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Malpractice Liability and Medical Costs

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Abstract

A great deal of concern has been raised about the contribution of medical malpractice liability to growth in overall medical costs. We use county-level variation in the generosity of juries to identify the causal impact of malpractice liability on medical costs and its components. In stark contrast to the earlier literature, which has focused on costs and outcomes for *high-risk patients*, we find extremely modest effects on total costs—growth in malpractice payments over the last decade and a half contributed no more than 5.7% to the total real growth in hospital expenditures, which topped 33% over this period. While the overall impact on costs is modest, we do find that malpractice lowers the number of patients treated, and raises expenditures per day for those who remain. Nonetheless, this suggests that—contrary to arguments advanced by many policymakers and intimated by previous research—tort reform is unlikely to have much impact on US health care spending.

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A. Introduction

Both physicians and the broader public identify the spiraling costs of malpractice insurance and lawsuits, not medical errors or medical coverage, to be the largest and most important problem facing health care today (cf, Blendon et al., 2002). Physician groups such as the American Medical Association have advocated federal limits on the damages that can be assessed in malpractice cases. President George W. Bush has echoed this sentiment, and repeatedly pointed to rising malpractice costs as a major driver of growth in health care spending,[1] a view shared by a number of governors and state legislators.[2]

There is little question that malpractice costs have been rising rapidly in recent years, as Figure 1 documents. According to data from the National Practitioner Data Bank (NPDB), from 1991 to 2002 physicians' real annual medical malpractice payments grew from $2.3 billion to $3.8 billion (65% growth).[3] Over the same time period, real health expenditures on physician services grew from $221 billion to $325 billion (47% growth).[4] On the one hand, malpractice liability and medical costs are growing at similar rates, but malpractice payments account for a tiny fraction — between 1 and 2 percent — of total expenditures. Even if we were to include

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[1] President Bush recently reiterated his longstanding position in the 2007 State of the Union Address.

[2] To provide a few recent examples, Rhode Island's Governor Donald Carcieri recently proposed the Health Care Tort Reform Act of 2006 (Insurance Journal, May 3, 2006, "R.I. Governor Carcieri Urges Medical Malpractice Reform"), and Maryland recently provide malpractice insurance premium relief to physicians (Baltimore Sun, May 11, 2005, "State clears way to give doctors relief on premiums").

[3] The malpractice payment figures are conservative, because they omit payments made by a state fund, and not all payments are included in the NPDB (Government Accounting Office, 2000).

[4] Data on health expenditures are from the U.S. Census Bureau's *Statistical Abstract of the United States*.

other factors, like time and transaction costs, one might question just how important malpractice could be.

Earlier research has provided a possible mechanism for a large effect. Kessler and McClellan (1996) find that the threat of liability from medical malpractice causes doctors to practice "defensive medicine," performing extraneous (and expensive) tests and medical procedures to ward off the possibility of a malpractice suit.[5] The cost implications of defensive medicine are often projected to be quite large. Kessler and McClellan (2002) find that a 10% increase in expected malpractice payments is associated with as much as a 3.9% increase in hospital expenditures on heart attack patients, or a 2.7% increase in expenditures on patients with ischemic heart disease. If one were to apply these elasticities to the trends in health care and malpractice costs, the 65% increase in total malpractice payments from 1991 to 2002 would have accounted for about half the increase in total physician expenditures over that time period.

This argument has resonated with policymakers. For example, President Bush has publicly and repeatedly stated that, "One of the major cost drivers in the delivery of health care are these junk and frivolous lawsuits."[6] The President was informed by a Department of Health and Human Services report that drew on Kessler and McClellan (1996) for its estimated cost impacts (Department of Health and Human Services, 2002).

However, as Kessler and McClellan clearly state, and others have long since recognized, their study was not designed to estimate the impact of malpractice on *total* health spending. As

[5] Kessler and McClellan (1996) focus on heart attack patients. Other work has identified a relationship between malpractice costs on the use of obstetric and pre-natal procedures (Tussing, Wojtowycz, and Maxwell Graduate School, 1994; Corrigan et al., 1996; Dubay et al., 1999; Dubay et al., 2001), as well as more general medical practices (Bovbjerg et al., 1996).

[6] See www.whitehouse.gov/news/releases/2004/01/20040126-3.html. The President reiterated this position in his most recent State of the Union Address (www.nytimes.com/2007/01/23/washington/23bush-transcript.html).

noted by both the Congressional Budget Office (CBO) and the General Accounting Office (GAO), the Kessler and McClellan study estimated the impacts of malpractice pressure on the costs and outcomes of *heart attack patients*, whose experiences may or may not mirror those of the average patient (General Accounting Office, 1999; Congressional Budget Office, 2004). Moreover, subsequent empirical work has found much smaller effects of malpractice on other groups of patients, such as expectant mothers (cf, Dubay et al., 1999). As a result, both the GAO and CBO studies concluded that the overall effects of malpractice pressure on health spending are simply not known.[7]

Perhaps more troubling have been the questions of validity raised regarding the results that *are* in the literature. Inspired by the path-breaking work of Kessler and McClellan, many researchers have relied on state-level tort reform policies as a source of identifying variation. This strategy relies on the identifying assumption that reforms are not driven by underlying trends or characteristics in the state's malpractice or health care system. However, Danzon (2000) argues that states with managed care may have been more likely to adopt tort reform measures, and that the cost savings attributed to tort reform could be a result of managed care. Recent CBO research finds direct empirical evidence consistent with this argument: controlling for pre-reform state trends in health care spending eliminates the savings attributed to the reforms (Congressional Budget Office, 2006). The CBO study also documents the problematic

[7] Two recent papers by Baicker and Chandra (2006; 2007) — post-dating the CBO and GAO reports — do focus on the overall effects. Using cross-sectional and longitudinal variation by state, they find that malpractice risk has little impact on overall costs, even though it has substantial impacts on specific kinds of procedures, like medical imaging. As Baicker and Chandra note, however, a strategy for causal inference is required, in order to confirm or reject their findings.

pattern that reform states are more likely to have slower growth in health care spending prior to the adoption of the reform.

In light of the policy-relevance of the issue, and the gaps in our existing knowledge, we need to explore the effect of malpractice on *overall* costs — not just for high-risk patients — and to do so with a new identification strategy. We use an empirical approach that relies on changes over time in the generosity of local juries to identify the impact of malpractice on medical expenditures. Areas experiencing growth in jury generosity can be expected to experience growth in malpractice claims that is exogenous with respect to medical spending growth.

Using this approach, we confirm that malpractice risk does increase medical spending, but that the total effects on overall medical costs are extremely small. A ten percent reduction in malpractice jury awards would reduce total hospital expenditures by, at most, 0.4 to 0.9 percent. Even during the malpractice "crisis" of the 1990s, we predict this would have added just 5 or 6 percent to real hospital expenditure growth, from 1991 to 2002.

Our results suggest that malpractice indeed lowers the number of patients treated, but this effect is also modest. A 10% decrease in malpractice costs would raise the quantity of hospital care (as measured by the number of inpatient days and outpatient visits) by about 0.2 to 0.7 percent. Holding constant the number of patients treated, our results suggest that a 10% increase in malpractice costs would increase expenditures per patient by at most 0.8 percent, about one-fifth to one-quarter the amount suggested by Kessler and McClellan (2002). This suggests that the defensive medicine effect of malpractice has considerably less impact on the average patient than on the high-risk patients — or elastic procedures — that have previously been studied.

We proceed as follows. In Section B, we discuss the economic issues framing the malpractice debate, and discuss how malpractice can affect the cost of care, the quantity of care provided, and hospital investments. In Section C, we outline our empirical approach and describe our data and identification strategy. In Section D, we discuss our results. Finally, we conclude with a discussion of directions for future research.

B. Conceptual Background

Throughout our study, we think of "malpractice risk" as the random variable m measuring total malpractice payments. This includes the actual cost of settlements and judgments, lawyer fees, time costs, and reputational costs.

To crystallize the important distinction between the direct and indirect effects of malpractice risk, we lay out a simple model of medical costs. Suppose we can decompose medical output into quantity q, which represents the number of patients treated, and intensity I, measuring the intensity with which each person is treated. Intensity includes the number of procedures performed, the level of technology employed, and other dimensions of quality or amenities.

Define $c(q,I)$ as the *medical cost* of serving q patients with intensity level I, and define $m(q,I;J)$ as the total malpractice payments associated with these levels of production. J is some driver of malpractice payments that is independent of medical production levels. We propose the local generosity of juries as the variable J. The total cost of medical production is given by:

$$T(q,I;J) = m(q,I;J) + c(q,I) \qquad (1)$$

The *direct effect* of malpractice on costs is the effect holding medical production constant. In elasticity terms, a one percent change in malpractice risk, holding quantity and intensity fixed, changes costs by:

$$\frac{\dfrac{T_J(q,I;J)}{T(q,I;J)}}{\dfrac{m_J(q,I;J)}{m(q,I;J)}} = \frac{m(q,I;J)}{T(q,I;J)} \tag{2}$$

If malpractice constitutes an $s\%$ share in total costs, the elasticity of the direct effect is simply $s\%$. Empirically, malpractice costs represent one to two percent of total medical costs, suggesting the direct effect elasticity ought to lie between 0.01 and 0.02.

Previous research has identified elasticities much higher than these levels, thus implying significant *indirect* effects of malpractice. The indirect effects arise, because changes in malpractice risk can affect the marginal costs of providing quantity and/or intensity, which are given by:

$$\begin{aligned} T_q &= m_q(q,I;J) + c_q(q,I) \\ T_I &= m_I(q,I;J) + c_I(q,I) \end{aligned} \tag{3}$$

If $m_{qJ} \neq 0$, or $m_{IJ} \neq 0$, we can expect indirect effects on quantity, or intensity, respectively. The sign or size of these cross-partials is not obvious from theory, but several important effects may be present.

There are at least two offsetting forces determining m_{qJ}. These could explain the empirical existence of indirect effects of malpractice on quantity (Kessler and McClellan, 1996, 2002). Since each patient represents a potential lawsuit, increases in the quantity of patients mechanically expose providers to greater risk of a lawsuit. Therefore, doctors practicing in the presence of more generous juries might be more reluctant to take on an additional patient, because the potential cost of a lawsuit is higher. On the other hand, increases in quantity may increase provider skill and decrease the probability of a lawsuit. As a result, there may be a higher pay-off to increasing quantity when juries are more generous.

A similar pair of effects operates on m_{IJ}. Increases in the number of procedures, for instance, mechanically expose providers to greater risk, but they can also serve as "defensive medicine" that decreases the probability of a lawsuit, or a finding of negligence.[8] Since all these effects are theoretically ambiguous, empirical estimates are needed.

This simple framework also demonstrates why the effect of malpractice may differ for high-risk patients. For patients whose treatments involve little risk of an adverse outcome, the marginal effects m_I and m_q may be quite low. This would suggest that malpractice has smaller effects on the treatment of low-risk patients. On the other hand, it might be easier to legally establish the fault of a provider given an adverse outcome, because low-risk patients may have lower background rates of illness or death. For example, it may be harder to trace the death of a sick, elderly patient to provider fault, than the death of a young, healthy patient.

Incorporating insurance into the analysis is straightforward. If a hospital or provider is self-insured, they bear all malpractice costs directly, and the above arguments apply in full. The same would be true if they own an insurance policy that is fully risk-rated. If, however, they transfer some of their risk to an insurance company, only the uninsured portion of malpractice risk affects behavior (Ehrlich and Becker, 1972). Only uninsured risk induces self-protective behavior on the part of providers; it is this self-protective behavior that constitutes the indirect effects of malpractice.

[8] The term defensive medicine is generally taken to mean the use of extraneous procedures, or at least procedures whose expected marginal benefit is outweighed by marginal cost. However, in our model, and more generally, it could represent an efficient deterrence of negligent care by the liability system. Whether defensive medicine is wasteful or efficient is beyond the scope of this paper.

C. Empirical Framework

We are interested in the effect of malpractice costs on total costs, quantity, intensity (cost per patient), and outcomes. The key empirical problem for us is one of reverse causality: malpractice costs influence all these outcomes, but provider decisions about cost, quantity, and intensity also influence the risk of malpractice litigation. This suggests the following instrumental variables model of the outcome Y_{it} for unit i at time t:

$$\begin{aligned} Y_{it} &= \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 E(MedMal_{ct}) + \boldsymbol{\beta}_2 X_{ct} + \boldsymbol{\phi}_i + \boldsymbol{\gamma}_t + \boldsymbol{\varepsilon}_{it} \\ E(MedMal_{ct}) &= \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1 J_{ct} + \boldsymbol{\alpha}_2 X_{ct} + \boldsymbol{\phi}_i + \boldsymbol{\gamma}_t + \boldsymbol{\delta}_{it} \end{aligned} \tag{4}$$

In addition to reverse causality, this specification illustrates the other key empirical issue: providers respond to *expected malpractice costs*, rather than actual current costs.

We propose to study several outcomes Y_{it} at two levels of aggregation.

1. County-level: Medicare spending per Medicare beneficiary;
2. Hospital-level: overall medical spending per patient-day, number of patient-days, adoption and utilization of technology;

As we discuss below, we have data from the Medicare population, and at the hospital level. Below, we discuss the measurement of $E(MedMal_{ct})$, which represents expected per capita medical malpractice jury awards in county c and at time t. X_{ct} is a vector of time-varying county characteristics, and $\boldsymbol{\phi}_i$ is a fixed-effect either at the hospital-level, or the county-level, depending on how Y_{it} is measured. We also include a year fixed-effect γ_t.

We begin with issues of measurement: our strategy for measuring malpractice costs (Section C.1), and our measurement of covariates and outcomes (Section C.2). We then provide empirical evidence in defense of our identification strategy (Section C.3).

C.1 Measurement of Expected Malpractice Costs

The empirical strategy requires that we be able to measure *expected malpractice costs*. Our approach is to use data on malpractice jury verdicts from the RAND Jury Verdicts Database (JVDB). We begin by describing the JVDB data, and then justify our measurement strategy in two steps: demonstrating the viability of measuring *malpractice costs* using data on malpractice jury verdicts, and addressing the problem of measuring *expected* malpractice costs.

C.1.1 *RAND Jury Verdicts Database*

As our instrument, we use average jury awards of non-economic damages, also called "pain and suffering," payments in awards for plaintiff victories. Non-economic damages represent a significant portion of damage awards,[9] and are frequently the target of reform efforts. In general, areas in which the average non-economic damages are higher should also have higher overall damages. As our measure of malpractice, we use total jury verdict awards in malpractice cases. Both are measured at the county- and year-level.

We use the RAND Jury Verdicts Database (JVDB) to recover the verdicts data. The RAND JVDB contains information on jury verdicts occurring in all counties in New York state and California, as well as Cook County, IL (Chicago), Harris County, TX (Houston), King County, WA (Seattle) and the counties in the greater St. Louis, MO area from 1985-1999 (125 counties in all). Our data cover 3.9% of US counties, but 23.6% of total US population, as of the year 2000.

The data in the JVDB were collected from court reporter publications. Relevant for our purposes, the JVDB includes data on plaintiff win rates, average economic and non-economic damage awards and type of injury for medical malpractice and other tort cases. While jury

[9] In the data we use, non-economic damages amount to approximately 28% of awards in all tort cases, and approximately 35% of awards in medical malpractice cases.

verdicts represent a relatively small sample of disputes (more than 90 percent of disputes settle out of court) they are important because expectations about juries influence all pre-trial negotiations. As we show later, jury awards and settlements are highly correlated, so information on jury verdicts will be a good proxy for malpractice liability.[10]

In Table 1, we present some summary statistics from the JVDB. The table presents the county-level averages for: total malpractice awards, malpractice awards per capita, average non-economic damages awards per plaintiff win, and total jury verdict awards in all tort cases. Both unweighted and population-weighted statistics are presented. As discussed below, we use lagged moving averages in the regression model; therefore, we present both statistics for the current year, and lagged moving averages. The columns of the table present the current year's average (year t), along with 3-year moving averages at various lags, beginning with a moving average across years $t-1$, $t-2$, and $t-3$), and ending with the moving average across $t-4$, $t-5$, and $t-6$.

C.1.2 *Measuring Jury Verdicts*

We use county-level jury verdicts in malpractice cases as a proxy for total malpractice costs in a county. The first step in defending this measurement strategy is to show that the RAND JVDB data accurately measure non-economic damage and malpractice awards in actual jury verdicts.

The RAND JVDB collects jury verdict award data from jury verdict reporters. Some researchers have objected that jury verdict reporters do not comprehensively cover all verdicts.[11] Court systems do not collect much detailed data about their own verdicts, so jury verdict

[10] If the propensity to settle is higher in areas with higher expected non-economic damages then this will affect our ability to estimate the structural relationship between malpractice and medical costs, but as long as non-economic damages are not driven by higher medical costs we can still obtain consistent reduced form parameters.

[11] For example, see Vidmar (1994), Moller, et al. (2004), and Eisenberg (2001).

reporters must typically rely on voluntary reports from the individual attorneys involved in cases. Earlier studies on the RAND JVDB used samples of public records to validate the data from several of the reporters used in this study.[12] Peterson and Priest (1982) found that the *Cook County Jury Verdict Reporter* contained more than 90 percent of all verdicts in almost every year from 1960-1978. Shanley and Peterson (1983) found that the *California Jury Verdicts Weekly* contained more than 84 percent of 1974 and 1979 verdicts in San Francisco County. Moreover, the verdicts most likely to be omitted were contract and financial injury cases, which do not enter into the non-economic damages instrument or the malpractice awards measure. These relatively high reporting rates as well as the confinement of the error to unrelated types of cases justify the accuracy of the measures for the purposes of this study.

C.1.3 *Measuring Expected Malpractice Cost*

The model requires measurement of $E(MedMal_{ct})$, because forward-looking providers are influenced not by realized malpractice costs, but by the malpractice liability they *expect* in a given year. Clearly, the true expected cost is always unobserved. However, providers will infer the expected cost from their observations about past cost. They may also use additional information — some of which is unobservable to us.

In practice, relatively few malpractice cases actually come to trial. However, theory predicts that the outcomes of malpractice verdicts ought to influence pre-trial negotiation and out-of-court settlements. The empirical question is how well malpractice verdicts approximate the quantity of interest — namely, all malpractice payments made by providers to plaintiffs. We explicitly think about malpractice expectations as being formed from information about past trends in jury verdicts. Total malpractice payments are equal to jury verdicts plus malpractice

[12] Seabury, Pace, and Reville (2004) provide additional background on the JVDB and the research on its validation.

settlements. From a theoretical point of view, settlements are determined by negotiations between plaintiffs and defendants, based on their expectations about a verdict from a potential jury trial. Therefore, as long as expected jury verdicts today can be forecast from previous jury verdicts, it is theoretically plausible to write:

$$E(MedMal_{ct} \mid V_{c,t-1}, V_{c,t-2}, \ldots), \tag{5}$$

where $V_{c,t-i}$ represents malpractice jury verdicts in county c and time $t-i$.

This formulation raises two questions: (1) How reliable is a forecast of current malpractice payments based on past verdicts and other observable information? (2) What is the best way to combine historical information on past verdicts in producing such a forecast? To answer these questions, we analyze state-level data from the 1990-2005 National Practitioner Data Bank (NPDB), which reports both malpractice jury verdicts and total malpractice settlements, but at the state-level. In particular, we estimate the following regression:

$$MedMal_{st} = \boldsymbol{\varphi}_0 + \sum_{i=1}^{6} \boldsymbol{\varphi}_i V_{s,t-i} + \boldsymbol{\omega}_{st} \tag{6}$$

We can think about expected malpractice payments as the fitted values from this regression, which takes as information historical trends in malpractice verdicts.

The results of this regression appear in Table 3. The table reports models using 5 different specifications, differing in the included lags. Column 1 reports a regression of $MedMal_{st}$ on $V_{s,t-1}$ through $V_{s,t-6}$, and the corresponding regression of $MedMal_{st}$ on the moving average of $V_{s,t-1}$ through $V_{s,t-6}$. Similarly, column 2 repeats this for lags $V_{s,t-1}$ through $V_{s,t-3}$, and so on. In addition to the regression coefficients, the table reports R-squared statistics, and the results of testing for equality between the coefficients $\boldsymbol{\varphi}_i$.

On their own, lagged malpractice verdicts explain a significant amount of the variation in current malpractice payments. Six lags explain 74% of variation in payments, while the first three lags alone explain 72%. Even historical lags have good explanatory power: lags 4 through 6 explain about 66% of the variation in current malpractice payments. This suggests that, while malpractice verdicts are not perfect proxies of payments, they perform reasonably well.

Second, for all models, we cannot reject the possibility that the coefficients on all the lags are equal. As a result, we cannot reject the simplest measurement strategy of using moving averages of jury verdicts, as proxies for malpractice payments. The regressions at the bottom of the table explicitly test the relationship between moving average verdicts, and current malpractice payments. In terms of R-squared, little is lost by moving from the specification with individual lags to one with a combined, equal-weighted moving average.

From the point of view of accurately measuring expected malpractice costs, there is no clear advantage in using lags that are closer or farther in time. As such, we explored a variety of moving average lag structures in our analysis. We do not present all of these in the paper, but fully document a variety of permutations in Appendix B.

Finally, if our IV estimates are consistent, and if verdicts reliably proxy expected costs, they will generate elasticity estimates statistically similar to those obtained from regressions using actual measures of expected costs. Suppose we could directly measure expected malpractice costs. In this case, our estimator of the elasticity of medical costs with respect to malpractice converges to the desired value, as in:

$$\text{plim}\left(\frac{\hat{\beta}_1 \overline{MedMal_{ct}}}{\overline{MedCosts_{ct}}}\right) = \frac{\beta_1 E(MedMal_{ct})}{E(MedCosts_{ct})} \tag{7}$$

What we have instead is a proxy for $E(MedMal_{ct})$, defined as V_{ct}. Suppose that our proxy is an unbiased predictor, such that $E(MedMal_{ct}) = \pi E(V_{ct})$. In this case, the coefficient we recover from an instrumental variables model is an estimate of $\beta_1 \pi$, not β_1. However, our estimator for the elasticity of medical costs with respect to malpractice still converges to the same value as in 7:[13]

$$\text{plim}\left(\frac{\hat{\beta}_1 \hat{\pi} \overline{V}_{ct}}{\overline{MedCosts}_{ct}}\right) = \frac{\beta_1 \pi E(V_{ct})}{E(MedCosts_{ct})} = \frac{\beta_1 E(MedMal_{ct})}{E(MedCosts_{ct})} \tag{8}$$

As emphasized above, this is subject to the caveats that our proxy must be unbiased, and that our model must consistently estimate the underlying parameter of interest.

C.2 Measuring Covariates and Outcomes

The previous section described the measurement of expected malpractice costs $E(MedMal_{ct})$, malpractice jury verdicts, V_{ct}, and overall non-economic damages J_{ct}. We now describe our measurement of county characteristics X_{ct}, along with unit-level outcomes Y_{it}.

C.2.1 *County-Level Characteristics from the Area Resource File*

Information on county-level demographics is taken from the Area Resource File (ARF). The ARF collects county-level per capita income from the Bureau of Economic Analysis (BEA) Local Area Income Tapes. The BEA collects these data as part of its annual estimates of local economic activity. The data on population are from the Census; the Census Bureau produces estimates for intercensal years based on a demographic model of their own. The vector X_{ct} includes time-varying county-level demographic characteristics: proportion male, proportion

[13] The standard errors will also be computed appropriately, because the model computes the standard error around the estimate of $\beta_1 \pi$, as a whole.

black, proportion white, income per capita and its square, and proportion of the population in 5-year age categories (one category for every five-year age interval between 0 and 85, and a single category for 85+).

These demographic data are summarized in the bottom panel of Table 2. The average person in our sample has higher per capita income than the average American, and is more likely to be non-white (likely due at least in part to the inclusion of California and New York). Still, the samples are close enough overall that it appears our results should be broadly generalizable to the larger population.

In addition, we also control for the time-varying characteristics of the county's jury verdicts, based on the JVDB data, with a set of variables measuring the proportion of cases that fall into each of the following mutually exclusive and exhaustive categories: no injury, physical injury but no permanent disability, partial disability, permanent and total disability, death, or multiple plaintiffs in the suit. This accounts for changes in the severity of injuries, which might affect the size of awards. These covariates appear in both the first- and second-stage models and thus play no identifying role.

C.2.2 *County-Level Medicare Costs*

One of the outcomes we study is county-level Medicare spending. From CMS, we have obtained county-level data on Medicare expenditures, from 1980 to 2003. In particular, CMS reports total Medicare Part A and B enrollees residing in a county; these are based on their administrative records. They also report total Medicare Part A and B expenditures for the residents of each county; these are based on administrative claims data linked to each Medicare beneficiary. Ideally, we would have preferred measures of Medicare utilization by Medicare beneficiaries who sought care in a particular county, rather than who live in a particular county. This would have matched up better with our measurement of malpractice risk faced by the providers in a particular county. The mismatch induces measurement error, because some of our beneficiaries are receiving care outside the county for which we are measuring malpractice risk.

This biases down our estimates. However, our instrumental variables strategy helps remove the effect of measurement error, provided that changes in the propensity to seek care outside the county are uncorrelated with changes in juries' propensity to award non-economic damages.

The Medicare data are summarized in the middle panel of Table 2. Part A is the inpatient hospital insurance portion of Medicare that is free to all eligible Americans over the age of 65. Part B covers physician visits, outpatient procedures, and diagnostic imaging. Eligible individuals must pay a premium for Part B, but approximately 94 percent[14] of Part A beneficiaries are enrolled in Part B. The table also reports geographically deflated expenditures, which represent indices of quantity. Medicare reimburses procedures differently across counties. Part A reimbursements are adjusted using the Medicare wage-index. Part B reimbursements are adjusted using the Geographic Practice Cost Index (GPCI). Therefore, the overall variation in expenditures across counties reflects both differences in utilization and differences in reimbursement rates. We deflated Parts A and B expenditures to construct series that reflect only differences in quantity, and not differences in reimbursement. The deflated series should be interpreted as the expenditures that would obtain if a particular county received the average national reimbursement rate.

C.2.3 *Hospital-Level Costs and Utilization*

We also study cost and utilization outcomes at the hospital level. These data — on hospital spending, utilization, and facilities — come from the American Hospital Association

[14] Based on CMS enrollment data from 2004. See http://www.cms.hhs.gov/MedicareEnRpts/Downloads/Sageall04.pdf

(AHA). The AHA conducts an annual census of its member hospitals. The survey has been conducted since 1946. We use data from the 1980 to 2003 survey years.

Hospital administrators are surveyed about their total facility expenditures over the most recent 12-month fiscal year, available resources at the end of that 12-month reporting period, and resource utilization during that period. A particular advantage of these data is the availability of facility identifiers that are consistent over time, and allow us to treat the data longitudinally. Note that all the data are reported at the facility level, not the firm-level. Therefore, ten hospitals owned by a single corporation would appear as ten separate observations with ten separate (and consistent) identifiers. In some cases, the length of reporting periods may vary, due for example to a hospital closure. In these cases, we annualize the expenditure and utilization numbers, based on the actual length of the reporting period. All costs, here and throughout the paper, are deflated over time using the overall Consumer Price Index. [15]

The upper panel of Table 2 summarizes the expenditure and utilization data from the AHA survey. Since our core regression models work with the 1985-2003 data, we have restricted the summary statistics to cover these years. The table shows the weighted and unweighted statistics over the counties in our JVDB sample, as well as the corresponding numbers for all counties. The table demonstrates that more populous areas have higher expenditures, but lower utilization per available bed. Moreover, the average person in our sample tends to live in a county with higher expenditures and lower utilization than the average American. However, these differences are not that large. It is relevant to note that, while our

[15] For the usual well-known reasons, we do not use the medical care CPI (Boskin et al., 1997; Berndt et al., 1998). Therefore, our estimates include real growth in medical care costs compared to other goods.

sample covers only about 5% of American counties, these are large, populous counties that together account for about one-quarter of the US population.

C.3 Identification

As our instrument, we use non-economic damage awards per case awarded by juries in the county. The idea is to use the generosity (or stinginess) of local juries as a means of exogenously shifting malpractice risk. We show that this is a sufficiently powerful instrument for total malpractice verdicts, and argue that it is valid. Finally, we discuss the source of the identifying variation.

C.3.1 *Instrument Power*

Table 4 displays the first-stage regression results associated with regression equation 4. The instrument is the average non-economic damage award, per plaintiff win, granted by juries in the county. This is calculated across *all* types of cases, not just malpractice, and measures the overall generosity of local juries. On the other hand, the included endogenous variable is the total value of malpractice awards, per county resident. Since we run models at both the hospital- and county-level, we report first-stage regression results that correspond to each level of aggregation. The results differ in that the hospital-level model includes hospital fixed-effects, and the county-level includes county fixed-effects. In both, standard errors are clustered at the county-level. For all the models with lagged non-economic damages as the instrument, first-stage power meets the rule of thumb suggesting a Wald statistic of 10.0 or better.

An issue that affects power and potentially validity is the set of cases to include in constructing the instrument. We compute average non-economic awards in plaintiff wins, across *all* cases. A plausible alternative configuration is to compute average non-economic awards in non-malpractice cases. This configuration arguably makes a stronger *a priori* case for validity

— since non-malpractice awards are less likely to be related to health care costs — but likely makes a weaker case for instrument power. Empirically, this configuration produces a weak instruments problem, with its attendant consequences of poor coverage rates (Staiger and Stock, 1997).

Including malpractice awards provides us with a much more powerful instrument, but it adds a stronger identifying assumption.[16] We implicitly contend that health care costs do not affect the size of pain and suffering awards in *successful* malpractice suits. However, we allow for the possibility that defensive medicine reduces the probability of a lawsuit, and reduces the probability of a plaintiff win. The exclusion restriction rules out the scenario under which defensive medicine reduces the size of non-economic damage awards, conditional on a plaintiff win.[17] In other words, we assume that jury awards for "pain and suffering" in malpractice cases are unaffected by local trends in medical spending. We test this assumption in two ways, and find that it clearly passes both tests.

The simplest test assesses the direction of causality between non-economic damages and medical spending. Second, we test for a specific mechanism that would invalidate the instrument. Suppose juries mechanically calculate non-economic damages as some increasing function of the economic damage award. Since counties with higher medical costs will necessarily award higher economic damages, this would create an invalid dependence between medical spending and non-economic damages. However, we show that plaintiffs' claimed

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[16] An alternative identification strategy would include non-economic awards from *only* malpractice cases, if the awards in non-malpractice cases were thought to have no causal impact on physician's perceived malpractice risk. In Appendix E, we present results from this strategy, which are extremely similar. If anything, limiting the instrument to malpractice cases alone results in smaller effects of malpractice liability on medical costs.

[17] In the language of Ehrlich and Becker (1972), we allow for self-protection, but rule out the possibility of self-insurance.

medical losses are highly and positively correlated with economic damage awards, but entirely uncorrelated with non-economic damages. This is evidence against a "rule of thumb" for jury awards. It also provides evidence that jury behavior is consistent with the principle that "pain and suffering" awards ought to be regarded as independent of a plaintiff's tangible economic losses.

If the identifying assumption fails to be valid, current and past health expenditures (as we measure them) ought to reduce current non-economic damage awards made in successful lawsuits. However, as we show below, the data demonstrate that this is not the case, *and* that higher current non-economic damage awards raise future health expenditures. These results lend empirical support to our identifying assumption.

C.3.2 *Instrument Validity*

Medical expenditures directly enter into jury verdicts when an injured plaintiff requires medical care, but only in the economic damages portion of the award. In principle, non-economic damages should be uncorrelated with this. In practice, however, juries might adopt rules of thumb that peg non-economic damages to economic damages, and this might create a relationship with medical costs that would invalidate the instrument. A more complex relationship might arise if defensive medicine changes the composition of plaintiffs, and the non-economic damage awards they end up with. In principle, either of these relationships might obtain. Therefore, we test the validity of the instrument in two ways, and find that the data support it.

First, we find that past medical costs do not affect current non-economic damage awards, but past non-economic damages do affect current medical spending. This suggests that causality runs from the instrument to medical spending, and not in the opposite (and invalid) direction. To implement this test, we ran reduced-form versions of equation 4, where health expenditures are regressed on the instrument, state and year fixed-effects, and all the covariates X. In addition to current and lagged moving averages of non-economic damage awards, we also use leading moving averages of non-economic awards, to see if these are correlated with current health costs. If health costs cause verdicts, we ought to see a relationship between current costs and future non-economic awards. Table 5 presents the results for seven different dependent variables. There are 28 regressions testing the causal link from lagged non-economic damages to current medical spending (i.e., the 4 right-most columns); 17 yield significant effects at the 10% level. On the other hand, only one of the 28 regressions testing the opposite effect — of current health care spending on leads of non-economic damages —is significant at the 10% level. Moreover, the regressions have more power when we test for the reverse causality running from medical spending to non-economic damages, but we find fewer significant effects. Finally, note that the current year regressions have the most power, but fail to find any significant relationship. This is an important argument against the possibility that juries use current growth in medical spending as a reason to raise awards. In addition, it demonstrates that serial correlation in the instrument likely does not cause bias by introducing a relationship from health care spending to future non-economic damages.

Second, we conduct a direct test of “rule of thumb” behavior by juries. According to this hypothesis, counties with high growth in medical spending exhibit higher economic damage awards, and this spills over into non-economic damages. However, we find that plaintiffs’ claimed medical losses are highly correlated with economic damage awards, but entirely uncorrelated with non-economic damage awards. This suggests that juries do not tie economic and non-economic damages, and that high growth in medical spending does not seem to spill over into “pain and suffering” awards. Table 6 presents the results of this test for the 2,328 malpractice cases that involved a plaintiff win in our sample. The first two columns of the table illustrate the estimated impact of claimed economic losses on the compensatory economic award granted by the jury, with and without non-medical losses, respectively. The second two columns provide similar estimates for the non-economic award.

As we would expect, claimed losses have a large impact on the economic awards. An additional dollar of claimed medical and non-medical damages is associated with about a $0.34 and $0.22 higher award, respectively (though only the medical losses are statistically significant alone, they are jointly significant). However, there is virtually no impact of claimed economic losses on noneconomic awards. The point estimates are smaller by at least an order of magnitude, and they are not statistically significant (either individually or jointly). These results are inconsistent with the notion that juries simply pin noneconomic awards to the severity of economic damages claimed by plaintiffs.

To summarize, the non-economic damages instrument has the following advantages:

- Both *a priori* and empirically, non-economic damages seem to be set independently of wages, medical care prices, and other incentives that affect health care;
- Non-economic damage awards in general are highly predictive of malpractice jury verdict sizes;

Non-economic damages seem to influence future health care costs, but health care costs do not influence future non-economic damages; this is consistent with the power of the instrument to affect malpractice verdicts and costs, but inconsistent with causality running from health care costs to non-economic damages.

As an additional test of instrument validity, we compute instrumental variables estimates with and without the auxiliary controls X_{ct}; this is analogous to the so-called “Wald estimator” in an instrumental variables model. If the instrument is valid, it ought to be uncorrelated with county-level trends in demographic characteristics, jury verdicts, and local economic variables.

Therefore, including or excluding those additional controls should not affect the point estimate of β_1, even though they may reduce the standard errors by soaking up "nuisance" variation. This is in fact consistent with our findings. Appendix C demonstrates that the estimates of β_1 are indistinguishable, with and without controls, but that adding controls reduces the standard errors of the point estimate.

The occurrence of tort reform generates a final potential validity issue that needs to be addressed. If in fact tort reform is driven by medical expenditures, and if tort reform affects non-economic damages in our data, the instrument could be compromised. However, it seems unlikely that this is a significant concern for us, primarily because there are relatively few reforms adopted in our sampled states during the time period of study. California has the strictest reforms in our sample, and perhaps in the country, but these were adopted in 1979. Conceivably, one might still be concerned that California's non-economic damage growth is systematically different than other states, in a way that is related to health spending. However, we get substantially similar results when we run the analysis on California alone, and on New York alone, which has no malpractice reforms in place during this period. Missouri adopted a damage cap at the very beginning of our sample (1986), but excluding the initial year has no impact on our results. Illinois adopted reform in 1987, but it was ruled unconstitutional that same year. Other observed reforms likely had little effect on damage awards. For example, Texas adopted a cap on punitive damages in 1995, but punitive damages are rare in medical malpractice cases, and should have little effect (Eisenberg et al., 1997).

C.3.3 ***Source of the Identifying Variation***

Our identification strategy relies on local trends in jury generosity with respect to pain and suffering awards – conditional on the observable severity of the plaintiff's injury, nationwide time-trends, county fixed-effects, and county demographics. We have provided evidence for instrument validity by showing that it is unrelated to past medical costs, but predicts future costs. However, it is also helpful to understand what drives the instrument's variation, in order to be sure that jury generosity is not obviously driven by other factors that plausibly affect health care.

Some insight is provided by the literature in psychology and legal studies on how and why juries and jurors make the decisions they do. The primary determinants of jury pain and suffering awards have been found to be the actual extent of the injury suffered by the plaintiff, and the jurors' own internal monetary evaluations of an injury. Other factors, such as jurors' perceptions of how liable the defendant actually is, or the pre-existing characteristics of the plaintiff (e.g., race or income) have been found less relevant (McCaffery, Kahneman, and Spitzer, 1995; Wissler et al., 1997). To a large extent, we observe and control for the severity of plaintiff injury in our verdict data: we observe the type of injury, as well as whether the injury resulted in temporary disability, permanent partial disability, permanent total disability, or death. Unobservable variation in severity seems not to drive identification, because adding controls for plaintiff injury had virtually no quantitative effect on our point-estimates for county-level costs and health utilization. If the observable injury measures were unrelated, it is less likely that the unobservables matter. By elimination, therefore, the residual variation in non-economic damage awards may be related to the last factor — jurors' internal evaluations of monetary damages, which jurors report being very difficult to estimate (Wissler et al., 1997). This is not surprising, since the jury behavior literature consistently finds this factor to be of great importance.

Internal evaluations have been related to certain observable characteristics — notably poverty, race, and educational attainment — but county trends in these observables explain relatively little of the county trends in non-economic damages. Including or excluding county-level measures for these variables had no appreciable quantitative impact on the first-stage or second-stage estimates (in the presence of county fixed-effects).

This finding is consistent with the experimental literature on jury behavior, which has found that jurors very frequently rely on heuristics or "rules of thumb" that are not systematic. Many of these rules of thumb will be random in an individual jury, but arbitrary changes in local norms would generate trends over time. Several researchers have found "anchoring," whereby jurors respond to a fixed but arbitrary point of reference, to be particularly important in the determination of non-economic damages (Sunstein, 1997; Robbennolt and Studebaker, 1999; Marti and Wissler, 2000). That is, jurors take arbitrary reference points — e.g., previous verdicts, suggested maximum verdicts, and the like — and use these to determine the appropriate award. Experimental evidence with mock juries shows that random changes in an arbitrary reference point can change jury verdicts substantially (cf, Robbennolt and Studebaker, 1999). This would imply that variation in jury awards is generated by a relatively random process. This has been used as an explanation of an apparent puzzle in jury behavior, that there are regions of the country where jurors consistently seem to award higher damages, and that these "judicial hell-holes" (a phrase coined by defense lawyers) emerge and fade away at particular points in time. For example, the tort reform advocacy group ATRA (American Tort Reform Association) maintains an annual ranking of the highest-verdict jurisdictions. The changes over time in the rankings are of interest: from 2002 to 2005, there were 6 new entrants into the "top ten" most generous jurisdictions. At the same time, it has proven extremely difficult in the legal studies literature to identify measurable county characteristics that predict increases or decreases in local jury generosity.

D. Results

D.1 Effects of Malpractice on Hospital Costs and Output

Ordinary least squares and instrumental variables estimates of equation 4 at the hospital level are given in Table . We model costs per bed-day, and bed-days per bed. Taken together, we can infer changes in cost per bed. The decomposition provides some insight into changes in quantity (bed-days per bed), versus changes in price (cost per bed-day). It is worth noting that we found few if any consistent relationships between malpractice and the total number of beds. Bed-days are defined as the sum of inpatient and outpatient bed-days.[18]

While the OLS models show little relationship between malpractice and either expenditures or bed-days, the IV models suggest that malpractice risk raises "price" but lowers "quantity." Hospitals treat fewer patients, but the treated patients end up spending more per day in the hospital. The elasticities are nearly equal in absolute value: a ten percent increase in malpractice risk raises costs per day by 0.9 percent at most, but reduces days by more than 0.7 percent. The offsetting effects on price and quantity minimize the effect on total spending, but fewer patients end up getting treated, and at higher cost.

The last model in the table shows that the reduction in inpatient days per bed is significantly smaller than the total reduction in days per bed. Most of the reduction in output comes from fewer outpatient procedures. We can think of this as a shift away from less intensive (outpatient) procedures, or as the inability of providers to avoid providing more critical inpatient care. Regardless of the precise mechanism, the most striking feature of these estimates is the modest size. The overall impact of malpractice risk on spending (price multiplied by quantity) is

[18] An outpatient procedure is defined as equivalent to an outpatient bed-day, since outpatient procedures are completed in one day.

extremely small, and even the separate impacts on price and quantity are relatively modest, with elasticities below 0.1.

The IV models are identified primarily by the instrument itself, and not by the auxiliary variables. As demonstrated in Appendix C, the effect of malpractice is statistically identical (although imprecisely estimated) in a sparse model that uses only the non-economic damages instrument, along with the year and geography fixed-effects. Precise estimates are obtained simply by adding controls for age, but no other covariates. This is sensible: without conditioning on age, there is a great deal of unexplained variation in medical care spending that adds noise to our estimates.

The modest size of these effects is robust. In the appendix, we present tables that experiment with different moving average windows for the malpractice measures, and include measures of HMO penetration, which is often thought of as a confounding factor in malpractice analyses (Danzon, 1991, 2000). The magnitudes remain small in these alternate specifications as well.

At most, the population-weighted elasticity on intensity is around 0.09, while the elasticity on quantity is -0.07. This would imply a total-cost elasticity of 0.02. Even though total malpractice payments per capita grew by 65% (in real terms) over this period of time,[19] this would translate into just 3.25% real growth in medical costs over this 13-year period, a tiny fraction of the greater than 50% real growth that actually took place. While doctors do seem to respond to malpractice, therefore, it is not a significant source of cost growth, or of major changes in behavior.

[19] Technically, it would be more accurate to compare our numbers to the real growth in per capita malpractice payments. (Chandra, Nundy, and Seabury, 2005) show this was about 41%, similar though slightly less than the overall growth in payments.

D.2 Effects of Malpractice on Medicare Costs

Table studies the relationship between malpractice risk and Medicare costs. We study both overall Medicare costs per enrollee, as well as geographically deflated Medicare costs. The former illustrates the growth in costs experienced by the average Medicare enrollee. The latter quantifies the growth that would have been experienced by the average enrollee, if she had faced average nationwide Medicare prices. There is relatively little difference between these two analyses, suggesting that growth is similar in high-cost and low-cost areas.

The IV estimates suggest that malpractice risk raises Medicare Part A (inpatient) expenditures per enrollee, but has a much smaller impact on Part B spending. The elasticity for Part A spending is around 0.08. The elasticity for Part B is around 0.02. As before, the OLS estimates are largely insignificant. This is consistent with measurement error in the OLS, or with the possibility that spending reduces malpractice risk. As with the earlier estimates, the modest size of these effects is robust to changes in the moving average window for the malpractice measures, as well as the inclusion of HMO penetration.

These results are qualitatively consistent with our findings for hospital costs. Recall that inpatient days per bed fell relatively little, but that outpatient days per bed fell by more. This would tend to mute the effect of malpractice on per capita outpatient spending, but not on inpatient spending. When evaluating the impact of malpractice risk on total spending, as we do in this exercise, we must add the elasticities with respect to price and quantity. The offsetting effects on price and quantity lead to small elasticities on outpatient spending, but the lack of an inpatient quantity effect permits a larger effect on inpatient spending. Finally, as before, identification is achieved primarily by our instrument, as shown in Appendix C.

D.3 Malpractice Growth and Medical Care Costs

We have identified positive relationships between malpractice risk and medical spending, particularly spending per bed-day. However, from a larger perspective, the effect of malpractice risk on medical costs is rather small, and significantly smaller than previously suggested. Figure 2 illustrates this graphically, by comparing the actual growth in hospital expenditures from 1991-2002, to the growth that would be predicted to have followed from the 65% increase in malpractice costs that took place during the same period. We calculate the predicted growth using four different elasticities relating malpractice payments to hospital expenditures: two from Kessler and McClellan (2002), our preferred estimate, and our upper bound estimate.

The most directly comparable elasticity from Kessler and McClellan (2002) is that of hospital expenditures with respect to expected claims payments made by physicians to patients. Kessler and McClellan estimate that this elasticity is equal to 0.39 for Acute Myocardial Infarction (AMI) patients, and 0.27 for patients with Ischemic Heart Disease (IHD). Our preferred estimate is the largest estimated effect we find of malpractice on hospital expenditures — 0.087 (the $t-2$ through $t-4$ moving average from Table). The upper bound estimate is the upper bound of the 95% confidence interval of this estimate — 0.126.

The Kessler and McClellan numbers would imply that 52%-75% of the 34% growth in medical expenditures from 1991-2002 is due to the increase in malpractice risk. Our numbers, in contrast, suggest much more modest effects. Our estimates suggest that between 17% and 24% of the growth is due to malpractice. In absolute terms, our preferred estimate implies that the substantial growth in malpractice from 1991 to 2002 added just 5% to medical expenditures. Given the extraordinary growth in malpractice premia over this period of time, this is a relatively small effect. Moreover, even our preferred estimate likely overstates the effect, since we have chosen to use the largest coefficient in our study for this calculation.

Unfortunately, it is not possible to compare our findings directly to Kessler and McClellan (1996), which found that direct malpractice reforms reduce hospital expenditures by 5 to 9 percent for heart attack patients. If in fact state-level tort reforms fail to be exogenous, it is not clear how to recover this parameter accurately. However, our results suggest that eliminating malpractice costs *entirely* would lower hospital spending by at most 8 percent. It thus seems likely that the effects of tort reform on the average patient's spending are smaller than 5 to 9 percent.

It is worth re-emphasizing that Kessler and McClellan do not explicitly claim that their results are quantitatively the same as the effect on overall expenditures. They merely suggest that they may serve as a guideline for the overall effects.[20] However, in the absence of alternatives, several policymakers have relied upon their findings as a predictor of how malpractice and tort reform affect total health care expenditures.

E. Conclusions

Malpractice payments have grown enormously over the past 15 years, but we have presented evidence that this has likely had a limited impact on the cost of health care in the US. To be sure, it may have had or continue to have other effects, such as decreasing the number of patients treated, and increasing the intensity of treatment on patients who do arrive. Our

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[20] Consider, for example, their suggestion that "Our results provide an empirical foundation for simulating the effects of untried malpractice reforms on health care expenditures and outcomes, based on their predicted effects on the malpractice pressure facing medical providers" (Kessler and McClellan, 2002, p. 931).

findings, however, suggest that limiting malpractice liability is no panacea for rising health care costs.

Putting our results together with earlier work suggests that malpractice may have substantial impacts on the care and costs of specific patient subgroups — like heart attack patients — but may have much more modest impacts on the average patient, and on health care spending as a whole.

An important question for future work is whether these limited impacts on costs are accompanied by large, small, or zero impacts on patient outcomes. If, for example, malpractice risk has had limited impacts on costs but appreciable positive impacts on average outcomes, the malpractice "crisis" may be anything but. If, on the other hand, it has negative impacts on outcomes, the real costs of malpractice may be on health rather than in dollars.

Appendix

A. HMO Penetration

Danzon has argued that HMO penetration can serve as a third factor that creates a spurious link between malpractice risk and medical costs. She argues that HMO's work to reduce both medical and malpractice costs. To assess the impact of this effect on our estimates, we included measures of HMO penetration in our models.

The HMO data on number of enrollees in a county come from two sources. The 1990-1994 data come from publications of the Group Health Association of America, whereas the 1995-2003 data come from Interstudy. With both data sources, penetration is defined as the number of enrollees per people in the county. See Baker (2000) for an example of these data used in past work.

Table 9 presents the results when HMO penetration estimates are included. Inclusion of the HMO data has few impacts on our estimates, which remain quantitatively stable and similar to those presented in the text. If anything, the impact of malpractice appears stronger when the data on HMO penetration is included.

B. Alternate Lag Structures

We argued that providers respond to expected malpractice risk, not actual risk. This required some way of estimating expected risk. In the text, we used a three-year moving average as a measure of expected risk. We demonstrated that expected malpractice costs could be proxied for by a variety of lag lengths and structures. Therefore, it is important to show that our results are robust to different lag structures. Table presents the estimates that result when the length of this moving average window is varied. The table demonstrates that we continue to get modest and insignificant effects at longer or shorter lag lengths.

C. Sources of Identification

In the paper, we focused on the validity of our IV strategy. It is important to note that the identification in our models was in fact driven primarily by the instrument itself, and not by other auxiliary variables or specification choices. Table makes this point by comparing the full model to models run with: the instrument and fixed-effects; and, the instrument, fixed-effects, and age dummies. Coefficients from all three are statistically indistinguishable, although the estimates with the instrument and fixed-effects alone are much less precise. Adding the age dummies provides precision. The auxiliary covariates do not greatly affect the instrumented estimate.

D. Alternative Identification Strategy: Tort Reform

Because the identification strategy we use in the paper is novel, it is natural to question whether our results represent the true effect, or simply an artifice of the approach we use. In particular, despite the evidence from the NPDB, we might be concerned that the small effects we find are due to jury verdicts being a bad predictor of malpractice costs. The dominant approach prior to ours has been to use tort reform as a predictor of malpractice risk, and to examine the impact on costs or other outcomes. To test this, we duplicate our analysis using tort reforms as a predictor of malpractice risk.

Various kinds of tort reforms were coded into the state-year level using the National Conference of State Legislature's "State Medical Liability Laws Table." Because the vast majority of malpractice reforms were adopted in the 1980s (particularly around 1986), we restrict our sample to 1984-1994. From our standpoint this is a conservative approach, because adding in additional years with less variation in laws “waters down” the effect of tort reform and drives it towards zero. Data on hospital expenditures, days per bed, Part A and Part B medical expenditures, and other demographics were aggregated to the state-year level, and the health care spending variables were regressed against tort reform, demographics and state and year fixed effects. Standard errors in the regressions were adjusted to allow for clustering by state.

Table 11 presents results on the estimated impact of “direct” and “indirect” tort reform, the classifications used by Kessler and McClellan (1996, 2002) and Kessler, Sage and Becker (2005). The first column represents the effect of the presence of tort reform in the current year on medical expenditures. The next three columns represent the effect of the presence of tort reform lagged 1, 2, and 3 years, respectively, on expenditures in the current year, allowing for the possibility that it takes reform some time to have an impact on expected malpractice costs. The final three columns test for impact of the presence of tort reform leading 1, 2, and 3 years, respectively. As with the modified Granger test in the paper, the inclusion of the leading tort reform variables (for which the expected coefficient is zero if tort reform is exogenous) tests for the possibility that the adoption of reform was endogenous to trends in medical expenditures.

The results reported in Table 12 suggest that tort reform has a negative impact on medical expenditures. In other words, tort reform lessens medical malpractice costs, which leads to a decline in medical expenditures. Similarly, the presence of tort reform is associated with increased utilization (in terms of hospital days per bed). In general, and consistent with our findings, the coefficients on these effects are extremely small. Consider the impact of tort reform on hospital expenditures. The combined effect of direct and indirect reforms in the current year is to reduce hospital expenditures by 3.5%. Kessler and McClellan (2002) suggest that the combined effect of these reforms is to reduce the frequency of malpractice claims by about 34%. Using this, we impute from the coefficient estimates that the elasticity of hospital expenditures with respect to malpractice cost is about 0.10, very close to our own estimates.

We draw similar conclusions if we consider the estimated coefficients on the other variables. If we consider the combined effect of tort reform on utilization we find somewhat larger estimates than in the paper, but in most cases the effect of the indirect reform is insignificant. The effect of malpractice on Part A expenditures is also larger than the effect on Part B expenditures, but again the coefficients are small and often not significant. Interestingly, we find little evidence that the adoption of tort reform was endogenous, with virtually none of the coefficients on the leading variables being significant.

Overall, these findings indicate that malpractice has a small effect on overall medical expenditures when tort reform is used as the predictor of changes in malpractice cost. This suggests that the effect of malpractice on the average patient really is less than the effect on patients with high risk conditions, and our findings are not due to measurement error or model misspecification.

E. Alternative Identification Strategy: Noneconomic Damages in Malpractice Cases Only

The instrument used throughout the paper is the average dollar amount of noneconomic awards granted by jury verdicts in all tort cases in a county in a year. Using all tort cases is appropriate if we think that they capture something about the average generosity of juries, either perceived or imaginary, for a case selected at random. For example, suppose large awards in non-malpractice cases lead to publicity that raises physicians' perceived malpractice risk, then it might be best to include noneconomic awards in all cases to construct the instrument. On the other hand, if jury generosity is systematically different across different types of cases, and non-malpractice cases had no impact on physician perceptions of malpractice costs, including these cases would only dilute the power of the instrument.

Table 13 presents results using noneconomic awards in malpractice cases only as the instrument for total malpractice costs. In general the results are consistent with previous findings. The impact of malpractice on hospital costs is positive, and mostly negative on the number of days per hospital bed. The impact on county Medicare costs is positive and stronger for Part A than Part B. However, the estimates are much weaker, with elasticities of no more than 0.03 for costs and -0.042 for hospital days. Moreover, the impact on Medicare expenditures is not significantly different from zero in any specification for Part A or Part B.

From these results alone, it is difficult to say whether the preferred instrument is to use noneconomic awards in malpractice cases only or in all tort cases. Ultimately, it appears to make little difference. Given our central conclusions that the impact of malpractice is relatively small, using the noneconomic awards in all tort cases appears to be the conservative approach. It is thus the one we adopt in the body of the paper.

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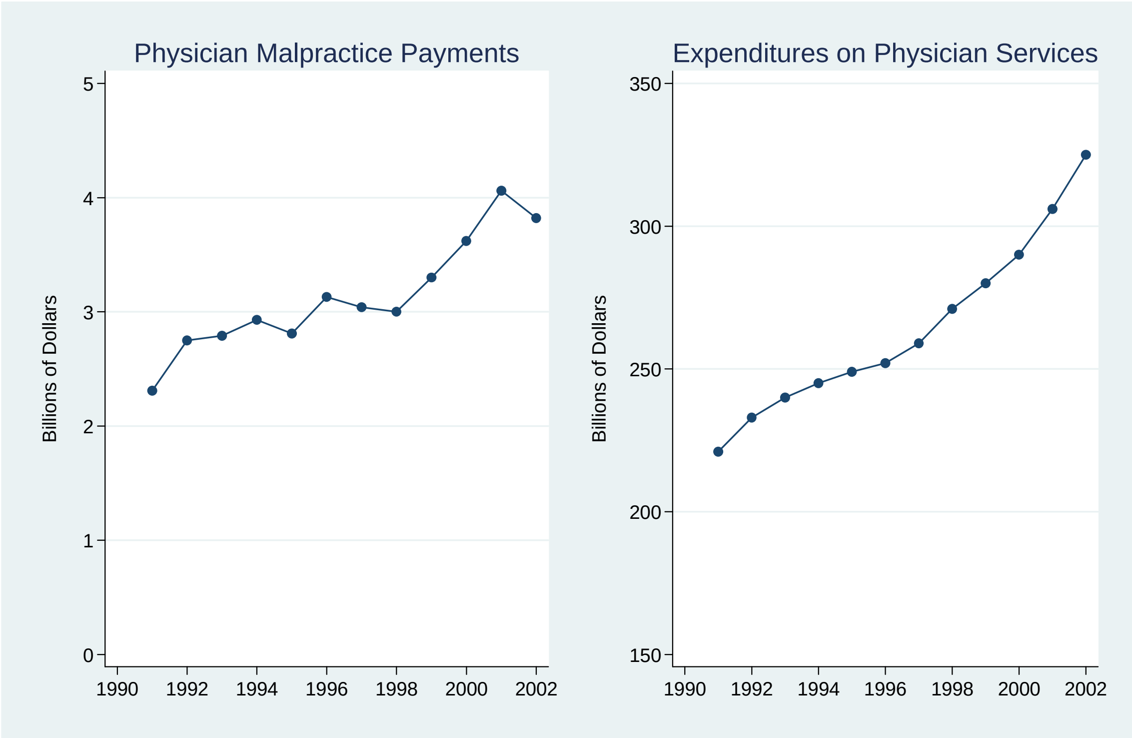
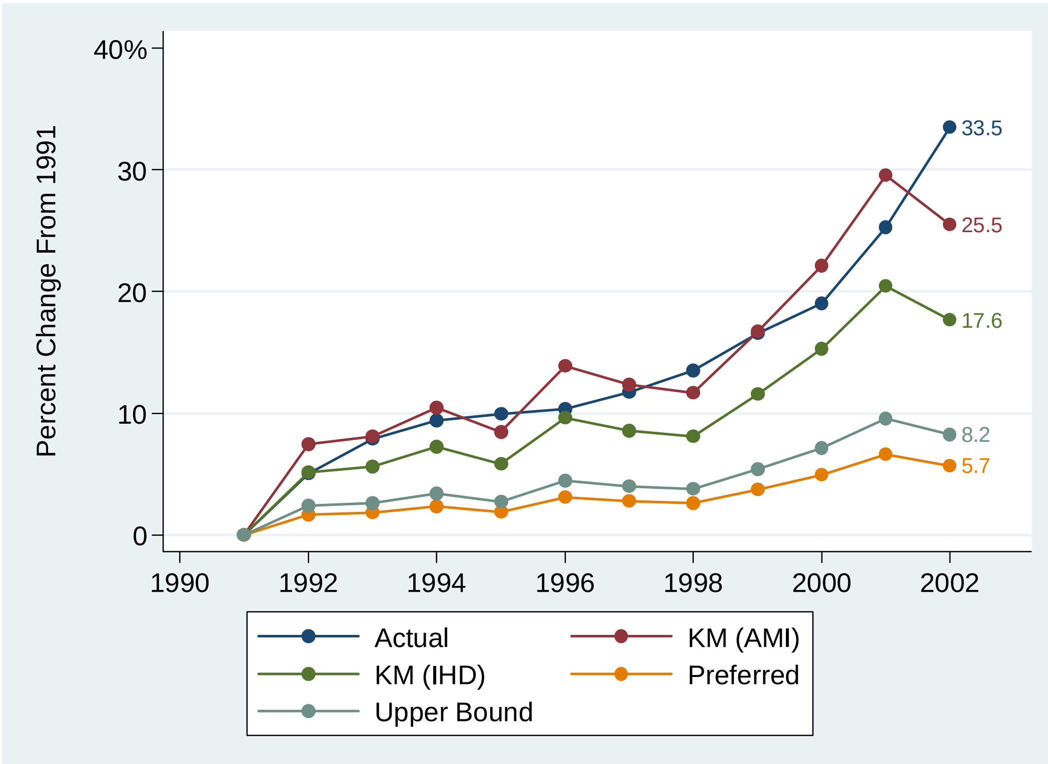
Figure 1: Physicians' Medical Malpractice Payments and Expenditures on Physician Services.



Notes: Data on physicians' medical malpractice payments consist of payments for settlements and judgments, as reported in the National Practitioners' Databank (NPDB). Expenditure data on physician services is based on the "Physician and Clinical Services" expenditures series in National Health Expenditures data (Bureau of the Census, 2007).

Figure 2: The effect of rising malpractice payments on hospital expenditures, alternative estimates.



Notes: The trend in actual hospital expenditures is based on the "Hospital Care" series in National Health Expenditures data (Bureau of the Census, 2007). The other series apply different hospital expenditure elasticities to the actual trend in total physician malpractice payments, shown in Figure 1: "KM (AMI)" uses Kessler and McClellan's 0.39 elasticity for heart attack patients; "KM (IHD)" uses their 0.27 elasticity for patients with ischemic heart disease; "Preferred" uses our largest estimate from Table , of 0.087; and "Upper Bound" uses the top of the 95% confidence interval associated with this estimate, or 0.126.

Table 1: Unweighted and population-weighted means for malpractice variables.

	Single Year		3-Year Moving Average	
	Unweighted	*Weighted*	*Unweighted*	*Weighted*
Total Malpractice Awards (thousands)	2,925	16,539	2,748	16,280
	(12,665)	(26,443)	(10,011)	(21,113)
Malpractice Awards Per Capita	2.82	6.05	2.00	5.67
	(17.80)	(14.36)	(7.50)	(8.30)
Average Noneconomic Award (thousands)	140	307	104	286
	(528)	(590)	(298)	(423)
Total Awards in All Tort Cases (thousands)	9,143	88,280	7,177	55,031
	(124290)	(521751)	(50,454)	(195993)
N	1,800	1,800	1,560	1,560

Notes: The table presents means (standard deviations in parentheses) of the average total jury awards in medical malpractice cases, average total malpractice awards per capita, average award for noneconomic damages in all tort cases with a plaintiff victory (defined as a nonzero damage award), and the total amount of awards in all tort cases. The unit of analysis is a county-year. Data come from the RAND JVDB, and include all counties in New York and California, as well as Cook County, IL (Chicago), King County, WA (Seattle), Harris County, TX (Houston) and all counties in the St. Louis, MO metropolitan area. The columns reporting lagged data represent the average of three years of lags. Data are available in the JVDB for 120 counties covering 15 years (1985-1999), but 2 years of data are lost to compute the 3-year moving average.

Table 2: Unweighted and population-weighted means of medical expenditures, utilization, and county characteristics.

	Counties in Sample		All Counties	
	Unweighted	Weighted	Unweighted	Weighted
	Hospital Level Expenditures			
Hospital Facility Expenditures Per Bed Day	436	530	363	508
	(298)	(320)	(1,437)	(2,143)
Total Days Per Hospital Bed	699	564	692	629
	(752)	(526)	(883)	(779)
Inpatient Days Per Hospital Bed	241	229	219	235
	(71)	(69)	(76)	(70)
N	23,288		161,784	
	County Medicare Expenditures			
Part A Expenditures Per Enrollee	1,974	2,283	1,895	2,081
	(840)	(985)	(881)	(903)
Part A Expenditures Per Enrollee (Deflated)	1,943	2,117	2,068	2,097
	(776)	(786)	(1,076)	(908)
Part B Expenditures Per Enrollee	1,219	1,345	1,154	1,265
	(553)	(557)	(716)	(658)
Part B Expenditures Per Enrollee (Deflated)	1,201	1,276	1,213	1,268
	(532)	(481)	(783)	(662)
	County Demographics			
Per Capita Income	19,129	23,316	15,579	20,031
	(8,494)	(10,103)	(6,296)	(8,867)
Fraction Male	0.50	0.49	0.49	0.49
	(0.02)	(0.01)	(0.02)	(0.01)
Fraction White	0.89	0.79	0.89	0.83
	(0.10)	(0.11)	(0.15)	(0.14)
Fraction African-American	0.06	0.13	0.09	0.12
	(0.08)	(0.11)	(0.14)	(0.13)
N	2,640		70,668	

Notes: The table presents means (standard deviations in parentheses) of the average cost of medical care and other demographic characteristics. The unit of analysis for the hospital data is a hospital-year, and for the county-level data it is a county-year. The counties "in sample" include all counties in New York and California, as well as Cook County, IL (Chicago), King County, WA (Seattle), Harris County, TX (Houston) and all counties in the St. Louis, MO metropolitan area. The "all counties" data include all counties in the U.S. for which data are available. All variables cover the time period from 1985 to 2003. All dollar amounts are reported in thousands of year 2000 dollars, adjusted by the Consumer Price Index (series CUUR0000SA0).

Table 3: Past malpractice verdicts as a measure of expected malpractice costs

	Dependent Variable: Current Total Malpractice Payments at year t				
	(1)	(2)	(3)	(4)	(5)
Individual Lagged Malpractice Verdicts					
Coefficients					
Year t-1	3.143***	5.261***			
	(1.203)	(1.090)			
Year t-2	3.752***	5.216***	6.132***		
	(1.055)	(1.003)	(1.147)		
Year t-3	3.261***	5.145***	5.398***	6.167***	
	(1.235)	(1.179)	(1.150)	(1.236)	
Year t-4	1.277		3.764***	3.571***	4.201***
	(1.153)		(1.176)	(1.294)	(1.324)
Year t-5	2.102			5.746***	5.063***
	(1.546)			(1.646)	(1.611)
Year t-6	3.934***				7.485***
	(1.395)				(1.562)
Testing for equality of coefficients					
F-statistic	0.7482	0.0019	0.8278	0.7917	1.0682
p-value	0.5877	0.9981	0.4375	0.4536	0.3444
Regression statistics					
R^2	0.7436	0.7229	0.6998	0.6726	0.6605
N	508	661	610	559	508
Moving Average of Lagged Verdicts					
Coefficients					
Average of Lagged	17.300***	15.623***	15.354***	15.391***	16.343***
Trial Verdicts	(1.288)	(1.081)	(1.164)	(1.310)	(1.433)
Regression statistics					
R^2	0.7375	0.7229	0.6957	0.6682	0.6550
N	508	661	610	559	508

Notes: The table illustrates the predicted relationship from regressions of total malpractice payments (from verdicts at trial and settlements) in the current year as the dependent variable against lagged values of payments from trial verdicts as the independent variable. Each column represents a separate regression, including the indicated lags. The coefficients for the moving averages also come from separate regressions, with each moving average defined as the average of the lags included in the top part of the table in the same column. Data come from the National Practitioner Data Bank (NPDB) from years 1990-2005, aggregated to the state-year level. Robust standard errors are in parentheses. A *** indicates statistical significance at the 1% level.

Table 4: First-Stage regression results.

	Instrument: Average Noneconomic Damages in All Tort Cases (Tens of Thousands)				
	Current Year	Lagged: 1, 2 and 3 Years	Lagged: 2, 3 and 4 Years	Lagged: 3, 4 and 5 Years	Lagged: 4, 5 and 6 Years
Hospital Level					
Malpractice Award Dollars Per Capita	0.562**	0.600***	0.668***	0.623***	0.509***
	(0.215)	(0.149)	(0.169)	(0.161)	(0.137)
Wald Statistic	6.811***	16.135***	15.666***	14.959***	13.754***
R^2	0.601	0.812	0.788	0.766	0.781
N	15,237	12,835	12,632	12,445	12,264
County Level					
Malpractice Award Dollars Per Capita	1.156**	0.665***	0.642***	0.582***	0.516***
	(0.557)	(0.141)	(0.125)	(0.122)	(0.112)
Wald Statistic	4.315**	22.336***	26.289***	22.837***	21.239***
R^2	0.508	0.789	0.776	0.748	0.754
N	1,785	1,547	1,547	1,547	1,547

Notes: The table reports the estimated effect of average noneconomic damage awards in all tort cases with a nonzero award to the plaintiff on total malpractice awards per capita. Each coefficient is from a separate regression, with each column representing a different lag for the dependent variable *and* the instrument. The unit of analysis for the top panel is a hospital-year, while the unit of analysis for the bottom panel is a county-year. The time periods for each regression are restricted by data availability; regressions for the current year period cover 1985-1999, the regression for lags 1 through 3 cover 1988-2000, the regressions for lags 2 through 4 cover 1989-2001, the regressions for lags 3 through 5 cover 1990 to 2002, and the regressions for lags 4 through 6 cover 1991 through 2003. County population is used as a weight in all regressions. Other explanatory variables include county and year fixed-effects, as well as county personal income per capita, the percent of the population that is male, white, African American, and that falls into 5-year age ranges. Robust standard errors allowing clustering at the county level are reported in parentheses. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.

Table 5: Health costs and non-economic damage awards: causality tests of the instrument.

	Lead: 4, 5 and 6 Years	Lead: 3, 4 and 5 Years	Lead: 2, 3 and 4 Years	Lead: 1, 2 and 3 Years	Current Year	Lagged: 1, 2 and 3 Years	Lagged: 2, 3 and 4 Years	Lagged: 3, 4 and 5 Years	Lagged: 4, 5 and 6 Years
Hospital Level Estimates									
Dependent Variable: Hospital Facility Expenditures Per Bed Day									
conomic Award	-0.275	0.666	1.462**	0.484	0.063	1.173	5.260***	4.684***	2.402*
eds of Thousands)	(1.125)	(0.790)	(0.640)	(0.629)	(0.413)	(1.031)	(1.343)	(1.240)	(1.366)
Dependent Variable: Total Hospital Days Per Bed									
conomic Award	0.108	0.243	0.998	0.006	-0.298	0.348	-3.976	-4.078	-4.200*
eds of Thousands)	(1.351)	(1.564)	(1.472)	(1.267)	(0.535)	(1.717)	(3.216)	(2.569)	(2.362)
Dependent Variable: Inpatient Hospital Days Per Bed									
conomic Award	0.187	0.012	-0.285	0.060	-0.044	-0.098	-0.430	-0.470	-0.623**
eds of Thousands)	(0.271)	(0.257)	(0.218)	(0.325)	(0.066)	(0.199)	(0.267)	(0.311)	(0.312)
County Level Estimates									
Dependent Variable: Medicare Part A Expenditures Per Enrollee									
conomic Award	2.740	0.667	4.531	5.916	1.567	21.989**	31.932***	24.780**	24.569*
eds of Thousands)	(4.563)	(4.600)	(5.071)	(7.043)	(1.978)	(8.470)	(11.143)	(10.477)	(12.589)
Dependent Variable: Medicare Part B Expenditures Per Enrollee									
conomic Award	1.060	1.513	2.442	3.078	1.490	10.317**	7.799*	5.131	5.232
eds of Thousands)	(1.901)	(2.440)	(2.496)	(2.852)	(1.137)	(3.998)	(4.424)	(4.689)	(5.168)

able shows the reduced-form estimates of average noneconomic damage awards on hospital and Medicare expenditures. Each coefficient is from a separate on, with each column representing a different lag or lead for the noneconomic damages. The unit of analysis for the top panel is a hospital-year, while for om panel it is a county-year. County population is used as a weight in all regressions. Other explanatory variables include hospital or county fixed-effects, ed-effects, as well as a quadratic for personal income per capita, the percent of the population that is male, white, African American, of Hispanic ethnicity, falls into 5-year age ranges. Robust standard errors allowing clustering at the county level are reported in parentheses. A *, **, or *** ts statistical significance at the 10, 5, or 1% level, respectively.

Table 6. The Impact of Claimed Economic Losses on Jury Awards in Medical Malpractice Cases

	(1) Jury Award: Economic	(2) Jury Award: Economic	(3) Jury Award: Non-Economic	(4) Jury Award: Non-Economic
Claimed Economic Losses: Medical	0.337**	0.380***	0.0002	0.005
	(0.147)	(0.141)	(0.048)	(0.050)
Claimed Economic Losses: Non-medical	0.216		0.025	
	(0.208)		(0.066)	
R-squared	0.29	0.29	0.09	0.09

Notes: Table presents the coefficients from OLS regression of different components of the compensatory jury award (economic and non-economic) against claimed economic losses (medical and non-medical). The unit of observation is a verdict of a malpractice case with a plaintiff "win" (i.e., a nonzero dollar amount awarded to the plaintiff). Each regression has 2,328 observations. Regressions include county-, year-, and injury type fixed-effects. Standard errors clustered by county. A ** or *** represents statistical significance at the 5% or 1% level, respectively.

Table 7: The Impact of malpractice on hospital costs.

	Current Year	Lagged: 1, 2 and 3 Years	Lagged: 2, 3 and 4 Years	Lagged: 3, 4 and 5 Years	Lagged: 4, 5 and 6 Years
OLS Estimates					
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards Per Capita	-0.278*	0.122	-0.033	0.281	-0.188
	(0.150)	(0.604)	(0.550)	(0.361)	(0.624)
Elasticity	-0.0028	0.0012	-0.0003	0.0026	-0.0017
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards Per Capita	0.055	-0.743	-1.141	-1.297*	-0.968
	(0.284)	(1.135)	(0.910)	(0.757)	(0.994)
Elasticity	0.0006	-0.0072	-0.0108	-0.0120	-0.0087
Dependent Variable: Inpatient Hospital Days Per Bed					
Malpractice Awards Per Capita	-0.037*	-0.156*	-0.009	-0.023	0.138
	(0.020)	(0.081)	(0.095)	(0.083)	(0.103)
Elasticity	-0.0009	-0.0040	-0.0002	-0.0006	0.0035
IV Estimates					
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards Per Capita	0.644	-0.215	8.911***	7.525***	4.744*
	(1.289)	(2.960)	(2.045)	(1.432)	(2.751)
Elasticity	0.0067	-0.0022	0.0870	0.0703	0.0432
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards Per Capita	-1.109	4.143	-7.626	-6.555*	-8.294**
	(2.007)	(5.260)	(5.384)	(3.552)	(4.122)
Elasticity	-0.0118	0.0419	-0.0743	-0.0606	-0.0741
Dependent Variable: Inpatient Hospital Days Per Bed					
Malpractice Awards Per Capita	-0.022	0.608	-0.440	-0.755*	-1.229**
	(0.146)	(0.668)	(0.458)	(0.426)	(0.551)
Elasticity	-0.0006	0.0167	-0.0118	-0.0194	-0.0313

Notes: The table reports the estimated effect of per capita malpractice jury award dollars on medical expenditures. In the IV models, malpractice awards are instrumented by the average noneconomic awards in all tort cases with a plaintiff win. Each coefficient is from a separate regression, and each column represents a different lag for the malpractice variable. The unit of analysis is a hospital-year. County population is used as a weight in all regressions. Other explanatory variables include hospital and year fixed-effects, a quadratic for per capita income, the percent of the population that is male, white, African-American, and that falls into 5-year age ranges. Elasticities are evaluated at the mean values of the dependent and independent variables. Robust standard errors allowing clustering at the county level are reported in parentheses. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.

Table 8: The Impact of malpractice on county Medicare costs.

	Current Year	Lagged: 1, 2 and 3 Years	Lagged: 2, 3 and 4 Years	Lagged: 3, 4 and 5 Years	Lagged: 4, 5 and 6 Years
OLS Estimates					
Dependent Variable: Medicare Part A Expenditures Per Enrollee					
Malpractice Awards	0.270	6.530**	8.291**	5.844*	3.868
Per Capita	(0.852)	(3.275)	(3.250)	(3.320)	(4.310)
Elasticity	0.0007	0.0145	0.0178	0.0122	0.0077
Dependent Variable: Medicare Part A Expenditures Per Enrollee (Deflated)					
Malpractice Awards	0.060	3.500	5.928**	4.444*	2.775
Per Capita	(0.889)	(2.352)	(2.482)	(2.410)	(3.472)
Elasticity	0.0002	0.0085	0.0141	0.0103	0.0061
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards	0.830*	3.351**	1.180	-0.923	-1.400
Per Capita	(0.474)	(1.608)	(1.835)	(2.086)	(2.155)
Elasticity	0.0035	0.0124	0.0042	-0.0032	-0.0046
Dependent Variable: Medicare Part B Expenditures Per Enrollee (Deflated)					
Malpractice Awards	0.717*	1.738	0.297	-1.011	-1.904
Per Capita	(0.430)	(1.266)	(1.450)	(1.543)	(1.812)
Elasticity	0.0032	0.0068	0.0011	-0.0038	-0.0067
IV Estimates					
Dependent Variable: Medicare Part A Expenditures Per Enrollee					
Malpractice Awards	1.355	33.063**	49.765***	42.579***	47.640*
Per Capita	(1.924)	(13.977)	(14.562)	(15.782)	(24.593)
Elasticity	0.0035	0.0732	0.1069	0.0888	0.0954
Dependent Variable: Medicare Part A Expenditures Per Enrollee (Deflated)					
Malpractice Awards	0.542	22.787*	36.739***	33.562***	27.979**
Per Capita	(1.324)	(11.663)	(9.440)	(11.633)	(12.465)
Elasticity	0.0015	0.0552	0.0871	0.0779	0.0616
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards	1.289	15.514**	12.154*	8.816	10.145
Per Capita	(1.246)	(6.793)	(7.296)	(8.246)	(10.408)
Elasticity	0.0055	0.0573	0.0433	0.0305	0.0335
Dependent Variable: Medicare Part B Expenditures Per Enrollee (Deflated)					
Malpractice Awards	0.989	11.924**	9.363	7.890	9.758
Per Capita	(0.998)	(5.932)	(6.080)	(6.498)	(7.163)
Elasticity	0.0045	0.0469	0.0356	0.0293	0.0343

Notes: The table reports the estimated effect of per capita malpractice jury award dollars on medical expenditures. In the IV models, malpractice awards are instrumented by the average noneconomic awards in all tort cases with a plaintiff win. Each coefficient is from a separate regression, and each column represents a different lag for the malpractice variable. The unit of analysis is a county-year. County population is used as a weight in all regressions. Other explanatory variables include county and year fixed-effects, a quadratic for per capita income, the percent of the population that is male, white, African-American, and that falls into 5-year age ranges. Elasticities are evaluated at the mean values of the dependent and independent variables. Robust standard errors allowing clustering at the county level are reported in parentheses. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.

Table 9: HMO Penetration and the effects of malpractice.

	Current Year	Lagged: 1, 2 and 3 Years	Lagged: 2, 3 and 4 Years	Lagged: 3, 4 and 5 Years	Lagged: 4, 5 and 6 Years
IV Estimates					
Hospital Level Estimates					
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards Per Capita	0.806	-0.527	8.688***	7.797***	5.219*
	(1.364)	(2.867)	(1.848)	(1.567)	(2.759)
Elasticity	0.0084	-0.0053	0.0848	0.0729	0.0475
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards Per Capita	-1.328	4.567	-7.259	-6.963**	-9.311**
	(2.053)	(5.120)	(5.014)	(3.346)	(3.570)
Elasticity	-0.0142	0.0462	-0.0707	-0.0643	-0.0832
Dependent Variable: Inpatient Hospital Days Per Bed					
Malpractice Awards Per Capita	-0.048	0.658	-0.401	-0.810**	-1.398***
	(0.130)	(0.637)	(0.408)	(0.363)	(0.476)
Elasticity	-0.0014	0.0181	-0.0107	-0.0208	-0.0356
County Level Estimates					
Dependent Variable: Medicare Part A Expenditures Per Enrollee					
Malpractice Awards Per Capita	1.880	23.458**	44.905***	43.946***	49.876**
	(1.754)	(10.830)	(13.470)	(13.611)	(23.069)
Elasticity	0.0048	0.0517	0.0971	0.0916	0.0999
Dependent Variable: Hospital Payroll Expenditures Per Bed Day(Deflated)					
Malpractice Awards Per Capita	0.668	8.821	31.044***	34.655***	29.853***
	(1.060)	(10.370)	(8.676)	(10.171)	(11.006)
Elasticity	0.0019	0.0215	0.0746	0.0804	0.0657
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards Per Capita	0.898	10.921*	8.074	9.364	11.418
	(1.046)	(6.278)	(6.733)	(7.566)	(9.625)
Elasticity	0.0038	0.0401	0.0289	0.0324	0.0377
Dependent Variable: Medicare Part B Expenditures Per Enrollee (Deflated)					
Malpractice Awards Per Capita	0.588	6.836	5.569	8.404	10.830*
	(0.793)	(5.586)	(5.695)	(5.829)	(6.455)
Elasticity	0.0027	0.0269	0.0214	0.0312	0.0381

Notes: The table reports the estimated IV effects of per capita malpractice jury award dollars on medical expenditures. Each coefficient is from a separate regression, and each column represents a different lag for the malpractice variable. The unit of analysis for the top panel is a hospital-year, and for the bottom panel it is a county year. County population is used as a weight in all regressions. Other explanatory variables include hospital or county fixed-effects, year fixed-effects, a quadratic for per capita income, the percent of the population that is male, white, African-American, and that falls into 5-year age ranges. Elasticities are evaluated at the mean values of the dependent and independent variables. Robust standard errors allowing clustering at the county level are reported in parentheses. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.

Table 10: Varying the construction of expected malpractice risk.

	Current Year	Lagged: 1 Year	Lagged: 1 and 2	Lagged: 1, 2 and 3 Years	Lagged: 1, 2, 3 and 4 Years	Lagged: 1, 2, 3, 4 and 5 Years	Lagged: 1, 2, 3, 4, 5 and 6 Years
IV Estimates							
Hospital Level							
Dependent Variable: Hospital Facility Expenditures Per Bed Day							
Malpractice Awards Per Capita	0.644	-3.318	-1.924	-0.215	2.194	2.292	0.224
	(1.289)	(2.626)	(1.422)	(2.960)	(1.978)	(3.488)	(5.214)
Elasticity	0.0067	-0.0350	-0.0200	-0.0022	0.0214	0.0218	0.0021
Dependent Variable: Total Hospital Days Per Bed							
Malpractice Awards Per Capita	-1.109	5.330	4.608*	4.143	3.003	3.519	1.264
	(2.007)	(3.528)	(2.604)	(5.260)	(4.802)	(5.354)	(8.399)
Elasticity	-0.0118	0.0568	0.0482	0.0419	0.0296	0.0338	0.0119
Dependent Variable: Inpatient Hospital Days Per Bed							
Malpractice Awards Per Capita	-0.022	0.228	0.182	0.608	0.245	-0.501	-1.996***
	(0.146)	(0.171)	(0.224)	(0.668)	(0.554)	(0.511)	(0.626)
Elasticity	-0.0006	0.0066	0.0052	0.0167	0.0066	-0.0130	-0.0519
County Level							
Dependent Variable: Medicare Part A Expenditures Per Enrollee							
Malpractice Awards Per Capita	1.355	4.379	10.839	33.063**	42.519***	36.163**	62.274*
	(1.924)	(3.898)	(8.709)	(13.977)	(13.844)	(17.702)	(32.269)
Elasticity	0.0035	0.0108	0.0254	0.0732	0.0912	0.0758	0.1261
Dependent Variable: Medicare Part B Expenditures Per Enrollee							
Malpractice Awards Per Capita	1.289	1.984	4.834	15.514**	14.859**	12.483	19.468
	(1.246)	(1.584)	(3.353)	(6.793)	(7.340)	(8.721)	(14.872)
Elasticity	0.0055	0.0081	0.0189	0.0573	0.0531	0.0436	0.0658

Notes: The table reports the estimated effect of per capita malpractice jury award dollars on medical expenditures. Malpractice awards are instrumented by the average noneconomic awards in all tort cases with a plaintiff win. Each coefficient is from a separate regression, and each column represents a different lag for the malpractice variable. The unit of analysis for the top panel is a hospital-year, while for the bottom panel it is a county-year. County population is used as a weight in all regressions. Other explanatory variables include hospital or county fixed-effects, year fixed-effects, a quadratic for per capita income, the percent of the population that is male, white, African-American, and that falls into 5-year age ranges. Elasticities are evaluated at the mean values of the dependent and independent variables. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively. Robust standard errors allowing clustering at the county level are reported in parentheses.

Table 11: Wald estimates and the source of identifying variation.

	Current Year	Lagged: 1, 2 and 3 Years	Lagged: 2, 3 and 4 Years	Lagged: 3, 4 and 5 Years	Lagged: 4, 5 and 6 Years
			Total Hospital Expenditures Per Bed Day		
Full Model	0.643	-0.213	8.782***	7.404***	4.643*
	(1.286)	(2.928)	(1.977)	(1.399)	(2.663)
FE + IV + Age	0.602	-0.003	5.532***	5.230***	3.684
	(1.111)	(1.539)	(1.791)	(1.873)	(2.530)
Fixed Effects + IV	2.968	12.220	13.394	10.152	4.999
	(4.055)	(11.383)	(10.257)	(7.492)	(6.049)
			Days Per Hospital Bed		
Full Model	-1.108	4.102	-7.516	-6.450*	-8.113**
	(2.002)	(5.208)	(5.270)	(3.464)	(3.979)
FE + IV + Age	-1.931	-0.519	-5.774	-6.953**	-8.988**
	(2.266)	(3.684)	(4.352)	(3.267)	(3.486)
Fixed Effects + IV	-1.691	-6.694	-9.669	-8.151	-7.287
	(3.241)	(8.641)	(8.057)	(5.270)	(4.682)
			Medicare Part A Expenses Per Enrollee		
Full Model	1.355	33.063**	49.765***	42.579***	47.640*
	(1.924)	(13.977)	(14.562)	(15.782)	(24.593)
FE + IV + Age	1.359	27.919**	46.705***	45.750***	48.535**
	(1.970)	(12.294)	(14.050)	(16.047)	(21.125)
Fixed Effects + IV	7.563	50.954***	66.203***	72.014***	81.426***
	(6.502)	(19.211)	(20.337)	(22.135)	(26.517)
			Medicare Part B Expenses Per Enrollee		
Full Model	1.289	15.514**	12.154*	8.816	10.145
	(1.246)	(6.793)	(7.296)	(8.246)	(10.408)
FE + IV + Age	1.019	10.155*	7.988	4.971	3.852
	(1.169)	(5.735)	(6.150)	(7.561)	(8.748)
Fixed Effects + IV	4.322	20.015*	16.498*	13.385	13.418
	(3.222)	(10.109)	(9.458)	(9.440)	(9.634)

Notes: The table reports the instrumental variable coefficients for: the full model with all independent variables; a more limited model with the instrument, fixed-effects (year and hospital or county), and controls for the age distribution in a county; and a third, even more limited model with the instrument, and fixed-effects.

Table 12: The Impact of Direct and Indirect Tort Reform on Medical Costs.

	Current	Lagged Reform			Leading Reform		
Tort reform in year:	*t*	*t-1*	*t-2*	*t-3*	*t+1*	*t+2*	*t+3*
Dependent variable: Hospital Facility Expenditures per Bed Day							
Direct reform	-0.016**	-0.012*	-0.010	-0.009	-0.014	-0.014	-0.003
	(0.008)	(0.007)	(0.007)	(0.006)	(0.009)	(0.009)	(0.010)
Indirect reform	-0.019*	-0.019**	-0.019**	-0.012	-0.002	-0.005	-0.006
	(0.010)	(0.009)	(0.008)	(0.008)	(0.012)	(0.014)	(0.012)
Dependent variable: Hospital Days per Bed							
Direct reform	0.029**	0.032***	0.022*	0.027**	0.011	-0.008	-0.022*
	(0.012)	(0.012)	(0.012)	(0.011)	(0.015)	(0.016)	(0.012)
Indirect reform	0.021	0.025	0.023	0.010	0.021	0.027	0.009
	(0.019)	(0.016)	(0.015)	(0.016)	(0.024)	(0.028)	(0.027)
Dependent variable: Part A Medicare Expenditures per Enrollee							
Direct reform	-0.005	-0.031*	-0.020	-0.011	-0.001	0.017	0.039**
	(0.020)	(0.018)	(0.017)	(0.016)	(0.017)	(0.018)	(0.015)
Indirect reform	-0.035	-0.013	-0.019	-0.030*	-0.004	0.025	0.026
	(0.023)	(0.020)	(0.017)	(0.017)	(0.027)	(0.028)	(0.027)
Dependent variable: Part B Medicare Expenditures per Enrollee							
Direct reform	-0.013	-0.017	-0.001	0.004	-0.014	0.014	-0.017
	(0.012)	(0.013)	(0.013)	(0.011)	(0.014)	(0.014)	(0.012)
Indirect reform	-0.011	0.007	0.002	-0.019	-0.028	0.001	0.008
	(0.022)	(0.019)	(0.017)	(0.016)	(0.033)	(0.048)	(0.036)

Note: The table reports the estimated effect of direct and indirect tort reform on medical costs. Each column of each panel reports results from a separate regression, with some measure of medical costs as the dependent variable. Data are at the state-year level and cover 1984-1994. Controls for demographic characteristics of the state, state fixed effects and state year effects are included in each regression. All regressions have 550 observations. Robust standard errors are reported in parentheses. A ***, ** and * indicates statistical significance at the 1, 5 or 10% level, respectively.

Table 13. Cost Effects of Malpractice Dropping Non-Malpractice Cases from the Instrument

	Current Year	Lagged: 1, 2 and 3 Years	Lagged: 2, 3 and 4 Years	Lagged: 3, 4 and 5 Years	Lagged: 4, 5 and 6 Years
Hospital Costs					
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards	-0.780	2.737*	2.982***	2.252***	0.153
Per Capita	(0.500)	(1.618)	(0.983)	(0.710)	(0.930)
Elasticity	-0.0082	0.0275	0.0291	0.0211	0.0014
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards	2.165**	1.609	-2.653	-4.488***	-3.529**
Per Capita	(0.972)	(3.307)	(2.636)	(1.648)	(1.581)
Elasticity	0.0231	0.0163	-0.0258	-0.0415	-0.0315
Dependent Variable: Inpatient Hospital Days Per Bed					
Malpractice Awards	0.094	0.557**	0.234	0.009	-0.053
Per Capita	(0.082)	(0.223)	(0.189)	(0.208)	(0.250)
Elasticity	0.0027	0.0153	0.0063	0.0002	-0.0014
County Medicare Costs					
Dependent Variable: Medicare Part A Expenditures Per Enrollee					
Malpractice Awards	-3.109	3.760	8.006	5.654	1.635
Per Capita	(2.785)	(9.058)	(6.546)	(6.387)	(10.314)
Elasticity	-0.0079	0.0083	0.0172	0.0118	0.0033
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards	0.721	4.047	2.496	0.204	0.653
Per Capita	(0.991)	(4.284)	(3.882)	(4.093)	(4.618)
Elasticity	0.0031	0.0149	0.0089	0.0007	0.0022

Notes: The table reports the estimated effect of per capita malpractice jury award dollars on medical expenditures. Malpractice awards are instrumented by the average noneconomic awards in medical malpractice verdicts with a plaintiff win. Each coefficient is from a separate regression, and each column represents a different lag for the malpractice variable. The unit of analysis is a hospital-year. County population is used as a weight in all regressions. Other explanatory variables include hospital and year fixed-effects, a quadratic for per capita income, the percent of the population that is male, white, African-American, and that falls into 5-year age ranges. Elasticities are evaluated at the mean values of the dependent and independent variables. Robust standard errors allowing clustering at the county level are reported in parentheses. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.