationship between noneconomic damage caps and cesarean utilization is positive in nature (i.e., contrary to the conventional wisdom), it is likely to be at a magnitude lower than that estimated by Currie and MacLeod, given that their estimated damage cap coefficient of 1.2 is above the upper end of the 95 percent confidence interval for the estimated damage cap coefficient indicated in Column 2 of Table 6.

D. Health Outcomes

The above results indicate that malpractice pressure may lead to certain increases in the intensity of care provided by obstetricians in the delivery of children, including increased utilization rates for episiotomies and increased delivery lengths of stay. It is of course possible that these positive utilization forces do not meet the definition of "defensive" behavior put forth above, in that they are actually associated with improvements in relevant health outcomes in the affected states. I explore this possibility in Table 7, presenting

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is in contrast to the estimated –1.7 percentage point reduction in cesarean rates following joint and several liability reforms estimated in Currie and MacLeod (2008). Even at the bottom of the estimated confidence interval, I estimate an impact of these reforms that exceeds Currie and MacLeod's estimate (i.e., that is less negative than their estimate).

estimates of the relationship between the adoption of noneconomic damage caps (and related tort reforms) and the five-minute Apgar scores assigned to each newborn in a sample of both cesarean and vaginal deliveries from the 1978–2004 Natality Detail files. Given the disconnect in the timing of this outcome measure (i.e., at delivery) and the delivery length-of-stay measure (i.e., postdelivery), this exercise is only useful in assessing the potential defensive nature of delivery lengths of stay to the extent that this measure also proxies for other utilization decisions made at or prior to delivery.

In Column 1 of Table 7, I present estimation results for difference-in-difference specifications that use the log of the five-minute Apgar score as the dependent variable (with full control variables and state-specific linear time trends). Column 2 analogously estimates specifications that use the incidence of a "good" Apgar score (greater than or equal to 7) as the dependent variable. In each specification, I estimate very small, statistically insignificant coefficients for the damage cap and collateral source rule reform measures. For instance, while noneconomic damage cap adoptions are associated with a 10–20 percent reduction in the episiotomy utilization rate (4–8 percentage point reduction) and a roughly 4 percent reduction in the average delivery length of stay, I estimate that they are associated with a 0.01 percentage point increase in individual five-minute Apgar scores. Even assuming that the estimated effect of damage cap adoptions is at the lower end of the relevant 95 percent confidence interval, the estimated effect would still entail a minor 0.2 percentage point reduction in individual Apgar scores.

Consistent with a defensive medicine story, these findings suggest that the positive utilization pressures created by malpractice fears do not generate corresponding improvements in health outcomes to the affected patient population.[23] If anything, the estimated positive coefficients presented in Table 7 for the "indirect" malpractice reforms suggest that the documented reductions in utilization rates stemming from "indirect" reform adoptions may be associated with improvements in neonatal health, implying that malpractice pressures felt prior to these "indirect" reforms both increased utilization rates and depressed neonatal health outcomes.

Of course, the implications of this health outcomes analysis (and, in turn, the implications for the defensive medicine determination) are limited by the unavailability of state-year data covering a more complete set of relevant health outcomes over the sample period, including those outcomes respecting maternal conditions. Episiotomies, for instance, have not always been indicated solely for the benefit of the newborn. Though certainly at a decreasing rate over time, many physicians have also performed episiotomies under the belief that the incision would reduce the risk of severe tears to the perineal skin. Absent data on the proper maternal morbidity measures, it is difficult to assess whether any malpractice-induced increases in episiotomy utilization are accompanied by associated improvements in maternal health outcomes.

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[23]Moreover, the pattern of results presented in Table 7 remains largely unchanged when I drop those states that repeal previously adopted damage cap laws over the sample period.

VI. Conclusion

This article contributes to an understanding of the manner in which malpractice pressure may shape physician behavior. For the purposes of this investigation, I focus on certain decisions faced by obstetricians during (and immediately after) the delivery of a child, a medical context that often implicates significant malpractice concerns. Consistent with much of the relevant literature, I use adoptions of various malpractice/tort reforms to identify within-state variations in malpractice pressure. However, unlike the analysis undertaken by Currie and MacLeod (2008), which addresses a substantially similar set of questions, I draw on a data set of hospital discharge records that allows for the identification of variation in physician behavior over the entire 1980s, a time period during which the most significant and relevant sources of legal variation occurred. The richness of the legal variation provided by this longer sample period provides greater confidence in the consistency of the estimated results. The results of this analysis confirm the findings of Currie and MacLeod to the extent that I find evidence inconsistent with the conventional wisdom that malpractice pressure contributes significantly to the excessive cesarean utilization rates observed across regions.

Extending this analysis to other aspects of obstetric care, I find evidence consistent with positive defensive behavior in the utilization of episiotomies during vaginal deliveries and in the durations of maternal lengths of stay. In each instance, I estimate that the adoption of a noneconomic damage cap leads to a reduction in the relevant utilization measure without a corresponding change in observed neonatal outcomes, implying that malpractice pressure may have previously induced overutilization of these measures. These findings are intuitive considering that the imbalance of risks involved in the relevant decision context may induce a risk-averse physician to elect the arguably unnecessary procedure (or additional bed day). In the cesarean context, however, the risks associated with the procedure may be high enough and the medical circumstances of the marginal cesarean mother may be minor enough that the balance of risks may not tip strongly in the direction of positive defensive medicine.

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