The Welfare Effects of Medical Malpractice Liability

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Abstract

Policymakers and the public are concerned about the role of medical malpractice liability in driving up the total cost of medical care. We use variation in the generosity of local juries to identify the causal impact of malpractice liability on medical costs and mortality. In stark contrast to the literature on malpractice costs and outcomes for high-risk patients, we find modest effects of malpractice on total medical costs—growth in malpractice payments over the last decade and a half contributed no more than 5.7% to the total real growth in medical expenditures, which topped 33% over this period. Moreover, while malpractice liability leads to only modest improvements in patient health outcomes, the value of these improvements more than likely exceeds the impact of malpractice on medical costs. Therefore, tort reform is unlikely to have a major impact on US health care spending, and is also unlikely to be cost-effective over conventionally accepted ranges for the value of a statistical life.

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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 major 2008 presidential candidate, along with 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] While malpractice payments are growing very rapidly, they account for a tiny fraction — between 1 and 2 percent — of total expenditures on physicians, let alone medical spending overall. Even if we were to include other factors, like time and transaction costs, one might question just how important malpractice liability could be in overall medical cost growth.

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

[2] John McCain has publicly stated his support of tort reform (Washington Post, http://projects.washingtonpost.com/2008-presidential-candidates/issues/candidates/). 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 appear in the NPDB (Government Accounting Office, 2000).

[4] Health expenditures are from the Census Bureau's *Statistical Abstract of the United States*.

Earlier research has provided a possible mechanism for a large effect. Kessler and McClellan (1996; 2002a; 2002b) 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] Quantitatively, Kessler and McClellan (2002b) 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 nationwide trends, the 65% increase in total malpractice payments from 1991 to 2002 would have accounted for about half the contemporaneous increase in total physician expenditures.

These numbers have 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 looked to Kessler and McClellan (1996) for its estimated cost impacts (Department of Health and Human Services, 2002).

Malpractice costs may indeed be a major driver of health care costs, but there are three factors limiting the generalizability of existing estimates to this issue. First, Kessler and McClellan themselves caution that their study was not designed to estimate the impact of malpractice risk on *total* medical spending. As discussed by both the Congressional Budget Office (CBO) and the General Accounting Office (GAO), the Kessler and McClellan study

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[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 et al., 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).

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 total health spending are simply not known.[7]

Second, the cost impacts of malpractice are typically presented alongside estimates suggesting that the malpractice risk faced by providers has statistically insignificant effects on patient outcomes, measured using mortality. The lack of a consistent and significant mortality effect in past studies is often taken to mean that malpractice reforms are strictly beneficial, since they lower cost without affecting patient outcomes. However, the mortality estimates often are not precise enough to support this conclusion, and fail to reject values consistent with substantial mortality benefits of malpractice.

The final issue has been the validity of the underlying identification strategy. Inspired by the work of Kessler and McClellan, many researchers have relied on state-level tort reform policies as a source of identifying variation. This strategy presumes 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

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[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.

care. Recent CBO research finds 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 pattern that reform states are more likely to have slower growth in health care spending prior to the adoption of the reform. In a more recent paper, Kessler and McClellan (2002b) find that controlling for HMO penetration indeed weakens the cost savings of tort reform.

In light of the policy-relevance of the issue, and the absence of critical information about the malpractice regime, 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. Finally, we need to deliver policy-relevant conclusions that properly account for the degree of imprecision in measuring malpractice's mortality effects.

We propose a novel identification strategy that relies on changes over time in the generosity of local juries to identify the impact of malpractice on medical expenditures. Using this approach, we confirm that malpractice risk does increase medical spending, but that the total effects on overall medical costs are somewhat modest. A ten percent reduction in malpractice costs 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.

Using the same identification strategy, we find that malpractice costs reduce mortality, but that the effects are modest and of borderline significance. However, using a bootstrap approach to construct a distribution of cost-effectiveness, we find that — for the most commonly used values of statistical life —malpractice cost reductions are unlikely to be cost-effective, even on the margin. This casts significant doubt on the social value of tort reform policies.

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 patient outcomes. In Section C, we outline our empirical approach and describe our data and identification strategy. Section D presents our results. Finally, we conclude with a discussion of directions for future research.

B. Conceptual Background

The term "malpractice risk" denotes different concepts, depending on the agent experiencing the risk. For providers, "malpractice risk" represents the risk of paying a malpractice claim. For patients, it is the risk of receiving negligent and hazardous medical care. Throughout our paper, we use the term to refer to the risk faced by providers. Ultimately, this will embed the risk faced by patients, but only to the extent that patient losses from malpractice are compensated by providers. Therefore, we do not measure the uncompensated costs to patients of physician negligence.[8] Correspondingly, we use the term "malpractice cost" to denote the direct and indirect costs borne by the physician as a result of the malpractice risk they face.

B.1 A Model of Malpractice Expectations

A simple theoretical model helps illustrate and formalize the assumptions that are critical to our empirical strategy. At time t, each provider chooses to treat q_t patients, with treatment intensity I_t. The provider pays the market insurance premium π_t. As is the case in actual

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[8] This omission is primarily an empirical one, as it is quite difficult to measure uncompensated, and certainly nonpecuniary, losses suffered by patients.

malpractice markets, insurance premiums are not experience-rated at the physician-level.[9] However, insurance is incomplete, in the sense that the provider faces uninsured malpractice costs M_t. The latter include the time, reputational and uninsurable financial costs of malpractice suits, as well as all self-insured liabilities. Generalized to a dynamic environment, these would also include the risk of being dropped by an insurance carrier.

B.1.1 *Provider Behavior*

Due to the lack of experience-rating, the physician's choices of *q* and *I* do not affect her insurance premium, but do affect the distribution of uninsured malpractice costs M. Expectations about M depend on current-period decisions, q_t and I_t, along with a vector containing all publicly available information about *past* malpractice payments made by all providers, which we represent as $\vec{\boldsymbol{\mu}}_{t-1}$.[10] Currently expected uninsured malpractice costs are given by $E[M_t \mid q_t, I_t; \vec{\boldsymbol{\mu}}_{t-1}]$.[11]

A provider with utility v over a single consumption good, and income only from her medical practice, chooses *q* and *I* to solve:[12]

$$\max_{q,I}\ v\{R(q,I) - \pi(\vec{\mu}_{t-1}) - E(M_t \mid q, I; \vec{\mu}_{t-1}) - c(q,I))\} \qquad (1)$$

[9] The economic literature suggests that insurers rarely experience-rate physicians, except in extreme cases with unusually bad claims histories (Danzon, 1991).

[10] In principle, the provider may also have private information about his past malpractice costs, but we abstract from this possibility, since it adds little to the analysis of interest to us.

[11] We assume that the malpractice experience of other providers provides useful information to an individual provider, since a single provider's malpractice history will typically be somewhat limited and idiosyncratic.

[12] For ease of exposition, we analyze this problem in a static setting, in spite of the time dimension. This abstracts from the impact of the provider's current decisions on future malpractice costs. While direct experience-rating is rare, risky behavior today might make it harder to secure coverage in the future. However, this dynamic linkage merely reinforces our findings here and can be ignored without sacrificing too much relevance.

where R is total revenue, $\pi(\bar{\mu}_{t-1})$ is the malpractice insurance premium, and c is the cost of treatment that is unrelated to malpractice. As long as $v()$ is monotonic, we can safely ignore it.

The solution to this problem is given by the following first-order conditions:

$$\begin{aligned} R_q - \frac{\partial E(M)}{\partial q} - c_q &= 0 \\ R_I - \frac{\partial E(M)}{\partial I} - c_I &= 0 \end{aligned} \tag{2}$$

These conditions highlight three issues that the estimation approach must address.

Role of expectations. Providers adjust their behavior when malpractice risk affects *expected* marginal returns, not actual marginal returns.

Importance of uninsured losses. Insurance premiums π play no role in provider behavior, because they do not vary with provider decisions. Note, however, that any uninsured costs associated with insurance, such as the costs of getting dropped by a provider, or any other dynamic incompleteness in the insurance market, would enter $E(M)$.

Reverse causality. Changes in quantity or intensity can influence malpractice costs. This induces a causal relationship running from total medical costs to malpractice costs. For example, suppose that "defensive medicine" works, in the sense that increased intensity, I, wards off lawsuits. In this case, it would be true that higher medical cost areas have lower malpractice costs. However, it would be incorrect to infer from this correlation that malpractice had no (or a negative) influence on medical costs.

B.1.2 *Malpractice and Medical Costs*

According to the model above, equilibrium medical costs per provider — which we define as C_t^* — can be written as:

$$C_t^* \equiv \boldsymbol{\pi}(\vec{\boldsymbol{\mu}}_{t-1}) + E(M_t \mid q_t, I_t; \vec{\boldsymbol{\mu}}_{t-1}) + c(q_t, I_t) \tag{3}$$

Equilibrium expected malpractice costs, on the other hand, can be written as:

$$L_t^* \equiv \boldsymbol{\pi}(\vec{\boldsymbol{\mu}}_{t-1}) + E(M_t \mid q_t, I_t; \vec{\boldsymbol{\mu}}_{t-1}) \tag{4}$$

This formulation clarifies that malpractice has direct and indirect effects on medical costs. When L^* rises, C^* rises, even holding all else constant. This "direct" effect of malpractice on medical costs is easy to quantify:

$$\frac{dC_t^*}{dL_t^*}\frac{L_t^*}{C_t^*} = \frac{L_t^*}{L_t^* + c(q_t, I_t)} \tag{5}$$

The *direct* elasticity of medical costs with respect to malpractice costs is equal to the share of malpractice costs in total medical costs. Empirically, actual 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. These arise, because changes in malpractice risk can affect the provision of quantity and/or intensity, as our model predicts.

B.2 Estimation Approach

The theoretical model helps illustrate how we approach the three problems above: measuring expectations, isolating variation in uninsured malpractice costs, and addressing reverse causality. Partition the vector $\vec{\boldsymbol{\mu}}_{t-1}$ into two components, $\vec{Y}_{t-1}$ and $\vec{Z}_{t-1}$. We define $\vec{Z}_{t-1}$ as all information about past *non-economic* damages awarded by local juries. $\vec{Y}_{t-1}$ is all remaining information about malpractice verdicts. Noneconomic damages represent a significant portion of damage awards,[13] and are frequently the target of reform efforts. As such, they provide a policy-relevant source of identifying variation. Their exogeneity relies on the premise that juries are instructed

[13] In our data, noneconomic damages amount to approximately 28% of awards in all tort cases, and approximately 35% of awards in medical malpractice cases.

to separate non-economic damages from economic factors like medical costs. The overall empirical strategy relies on the following three assumptions.

A1. *Public information about past malpractice awards affects current expected malpractice costs*. The model assumes that $\overline{\mu}_{t-1}$ affect L_t^*; we exploit this assumption to measure expectations. This is a non-trivial assumption: if the distribution of costs were viewed as perfectly stationary and well-understood, agents may regard all observed variation as primarily noise and fail to update their beliefs accordingly.

A2. *Non-economic damage awards vary over time in a way that affects expected* <u>*uninsured*</u> *malpractice costs.* This assumption generates the required first-stage power of the instrument, and isolates the appropriate local average treatment effect. We assume $\overline{Z}_{t-1}$ varies over time, and that this variation has a causal effect on $E(M_t)$ through its relationship to $\bar{\boldsymbol{\mu}}_{t-1}$. If the distribution of $\overline{Z}_{t-1}$ were presumed to be stationary and well-understood, agents may not respond to variation over time. Moreover, if $\overline{Z}_{t-1}$ affected only insured costs, it would not identify the indirect effects of malpractice on medical costs.[14]

A3. *Jury generosity in non-economic damages is independent of medical costs, quantity, and intensity*. This is the standard instrument validity assumption requiring that $\bar{Z}_{t-1}$ be independent of $c(\circ)$, $R(\circ)$, q, and I, except through its effects on $E(M_t)$ and L_t^*.

In our empirical presentation, we present evidence justifying each of these assumptions.

C. Empirical Framework

The theoretical model and assumptions above suggest the following instrumental variables model of the cost outcome Y_{it} for unit i at time t:

$$\begin{aligned} Y_{it} &= \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 E(MedMal_{ct}) + \boldsymbol{\beta}_2 X_{ct} + \boldsymbol{\phi}_i + \boldsymbol{\gamma}_t + \boldsymbol{\varepsilon}_{it} \\ E(MedMal_{ct}) &= \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1 J_{ct} + \boldsymbol{\alpha}_2 X_{ct} + \boldsymbol{\phi}_i + \boldsymbol{\gamma}_t + \boldsymbol{\delta}_{it} \end{aligned} \tag{6}$$

We propose to study several measures of cost Y_{it} at the county-level and the hospital-level. $E(MedMal_{ct})$ is expected per capita medical malpractice jury awards in county c and at time t. The identifying instrument is J_{ct}, per capita noneconomic damage awards in plaintiff wins for

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[14] In other words, we need to rule out the case that $E\left(M_t \mid \vec{Y}_{t-1}, \overline{Z}_{t-1}\right) = E\left(M_t \mid \vec{Y}_{t-1}\right)$.

county c at time t. X_{ct} is a vector of time-varying county characteristics, and ϕ_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.

Assumption A1 above implies that we can measure $E(MedMal_{ct})$ using information on past malpractice awards. Assumption A2 implies that $\alpha_1 > 0$, while A3 implies that $J_{ct} \perp \varepsilon_{it}$. We now justify each of these in turn.

C.1 Measurement of Expected Malpractice Costs

Assumption A1 postulates that past jury verdicts drive expectations of current malpractice cost. In this section, we justify that assumption empirically. We first demonstrate the viability of measuring *malpractice costs* using data on malpractice jury verdicts, and then justify our approach to measuring *expected* malpractice costs.

C.1.1 *Measurement of Malpractice Costs*

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 23.6% of total US population, as of the year 2000 Census.

The data in the JVDB were collected from court reporter publications, trade publications that provide trial attorneys with information on verdicts in local courts.[15] The JVDB includes

[15] Some researchers have objected that jury verdict reporters do not comprehensively cover all verdicts (cf, Vidmar, 1994; Moller et al., 1999; Eisenberg, 2001; Seabury et al., 2004). Earlier studies on the RAND JVDB used samples of public records to validate the data from several of the reporters used in this study. Peterson and Priest (1982) found that the *Cook County*

data on plaintiff win rates, average economic and noneconomic damage awards and type of injury for medical malpractice and other tort cases.

In Table 1, we present JVDB county-level averages for: total malpractice awards, malpractice awards per capita, average noneconomic damages awards per plaintiff win, and total jury verdict awards in all tort cases. Both unweighted and population-weighted statistics are presented. The columns of the table present the current year's average (year t), along with 3-year moving averages, defined as the mean across years $t-1$, $t-2$, and $t-3$.

On a per capita basis, the average county hands out $2.82 in malpractice awards. Larger counties tend to award more per capita: the population-weighted county average is $6.05. The average verdict in our sample involves a noneconomic award of $140,000, and an economic award of $328,000, where both means are higher on a population-weighted basis. Malpractice cases involve higher verdicts, due to higher noneconomic and economic damage awards.

C.1.2 *Measuring Expected Malpractice Cost*

While the true expected cost of malpractice is always unobserved, providers will infer the expected cost from their observations about past cost. We hypothesize that expectations about malpractice cost are based partly upon observations about past trial verdicts, for several reasons.

First, necessity may dictate the use of trial verdicts in forecasting costs, since verdicts are publicly available, but out-of-court settlements are typically confidential. Second, while this limits the information available, verdicts may perform well at prediction: even though relatively few malpractice cases proceed to trial, the expected size of a verdict will influence pre-trial

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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 noneconomic damages instrument or the malpractice awards measure we use.

negotiation and settlement. Therefore, information about past trial verdicts may be enough to draw educated inferences about expected total costs, of verdicts plus settlements.

We model expected costs as a function of past verdicts in a county, according to:

$$E(MedMal_{ct} \mid V_{c,t-1}, V_{c,t-2}, \ldots), \tag{7}$$

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? (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 only at the state-level. We estimate the following regression:

$$MedMal_{st} = \boldsymbol{\varphi}_0 + \sum_{i=1}^{6} \boldsymbol{\varphi}_i V_{s,t-i} + \boldsymbol{\omega}_{st} \tag{8}$$

We think about expected malpractice payments as the fitted values from this regression, which takes as information historical trends in malpractice verdicts. To normalize across differently sized states, both the payments and verdicts variables are calculated on a per capita basis.[16]

The results of this regression appear in Table 2. 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 $V_{s,t-3}$,

[16] This also eliminates the mechanical correlation induced by variation in population size.

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 φ_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 total malpractice costs. The regressions at the bottom of the table explicitly test the relationship between moving averages of verdicts, and current malpractice payments. In terms of R-squared, almost nothing is lost by moving from the specification with individual lags to one with a combined, equal-weighted moving average.[17]

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. If we could observe expected malpractice costs directly, that idealized regression would yield an elasticity that converges to the following value:

$$\text{plim}\left(\frac{\hat{\beta}_1 \overline{MedMal_{ct}}}{\overline{MedCosts_{ct}}}\right) = \frac{\beta_1 E(MedMal_{ct})}{E(MedCosts_{ct})} \tag{9}$$

[17] 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.

What we have instead is a proxy for $E(MedMal_{ct})$, defined as V_{ct}. If our proxy is an unbiased predictor, $E(MedMal_{ct}) = \pi E(V_{ct})$. However, our estimator for the elasticity of medical costs with respect to malpractice still converges to the same value as in equation 9:[18]

$$\text{plim}\left(\frac{\hat{\beta}_1 \hat{\pi} \overline{V}_{ct}}{\overline{MedCosts}_{ct}}\right) = \frac{\beta_1 \pi E(V_{ct})}{E(MedCosts_{ct})} = \frac{\beta_1 E(MedMal_{ct})}{E(MedCosts_{ct})} \qquad (10)$$

Therefore, we will focus on estimated elasticities, rather than slope coefficients.

C.2 Measuring Covariates and Outcomes

C.2.1 *County-Level Characteristics*

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 data on population are from the Census Bureau, which produces estimates for intercensal years based on a demographic model of its own. The vector X_{ct} includes time-varying county-level demographic characteristics: proportion male, proportion 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 3.

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

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[18] 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.

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*

From the Centers for Medicare and Medicaid Services (CMS), we obtained county-level data on Medicare expenditures, from 1980 to 2003. Based on their administrative records, CMS reports total Medicare Part A and B enrollees residing in a county, and total Parts A and B expenditures for the residents of each county. Due to inconsistencies over time in the reporting of Medicare HMO data, we use Medicare fee-for-service expenditures and enrollees, with a focus on aged (not disabled or end-stage renal disease) enrollees.

Ideally, we would have preferred measures of Medicare utilization by Medicare beneficiaries who sought care in a particular county, rather than those 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. Our instrumental variables strategy removes bias due to measurement error, provided that changes in the propensity to seek care outside the county are uncorrelated with changes in juries' propensity to award noneconomic damages.

The Medicare data are summarized in the middle panel of Table 3. Part A is the inpatient hospital insurance portion of Medicare that is free to all eligible Americans (over age 65 or disabled). Part B covers physician visits, outpatient procedures, and diagnostic imaging. Eligible individuals must pay a premium for Part B, but approximately 94 percent[19] of Part A

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

beneficiaries are enrolled in Part B. Therefore, we focus on costs per enrollee, rather than impacts on enrollment per se.

C.2.3 *Hospital-Level Costs and Utilization*

Data on hospital spending, utilization, and facilities come from the American Hospital Association (AHA). Since 1946, the AHA has conducted an annual census of its member hospitals. 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.[20] Hospitals report information longitudinally. All costs, here and throughout the paper, are deflated over time using the overall Consumer Price Index.[21]

The upper panel of Table 3 summarizes the expenditure and utilization data from the AHA survey. Since our core regression models use 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 average person in our sample tends to live in a county with slightly higher expenditures and lower utilization than the average American, but these differences are typically just 5-10 percent.

C.3 First-Stage Power

Assumption A2 stipulates that "non-economic damage awards vary over time in a way that affects expected uninsured malpractice costs." We provide evidence that: (1) Non-economic

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[20] 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.

[21] 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.

awards vary considerably; (2) The variation in non-economic awards predicts expected malpractice costs; and (3) Non-economic awards have particular effects on *uninsured* malpractice costs.

C.3.1 *Variation in Non-Economic Awards*

The psychology and legal studies literature on jury behavior has found a primary determinant of pain and suffering awards to be jurors' own internal monetary evaluations of the physical and emotional pain caused by an injury.[22] 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 et al., 1995; Wissler et al., 1997).

Jurors report great difficulty in estimating this monetary value of pain and suffering (Wissler et al., 1997). Perhaps as a result, experimental data with mock juries and actual verdict data reveal wide variation in pain and suffering awards made, even for similar cases, and by similar juries (cf, Saks, 1992; Saks et al., 1997). Absent concrete, external guidelines for calculating awards, jurors appear to respond to arbitrary reference points and to exhibit "anchoring" behavior (Sunstein, 1997; Robbennolt and Studebaker, 1999; Marti and Wissler, 2000). For example, experimental evidence with mock juries shows that random changes in an arbitrary reference point can change jury verdicts substantially (cf, Robbennolt and Studebaker, 1999). Scholars often emphasize the "under-specified" nature of "pain and suffering" as a

[22] The other primary determinant is the actual extent of the injury suffered by the plaintiff. Trends in injury severity appear not to drive our county-level trends in awards. Adding controls for plaintiff injury (the type of injury, as well as whether the injury resulted in temporary disability, permanent partial disability, permanent total disability, or death) had virtually no quantitative effect on our point-estimates for the impact of malpractice on county-level costs and utilization. Given this fact, it also seems less likely that unobservable variation in severity matters.

concept, which leads to wide "internal variance" of damage awards made by even a single juror, when faced with different cognitive cues or frames of reference (McCaffery et al., 1995).

These findings imply that a particular trial can lead to widely disparate outcomes, which depend on random cognitive factors influencing jurors. If these factors were purely random from one trial to the next, with no persistent component, they would never be used to update beliefs. However, the next sections present three pieces of evidence that local variation in non-economic damages causes agents to update beliefs about expected malpractice costs. In particular, past increases in non-economic damages predict: (1) Current malpractice premia, indicating that insurers update their beliefs; (2) The current probability of claims against physicians, indicating that patients update their beliefs about expected gains from a malpractice claim; and (3) Current medical costs, suggesting that providers adjust their behavior as well.

C.3.2 *Effect of Non-Economic Awards on Expected Malpractice Costs*

Table 4 displays the first-stage relationship between local trends in non-economic damages, and expected malpractice costs. The instrument is the average noneconomic damage award, per plaintiff win, granted by juries in the county. 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 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 noneconomic damages as the instrument, first-stage power meets the "rule of thumb" suggesting a Wald statistic of 10.0 or better.

As an auxiliary piece of evidence that this first-stage relationship reflects a relevant causal effect, we show in Appendix C that past trends in noneconomic damages positively predict current malpractice premia. This finding is consistent with the idea that insurers use variation in pain and suffering awards to forecast expected malpractice costs. By extension, this also suggests that variation in pain and suffering awards affects the settlement negotiations that primarily drive insurers' average malpractice costs.

One issue that affects power and potentially validity is the set of cases to include in constructing the instrument. We calculate average noneconomic awards in plaintiff wins for all types of cases, in measuring non-economic damage propensities. A plausible alternative configuration is to compute average noneconomic 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 it decreases instrument power. Empirically, this configuration produces a fairly severe weak instruments problem, with its attendant consequences of poor coverage rates (Staiger and Stock, 1997).

Including malpractice awards provides us with more power, but adds a stronger identifying assumption.[23] 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 possibilities that defensive medicine reduces the probability of a lawsuit, reduces the probability of a plaintiff win, and increases (or decreases) the size of the economic damage awards that depend most directly on medical costs. We test this assumption in our discussion of validity.

C.3.3 *Non-Economic Awards and Uninsured Costs*

If the first-stage relationship were driven entirely by a link between non-economic awards and *insured* malpractice costs, we would fail to identify any indirect effects of

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[23] An alternative identification strategy would include noneconomic 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 G, 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.

malpractice risk on behavior. Our instrument, however, has demonstrable effects on uninsured losses.

Increases in jury generosity will have direct effects on uninsured losses for hospitals, because 40% of them are self-insured (GAO, 2003). Among providers, only about 9.6% are self-insured,[24] but they cannot insure against the substantial time or reputational costs associated with being sued (Kessler and McClellan, 2002b). Appendix D shows our results that growth in noneconomic damage awards increase the probability of a malpractice claim, presumably by raising the pay-off to suing a provider.[25] Local increases in non-economic awards make it more likely that physicians will face claims, and even more likely that they will face (non-frivolous) claims that incur nonzero defense costs.

Finally, we should emphasize that our local average treatment effect is a policy-relevant parameter, because malpractice reform efforts often focus on capping noneconomic damages. The key element of the proposed Health Reform Act of 2003, which ultimately passed in the U.S. House of Representatives but failed in the Senate, was a $250,000 cap on noneconomic damages in medical malpractice cases. A noneconomic damage cap remains a key legislative priority for physician lobbying organizations such as the American Medical Association (AMA).[26]

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[24] This figure is an estimate based on the fraction of non state-fund payments in the NPDB that are made by an individual (specifically, not an insurance company). Applying this to the general population of physicians assumes that there is no selection of low risk physicians out of the insurance market. However, even if such an effect existed, it is unlikely to be strong enough to explain the difference in the fraction of physicians and hospitals that self-insure.

[25] For a summary of the literature on tort reform and lawsuit frequency, see Studdert, Brennan and Mello (2004).

[26] See http://www.ama-assn.org/ama/pub/category/7861.html.

C.4 Instrument Validity

The idea behind the noneconomic damages instrument is that juries are instructed to disregard medical costs, wages, and other economic variables in determining the size of a non-economic damage award.[27] While there is no direct mechanism transferring medical expenditures to the noneconomic award, indirect links are possible. For example, juries might increase the size of the noneconomic award in an environment where the plaintiff faces rapid medical cost growth. A more complex relationship might arise if defensive medicine changes the composition of plaintiffs, and the noneconomic damage awards they end up with. Therefore, we test the validity of the instrument in three ways.

C.4.1 *Temporal Tests of Validity*

First, we find that past medical costs do not affect contemporaneous or current noneconomic damage awards, but past noneconomic 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 the model in equations 6, where health expenditures are regressed on the (lagged values of the) instrument, state and year fixed-effects, and all the exogenous covariates X. In addition to the reduced-forms, we ran analogous models regressing on future values of the instrument, 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 noneconomic awards. Table 5 presents the results for five different dependent variables. There are 20 regressions testing the causal link from lagged noneconomic damages to

[27] We are emphasizing the levels of awards, conditional on plaintiff victory, as an identification strategy. An additional option is to use the probability of plaintiff victory. While this is also a plausibly valid instrument, we have found the probability of plaintiff wins to be an insignificant predictor of variation in malpractice costs.

current medical spending (i.e., the 4 right-most columns); 10 yield significant effects at the 10% level. On the other hand, only two of the 20 regressions testing the opposite effect — of current health care spending on leads of noneconomic damages —is significant at the 10% level. This result is not an artifact of differences in power, since the regressions have more power (i.e., narrower confidence intervals) when we test for the reverse causality running from medical spending to noneconomic damages. 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, and against the notion that an unobserved third factor simultaneously drives verdicts and medical spending.[28]

C.4.2 *Tests of Jury Behavior*

Second, we conduct a direct test of whether juries link medical costs to noneconomic damage awards. According to this hypothesis, juries award noneconomic damages as a simple function of medical losses. If true, high growth in medical costs would cause higher noneconomic damage awards. However, we find that plaintiffs' claimed medical losses are highly correlated with economic damage awards, but entirely uncorrelated with noneconomic damage awards. Table 5 presents the results of this test for the 2,328 malpractice cases that involved a plaintiff win in our sample.[29] 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 noneconomic award.

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[28] 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 noneconomic damages.

[29] Mean medical costs are relatively insignificant for non-malpractice cases.

As we would expect, claimed medical losses have a large impact on the economic awards. An additional dollar of claimed medical and non-medical losses is associated with about a \$0.34 and \$0.22 higher award, respectively (medical and non-medical damages are jointly significant, but only medical losses are statistically significant on their own). 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).

C.4.3 *Effects of Regression Covariates*

As an additional test of instrument validity, we compute instrumental variables estimates with and without the auxiliary controls X_{ct}. 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 D 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.

C.4.4 *Impact of Tort Reform*

The occurrence of tort reform generates a final potential validity issue to address. If in fact tort reform is driven by medical expenditures, and if tort reform affects noneconomic damages in our data, the instrument could be compromised. However, 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.[30] 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 on expected payments (Eisenberg et al., 1997). It remains possible that the presence of tort reform at baseline creates heterogeneity in the treatment effects; we take this question up in Section D.3.

D. Results

D.1 Effects of Malpractice on Costs

Consistent with the previous literature, we find clear evidence of "defensive medicine," in the sense that malpractice risk increases per capita costs, by more than the direct effects would suggest. Overall, however, the effects of malpractice risk on medical costs are modest. Doubling expected malpractice cost raises per capita medical spending by at most 9%.

D.1.1 *Hospital Costs*

Hospital costs account for the majority — approximately 60% — of total spending on hospitals, physicians, and clinical services, which represent the segment of the health care market exposed to malpractice risk. At the hospital level, we find that increases in expected malpractice cost raise the daily cost of hospital care, at the same time that it reduces the total number of visits. The reduction in quantity is particularly pronounced for outpatient procedures.

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[30] Conceivably, one might still be concerned that California's noneconomic damage growth is systematically different than that of other states, in a way that is related to health spending. However, we get substantially similar results when we estimate the effects excluding all counties in California, a finding discussed further in Section D.3.

Effectively, malpractice risk functions like a downward shift in the supply of hospital bed-days. These offsetting effects on “prices” and “quantities” create smaller impacts on total costs, but even focusing exclusively on the increased daily cost of care leads to rather modest implications: 10% increases in expected malpractice costs raise hospital costs per bed-day by at most 0.8%.

Ordinary least squares and instrumental variables estimates of equation 6 at the hospital level are given in Table 7. We model costs per bed, costs per bed-day, and days per bed.[31] We think of the first as an overall expenditures measure, the second as a price measure, and the third as a quantity. We found no relationship between malpractice risk and changes in hospital beds, suggesting that one can treat this quantity as constant.

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 has minimal effects on “quantity,” at least overall.[32] As such, the impacts on total expenditure are quite similar to the change in cost per unit of output. The most striking feature of the table is the modest size of the estimates. The overall impact of malpractice risk on spending (price multiplied by quantity) is extremely small, and even the separate impacts on price and quantity are relatively modest, with elasticities below 0.1.[33]

D.1.2 *Medicare Costs*

Medicare costs account for 30% of hospital costs and 20% of spending on physicians and clinical services (2000 National Health Expenditures data). Table 8 studies the relationship

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[31] We define bed-days as inpatient bed-days plus outpatient procedures. Implicitly, we regard an outpatient procedure as filling a hospital bed for one day.

[32] We find more robust evidence that malpractice risk reduces inpatient days per bed, but there is no effect on outpatient procedures.

[33] In Appendix B we verify that that the small affects are robust to other specifications, such as the inclusion of HMO penetration. In Appendix D we verify that these effects are identified primarily off the instrument itself, rather than on auxiliary variables.

between expected malpractice costs and Medicare costs per enrollee. The IV estimates suggest that malpractice risk raises Medicare Part A expenditures per enrollee, but has a somewhat smaller impact on Part B spending, which consists of outpatient and physician services spending. The elasticity for Part A spending ranges from 0.07 to 0.10. The elasticity for Part B is around 0.04 to 0.06. Our numbers suggest that doubling expected per capita malpractice costs raises per capita Part A spending by 7% to 10%, and Part B spending by 4% to 6%. The evidence suggests a substantial indirect effect of malpractice — approximately 5-8% — on Part A spending.

The modest size of the Part B elasticities is consistent with earlier research finding small overall effects of malpractice on Medicare Part B, in spite of considerable impacts on specific diagnostic and imaging procedures (Baicker and Chandra, 2007). The modest size of these effects is robust to the inclusion of HMO penetration variables, and to changes in the moving average window for the malpractice measures, as shown in Appendices A and B, respectively.

D.1.3 *Overall Effect on Costs*

By analyzing total Medicare spending and total hospital spending, we cover approximately 66% of total US health care spending on hospitals, physicians, and clinical services, which is the segment of health care spending exposed to malpractice risk.[34] Moreover, the uncovered portion is non-Medicare spending on physician services. Our analysis suggests that physician costs are less responsive to malpractice risk; this relative insensitivity is confirmed by other research (Baicker and Chandra, 2007). Therefore, it seems reasonable to assume that

[34] According to 2000 National Health Care Accounts data, total hospital spending was $417bn, and total physician and clinical services spending was $289bn. Of the latter, $58bn was paid by Medicare, also according to the National Health Care Accounts. Finally, according to our CMS county-level data, fee-for-service spending on the aged was approximately 84% of the Medicare program, in the year 2000. Applying this ratio would suggest that we cover $49bn of Medicare physician spending.

the effects of malpractice on physician costs are no higher than our maximum estimate for hospital costs.

As such, we can perform a conservative calculation of the impact of malpractice risk on total medical costs. We take our largest estimated effect of malpractice on daily hospital expenditures — 0.077 (the $t-2$ through $t-4$ moving average from Table 7), or alternatively, the upper bound of this estimate's 95% confidence interval — 0.110. Between 1991 and 2002, medical expenditures grew by 34%, while malpractice payments grew by 65%. Our highest point estimate would imply that, over this period, the growth in malpractice payments added 5.0% to the growth in medical expenditures, while the upper bound of its confidence interval implies an increment of 7.2%. In relative terms, between 15% and 21% of the growth in medical expenditures is due to malpractice. In spite of the extraordinary growth in malpractice premia over this period of time, it was not a dominant driver of real medical cost growth. Moreover, these calculations are based on the largest estimated effect on hospital spending.

Nonetheless, the numbers reveal a sizeable indirect effect of malpractice risk on behavior. Doubling malpractice risk has a direct 2% impact on spending at most, but the total effect could be as high as 10%. Therefore, doctors do change their behavior in response to malpractice risk, even though the latter is not a significant source of cost growth.

D.2 Malpractice Growth and Changes in Mortality

Faced with the threat of malpractice liability, physicians may undertake actions that limit risk to patients. As such, part or all of their behavioral response *may* improve outcomes for patients. Past studies have found virtually no evidence of such improvements. Kessler and McClellan (1996; 2002b) find no consistently positive and significant effect of malpractice reforms on the mortality or 1-year hospital readmission rates for heart attack patients in the

Medicare population. Rubin and Shepherd (2007) find that tort reform appears to reduce the number of (non-automobile) accidental deaths, which they attribute to behavioral responses — for example, individuals who are less protected by the tort system may take more care. These and similar papers argue that tort reform lowers costs without raising mortality. However, these studies tend to be confined to particular subgroups — for example, heart attack patients in Medicare, or a subset of accidental death victims. Moreover, the statistically insignificant mortality effects often cannot rule out substantial mortality benefits, based on their estimated confidence intervals.

Given the limits of the previous literature, we revisit this question, by using jury generosity to estimate the impact of malpractice on total mortality rates at the county level, taken from the Multiple Cause-of-Death Mortality Data.[35] Since we argued earlier that the instrument is exogenous with respect to medical costs, it is also likely exogenous to variation in mortality driven by medical care. One unique identification question concerns whether or not noneconomic damages affect mortality through channels other than medical care. In Appendix H, we show in reduced-form specifications that noneconomic damages have significant effects on non-accidental mortality, but not accidental deaths. While not dispositive, this evidence suggests that medical care is playing an important role in causation.[36]

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[35] These are taken from the National Vital Statistics System of the National Center for Health Statistics (NCHS). The NCHS data provide detailed cause-of-death information on all deaths that occur in the United States. We aggregate to the county level, for every year between 1982 and 2003. Separate death rates are calculated for: total population, 20-64 year-olds, and 65+ year-olds. To protect individual privacy, county identifiers are provided only for counties with 100,000 people or more. Rather than exclude these deaths, we construct an aggregate "small California county" and "small New York county" by using weighted means for the other variables.

[36] A final concern is the theoretical possibility that non-medical variation in mortality (e.g., from accidental deaths) drives juries' non-economic damage awards. However, Appendix H also

We estimate the effect of malpractice risk on the total mortality rate, the number of non-accidental deaths (as these seem more likely to be influenced by medical care), the deaths attributed to ischemic heart disease, and deaths among individuals 20 to 64 or 65 and older. We did not disaggregate by more specific causes of death because these fields are generally considered unreliable. The death rate is calculated as the number of deaths per 1,000 members of the county population.

The results of the analysis appear in Table 9. Our point estimates on total deaths are uniformly negative, but statistical significance is less consistent. The results are strongest for non-accidental deaths, with 3 of the five coefficients negative and significant at the 10% level. The age-specific mortality effects are also negative, though the significance is weak (particularly for those 65 and older). The point estimates suggest that doubling malpractice costs leads to modest reductions in death rates of approximately one to two percent. Interestingly, we found uniformly insignificant but mostly *positive* relationships between malpractice costs and ischemic heart disease deaths. This is consistent with the Kessler and McClellan finding that malpractice reforms have insignificant effects with inconsistent sign on the mortality of heart patients.

Overall, the results of Table 9 provide some weak evidence that malpractice risk leads to reduced mortality. The estimates are imprecise, but consistently negative and significant in several instances (more than one would expect by chance). Still, it is hard to draw policy inferences from these results, because the confidence intervals often span ranges with wildly inconsistent implications for net social benefit.

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shows that past mortality does not statistically precede contemporaneous or current noneconomic damages, but noneconomic damages do sometimes statistically precede mortality.

D.3 The Cost-Effectiveness of the Malpractice Regime

To develop a more rigorous approach to policy evaluation in the face of this parameter uncertainty, we note that neither the cost elasticity nor the mortality elasticity is the parameter of interest. The policy-relevant output is the number of dollars saved per life lost that is associated with a particular policy change. Moreover, the statistical uncertainty around our estimates implies that we ought to examine the *distribution* of this parameter, not just its point-estimate. Comparing this distribution to the value of a statistical life-year yields a conclusion about the probability that a given policy change is cost-effective.

Our approach resembles the method of "cost-effectiveness acceptability curves," often used in the cost-effectiveness literature (cf, Lothgren and Zethraeus, 2000; Fenwick et al., 2004). The approach is to calculate the empirical distribution of the "dollars per life lost" parameter, via a bootstrap methodology (whose technical underpinnings are discussed in Appendix I). This distribution then implies a probability of whether or not malpractice cost-reduction is a cost-effective policy, conditional on a value of life.[37] For example, suppose we adopt the view that the value of a statistical life is \$6m. Suppose further that its empirical distribution implies a 40% probability that — for a marginal reduction in malpractice costs — dollars saved per life lost exceed \$6m. Under this scenario, a policy lowering malpractice costs at the margin has a 40% probability of improving net social welfare.[38]

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[37] Conceptually, this is defined over the probability space containing our estimator of dollars per life saved.

[38] Conceptually, we are calculating how many dollars of cost-saving per life lost are generated by a local average reduction in malpractice cost, due to lower noneconomic damage awards. At a minimum, this evaluates noneconomic damage caps, which are one of the most frequent malpractice reforms mentioned. If the local average treatment effects generalize, this provides insight into a broader class of reforms that limit malpractice cost.

To estimate the distribution of "dollars per life lost," we use a clustered or "block" bootstrap approach that samples with replacement at the level of the county. This embeds the underlying empirical assumptions that observations from different counties are statistically independent, but observations within a particular county exhibit dependence. We implement this procedure to evaluate policy reform as it affects the total population, and separately as it affects the Medicare population.

To illustrate, we explicitly lay out the bootstrap algorithm for the Medicare analysis:

1. Randomly draw a county, and include all observations (years) from that county;
2. Repeat step 1, sampling with replacement, until the bootstrap sample of counties is complete;
3. Using the bootstrap sample constructed in steps 1 and 2, estimate the model in equations 6 using per total Medicare spending per elderly beneficiary as the outcome Y_{it}.[39] This then yields an elasticity of malpractice costs on Medicare costs, defined as $\boldsymbol{\varepsilon}^{CM}$.[40]
4. With the *same* bootstrap sample used in step 3, estimate the model in 6 separately using county-level death rates for the over 65 population as the

[39] In each bootstrap replication, we run IV models using county population as weights: if smaller counties have smaller hospitals with more variance, weighting by population mitigates the effect of heteroskedasticity on the distribution of the bootstrap estimator.

[40] The "direct costs" of malpractice do not represent true social costs, since they are merely transfers to patients. However, several studies have concluded that the social costs of litigation are approximately equal to payments to victims (Kakalik and Pace, 1986; Studdert et al., 2006). Moreover, the social costs of settlement are likely to be lower than that of litigation. Therefore, we conservatively assume that the social costs associated with direct payments to victims are approximately equal to the payments themselves. This biases us toward finding benefits to malpractice cost reductions.

outcome Y_{it}. This provides the elasticity of malpractice costs on over 65 mortality, defined as $\boldsymbol{\varepsilon}^{M}$.

5. Using estimates of total nationwide deaths and costs in the year 2000 (the last year of our data),[41] our estimate of dollars saved per life lost in the Medicare population is then given by:

$$DPL^{M} \equiv \frac{\boldsymbol{\varepsilon}^{CM} * \text{Total Medicare Costs}}{\boldsymbol{\varepsilon}^{MM} * \text{Total Deaths Over Age 65}} \quad (11)$$

A similar procedure is used to estimate dollars per life in the overall population. We first estimate the model in equations 6, at the hospital level, using total hospital costs as the outcome Y_{it} to derive the impact of malpractice costs on total hospital costs, denoted by $\boldsymbol{\varepsilon}^{CT}$. Estimating the model using total county-level mortality as the outcome Y_{it} then yields its impact on deaths, called $\boldsymbol{\varepsilon}^{MT}$. (We must assume that our hospital cost elasticity is not substantially different from the elasticity for other medical costs.[42]) These are then combined with year 2000 data on total nationwide medical spending and total deaths, to estimate the aggregate impacts.[43] The estimate of dollars per life overall is:

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[41] The total number of deaths in the over 65 population was approximately 1.8m. Total Medicare spending was approximately $224.3bn.

[42] Our findings using Medicare Part B spending, which are corroborated by Baicker and Chandra (2007), suggest that non-hospital spending (other than non-specialty pharmaceuticals) responds at less than or equal to the rate of hospital spending, at least for the Medicare population. Moreover, our cost measures account for approximately two-thirds of all spending on hospitals and physician services, which together represent the portion of health care spending exposed to malpractice risk. In 2000, for example, hospital spending was $417bn; spending on physicians and clinical services was $289bn, of which Medicare paid $58bn.

[43] The total number of deaths in 2000 was approximately 2.4m. Total medical spending in that year was approximately $1.4tr. One might argue that we should use total hospital expenditures, but we use all expenditures to better proxy for the total impact of malpractice. Using only hospital expenditures would severely weaken the case for tort reform.

$$DPL^T \equiv \frac{\boldsymbol{\varepsilon}^{CT} * \text{Total Medical Costs}}{\boldsymbol{\varepsilon}^{MT} * \text{Total Deaths}} \tag{12}$$

We conducted 1,000 bootstrap replications. For completeness, we repeated this procedure for all the various lag specifications reported in Table 7, Table 8, and Table 9.

Table 10 demonstrates that the bootstrap estimates yield hypothesis test results similar to those of the asymptotic IV estimates.[44] For the sake of comparison, the table reports p-values for one-tailed hypothesis tests assessing whether the coefficients have the theoretically predicted sign — positive effect of malpractice on costs, and negative effect of malpractice on mortality.[45] The bootstrapped p-values are quite close to the asymptotic p-values, particularly in the 3, 4, and 5 lags case that we have focused on. Some departures are observed in the case with 1, 2, and 3 lags, where the asymptotic estimates seem to lead to over-rejection.[46]

Figure 2 depicts the empirical cumulative distribution function for the estimated dollars per life saved.[47] For each dollar value, the Figure plots the probability that dollars per life saved lies above that value, and thus the probability that malpractice cost-reduction is cost-effective.

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[44] Observe that the IV estimates reported here are not exactly identical to those in the earlier tables, because we adjusted the samples to be entirely comparable to the bootstrap methodology. First, small counties do not separately report mortality numbers. Therefore, in calculating mortality estimates, we had to group small counties, as described in Section D.2. To ensure that each pair of cost and mortality estimates is generated from the same sample, we use this grouping of counties prior to each bootstrap draw, and we do the same in constructing IV estimates of cost effects in Table 10. Second, to conserve computing power in the bootstrap, we ran all analyses at the county level, by aggregating hospital-level data. In Table 10, we apply this procedure to the IV estimates as well. We found, using one (most preferred) model specification that the county aggregation had no quantitative impact on the bootstrapped distribution.

[45] As discussed in Appendix I, the bootstrap should deliver valid p-values and confidence intervals for these coefficients, since IV is an asymptotically linear estimator.

[46] We are operating under the view that the bootstrap distribution performs better in a finite-sample context, and thus serves as the "gold standard."

[47] Technically, the figure, which is truncated above at $10m, illustrates one minus the empirical cumulative distribution function.

The figure can be interpreted as a "menu" of policy implications for malpractice cost-reduction, conditional on choices for the value of a statistical life.

In the Medicare population, tort reform is more likely than not to be cost-effective for values of a statistical life lower than $650,000. There is a stronger case for tort reform in the overall population, but it is much shakier than reliance on the borderline significance of the point estimates would suggest. Reductions in malpractice costs are more likely to be cost-ineffective for values at or above $3m. Three of the four estimated models imply a threshold lower than this, of $2.5m.

Using the figure to assess the desirability of tort reform requires clarity on the exact value of a statistical life. Unfortunately, the literature on this subject provides more controversy than clarity. In a prominent review of it, Viscusi and Aldy locate the value of a statistical life within the range of $4m to $9m (Viscusi and Aldy, 2003). In a separate paper, they argue that the value for 60 year-olds is about half that, between $2.5m and $3m (Aldy and Viscusi, 2004). However, others have dissented quite markedly. Ashenfelter and Greenstone (2004) use the impact of speed limit increases on mortality to conclude that the value of a statistical life is bounded above by $1.5m. According to our estimates, malpractice cost reduction has a better than even chance of being cost-effective for values lower than $1.5m, but is a poor bet for the values espoused by Viscusi and Aldy.

While the literature is ambiguous on the privately optimal value of a statistical life, one pragmatic approach is to follow the actual value thresholds employed by US regulators. The US Environmental Protection Agency makes decisions based on a value of at least $5.2m (U.S. Environmental Protection Agency, 2002), while the Department of Transportation (along with the Federal Aviation Administration) uses $3m (U.S. Department of Transportation, 2002).[48] Perhaps most directly relevant is the $5m number used by the Food and Drug Administration (FDA) to assess health risks.[49] All these thresholds would imply that, on the margin, malpractice reform is more likely to be cost-ineffective. Therefore, any policymaker wishing to defend tort reform would need to depart from these accepted US regulatory practices, and advocate a lower value of statistical life than conventionally used, in order to justify their case.

At a minimum, our analysis reveals the tenuousness of the case for tort reform, but it is important to note its limitations. First, we account only for impacts of tort reform on medical costs and mortality, excluding its impacts (if any) on morbidity, physician utility, and patient satisfaction. In addition, we do not account for the adjustment costs (e.g., on the utilization of the health care system) that would be induced by any large-scale reform project. The size and even direction of these excluded effects is not clear. Finally, even if we ignore these limitations and accept the estimates at face value, the probabilistic nature of our analysis means we cannot rule with (even approximate) certainty for or against tort reform without adopting extreme value of life thresholds.

A remaining issue is the presence of noneconomic damage caps in California, which were adopted before the start of our data. If the hypothetical policy experiment is the reduction of malpractice costs through the imposition of California-style caps, it does not make sense to compute the impact of equal reductions in malpractice costs for both California, and the

[48] Unfortunately for our analysis, the US Medicare program is barred from explicitly disclosing a value of statistical life decisionmaking criterion with respect to medical care decisions.

[49] If the Centers for Medicare and Medicaid Services (CMS) used such a number, that would be most relevant of all for our purposes, but they are discouraged from incorporating cost-effectiveness into their approval criteria.

uncapped states in our data. Econometrically, the issue is whether we have heterogeneous treatment effects across California and New York, which is uncapped. If so, the analysis above computes a "blended" treatment effect that may differ from the effect of adopting caps in an entirely unregulated state. To assess this hypothesis, we repeated the bootstrap procedure without the California data. The results are qualitatively the same, as shown in Figure 3.[50]

E. Conclusions

The impact of liability for medical malpractice on the cost of medical care has been one of the highest profile issues in debates over the U.S. health care system for many years. There is no question that 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 other effects, such as decreasing the number of patients treated, or increasing the intensity of treatment for the patients who remain. Our findings, however, suggest that limiting malpractice liability is no panacea for rising health care costs.

While the benefits of malpractice may be quite modest in terms of mortality reductions, these seem more likely than not to justify its costs to the health care system. Therefore, we conclude that — for values of statistical life traditionally employed by US regulators — additional reductions in malpractice cost are unlikely to be welfare-improving. 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. Future

[50] This supports the anecdotal claim that California suffers from more litigiousness, which magnifies the cost impacts of malpractice and may offset the effects of its damage caps.

research should endeavor to determine whether tort reform that somehow targets these subgroups would be cost-effective.

Another important avenue for future work is to evaluate whether malpractice has effects on more fine-grained outcomes in the health care system, such as morbidity, disability, or the nature of care delivery. Medical costs and mortality are likely to be the first-order costs and benefits of changes to the malpractice system, but the auxiliary effects may be quite significant. If, for example, malpractice risk has had limited impacts on costs but appreciable positive impacts on average outcomes other than mortality, the malpractice “crisis” may be anything but. If, on the other hand, it has negative impacts on outcomes, the major costs of malpractice may be on health rather than in dollars.

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Appendix

A. HMO Penetration

Danzon (2000) 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. Kessler and McClellan (2002a) confirm this, and find that HMO penetration weakens the estimated effect of tort reform on 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 11 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. This suggests that the impact of HMO penetration operates through the adoption of tort reform and not through impacts on pain and suffering damage awards.

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 13 presents the estimates that result when the length of this moving average window is varied. The table demonstrates that we continue to get modest effects on cost at longer or shorter lag lengths.

C. Noneconomic Awards and Malpractice Premiums

Our use of pain and suffering awards in jury cases is based on the theory that these act as shocks to expectations about future malpractice costs. Here we examine empirically whether changes in noneconomic damages are correlated with future changes in insurance premiums. Table 12 provides the estimated effect of lagged noneconomic damage awards on current malpractice premiums. The data on premiums come from the Medical Liability Monitor (MLM), an annual publication that surveys malpractice insurers about premium levels in each state. The MLM do not publish average rates, rather they publish rates for three specialties: internal medicine, obstetrics and general surgery. We run regressions at the insurer level, and the aggregated county-level. The average county-level premium is calculated as the mean across companies reporting in a county-year; the results are essentially the same if the weighted average of specialties within a county-year is used as the dependent variable (where the weights are based on the fraction of physicians in each specialty as computed from the ARF).. At the insurer level, we report results with fixed effects for county, year, and specialty, along with results that include fixed-effects for insurers.

These regressions indicate a positive relationship between lagged noneconomic damages and premiums, but no significant relationship between current damages and current premiums. This suggests that causality runs from damages to premiums, rather than in the opposite direction. For the lags, the coefficients range from \$650 to \$1200 increase in annual premiums for every hundred thousand dollar increase in average noneconomic damages per plaintiff victory. The coefficients are significant in 7 of the 12 specifications, and near significance in the others. For the 5 insignificant estimates, the associated p-values are: 0.121, 0.124, 0.126, 0.159, and 0.165.

D. 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 14 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 as covariates provides precision, but does not appear to drive identification. In general, the auxiliary covariates do not greatly affect the instrumented estimate.

E. Noneconomic Damages and the Probability of Lawsuits

The IV strategy in the paper isolates a relevant local average treatment effect if noneconomic damages affect the probability of lawsuits, and thus uninsurable costs for providers. To test this assumption, we use California closed-claim data from a large malpractice insurer covering approximately 20% of the California market. These data include the number and type of claims for all policyholders from 1991 to 2000, as well as the county in which each physician was rated and practiced. The data include information on 12,382 physicians for an average of about 11 years per physician located in 54 counties in California.

Table 15 presents the results from our regression analysis testing whether or not the probability of lawsuits faced by physicians vary with a county's average noneconomic damage awards. We estimate separate linear probability models for two dependent variables: an indicator for whether a physician faced any claim in a given year, and an indicator for whether a physician faced a claim in a given year that incurred a positive defense cost. Physicians are required to report any event in which they think there is a chance someone might sue, but about 25% of the time these are resolved with no cost (i.e., the claim is simply dropped or never pursued). In our data, the probability of a claim in a given year is 17%, whereas the probability that the claim incurs some defense cost is about 13%.[51] The table reports results with and without physician fixed-effects. The other demographic variables described in the text are also included in the regressions.

The probability that a physician is sued is increasing in the average noneconomic damage awards in tort cases. A $100K increase in noneconomic damages is associated with a 0.4 to 0.7 percentage point increase in the probability of a claim in a given year, for an elasticity of 0.069 to 0.138. The coefficients and elasticities change little when the physician fixed effects are introduced, though the standard errors do increase (enough so that the impact on all claims is not significant).

F. 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 are simply an artifice of our new approach. 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.

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[51] Note, the probability that a claim is filed that results in some kind of indemnity payment to the plaintiff is just 3%.

State-year level data on the presence of various kinds of tort reform was taken from the 2nd edition of the Database of State Tort Law Reforms (DSTLR) by Ronen Avraham, a detailed compilation of all state tort reform efforts going back to 1980 funded in part by the National Science Foundation (Avraham, 2006). 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 16 presents results on the estimated impact of "direct" and "indirect" tort reform, the classifications used by Kessler and McClellan (1996; 2002b) 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 16 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 (2002b) 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. It is significant to note that, compared to our estimates, the tort reform estimates are closer to the OLS numbers, consistent with invalidity of the tort reform strategy. Moreover, it also suggests that the large elasticities obtained by Kessler and McClellan has more to do with their choice of subpopulation — heart attack patients — than their choice of instrument.

G. 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. Using all tort cases is appropriate if they capture the average generosity of juries, either perceived or imaginary, for a randomly selected case. For example, suppose large awards in non-malpractice cases lead to publicity that raises physicians' perceived malpractice risk. As such, it would be justified 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 17 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.

H. Causality Test for County Level Mortality

In the paper, we argued that noneconomic damages were not driven by medical costs or other factors that determine it. This makes it unlikely that noneconomic damages are plausibly caused by mortality. To ensure that this is the case, we repeat in Table 18 the Granger causality test by regressing the different measures of county-level mortality against lags and leads of the noneconomic damage award instrument. As before, the test supports the validity of the instrument if the lags of noneconomic damages influence current mortality while the leads do not. In this case we expect a negative relationship between lagged noneconomic awards and mortality. The results in the table are consistent with the validity of the instrument. The lags are all negative, and a handful of them are significant—particularly the non-accidental deaths and the deaths among the non-elderly adult population. On the other hand, none of the leads are significant. However, it is worth noting that the results here are not quite as strong as those in Table 5 for the cost regressions, primarily because the reduced-form effects on mortality are weaker than for costs.

I. Bootstrapped Cost-Effectiveness Acceptability Curves

In this appendix, we demonstrate and justify the conditions under which the bootstrapped distribution of "dollars per life saved" is a valid approximation to the true distribution. Define $\boldsymbol{\varepsilon}^C$ as the estimator of the elasticity of malpractice with respect to medical costs, and $\boldsymbol{\varepsilon}^M$ as the estimator of the elasticity with respect to mortality. Define ε_0^C and ε_0^M as their respective probability limits. In calculating dollars per life saved, we take the ratio of two elasticities, $\frac{\boldsymbol{\varepsilon}^C}{\boldsymbol{\varepsilon}^M}$. Note, however, that all our policy conclusions are exactly symmetric if we take the inverse ratio, $\frac{\varepsilon^M}{\varepsilon^C}$. Therefore, without loss of generality, we will show that this ratio's distribution can be bootstrapped.

A sufficient condition for this result is that ε_0^C is bounded away from zero. If this is true, then the function $\frac{\varepsilon^M}{\varepsilon^C}$ is differentiable at $\frac{\varepsilon_0^M}{\varepsilon_0^C}$. In addition, provided that the underlying IV models are valid, both these estimators are scalar multiples of $\sqrt{n}$-consistent, asymptotically normal estimators. This fact coupled with the differentiability assumption implies — via the Delta Method — that $\frac{\varepsilon^M}{\varepsilon^C}$ is asymptotically normal. Since the ratio of elasticities is asymptotically linear, the bootstrap provides a valid approximation of its distribution (Mammen, 1992; Abadie and Imbens, 2006).

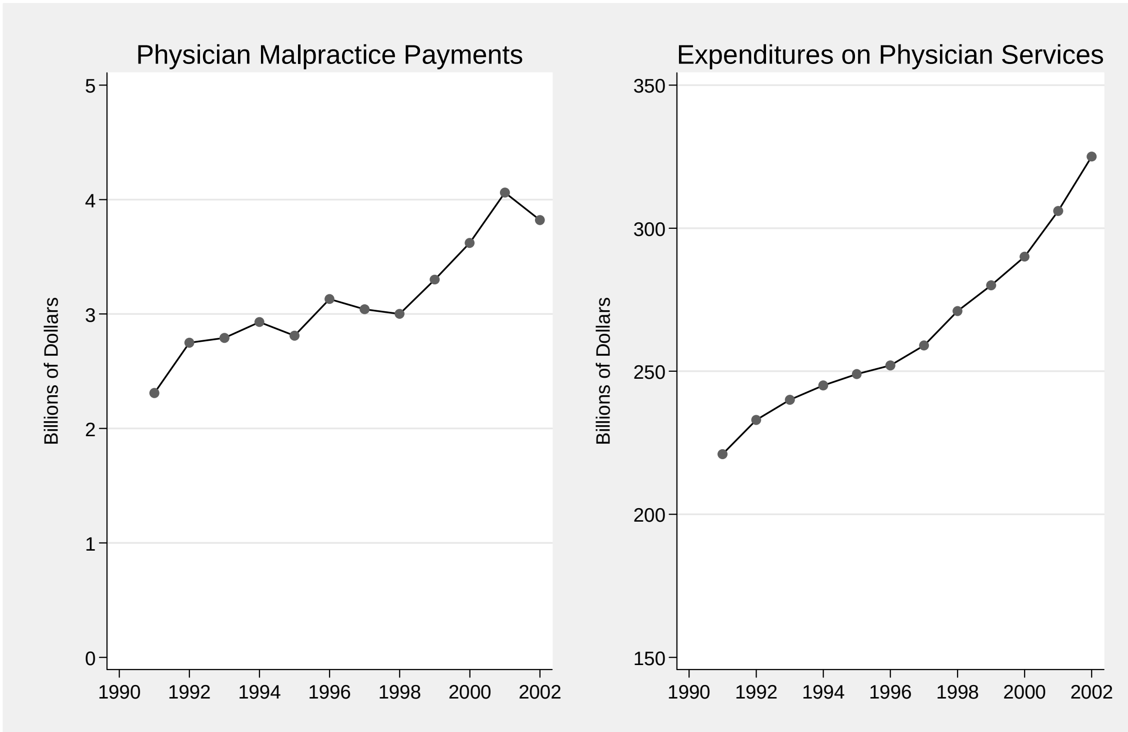
The key condition is that ε_0^C is bounded away from zero, which flows from the economics of the problem. Recall that the direct effect of malpractice on medical costs is equal to the share of malpractice costs in medical costs, $s > 0$. Moreover, the indirect effects must be nonnegative, since providers will weakly spend resources (not save them) in order to avoid risks that are imposed upon them. Risk-neutral providers will spend zero, but risk-averse providers will spend positive resources. Therefore, theory predicts that $\varepsilon_0^C \geq s > 0$, or that the cost elasticity is bounded away from zero.

Finally, note that the policy implications of the bootstrap procedure will be invalid only if $\varepsilon_0^C = \varepsilon_0^M = 0$, or that malpractice has no true effect on costs or mortality. If both these conditions held, the malpractice regime would have no costs and no benefits, rendering all policy reforms welfare-neutral.[52] Following this polar case to its logical conclusion, our policy recommendations would be welfare-neutral, rendering them at least weakly welfare-enhancing, even considering the possibility of invalidity for the bootstrap procedure.

[52] One might be concerned that there are other dimensions along which malpractice could affect welfare. However, all the major possibilities – e.g., morbidity, and legal costs – would have some effect on costs and/or mortality.

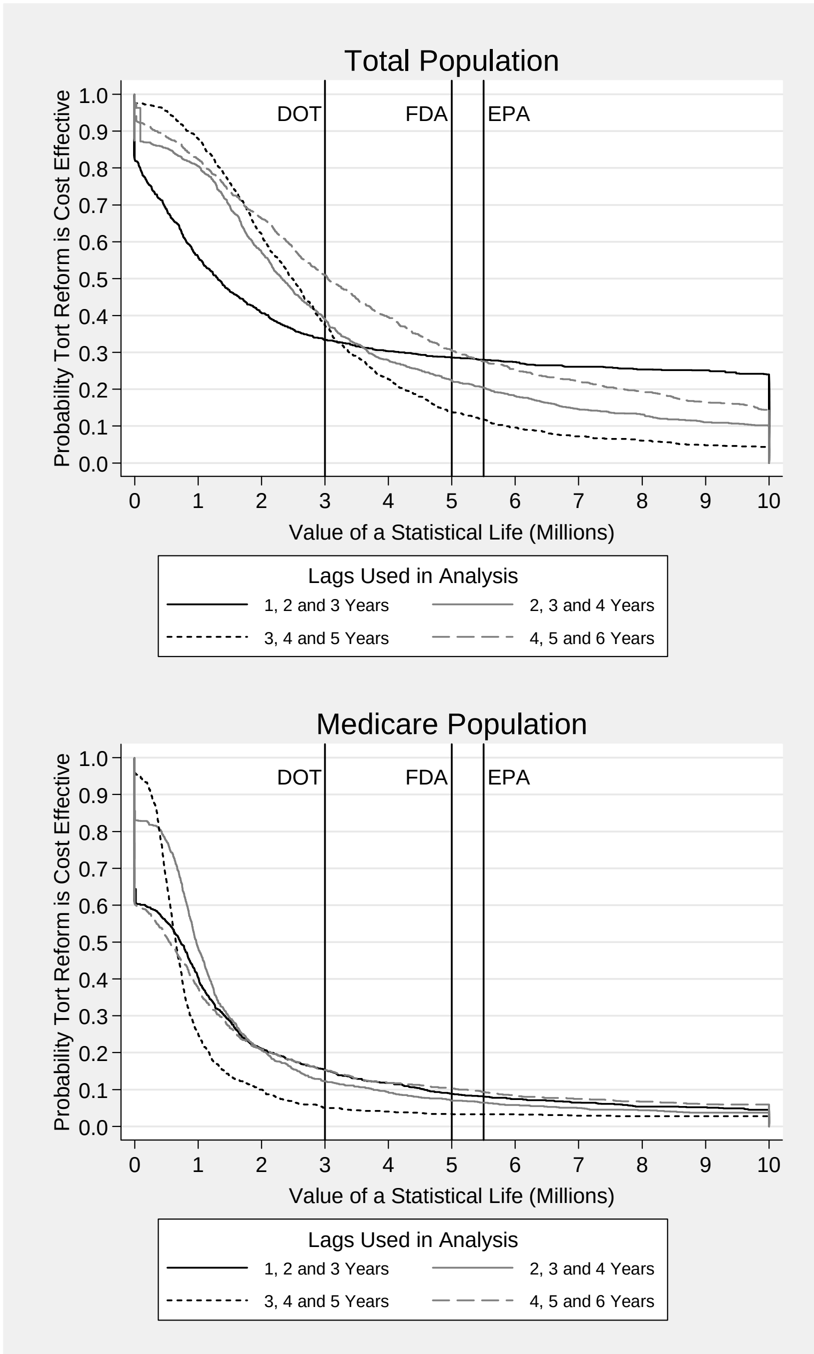
J. Figures

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



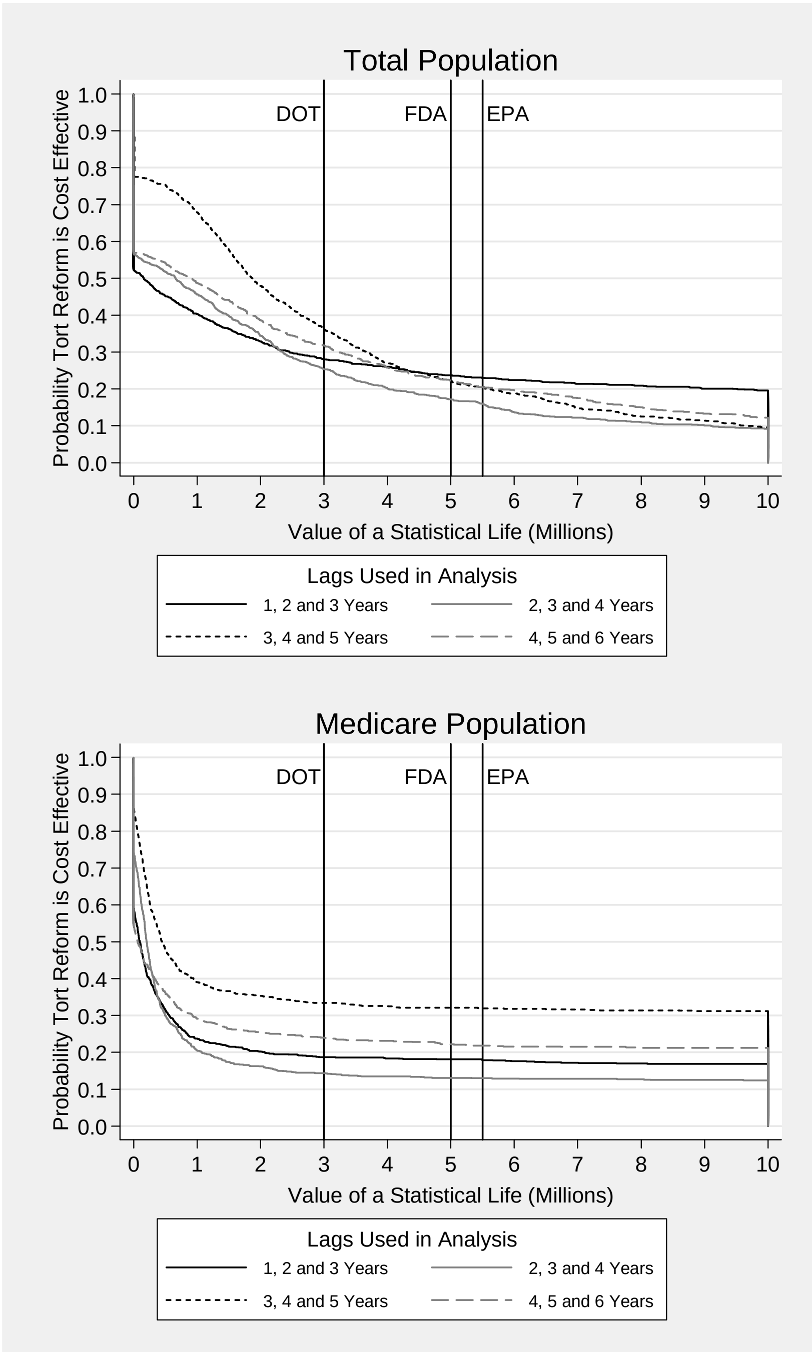
Notes: Data on physicians' medical malpractice payments consist of payments for settlements and judgments, as reported in the National Practitioners' Databank (NPDB). Expenditure data on physician services is based on the "Physician and Clinical Services" expenditures series in National Health Expenditures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

Figure 2: The welfare consequences of reducing malpractice costs.



Notes: The curves depict the empirical probabilities that the estimated dollars saved per life lost exceed the given value of a statistical life. The empirical probabilities are based on 1000 bootstrap replications of the IV models in equations 6, which yield elasticities of malpractice cost on county-level mortality, and separately on medical costs. Separate bootstrap procedures are run for models with different lag structures, as shown in the legend. Vertical lines correspond to values of statistical life (in year 2000 dollars) used by federal government regulatory agencies: Department of Transportation (DOT), Food and Drug Administration (FDA), and Environmental Protection Agency (EPA).

Figure 3: The welfare impact of reducing malpractice costs in states without noneconomic damage caps.



Notes: The curves depict the empirical probabilities that the estimated dollars saved per life lost exceed the given value of a statistical life. These are constructed identically as in Figure 2, except that in this figure, we drop data for California, which implement caps on noneconomic damages.

K. Tables

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

	Single Year		3-Year Moving Average	
	Unweighted	*Weighted*	*Unweighted*	*Weighted*
County-Level Means				
Total Malpractice Awards (thousands)	2,925	16,539	2,748	16,280
	(12,665)	(26,443)	(10,011)	(21,113)
Malpractice Awards Per Capita (dollars)	2.82	6.05	2.00	5.67
	(17.80)	(14.36)	(7.50)	(8.30)
Verdict-Level Means				
Average Noneconomic Award: All Cases (thousands)	140	307	104	286
	(528)	(590)	(298)	(423)
Average Economic Award: All Cases (thousands)	328	629	201	578
	(2,455)	(1,643)	(430)	(514)
Average Noneconomic Award: Malpractice Cases (thousands)	172	494	148	474
	(800)	(1,112)	(507)	(737)
Average Economic Award: Malpractice Cases (thousands)	381	1,096	291	1,049
	(2,690)	(2,649)	(896)	(1,452)
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, or a verdict, as appropriate. 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: Past malpractice verdicts as a measure of expected malpractice costs.

Dependent Variable: Current Total Malpractice Payments Per Capita at year t					
	(1)	(2)	(3)	(4)	(5)
Individual Lagged Malpractice Verdicts Per Capita					
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
Moving Average of Lagged Verdicts Per Capita					
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 per capita malpractice payments (from verdicts at trial and settlements) in the current year as the dependent variable against lagged values of per capita 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 3: 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 (Thousands)	295	301	240	299
	(233)	(231)	(220)	(252)
Hospital Facility Expenditures Per Bed Day	480	578	396	545
	(306)	(318)	(299)	(359)
Total Days Per Hospital Bed	737	589	707	637
	(796)	(560)	(821)	(697)
N	18,745		120,973	
County Medicare Expenditures				
Part A Expenditures Per Enrollee	2,223	2,517	2,226	2,385
	(828)	(1,033)	(843)	(879)
Part B Expenditures Per Enrollee	1,413	1,518	1,431	1,510
	(527)	(542)	(696)	(623)
County Demographics				
Per Capita Income	21,363	25,815	17,769	22,745
	(8,594)	(10,148)	(6,085)	(8,747)
Fraction Male	0.50	0.49	0.49	0.49
	(0.02)	(0.01)	(0.02	(0.01)
Fraction White	0.89	0.78	0.89	0.83
	(0.10)	(0.11)	(0.15	(0.14)
Fraction African-American	0.06	0.13	0.09	0.13
	(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 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	0.561**	0.627***	0.661***	0.653***	0.556***
	(0.228)	(0.154)	(0.170)	(0.154)	(0.136)
Wald Statistic	6.043**	16.606***	15.148***	17.935***	16.758***
R^2	0.594	0.794	0.790	0.780	0.790
N	15,237	12,835	12,632	12,445	12,264
County Level					
Malpractice Award Dollars Per Capita	1.164**	0.662***	0.648***	0.587***	0.530***
	(0.579)	(0.148)	(0.137)	(0.121)	(0.114)
Wald Statistic	4.038**	19.941***	22.404***	23.391***	21.684***
R^2	0.490	0.745	0.744	0.719	0.723
N	1,800	1,560	1,560	1,560	1,560

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 noneconomic 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									
Noneconomic Award (Hundreds of Thousands)	-53.016	490.978	619.709**	440.093	66.214	1,227.341***	2,455.520***	2,487.782***	1,008.758
	(356.230)	(325.314)	(312.037)	(384.229)	(157.818)	(450.318)	(588.735)	(506.919)	(646.066)
Dependent Variable: Hospital Facility Expenditures Per Bed Day									
Noneconomic Award (Hundreds of Thousands)	-0.303	0.712	1.271*	0.590	0.051	1.147	5.354***	4.702***	2.415
	(1.169)	(0.807)	(0.735)	(0.641)	(0.438)	(1.085)	(1.464)	(1.288)	(1.467)
Dependent Variable: Hospital Days Per Bed									
Noneconomic Award (Hundreds of Thousands)	0.073	0.261	1.128	-0.155	-0.296	0.422	-3.432	-4.504	-4.343*
	(1.412)	(1.636)	(1.522)	(1.299)	(0.567)	(1.790)	(3.047)	(2.956)	-2.509
County Level Estimates									
Dependent Variable: Medicare Part A Expenditures Per Enrollee									
Noneconomic Award (Hundreds of Thousands)	3.231	1.375	5.216	6.395	1.696	21.826**	32.030***	24.969**	24.559*
	(4.894)	(4.964)	(5.518)	(7.661)	(2.120)	(8.799)	(11.775)	(10.930)	(13.053)
Dependent Variable: Medicare Part B Expenditures Per Enrollee									
Noneconomic Award (Hundreds of Thousands)	1.239	1.760	2.608	3.235	1.497	10.287**	7.852	5.294	5.315
	(2.117)	(2.661)	(2.680)	(3.016)	(1.193)	(4.212)	(4.752)	(4.947)	(5.367)

Note: Table shows the reduced-form estimates of average noneconomic damage awards on hospital and Medicare expenditures. Each coefficient is from a separate regression, 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 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, as well as a quadratic for personal income per capita, the percent of the population that is male, white, African American, of Hispanic ethnicity, 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 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: Noneconomic	(4) Jury Award: Noneconomic
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 noneconomic) 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					
Malpractice Awards Per Capita	-138.819	18.865	-169.979	93.979	275.034
	(139.147)	(221.731)	(270.948)	(221.042)	(327.488)
Elasticity	-0.0030	0.0004	-0.0031	0.0016	0.0044
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards Per Capita	-0.411***	-0.200	-0.393	0.048	-0.566
	(0.155)	(0.632)	(0.598)	(0.430)	(0.603)
Elasticity	-0.0043	-0.0020	-0.0037	0.0004	-0.0051
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards Per Capita	0.162	-0.642	-0.936	-1.215	-0.406
	(0.281)	(1.009)	(0.872)	(0.752)	(0.905)
Elasticity	0.0017	-0.0063	-0.0089	-0.0111	-0.0036
IV Estimates					
Dependent Variable: Hospital Facility Expenditures Per Bed					
Malpractice Awards Per Capita	117.956	1,958.187***	3,712.756***	3,809.226***	1,813.872
	(296.050)	(618.148)	(912.643)	(1,091.975)	(1,250.834)
Elasticity	0.0026	0.0379	0.0674	0.0647	0.0288
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards Per Capita	0.090	1.829	8.095***	7.200***	4.342
	(0.789)	(1.687)	(1.797)	(1.506)	(2.713)
Elasticity	0.0009	0.0179	0.0771	0.0667	0.0391
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards Per Capita	-0.527	0.674	-5.189	-6.896	-7.809*
	(1.056)	(2.930)	(4.060)	(4.314)	(4.434)
Elasticity	-0.0055	0.0066	-0.0493	-0.0632	-0.0690

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 Per Capita	0.455	5.527**	7.951***	5.397*	3.160
	(0.865)	(2.496)	(2.899)	(2.782)	(3.281)
Elasticity	0.0012	0.0126	0.0174	0.0114	0.0064
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards Per Capita	0.806*	2.743**	1.193	-0.981	-1.489
	(0.421)	(1.077)	(1.349)	(1.724)	(1.747)
Elasticity	0.0035	0.0104	0.0043	-0.0034	-0.0050
IV Estimates					
Dependent Variable: Medicare Part A Expenditures Per Enrollee					
Malpractice Awards Per Capita	1.623	31.774***	46.884***	41.216***	41.881**
	(1.960)	(11.560)	(11.825)	(13.858)	(19.688)
Elasticity	0.0043	0.0723	0.1028	0.0872	0.0850
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards Per Capita	1.390	14.567**	12.149*	9.106	9.493
	(1.249)	(6.049)	(6.449)	(7.833)	(9.389)
Elasticity	0.0061	0.0554	0.0442	0.0320	0.0318

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: The Impact of Malpractice on County Level Mortality.

	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 Deaths Per 1,000 Population		
Malpractice Awards	-0.001	-0.017	-0.018	-0.023*	-0.011
Per Capita	(0.002)	(0.010)	(0.011)	(0.013)	(0.010)
Elasticity	-0.0010	-0.0123	-0.0129	-0.0169	-0.0086
			Non-Accidental Deaths Per 1,000 Population		
Malpractice Awards	-0.001	-0.020**	-0.021*	-0.023*	-0.012
Per Capita	(0.002)	(0.010)	(0.011)	(0.014)	(0.010)
Elasticity	-0.0010	-0.0153	-0.0159	-0.0178	-0.0093
			IHD Deaths Per 1,000 Population		
Malpractice Awards	-0.003	0.001	0.005	0.005	0.004
Per Capita	(0.002)	(0.004)	(0.006)	(0.008)	(0.007)
Elasticity	-0.0078	0.0019	0.0152	0.0165	0.0117
			Deaths Per 1,000 Age 20 to 64		
Malpractice Awards	-0.002	-0.019***	-0.017**	-0.019	-0.016
Per Capita	(0.002)	(0.007)	(0.007)	(0.013)	(0.014)
Elasticity	-0.0034	-0.0310	-0.0278	-0.0330	-0.0288
			Deaths Per 1,000 Age 65 and up		
Malpractice Awards	-0.000	-0.036	-0.063	-0.076	-0.009
Per Capita	(0.010)	(0.053)	(0.056)	(0.054)	(0.053)
Elasticity	-0.0000	-0.0041	-0.0074	-0.0090	-0.0011

Notes: The table reports the estimated effect of per capita malpractice jury award dollars on aggregate mortality rates. 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 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 10: Distribution of estimated effects of malpractice on mortality and cost.

Hospital Expenditures

		Lagged:			
		1, 2, 3	2, 3, 4	3, 4, 5	4, 5, 6
IV Model	Coeff.	1.59	5.04	7.00	5.55
	Std.Er.	1.39	1.99	2.36	2.74
	Elasticity	0.019	0.059	0.08	0.061
	Pr(b<0)	**0.13**	**0.007**	**0.002**	**0.02**
Bootstrap	**Pr(b<0)**	**0.24**	**0.004**	**0.003**	**0.05**

Total Medicare Expenditures

		Lagged:			
		1, 2, 3	2, 3, 4	3, 4, 5	4, 5, 6
IV Model	Coeff.	45.2	58.2	47.0	52.1
	Std.Er.	21.3	21.8	24.1	33.1
	Elasticity	0.064	0.08	0.062	0.066
	Pr(b<0)	**0.02**	**0.005**	**0.03**	**0.06**
Bootstrap	**Pr(b<0)**	**0.02**	**0.003**	**0.03**	**0.05**

Total Mortality

		Lagged:			
		1, 2, 3	2, 3, 4	3, 4, 5	4, 5, 6
IV Model	Coeff.	-0.017	-0.018	-0.023	-0.011
	Std.Er.	0.010	0.011	0.013	0.010
	Elasticity	-0.012	-0.013	-0.017	-0.009
	Pr(b>0)	**0.05**	**0.06**	**0.05**	**0.13**
Bootstrap	**Pr(b>0)**	**0.22**	**0.13**	**0.02**	**0.07**

Mortality Over Age 65

		Lagged:			
		1, 2, 3	2, 3, 4	3, 4, 5	4, 5, 6
IV Model	Coeff.	-0.036	-0.063	-0.076	-0.009
	Std.Er.	0.053	0.056	0.054	0.053
	Elasticity	-0.004	-0.007	-0.009	-0.001
	Pr(b>0)	**0.25**	**0.13**	**0.08**	**0.43**
Bootstrap	**Pr(b>0)**	**0.40**	**0.17**	**0.05**	**0.42**

Notes: Table illustrates, for consistent sampling schemes, properties of IV and bootstrap estimates for effects of malpractice on medical costs and mortality. For comparison, the tables report p-values for one-tailed tests of whether coefficients are greater than or less than zero.

Table 11: 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	0.806	-0.527	8.688***	7.797***	5.219*
Per Capita	(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	-1.328	4.567	-7.259	-6.963**	-9.311**
Per Capita	(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	-0.048	0.658	-0.401	-0.810**	-1.398***
Per Capita	(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	1.880	23.458**	44.905***	43.946***	49.876**
Per Capita	(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	0.668	8.821	31.044***	34.655***	29.853***
Per Capita	(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	0.898	10.921*	8.074	9.364	11.418
Per Capita	(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	0.588	6.836	5.569	8.404	10.830*
Per Capita	(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 12: Noneconomic awards and malpractice premiums.

	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
			Insurer Level Regressions		
			Dependent Variable: Annual Malpractice Premium		
Noneconomic Award	19.619	708.966**	613.526	782.747*	1,202.821*
(Hundreds of Thousands)	(64.222)	(334.244)	(394.698)	(424.125)	(638.348)
R^2	0.83	0.83	0.82	0.82	0.81
Fixed effects:			County, Year, Specialty		
Noneconomic Award	34.904	561.774	633.835	766.112*	1,073.796*
(Hundreds of Thousands)	(47.265)	(368.108)	(447.993)	(448.421)	(596.661)
R^2	0.84	0.84	0.83	0.84	0.83
Fixed effects:			Insurer, County, Year, Specialty		
			County Level Regressions		
			Dependent Variable: Average Annual Malpractice Premium		
Noneconomic Award	19.619	711.020**	490.136	627.775	954.619*
(Hundreds of Thousands)	(64.222)	(359.205)	(352.018)	(407.348)	(574.220)
R^2	0.83	0.85	0.85	0.85	0.84
Fixed effects:			County, Year, Specialty		

Notes: Table reports the regressions of annual malpractice premiums against different lags of the average noneconomic damage awards in tort cases. Premium data come from the Medical Liability Monitor (MLM), and are measured at the county-county-year level for three specialties: internal medicine, general surgery, and OBGYN. County level regressions take the mean across all companies reporting in a county-year. Regressions are weighted by the county population. Standard errors adjusted to reflect clustering by county are reported in parentheses.

Table 13: Varying the construction of expected malpractice costs.

	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 14: 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 15: The Impact of Average Noneconomic Damage Awards in a County on the Probability of Facing a Malpractice Suit.

	Any Claim		Claim with Defense Costs	
Noneconomic Award	0.006	0.005	0.007	0.006
(Hundreds of Thousands)	(0.003)**	(0.004)	(0.002)***	(0.003)*
Elasticity	0.083	0.069	0.138	0.122
Physician fixed effects:	No	Yes	No	Yes

Notes: The table reports the estimated effect of average noneconomic jury award dollars in tort cases on the probability of being sued. The model is estimated as a linear probability model with an indicator variable indicating a lawsuit reported against a physician in a year as the dependent variable. The coefficient is reported for the moving average of noneconomic damage awards in plaintiff wins in tort cases lagged 1, 2 and 3 years. Each coefficient is from a separate regression, and each row represents a different lag for the malpractice variable. The unit of analysis is a physician-year. County population is used as a weight in all regressions. Other explanatory variables include indicators for physician specialty and physician age, 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 physician level are reported in parentheses. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.

Table 16: 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 17: 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.

Table 18: County Mortality and Noneconomic Damage Awards: Causality Test 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
Dependent Variable: Total Deaths Per 1,000 Population									
Noneconomic Award (Hundreds of Thousands)	-0.014	-0.011	-0.008	-0.008	-0.002	-0.011	-0.011	-0.013	-0.006
	(0.010)	(0.012)	(0.011)	(0.009)	(0.002)	(0.008)	(0.007)	(0.008)	(0.005)
Dependent Variable: Non-Accidental Deaths Per 1,000 Population									
Noneconomic Award (Hundreds of Thousands)	-0.014	-0.012	-0.009	-0.008	-0.002	-0.013*	-0.013*	-0.013	-0.006
	(0.009)	(0.012)	(0.011)	(0.009)	(0.002)	(0.007)	(0.008)	(0.009)	(0.005)
Dependent Variable: IHD Deaths Per 1,000 Population									
Noneconomic Award (Hundreds of Thousands)	0.001	-0.006	-0.006	-0.006	-0.003**	0.000	0.003	0.003	0.002
	(0.005)	(0.005)	(0.004)	(0.004)	(0.001)	(0.003)	(0.003)	(0.004)	(0.003)
Dependent Variable: Deaths Per 1,000 Age 20 to 64									
Noneconomic Award (Hundreds of Thousands)	-0.010	-0.008	-0.008	-0.009	-0.002	-0.012**	-0.010**	-0.011	-0.008
	(0.007)	(0.007)	(0.007)	(0.007)	(0.002)	(0.005)	(0.005)	(0.008)	(0.007)
Dependent Variable: Deaths Per 1,000 Age 65 and up									
Noneconomic Award (Hundreds of Thousands)	-0.061	-0.035	-0.009	-0.002	-0.000	-0.023	-0.039	-0.042	-0.005
	(0.044)	(0.057)	(0.050)	(0.037)	(0.011)	(0.035)	(0.036)	(0.034)	(0.027)

Note: Table shows the reduced-form estimates of average noneconomic damage awards on county level mortality. Each coefficient is from a separate regression, with each column representing a different lag or lead for the noneconomic damages. 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. 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.