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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 the rising cost of medical care. We use variation in the generosity of local juries to identify the causal impact of malpractice liability on medical costs, mortality, and social welfare. The effect of malpractice on medical costs is large relative to its share of medical expenditures, but relatively modest in absolute terms—growth in malpractice payments over the last decade and a half contributed at most 5.0% to the total real growth in medical expenditures, which topped 33% over this period. On the other side of the ledger, malpractice liability leads to modest reductions in patient mortality; the value of these more than likely exceeds the cost impacts of malpractice liability. Therefore, policies that reduce expected malpractice costs are unlikely to have a major impact on health care spending for the average patient, and are 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 as a major problem facing the US health care system (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. Republican senators, governors, and even presidents, have echoed this sentiment, and repeatedly pointed to rising malpractice costs as a major driver of growth in health care spending.[1] Even many Democratic politicians who oppose limits on damages show little faith in the present tort liability system. For instance, President Obama has expressed opposition to damage caps, but he and Senator Hilary Clinton have identified the current liability environment as a major contributor to adverse outcomes for patients (Clinton and Obama, 2006).

The hardened political lines around malpractice reform belie major gaps in our understanding of the issue. Both the CBO (2006) and the GAO (1999) have concluded that the overall effects of malpractice exposure on total health spending and outcomes are simply not known.[2] Fueling the political debate have been several apparently contradictory facts about malpractice. Opponents of malpractice reform often note that malpractice payments are a relatively small fraction – between 1 and 2 percent – of total expenditures on physicians. However, supporters of reform point to the recent and dramatic rise in malpractice costs, as documented in Figure 1 (also see Mello et al., 2003). According to data from the National

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

[2] Two recent papers by Baicker and Chandra (2006; 2007) — post-dating the CBO and GAO reports — tackle the overall effects. Using cross-sectional and longitudinal variation by state, they find that malpractice risk has little impact on total costs, but substantial impacts on specific procedures, like medical imaging. As Baicker and Chandra note, however, a strategy for causal inference is required, in order to elicit the policy implications of these relationships.

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] Moreover, previous research provides a mechanism for even modest malpractice payments to have a disproportionately large effect on total medical costs. Using data on elderly heart attack patients, 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.

The identification of "defensive medicine" by Kessler and McClellan has been confirmed and deepened by a number of important studies. For instance, Currie and MacLeod (2008) recently demonstrated that tort reforms impact the behavior of obstetricians both in the large and even in subtly predictable ways. Baicker and Chandra (2007) find evidence that malpractice pressure is associated with more diagnostic imaging. 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), more general medical practices (Bovbjerg et al., 1996), and the labor supply of physicians (Encinosa and Hellinger, 2005; Kessler et al., 2005; Klick and Stratmann, 2007; Matsa, 2007; Helland and Showalter, 2008).[5]

While the mechanisms from malpractice to medical care have been carefully established, the overall welfare consequences of reducing malpractice costs have not been as well documented, for two reasons. First, much of the causal research on defensive medicine has

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[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*.

[5] In contrast, however, other research has found that malpractice premium growth does not adversely affect the net income of physicians (Pauly et al., 2006).

justifiably focused on particular kinds of patients, so as to homogenize the study population. For example, Kessler and McClellan caution that their studies were designed not to estimate the impact of malpractice risk on total medical spending, but rather the impacts on the treatment, costs, and outcomes of heart attack patients (Kessler and McClellan, 1996; Kessler and McClellan, 2002a; Kessler and McClellan, 2002b). Other researchers have focused on different subpopulations like expectant mothers (cf, Dubay et al., 1999; Currie and MacLeod, 2008). Unfortunately, the effects of malpractice are not always uniform and are difficult to generalize across the population (Congressional Budget Office, 2006).

Second, the dominant identification strategy to date has relied upon state-level tort reform. The empirical evidence supports tort reform's validity in the frequently studied contexts of heart attack patients and expectant mothers (cf, Kessler and McClellan, 2002a; Currie and MacLeod, 2008). However, questions have been raised about validity in the study of overall medical costs and outcomes. Danzon (2000) argues that states with managed care may have been more likely to adopt tort reform measures, and that the cost savings attributed to tort reform could be a result of managed care itself.[6] In the context of overall costs, the CBO finds empirical evidence consistent with this argument: reform states are more likely to have slower growth in total health care spending prior to the reform's adoption (Congressional Budget Office, 2006).

In this paper, we attempt to quantify the social welfare effects of medical malpractice, and to do so with a new identification strategy. Our strategy relies on changes over time in the generosity of local juries. Using this approach, we find that malpractice risk increases medical spending in the aggregate, above and beyond its direct effects, but the total impact on overall

[6] However, also see Arlen and MacLeod (2005), who point out that managed care organizations are generally not held liable for physician negligence.

medical costs is relatively modest. A ten percent reduction in malpractice costs would reduce total health care expenditures by, at most, 1.2 percent. During the malpractice "crisis" of the 1990s, we predict this would have added about 5 percent to real hospital expenditure growth, which exceeded 33% over this period. Using the same identification strategy, we find that, on the margin, 10 percent increases in malpractice costs reduce mortality by a modest amount, approximately 0.2%. Using values of a statistical life most commonly adopted by US regulators, the value of the mortality decline more than likely outweighs the increase in medical costs. On balance, reducing malpractice costs is more likely to harm than improve social welfare.

We proceed as follows. In Section B, we present our framework for analysis. Section C describes our data sources. Section D investigates issues of measurement and identification. Section E presents our results for costs, mortality, and overall welfare. Finally, we conclude with a discussion of directions for future research.

B. Analytical Approach

B.1 Theoretical Background

Our approach to welfare can be summarized in a stylized model with a single composite health care output, q. The *medical* cost of this output is $c(q)$. Total malpractice payments to patients, $MedMal$, vary with output, since providers can be sued for the failure to diagnose, treat, or treat appropriately (Kessler and McClellan, 1996). Therefore, the equilibrium value of q depends on providers' expectations about malpractice payments,[7] which in turn depend on an exogenous variable X.

[7] Specifically, quantity depends on the *uninsured* portion of expected malpractice costs, since fully insured providers have no incentives to reduce the probability of lawsuits. This

Malpractice payments themselves represent a pure transfer between providers and patients. However, several studies have concluded that the deadweight costs of litigation are approximately equal to payments to victims (Kakalik and Pace, 1986; Studdert et al., 2006).[8] Therefore, we approximate the expected social cost of medical care, including malpractice, as:

$$E\{SC(q,X)\} = E\{MedMal(q,X)\} + E\{c(q)\} \tag{1}$$

The causal effect of exogenous changes in malpractice payments on social cost is then:

$$E\left\{\frac{\frac{dSC(q,X)}{dX}}{\frac{dMedMal}{dX}}\right\} = \left(1 + E\left\{\frac{c'(q)\frac{dq}{dX}}{\frac{dMedMal}{dX}}\right\}\right) \tag{2}$$

The first right-hand term (equal to unity) is the direct effect of malpractice, holding behavior constant. The second captures the indirect effects on quantity often labeled “defensive medicine” – the provision of services designed to ward off lawsuits. Converting to an elasticity:

$$E\left\{\frac{\frac{dSC(q,X)}{dX}}{\frac{dMedMal}{dX}}\frac{MedMal}{SC}\right\} = E\left(\frac{MedMal}{SC} + \left\{\frac{c'(q)\frac{dq}{dX}}{\frac{dMedMal}{dX}}\frac{MedMal}{SC}\right\}\right) \tag{3}$$

The “direct” component of the elasticity equals the share of malpractice in total medical costs. An elasticity larger than this indicates the presence of an indirect behavioral effect.

If $V(q)$ is the gross value of output to the representative consumer, we can write the causal effect of expected malpractice cost on net social welfare as:

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distinction is less relevant for hospitals, 40% of whom are self-insured (GAO, 2003). However, even market insurance fails to cover the substantial time and reputational costs associated with being sued (Kessler and McClellan, 2002b; Currie and MacLeod, 2008).[7]

[8] While the social cost of out-of-court settlements are likely lower than that of litigation, we conservatively assume they are equal. This biases us in favor of malpractice cost reductions.

$$E\left\{\frac{\frac{dV(q)}{dX}}{\frac{dMedMal}{X}}-\frac{\frac{dSC(q,X)}{dX}}{\frac{dMedMal}{X}}\right\}=E\left\{\frac{V'(q)\frac{dq}{dX}}{\frac{dMedMal}{X}}-\left(1+\frac{c'(q)\frac{dq}{dX}}{\frac{dMedMal}{X}}\right)\right\} \qquad (4)$$

The first term is the gross value (if any) of the additional output induced by malpractice. Due to the relatively high economic value of life and the difficulty of measuring morbidity, we focus on mortality as a single index of health care output.[9] To evaluate welfare consequences, we assess whether the expected cost of malpractice in dollars per life saved is larger or smaller than the (approximately constant) dollar value of a life to society, or:

$$E\left\{\frac{\frac{c'(q)\frac{dq}{dX}}{\frac{dMedMal}{dX}}+1}{\frac{\frac{dq}{dX}}{\frac{dMedMal}{dX}}}\right\}\overset{?}{>}V'(q) \qquad (5)$$

B.2 Estimation Approach

The welfare criterion in expression 5 depends on the ratio between the cost effect of malpractice, and its mortality effect. Estimating these two quantities is complicated by reverse causality and measurement error. If defensive medicine is effective, areas with high medical costs (or better health outcomes) may have fewer lawsuits, precisely because providers are responding to malpractice incentives. This reverse causality problem may bias ordinary least squares estimates towards zero. In addition, providers respond not to actual malpractice cost, but to expected malpractice costs, which are unobservable. The resulting measurement error reinforces the problem of reverse causality by biasing estimated coefficients towards zero.

[9] In this respect, we follow Kessler and McClellan (1996).

B.2.1 *Empirical Framework*

To address reverse causality and measurement error, we employ instrumental variables models of the cost and mortality outcomes. Defining outcome Y_{ct} for county c at time t, we estimate:

$$
\begin{aligned}
Y_{ct} &= \beta_0 + \beta_1 E^*\left(MedMal_{ct}\right) + \beta_2 \vec{X}_{ct} + \phi_c + \gamma_t + \varepsilon_{ct} \\
E^*\left(MedMal_{ct}\right) &= a^* + b^* MA(\mathrm{V}_{c,t-i}) + e_{ct} \\
MA(\mathrm{V}_{c,t-i}) &= \alpha_0 + \alpha_1 Z_{ct} + \alpha_2 \vec{X}_{ct} + \phi_c + \gamma_t + \delta_{ct}
\end{aligned} \tag{6}
$$

$E^*\left(MedMal_{ct}\right)$ is the expected direct cost of malpractice in county c at time t. Providers' behavior depends upon their expectations about malpractice costs, rather than the realized costs in any given period. While these are unobserved, we proxy for currently expected malpractice costs using past malpractice jury awards, and specifically, a moving average of past awards, $MA(V_{c,t-i})$.[10] Z_{ct} is our instrument, which is discussed below. $\vec{X}_{c,t}$ is a vector of other county characteristics, including county demographics and the composition of cases (timed to coincide with $MA(V_{c,t-i})$). ϕ_c and γ_t are county and year fixed-effects, respectively. For some specifications, we run models at the hospital-, rather than county-level. These incorporate hospital, rather than county, fixed-effects.

Substituting for the unobserved variable $E^*(MedMal_{ct})$ yields the estimating equations:

$$
\begin{aligned}
Y_{ct} &= (\beta_0 + \beta_1 a^*) + \beta_1 b MA(\mathrm{V}_{c,t-i}) + \beta_2 \vec{X}_{ct} + \phi_c + \gamma_t + (\varepsilon_{ct} + \beta_1 e_{ct}) \\
MA(\mathrm{V}_{c,t-i}) &= \alpha_0 + \alpha_1 Z_{ct} + \alpha_2 \vec{X}_{ct} + \phi_c + \gamma_t + \delta_{ct}
\end{aligned} \tag{7}
$$

As long as the instrument Z_{ct} satisfies standard validity conditions, we can consistently estimate the extra medical cost incurred per life saved. This is the key welfare parameter on the left-hand

[10] An alternative proxy would be total payments, including settlements. Unfortunately, settlement data are not available at the county-level. Below, we present evidence that, at the state-level, jury awards perform reasonably well at predicting total settlements.

side of expression 5. In other words, estimation of the welfare parameter is robust to the error in measuring expected malpractice costs.

If the instrument satisfies the stronger condition of orthogonality to the measurement error e_{ct}, we can also recover consistent estimates of β_1. If this stronger condition fails to be met, the IV model will overstate the impact of malpractice on the outcome variables. All these results are proven in Appendix A.

B.2.2 *Approach to Identification*

Increases in the generosity of local juries will tend to shift out expected malpractice costs. Total jury awards consist of three components: economic damages, noneconomic damages, and punitive damages. Economic damages represent concrete, measurable losses to the plaintiff, including lost wages, medical costs, and other out-of-pocket costs. Since these incorporate medical costs directly, we might be concerned that local trends in medical costs will drive economic damages in an empirically problematic fashion. Punitive damages, very rare in malpractice cases, are also less relevant.

Noneconomic damages, in contrast, compensate a plaintiff for his/her unquantifiable "pain and suffering." We argue that, when juries value the severity of this suffering, their determinations are plausibly exogenous to local trends in medical costs, care, and outcomes. Therefore, as our instrument, we use $MA(Nonecon_{c,t-i})$, past trends in mean noneconomic damage awards per plaintiff victory, in county c. If the instrument is valid, unusually rapid growth in noneconomic damage awards will lead to growth in expected malpractice payments, but growth in medical costs, care, and outcomes will not cause changes in noneconomic damages. To support validity, we provide the following pieces of evidence.

1. Noneconomic damage awards are statistically unrelated to past and current trends in medical costs and mortality (Section D.2.1).
2. Current and past noneconomic damage awards predict future trends in medical costs and mortality (Section D.2.1).
3. Past growth in noneconomic damage awards is correlated with increases in the propensity of patients to file malpractice claims against providers (Section D.3).
4. Past growth in noneconomic damages is correlated with higher malpractice premia (Section D.3).

Finally, we address a specific threat to validity. If jurors use simple “rules of thumb” to construct noneconomic damages as a mechanical function of medical costs awarded, this would invalidate the instrument. We show that, while plaintiffs’ claimed medical losses are highly predicted of their economic damages award, they are entirely uncorrelated with the noneconomic damage award (Section D.2.2). To be clear, this does not imply that noneconomic awards are always uncorrelated with economic awards, rather it implies that the determinants of plaintiff medical costs appear unrelated to pain and suffering awards. It is the latter condition that serves as our identifying assumption, rather than a stronger claim about the independence between economic and noneconomic damage awards.

C. Data

Our econometric approach requires data on jury verdicts, medical costs, mortality, and county-level characteristics. We measure medical costs using data from Medicare and total hospital spending. These components together cover about two-thirds of health care spending exposed to malpractice liability. The remainder consists of physician services paid for outside Medicare.

C.1.1 *Jury Verdicts Data*

We use the RAND Jury Verdicts Database (JVDB) to recover the verdicts data. The RAND JVDB contains information from 1985 to 1999 on jury verdicts occurring in all counties in the states of New York 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 (125 counties in all). These data cover 23.6% of the total US population, as of the 2000 Census. Table 1 reports the geographic composition of the JVDB counties, ranked in decreasing order of population size. The Los Angeles metro area accounts for about 15% of the population covered by our sample, and the New York City metro area accounts for about the same. The cities outside New York and California represent about one-fifth of the data's population weight.

The data in the JVDB are collected from court reporter publications, trade publications that provide trial attorneys with information on verdicts in local courts.[11] The JVDB includes data on plaintiff win rates, average economic and noneconomic damage awards and type of injury for medical malpractice and other tort cases.

In Table 2, 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. 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 $t-1$, $t-2$,

[11] Some researchers have noted 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 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. Finally, verdict reporters represent a primary source of information upon which agents base expectations.

and $t - 3$. On a per capita basis, the average county hands out $2.87 in malpractice awards. Larger counties tend to award more per capita: the population-weighted county average is $6.07. The average verdict in our sample involves a noneconomic award of $142,000, 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 *Hospital Costs and Utilization*

Data on hospital spending, utilization, and facilities come from the American Hospital Association (AHA) database. 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.[12] Hospitals report information longitudinally. All costs, here and throughout the paper, are deflated over time using the overall Consumer Price Index.[13]

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 around 5 percent. While the differences are modest, the JVDB sample somewhat over-represents large counties.

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[12] 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.

[13] 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.

Appendix B presents a formal statistical analysis demonstrating that this tendency does not alter our primary empirical results. Since the data also over-represent New York and California, Appendix G demonstrates and discusses the similarity of our key results across California, which has noneconomic damage caps, and the uncapped state of New York.

C.1.3 *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.[14] 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.

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[15] of Part A beneficiaries are enrolled in Part B. Therefore, we focus on costs per enrollee, rather than impacts on enrollment *per se*.

C.1.4 *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)

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[14] 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. The mismatch induces measurement error in the dependent variable.

[15] Based on CMS enrollment data from 2004, available from the authors or at http://www.cms.hhs.gov/MedicareEnRpts/Downloads/Sageall04.pdf

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

D. Measurement and Identification

D.1 Measuring Expected Malpractice Cost

We measure the currently expected cost of malpractice using past malpractice trial verdicts from the JVDB. Even though approximately 90% of malpractice claims are settled out-of-court, this is theoretically plausible for two 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.[16] Second, the expected size of a verdict will influence pre-trial negotiation and settlement. Therefore, information about past trial verdicts may be enough to draw educated inferences about expected total costs, of verdicts plus settlements.

[16] As we exploit later, state-level data on malpractice settlements are publicly available, but more local measures are not made public.

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

$$E(MedMal_{ct} \mid V_{c,t-1}, V_{c,t-2}, \ldots), \tag{8}$$

where $V_{c,t-i}$ represents malpractice jury verdicts in county c and time $t-i$. To assess the signal-to-noise ratio in this strategy, we turn to an external data source, the 1990-2005 National Practitioner Data Bank (NPDB), which reports both malpractice jury verdicts and total malpractice settlements, but only at the state-level. For state s at time t, we estimate:

$$MedMal_{st} = \varphi_0 + \sum_{i=1}^{6} \varphi_i V_{s,t-i} + \omega_{st} \tag{9}$$

This regression tests past jury verdicts as predictors of current malpractice payments. Both the payments and verdicts variables are calculated on a per capita basis.[17]

The results of this regression appear in Table 4, which reports models using 5 different specifications, differing in the included lags. Column 1 reports a regression of $MedMal_{st}$ on $V_{s,t-1}$ through $V_{s,t-6}$, and the corresponding regression of $MedMal_{st}$ on the moving average of $V_{s,t-1}$ through $V_{s,t-6}$. Similarly, column 2 repeats this for lags $V_{s,t-1}$ through $V_{s,t-3}$, and so forth.

On their own, past 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 malpractice verdicts are defensible, if imperfect, proxies of payments.

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

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

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.

D.2 Instrument Validity

As an instrument for expected malpractice costs, we use mean noneconomic damages awarded by juries in plaintiff victories. The identifying assumption is the local trends in noneconomic damages are not caused by local trends in medical costs and outcomes, or their determinants. We provide evidence for this assumption, along with evidence that variation in noneconomic damages is relevant, in the sense that agents throughout the medical and legal systems update their beliefs and behavior in response to changing noneconomic damage awards.

D.2.1 *Temporal Tests of Validity*

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 instrumental variables model in equations 7, 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 current health expenditures on *future* values of the instrument, as a falsification test. If health costs cause verdicts, we should see a relationship between current costs and future noneconomic awards. Table 5 presents the results for four different expenditure measures as dependent variables, and one dependent variable measuring inpatient utilization. There are 16 regressions testing the causal link from lagged noneconomic damages to current medical spending (i.e., the 4 right-most columns); 11 yield significant effects at the 10% level. On the other hand, only one of the 20 regressions testing the opposite effect — of current health care spending on the current year or leads of noneconomic damages —are significant. This result is not an artifact of differences in power, since the regressions have narrower confidence intervals when we test for the reverse causality running from medical spending to noneconomic damages. Also 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, or that an unobserved third factor simultaneously drives verdicts and medical spending.[18]

For utilization, we find little evidence of reverse causality, but also no reduced-form effects. This is consistent with our later findings that malpractice has little estimated impact on inpatient utilization.

D.2.2 ***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.[19] The first two columns of the table illustrate the

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[18] 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.

[19] Mean medical costs are quantitatively insignificant for non-malpractice cases.

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.

As we would expect, claimed medical losses affect the size of the economic award. 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 (individually or jointly).

D.2.3 *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 overall 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.[20] 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).

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[20] 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 E.3, and Appendix G.

D.3 Power and Relevance of the Instrument

We begin by demonstrating a first-stage relationship between noneconomic damages and jury verdicts in malpractice cases. We then provide evidence that noneconomic damages also influence expectations about malpractice costs, and thus provide *meaningful* first-stage variation.

Table 7 displays the first-stage relationship between local trends in noneconomic 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. The first-stage model is always run 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.

Our first stage treatment effect is meaningful only if noneconomic awards influence expected malpractice costs for health care providers, insurers and patients. First, Section D.2.1 showed that past noneconomic damage awards predict current medical costs, but that the reverse is not true, consistent with the theory that provider behavior is influenced by noneconomic awards. Second, to verify that noneconomic awards influence the costs expected by insurers, we show in Appendix D that higher past trends in county noneconomic damages raise county malpractice premia currently charged by insurers. Finally, we show that noneconomic awards influence the behavior of patients. Theoretically, higher noneconomic damage awards raise the payoff to suing a provider,[21] but only if patients believe that trends in noneconomic damages meaningfully predict the size of expected malpractice payments. Appendix E uses closed-claims data from a California malpractice insurer to verify that local growth in noneconomic damages

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

increases the probability that physicians face malpractice claims, and the probability they will face a claim with nonzero defense costs. Taken together, these findings suggest that agents use past trends in noneconomic damages to update their expectations about malpractice cost.

E. Results

E.1 Effects of Malpractice on Costs

E.1.1 *Hospital Costs*

We estimate equation 6 at the hospital level. Since our first-stage equation for malpractice costs is estimated at the county-level, this requires a two-sample IV approach (Angrist and Krueger, 1992). We estimate the first-stage equation, use the predicted values for malpractice in the second-stage, and calculate the standard errors via a bootstrap procedure.[22]

The resulting instrumental variables estimates are given in Table 8. We model costs per bed, costs per bed-day, and days per bed (in the bottom panel).[23] We think of the first as an overall expenditures measure, the second as a price measure, and the third as a quantity. Separate estimates are provided for the current measure of malpractice as well as each set of moving averages.

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

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[22] Specifically, we use a cluster (or "block") bootstrap that sampled all hospitals in a given county for each bootstrap replication. This embeds the underlying empirical assumptions that observations from different counties are statistically independent, but observations within a particular county exhibit dependence. In each of 500 bootstrap replications, 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.

[23] 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.

exposed to malpractice risk. While the OLS models showed little relationship between malpractice and hospital costs, the IV models suggest that malpractice risk raises "price" and overall spending. There is no significant effect on total hospital quantity, although separate specifications revealed that malpractice significantly reduces total inpatient days and admissions. The overall elasticity of the hospital cost measures with respect to lagged malpractice cost ranges from approximately 0.02 to 0.08. While the elasticity for hospital costs never exceeds 0.1, it is considerably higher than the 0.01 or 0.02 that we would expect if we focused only on the direct costs of malpractice. This indicates that there is a clear behavioral response of health care providers to expected malpractice risk.

E.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). The lower panel of Table 8 studies the relationship between 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 0.08 to 0.12. The elasticity for Part B is around 0.03 to 0.06. 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 is also robust to the inclusion of HMO penetration variables, as shown in Appendix C.

E.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.[24] Moreover, the uncovered portion is physician spending paid for outside Medicare. Our analysis suggests that physician costs are less responsive to malpractice risk, as confirmed by other research (Baicker and Chandra, 2007). Therefore, it seems plausible to assume that the effects of malpractice on physician costs are no higher than our estimates for hospital costs.

Most of our estimated elasticities are quite close. For this calculation, we focus on the $t-3$ through $t-5$ moving average models, since those displayed the highest first-stage Wald statistic and are likely to display the best coverage rates as a result. The estimated elasticity of malpractice on daily hospital expenditures is 0.078. Between 1991 and 2002, medical expenditures grew by 34%, while malpractice payments grew by 65%. Our point estimate would imply that, over this period, the growth in malpractice payments added 5.1% to the growth in medical expenditures, or about 15% of the total growth.

In absolute terms, this is rather a modest effect, but disproportionate to the very small share of malpractice in total medical spending. Doubling malpractice risk has a direct 2% impact on spending at most, but the total effect could be as high as 8%. Therefore, doctors do change their behavior in response to malpractice risk, even though the latter is not a major driver of cost.

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[24] 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. 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.

E.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. We use our identification strategy to estimate the impact of malpractice on total county-level mortality rates, taken from the Multiple Cause-of-Death Mortality Data.[25]

Above, we argued empirically that our instrument is exogenous with respect to local trends in health care costs, which did not appear to cause variation in noneconomic damages. This empirical test also makes it unlikely that health-related trends in mortality cause variation in the instrument. Appendix F provides further evidence on this point: past or current mortality rates do not predict current noneconomic damages, but higher noneconomic damages in the past are significantly related to lower mortality today.

An additional concern is the possibility that noneconomic damages influence mortality through channels other than malpractice. For example, lower payoffs to litigation may encourage people to avoid taking risks, when adverse outcomes are less well compensated.[26] To test this, Appendix F provides evidence that noneconomic damages significantly reduce non-

[25] 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.

[26] Rubin and Shepherd (2007) find that tort reform appears to *reduce* the number of non-automobile accidental deaths, an effect they attribute to behavioral responses — for example, individuals who are less protected by the tort system may take more care. However, they find no effect on auto accidents, which represent the bulk of individuals' exposure to accident risk.

accidental deaths, but have little to no impact on accidental deaths. There and elsewhere, limiting our analysis to non-accidental deaths has no quantitative impact on our results.

We estimate the effect of malpractice risk on the total mortality rate, and deaths among individuals 20 to 64 or 65 and older. We did not disaggregate by specific causes of death because these data 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 appear in Table 9. The elasticities for total death rates with respect to malpractice costs are approximately 0.02. This implies that doubling malpractice costs lowers the total death *rate* by 2 percent. The age-specific breakdowns suggest these effects are stronger for the non-elderly, although this could be due to greater statistical noise in the estimates for the elderly.

Overall, the results of Table 9 provide some evidence that exposure to malpractice costs leads to modest reductions in mortality. The estimates are not uniformly precise, but consistently negative and significant, at least for total deaths. Still, it is hard to draw policy inferences from these results alone, because the confidence intervals often span ranges with inconsistent implications for net social benefit.

E.3 The Welfare Consequences of Changes in Malpractice Cost

As we argued earlier, the policy-relevant output is the number of dollars saved per life lost by exogenous changes in malpractice cost, or the left-hand side of expression 5. 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 yields a conclusion about the likely welfare consequences of a given policy change.

Our approach resembles the method of "cost-effectiveness acceptability curves" (cf, Lothgren and Zethraeus, 2000; Fenwick et al., 2004), which calculates the empirical distribution of the "dollars per life lost" parameter via a bootstrap methodology (whose technical underpinnings are discussed in Appendix G). This distribution then implies a probability for whether or not malpractice cost-reduction is cost-effective, conditional on a value of life.[27] For example, suppose we adopt the view that the value of a statistical life is \$6m. Suppose further that the "dollars saved per life lost" parameter exceeds \$6m about 40% of the time. In this case, lowering malpractice costs at the margin has a 40% probability of improving net social welfare.[28]

To estimate the distribution of "dollars per life lost," we use a bootstrap approach that jointly estimates the variance in the estimates of costs and mortality. We use the same bootstrap technique described earlier (see footnote 22), except here we use 1,000 replications, in order to gain more precision for the estimation of probabilities. We separately calculate distributions for the total population, and for 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;

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

[28] 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.

3. Using the bootstrap sample constructed in steps 1 and 2, estimate the model in equations 7 using total Medicare spending per elderly beneficiary as the outcome Y_{it}. This yields an elasticity of malpractice on Medicare costs, defined as $\boldsymbol{\varepsilon}^{CM}$.

4. With the *same* bootstrap sample used in step 3, estimate the model in 7 separately using county-level death rates for the over 65 population as the outcome Y_{it}. This provides the elasticity of malpractice costs on Medicare mortality, defined as ε^{MM}.

5. Using estimates of total nationwide deaths and costs in the year 2000,[29] dollars saved per life lost in the Medicare population is given by:

$$DPL^{M} \equiv \frac{\boldsymbol{\varepsilon}^{CM} * \text{Total Medicare Costs}}{\boldsymbol{\varepsilon}^{MM} * \text{Total Deaths Over Age 65}} \tag{10}$$

A similar procedure is used to estimate dollars per life in the overall population. We first estimate the model in equations 7 using total hospital costs per bed-day[30] aggregated to the county level[31] as the outcome Y_{it} to derive the impact of malpractice costs on 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.[32]) These are then combined

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

[30] Using hospital costs per bed or per capita would potentially understate the costs of malpractice, which seems to have at least small negative effects on hospital quantity.

[31] We aggregated by taking the mean of hospital expenditures in the county, weighted by the number of hospital beds. We obtain qualitatively identical results if we fail to aggregate, but it is computationally faster to do so.

[32] 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. Moreover, our cost measures account for approximately two-thirds of all health care spending exposed to malpractice risk. In

with year 2000 data on total nationwide medical spending and total deaths, to estimate the aggregate impacts.[33] The estimate of dollars per life overall is:

$$DPL^{T} \equiv \frac{\varepsilon^{CT} * \text{Total Medical Costs}}{\varepsilon^{MT} * \text{Total Deaths}} \tag{11}$$

We conducted 1,000 bootstrap replications. For completeness, we repeated this procedure for all the various lag specifications reported in Table 8 and Table 9. Here, we present results using the $t-3$ through $t-5$` moving average specification, as this yielded the most powerful first-stage. Appendix G demonstrates that the results are qualitatively similar for the other specifications, and across states with and without noneconomic damage caps.[34] This appendix also shows that the bootstrapped model yields hypothesis test results that are quite similar to the asymptotic IV results.

Figure 3 depicts the empirical cumulative distribution function for the estimated dollars per life saved.[35] For each dollar value, the Figure reveals the probability that dollars per life saved lies above that value, and thus the probability that malpractice cost-reduction is cost-effective. 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.

2000, for example, hospital spending was $417bn; spending on physicians and clinical services was $289bn, of which Medicare paid $58bn.

[33] 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.

[34] Appendix G demonstrates that these results are robust across the states in our sample with (California) and without (New York) noneconomic damage caps. 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.

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

In the Medicare population, tort reform is more likely than not to be cost-effective for values of a statistical life lower than $900,000. 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 $2.5m.

Using the figure to assess the desirability of reducing malpractice costs requires clarity on the exact value of a statistical life. In a prominent literature review, Viscusi and Aldy locate the value of a statistical life within the range of $5.5m to $7.5m (Viscusi and Aldy, 2003). In their study on the social value of life-extension, Murphy and Topel (2006) advocate $6.3m as a weighted average applicable to those aged 25 and 55. Others have dissented 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. Malpractice cost reduction has a better than even chance of being cost-effective for values lower than $2m, but is a poor bet for the values cited by Viscusi and Aldy, or used by Murphy and Topel. In general, a nearly five-fold difference in this value makes it hard to draw unambiguous policy conclusions.

One way through the controversy is to follow the actual thresholds employed by US regulators. The US Environmental Protection Agency typically 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) most often uses $3m (U.S. Department of Transportation, 2002). Perhaps most directly relevant is the $5m number often used by the Food and Drug Administration (FDA) to assess health risks.[36] All these thresholds

[36] If Medicare 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.

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.[37]

F. 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. Malpractice payments have grown enormously over the past 15 years, but this has likely had a modest impact on the cost of health care in the US. It may have other significant effects, such as decreasing the supply of physicians or changing the nature of treatment. Our findings, however, suggest that limiting malpractice liability is no panacea for rising health care costs.

Moreover, while the mortality benefits of malpractice may be quite modest, these seem more likely than not to justify its direct and indirect health care costs. Therefore, we conclude that — for values of statistical life traditionally employed by US regulators —reducing malpractice costs is not likely to be a worthwhile policy goal in itself. As emphasized by Currie and MacLeod (2008), however, specific policies must be evaluated on a case-by-case basis, as they can have unexpected effects on physicians' expected liability and incentives. In addition, there may be policies that reduce malpractice costs but have other social benefits; we do not rule those out, but note that the case for their adoption rests on their auxiliary effects.

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

[37] Agencies often take complex views that incorporate a range of values. The numbers given are "central tendencies" for each regulatory branch (Robinson, 2007).

costs and mortality, excluding its impacts (if any) on morbidity, physician utility, and patient satisfaction. These quantities are extremely difficult to measure objectively. 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 over conventionally accepted values of life.

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 much more modest impacts on the average patient, and on health care spending as a whole. Future research should endeavor to determine whether tort reform can be targeted toward these subgroups in a cost-effective manner.

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 in health rather than in dollars.

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Table 1: Geographical composition of the counties in the JVDB sample.

County	2000 Population	Population Share
Los Angeles County, CA	9,519,338	14.6%
Cook County, IL	5,376,741	8.3%
Harris County, TX	3,400,578	5.2%
Orange County, CA	2,846,289	4.4%
San Diego County, CA	2,813,833	4.3%
Kings County, NY	2,465,326	3.8%
Queens County, NY	2,229,379	3.4%
King County, WA	1,737,034	2.7%
San Bernardino County, CA	1,709,434	2.6%
Santa Clara County, CA	1,682,585	2.6%
Riverside County, CA	1,545,387	2.4%
New York County, NY	1,537,195	2.4%
Alameda County, CA	1,443,741	2.2%
Suffolk County, NY	1,419,369	2.2%
St. Louis County, MO	1,846,486	2.8%
Nassau County, NY	1,334,544	2.1%
Bronx County, NY	1,332,650	2.0%
Sacramento County, CA	1,223,499	1.9%
Rest of California	11,054,004	17.0%
Rest of New York	8,494,928	13.1%

Notes: The population numbers for St. Louis County include the population of Jefferson County and St. Charles County.

Table 2: 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)	2,949	16,837	2,772	16,613
	(12,718)	(26,590)	(10,053)	(21,198)
Malpractice Awards Per Capita (dollars)	2.87	6.07	2.04	5.77
	(18.06)	(14.57)	(7.96)	(8.87)
Verdict-Level Means				
Average Noneconomic Award: All Cases (thousands)	142	312	105	292
	(530)	(593)	(299)	(425)
Average Economic Award: All Cases (thousands)	328	634	200	585
	(2,465)	(1,656)	(430)	(514)
Average Noneconomic Award: Malpractice Cases (thousands)	173	501	117	445
	(803)	(1,120)	(484)	(740)
Average Economic Award: Malpractice Cases (thousands)	377	1,096	286	1,050
	(2,691)	(2,646)	(885)	(1,446)
N	1,785		1,547	

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 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	2,220	2,512	2,226	2,385
	(829)	(1,041)	(843)	(879)
Part B Expenditures Per Enrollee	1,413	1,518	1,431	1,510
	(528)	(545)	(696)	(623)
County Demographics				
Per Capita Income	21,325	25,782	17,769	22,745
	(8,600)	(10,209)	(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
	(0.07)	(0.10)	(0.14)	(0.13)
N	2,261		58,387	

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: 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 5: Health Costs and Noneconomic Damage Awards: Causality Tests of the Instrument.

	Timing of noneconomic award variable								
	Moving average of leading values				Current Year (t)	Moving average of lagged values			
	t+4, t+5, t+6	t+3, t+4, t+5	t+2, t+3, t+4	t+1, t+2, t+3		t-1, t-2, t-3	t-2, t-3, t-4	t-3, t-4, t-5	t-4, t-5, t-6
Hospital Cost Estimates									
Dependent Variable: Hospital Facility Expenditures Per Bed									
Noneconomic Award (Hundreds of Thousands)	-183.970	643.497	768.325*	309.522	83.116	1,198.059***	2,393.893***	2,486.269***	943.709
	(365.466)	(555.175)	(424.522)	(439.570)	(157.477)	(432.643)	(554.984)	(505.182)	(654.647)
Dependent Variable: Hospital Facility Expenditures Per Bed Day									
Noneconomic Award (Hundreds of Thousands)	-0.119	0.571	0.738	-0.346	0.059	1.161	5.242***	4.661***	2.191
	(1.163)	(0.950)	(1.084)	(0.783)	(0.433)	(1.084)	(1.403)	(1.297)	(1.438)
County Medicare Estimates									
Dependent Variable: Medicare Part A Expenditures Per Enrollee									
Noneconomic Award (Hundreds of Thousands)	2.778	0.821	4.774	6.143	1.578	21.940**	31.916***	24.734**	24.308*
	(4.767)	(4.811)	(5.283)	(7.370)	(2.050)	(8.786)	(11.537)	(10.853)	(12.920)
Dependent Variable: Medicare Part B Expenditures Per Enrollee									
Noneconomic Award (Hundreds of Thousands)	1.063	1.559	2.471	3.144	1.492	10.320**	7.789*	5.117	5.195
	(1.985)	(2.539)	(2.586)	(2.968)	(1.178)	(4.143)	(4.572)	(4.851)	(5.317)
Hospital Utilization Estimates									
Dependent Variable: Hospital Days Per Bed									
Noneconomic Award (Hundreds of Thousands)	-1.332	0.024	1.779	0.232	-0.300	0.188	-4.290	-4.026	-3.932
	(2.250)	(3.069)	(2.233)	(1.427)	(0.559)	(1.857)	(3.501)	(2.694)	(2.472)

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: First-Stage Regression Relationship Between Malpractice Award Dollars Per Capita and Average Noneconomic Damage Awards Per Plaintiff Victory.

	Timing of malpractice award and average noneconomic damages variables				
	Current Year (t)	Three-Year Moving Average of Lagged Values			
		t-1, t-2, t-3	t-2, t-3, t-4	t-3, t-4, t-5	t-4, t-5, t-6
Malpractice Award Dollars Per Capita	1.150**	0.631***	0.620***	0.581***	0.517***
	(0.579)	(0.169)	(0.173)	(0.161)	(0.153)
Wald Statistic	3.954**	13.885***	12.775***	13.028***	11.335***
R^2	0.519	0.747	0.733	0.731	0.737
N	1,785	1,547	1,547	1,547	1,547

Notes: The table reports the estimated effect of average noneconomic damage awards in all tort cases with a nonzero award to the plaintiff on total malpractice awards per capita. Each coefficient is from a separate regression, with each column representing a different lag for the dependent variable *and* the instrument. The unit of analysis 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 8: The Impact of Malpractice on Hospital Costs and County Medicare Costs.

	Timing of noneconomic damage awards and malpractice awards				
	Current Year (t)	Moving average of lagged values			
		t-1, t-2, t-3	t-2, t-3, t-4	t-3, t-4, t-5	t-4, t-5, t-6
Hospital and County Medicare Cost Estimates					
Dependent Variable: Hospital Facility Expenditures Per Bed					
Malpractice Awards Per Capita	72.252	1,897.295*	3,863.612***	4,282.672***	1,826.520
	(202.170)	(1,077.975)	(1,316.800)	(1,403.649)	(2,176.261)
Elasticity	0.002	0.037	0.072	0.076	0.030
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards Per Capita	0.051	1.838	8.461***	8.030***	4.241
	(0.402)	(2.184)	(3.375)	(3.177)	(4.384)
Elasticity	0.001	0.018	0.083	0.078	0.040
Dependent Variable: Medicare Part A Expenditures Per Enrollee					
Malpractice Awards Per Capita	1.371	34.744**	51.511***	42.605***	47.047*
	(1.939)	(15.316)	(13.534)	(15.102)	(24.127)
Elasticity	0.0036	0.0806	0.1162	0.0933	0.0989
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards Per Capita	1.297	16.343**	12.571*	8.815	10.055
	(1.255)	(7.399)	(7.125)	(7.939)	(9.962)
Elasticity	0.0057	0.0632	0.0470	0.0320	0.0348
Hospital Utilization Estimates					
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards Per Capita	-0.261	0.297	-6.924	-6.936	-7.609
	(0.654)	(4.855)	(7.500)	(5.757)	(6.755)
Elasticity	-0.003	0.003	-0.068	-0.066	-0.070

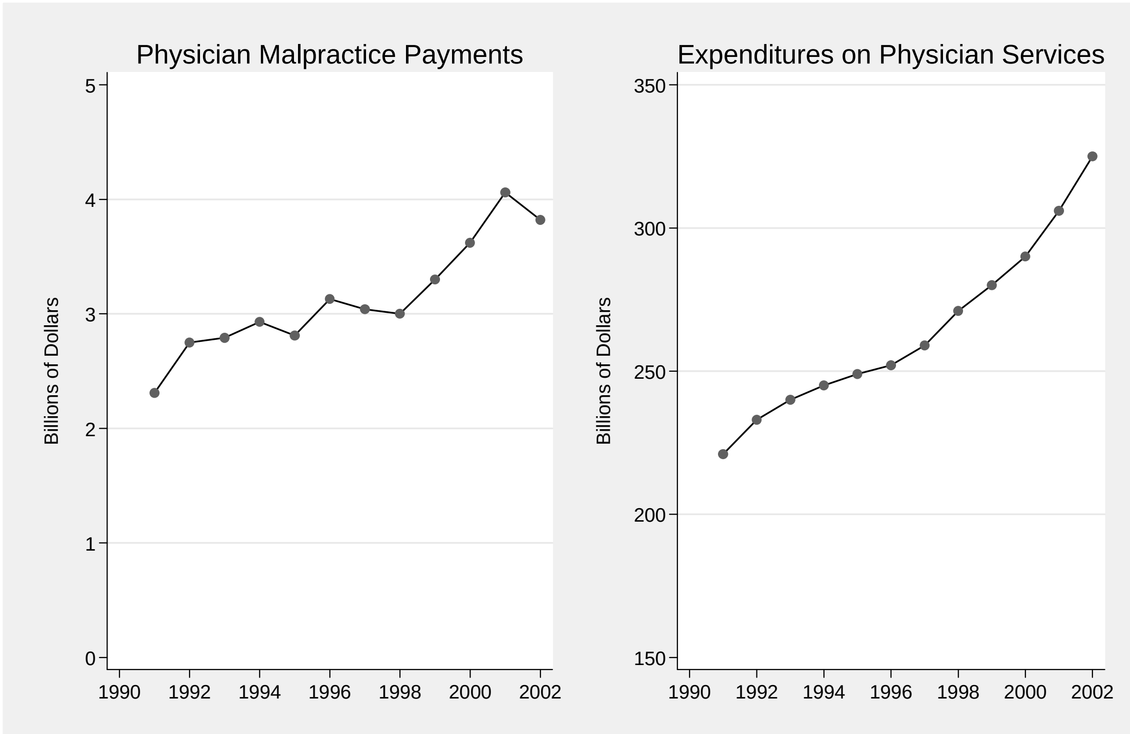
Notes: The table reports the estimated effect of per capita malpractice jury award dollars on medical expenditures. In the IV models, malpractice awards are instrumented by the average noneconomic awards in all tort cases with a plaintiff win. Each coefficient is from a separate regression, and each column represents a different lag for the malpractice variable. The unit of analysis is a hospital-year for the hospital spending regressions, and a county-year for the county Medicare regressions. 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. For the hospital level regressions, these standard errors are calculated using 500 bootstrap replications with bootstrap resampling done at the county level. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.

Table 9: The Impact of Malpractice on County Level Mortality.

	Timing of noneconomic damage awards and malpractice awards				
	Current Year	Moving average of lagged values			
	(t)	t-1, t-2, t-3	t-2, t-3, t-4	t-3, t-4,t-5	t-4, t-5, t-6
			Total Deaths Per 1,000 Population		
Malpractice Awards	-0.005	-0.035*	-0.026*	-0.023**	-0.012
Per Capita	(0.004)	(0.018)	(0.014)	(0.010)	(0.009)
Elasticity	-0.0036	-0.0258	-0.0192	-0.0171	-0.0094
			Deaths Per 1,000 Age 20 to 64		
Malpractice Awards	-0.004	-0.028**	-0.021**	-0.019	-0.017
Per Capita	(0.003)	(0.011)	(0.009)	(0.012)	(0.015)
Elasticity	-0.0060	-0.0457	-0.0351	-0.0328	-0.0299
			Deaths Per 1,000 Age 65 and up		
Malpractice Awards	-0.016	-0.112	-0.099	-0.079*	-0.015
Per Capita	(0.017)	(0.080)	(0.063)	(0.040)	(0.050)
Elasticity	-0.0019	-0.0131	-0.0117	-0.0095	-0.0018

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.

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: Trends in Noneconomic Damage Awards and Hospital Spending for Four Large Counties.

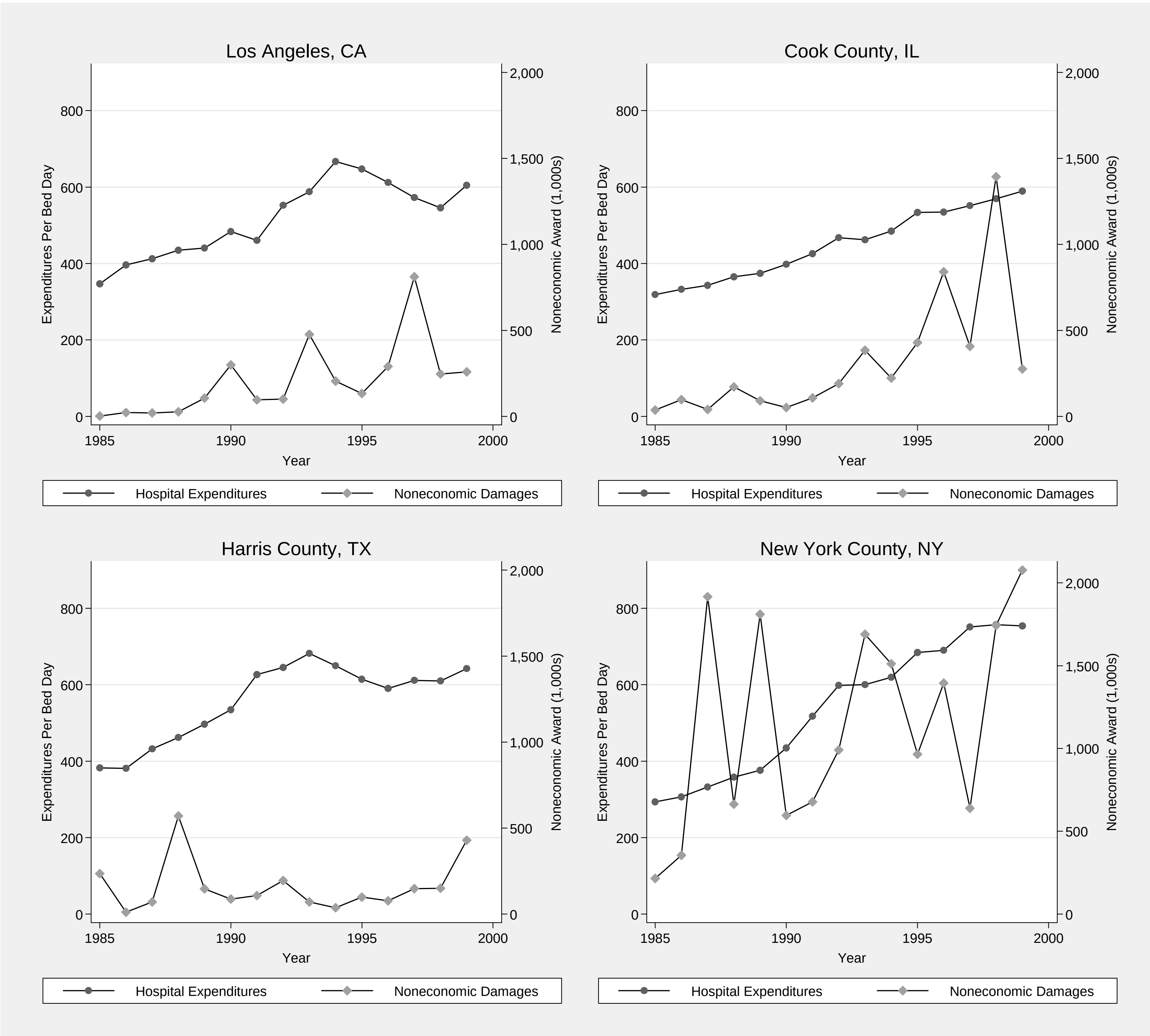
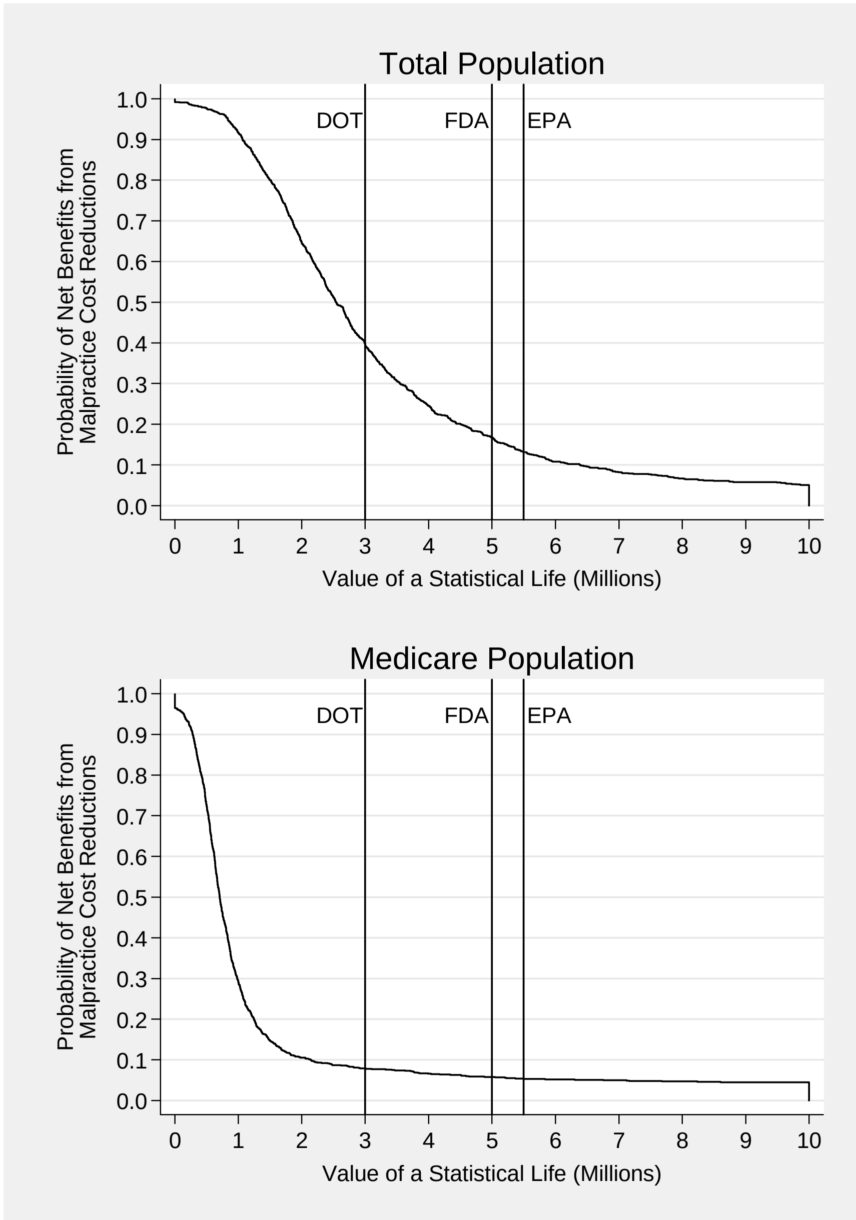


Figure 3: The Welfare Impact of Reducing Malpractice Costs.



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. The results illustrated here use the three year moving average of malpractice costs lagged 3, 4 and 5 years. Vertical lines correspond to values of statistical life (in year 2000 dollars) used by the following federal government regulatory agencies: Department of Transportation (DOT), Food and Drug Administration (FDA), and Environmental Protection Agency (EPA).

Appendix

A. Econometric Identification

A.1 Consistency of Elasticity Estimates

Assume that assumptions (A-1) and (A-2) in the text both hold. Under these assumptions, the instrumental variables model in 7 yields a consistent estimate of $\beta_1 b$. The true elasticity of medical costs with respect to expected malpractice is given by:

$$\text{plim}\left(\frac{\hat{\boldsymbol{\beta}}_1 \overline{MedMal_{ct}}}{\overline{MedCosts_{ct}}} \right) = \frac{\boldsymbol{\beta}_1 E(MedMal_{ct})}{E(MedCosts_{ct})} \tag{12}$$

We have a proxy for $E(MedMal_{ct})$, defined as V_{ct}. If our proxy is an unbiased predictor, $E(MedMal_{ct}) = bE(V_{ct})$. However, our estimator for the elasticity of medical costs with respect to malpractice still converges to the same value as in equation 12:[38]

$$\text{plim}\left(\frac{\hat{\beta}_1 \hat{b} \overline{V}_{ct}}{\overline{MedCosts_{ct}}} \right) = \frac{\beta_1 bE(V_{ct})}{E(MedCosts_{ct})} = \frac{\beta_1 E(MedMal_{ct})}{E(MedCosts_{ct})} \tag{13}$$

The argument for the mortality elasticity proceeds identically.

A.2 Estimating Upper Bounds on Elasticities

To simplify the algebra for the balance of this appendix, consider the residualized instrumental variables regression of Y^* on the single regressor V^* using the residualized instrument $NonEcon^*$. Suppose that assumption (A-1) holds, but assumption (A-2) does not. In particular, suppose that $Cov(NonEcon^*, e) \geq 0$. In words, growth in noneconomic damages leads to a higher forecast for expected malpractice costs, holding malpractice verdicts constant. The instrumental variable estimate has the following asymptotic value:

$$\text{plim}(\beta_1 b)_{IV} = \frac{\text{cov}(Y^*, NonEcon^*)}{\text{cov}(V^*, NonEcon^*)} = \frac{\text{cov}(\beta_1 bV^* + \beta_1 e + \varepsilon, NonEcon^*)}{\text{cov}(V^*, NonEcon^*)} =$$

$$\beta_1 b \left(\frac{\text{cov}(V^* + \frac{e}{b} + \frac{\varepsilon}{\beta_1 b}, NonEcon^*)}{\text{cov}(V^*, NonEcon^*)} \right) = \beta_1 b \left(1 + \frac{\text{cov}(\frac{e}{b}, NonEcon^*)}{\text{cov}(V^*, NonEcon^*)} \right)$$

---

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

So long as the proxy is positively related to expected malpractice costs ($b > 0$), this expression implies that $|\,\text{plim}(\beta_1 b)_{IV}\,| \geq |\,\beta_1 b\,|$. It then follows that the instrumental variables estimate yields an elasticity estimate that exceeds the true elasticity.

A.3 Estimating Welfare Parameters

Continue to suppose that assumption (A-1) holds, but that assumption (A-2) need not. The object of interest is dollars of malpractice cost per life saved, or $\dfrac{\beta_1^{Cost}}{\beta_1^{Deaths}}$. The coefficients β_1^{Cost} and β_1^{Deaths} come from the cost and mortality models, respectively.

Our estimator for this ratio will be $\dfrac{(\beta_1 b)_{IV}^{Costs}}{(\beta_1 b)_{IV}^{Deaths}}$. This converges to the following:

$$\text{plim}\frac{(\beta_1 b)_{IV}^{Costs}}{(\beta_1 b)_{IV}^{Deaths}} = \frac{\beta_1^{Costs} b \left(1 + \dfrac{\text{cov}(\frac{e}{b}, NonEcon^*)}{\text{cov}(V^*, NonEcon^*)}\right)}{\beta_1^{Deaths} b \left(1 + \dfrac{\text{cov}(\frac{e}{b}, NonEcon^*)}{\text{cov}(V^*, NonEcon^*)}\right)} = \frac{\beta_1^{Costs}}{\beta_1^{Deaths}}$$

This proves the required result.

B. Analysis of National-Representativeness

The JVDB covers about one-quarter of the US population, but it over-represents large counties in the US. To verify that this has no undue influence on our key findings, we replicated our welfare analysis using statistical methods to account for the over-representation (Little and Rubin, 1987).

We begin by taking the full set of counties for a single year (2000) and generating a binary variable indicating which counties are in our analytic sample. We then use a logistic regression to predict the probability of being included in our sample: the dependent variable is the indicator for sample inclusion, and the independent variables are population, and county demographic characteristics (age distribution, income, and race). We then use the inverse of these predicted probabilities to generate sampling weights. These weights are larger on average for the smaller counties in our sample, giving them additional weight in the analysis.

The sampling weights do not enter directly into our analysis. Rather, we use them to conduct weighted bootstrap draws. That is, we conduct 1,000 bootstrap replications of counties, just as before, but the probability of inclusion in each replication sample is proportional to the sampling weight. The resulting bootstrapped sample is adjusted to be nationally representative, based on the predicted probability of a county being in the sample.

Based on the bootstrapped samples, we estimate the cost and mortality effects of malpractice and use the full set of bootstrap replications to generate the cost-effectiveness acceptability curves. The results of the weighted bootstrap analysis are illustrated in Appendix Figure 1. The figure is nearly identical to before, indicating that our sample is highly representative. From the figure it appears that malpractice reform is likely to be cost-effective at values of life around $1 million or less, though perhaps not for the Medicare population. For the values of life used by U.S. regulatory agencies, however, malpractice reform appears more likely than not to reduce welfare.

C. 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.

Appendix Table 1 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.

D. 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. Appendix Table 2 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.

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.

Appendix Table 3 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%.[39] 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. 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 Appendix Table 4 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 total deaths and 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, because the standard errors are larger for the leads. This is in spite of larger sample sizes for the lead regressions, rather than the lags.

In Table 5 we also break down results for accidental and non-accidental deaths. The results for non-accidental deaths are consistent with the results for all deaths. There is a slight inconsistency with accidental deaths; the correlation is positive in most cases and significant for one of the lags. Nevertheless, the share of accidental deaths is too small for it to have any impact on our overall findings.

G. 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, $\dfrac{\boldsymbol{\varepsilon}^C}{\boldsymbol{\varepsilon}^M}$. Note, however, that all our policy conclusions are exactly symmetric if we take the inverse ratio, $\dfrac{\varepsilon^M}{\varepsilon^C}$. Therefore, without loss of generality, we will show that this ratio's distribution can be bootstrapped.

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

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.[40] 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.

Empirically, the bootstrap estimates yield hypothesis test results similar to those of the asymptotic IV analysis, as shown in Appendix Table 5.[41] 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.[42] The bootstrapped p-values are reasonably close to the asymptotic p-values. Some departures are observed in the case with 1, 2, and 3 lags, where the asymptotic estimates seem to lead to over-rejection.[43]

Finally, our results are fairly similar for our different lag specifications, and fairly similar across states with and without noneconomic damage caps. Appendix Figure 2 plots the empirical distribution of

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[40] 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.

[41] Observe that the IV estimates reported here are not exactly identical to those in the paper's 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 E.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 Appendix Table 1. Second, to conserve computing power in the bootstrap, we ran all analyses at the county level, by aggregating hospital-level data. In Appendix Table 1, 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.

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

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

dollars per life lost for all major lag specifications used in the paper. Appendix Figure 3 plots the empirical distribution for the parameter that is estimated using only the uncapped state of New York.

Appendix Tables

Appendix Table 1: 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 Cost Estimates					
Dependent Variable: Hospital Facility Expenditures Per Bed					
Malpractice Awards	31.576	2,023.300*	4,164.870***	4,340.461***	1,833.339
Per Capita	(182.665)	(1,174.366)	(1,433.954)	(1,361.019)	(2,121.661)
Elasticity	0.001	0.040	0.079	0.077	0.030
Dependent Variable: Hospital Facility Expenditures Per Bed Day					
Malpractice Awards	0.308	-0.509	8.755***	8.276***	4.577
Per Capita	(0.411)	(2.710)	(2.981)	(3.043)	(4.301)
Elasticity	0.003	-0.005	0.088	0.080	0.043
County Medicare Estimates					
Dependent Variable: Medicare Part A Expenditures Per Enrollee					
Malpractice Awards	1.874	22.450**	45.901***	43.848***	48.507**
Per Capita	(1.746)	(10.377)	(13.252)	(13.346)	(22.806)
Elasticity	0.0048	0.0513	0.1037	0.0961	0.1019
Dependent Variable: Medicare Part B Expenditures Per Enrollee					
Malpractice Awards	0.892	10.550*	8.201	9.325	11.068
Per Capita	(1.039)	(6.055)	(6.765)	(7.303)	(9.150)
Elasticity	0.0038	0.0402	0.0307	0.0339	0.0383
Hospital Utilization Estimates					
Dependent Variable: Total Hospital Days Per Bed					
Malpractice Awards	-0.510	3.396	-7.854	-7.327	-8.429
Per Capita	(0.785)	(5.515)	(7.596)	(5.537)	(6.352)
Elasticity	-0.005	0.035	-0.079	-0.070	-0.078

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 is a hospital-year for the hospital-level regressions or a county year for the county-level regression. 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. For the hospital level regressions, these standard errors are calculated using 500 bootstrap replications with bootstrap resampling done at the county level. A *, **, or *** represents statistical significance at the 10, 5, or 1% level, respectively.

Appendix Table 2: 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	12.748	681.614**	584.905	768.075*	1,192.766*
(Hundreds of Thousands)	(65.794)	(336.628)	(386.884)	(425.067)	(641.043)
R^2	0.83	0.83	0.82	0.82	0.81
Fixed effects:			County, Year, Specialty		
Noneconomic Award	34.814	556.848	626.990	764.533*	1,074.783*
(Hundreds of Thousands)	(47.207)	(368.333)	(444.700)	(448.321)	(600.943)
R^2	0.84	0.84	0.83	0.84	0.83
Fixed effects:			Insurer, 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. Standard errors adjusted to reflect clustering by county are reported in parentheses.

Appendix Table 3: 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 (Hundreds of Thousands)	0.005 (0.003)*	0.005 (0.003)*	0.007 (0.003)**	0.006 (0.003)**
Elasticity	0.055	0.068	0.093	0.111
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. Sample includes claims reported against insured physicians in California who purchase their policies from a single large insurance company from 1991-2000. 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. 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, 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.

Appendix Table 4: 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)	-0.021	-0.017	-0.015	-0.015	-0.004	-0.019*	-0.014**	-0.012*	-0.006
	(0.015)	(0.017)	(0.016)	(0.014)	(0.003)	(0.010)	(0.007)	(0.007)	(0.005)
Dependent Variable: Deaths Per 1,000 Age 20 to 64									
Noneconomic Award (Hundreds of Thousands)	-0.013	-0.011	-0.011	-0.011	-0.003	-0.015**	-0.011**	-0.009	-0.007
	(0.010)	(0.010)	(0.010)	(0.009)	(0.002)	(0.006)	(0.004)	(0.007)	(0.007)
Dependent Variable: Deaths Per 1,000 Age 65 and up									
Noneconomic Award (Hundreds of Thousands)	-0.091	-0.060	-0.043	-0.038	-0.015	-0.064	-0.060*	-0.049*	-0.010
	(0.065)	(0.074)	(0.066)	(0.054)	(0.014)	(0.043)	(0.033)	(0.027)	(0.025)
Dependent Variable: Accidental Deaths Per 1,000 Population									
Noneconomic Award (Hundreds of Thousands)	0.0002	0.0005	0.0004	0.0000	-0.0002	0.0017**	0.0019	0.0003	0.0002
	(0.0007)	(0.0005)	(0.0006)	(0.0006)	(0.0002)	(0.0008)	(0.0012)	(0.0007)	(0.0005)
Dependent Variable: Non-Accidental Deaths Per 1,000 Population									
Noneconomic Award (Hundreds of Thousands)	-0.022	-0.018	-0.015	-0.015	-0.004	-0.021**	-0.016**	-0.012*	-0.006
	(0.015)	(0.017)	(0.016)	(0.013)	(0.003)	(0.010)	(0.007)	(0.007)	(0.005)
N	739	801	801	801	925	801	801	802	803

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.

Appendix Table 5: 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.	1.359	5.134	6.894	5.677
	Std.Er.	(1.532)	(2.103)	(2.338)	(2.881)
	Elasticity	0.0169	0.0621	0.0818	0.0645
	Pr(b<0)	**0.188**	**0.007**	**0.002**	**0.025**
Bootstrap	**Pr(b<0)**	**0.313**	**0.008**	**0.008**	**0.051**

Total Medicare Expenditures

		Lagged:			
		1, 2, 3	2, 3, 4	3, 4, 5	4, 5, 6
IV Model	Coeff.	48.604	63.041	48.122	53.912
	Std.Er.	(24.619)	(21.388)	(22.511)	(33.659)
	Elasticity	0.0705	0.0887	0.0658	0.0705
	Pr(b<0)	**0.024**	**0.002**	**0.016**	**0.055**
Bootstrap	**Pr(b<0)**	**0.014**	**0.007**	**0.039**	**0.046**

Total Mortality

		Lagged:			
		1, 2, 3	2, 3, 4	3, 4, 5	4, 5, 6
IV Model	Coeff.	-0.035	-0.026	-0.023	-0.012
	Std.Er.	(0.018)	(0.014)	(0.010)	(0.009)
	Elasticity	-0.0258	-0.0192	-0.0171	-0.0094
	Pr(b>0)	**0.026**	**0.032**	**0.011**	**0.091**
Bootstrap	**Pr(b>0)**	**0.152**	**0.144**	**0.011**	**0.033**

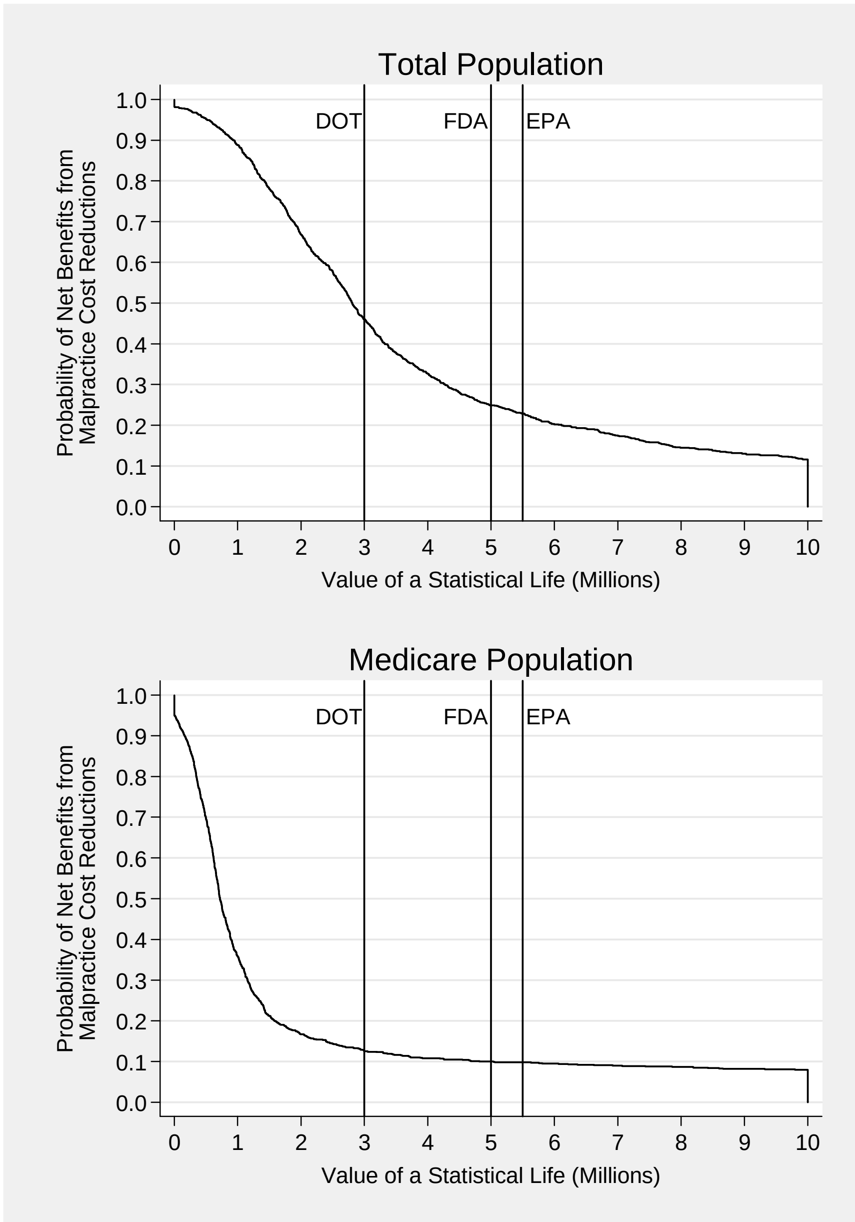
Mortality Over Age 65

		Lagged:			
		1, 2, 3	2, 3, 4	3, 4, 5	4, 5, 6
IV Model	Coeff.	-0.112	-0.099	-0.079	-0.015
	Std.Er.	(0.080)	(0.063)	(0.040)	(0.050)
	Elasticity	-0.0131	-0.0117	-0.0095	-0.0018
	Pr(b>0)	**0.081**	**0.058**	**0.024**	**0.382**
Bootstrap	**Pr(b>0)**	**0.272**	**0.188**	**0.039**	**0.373**

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.

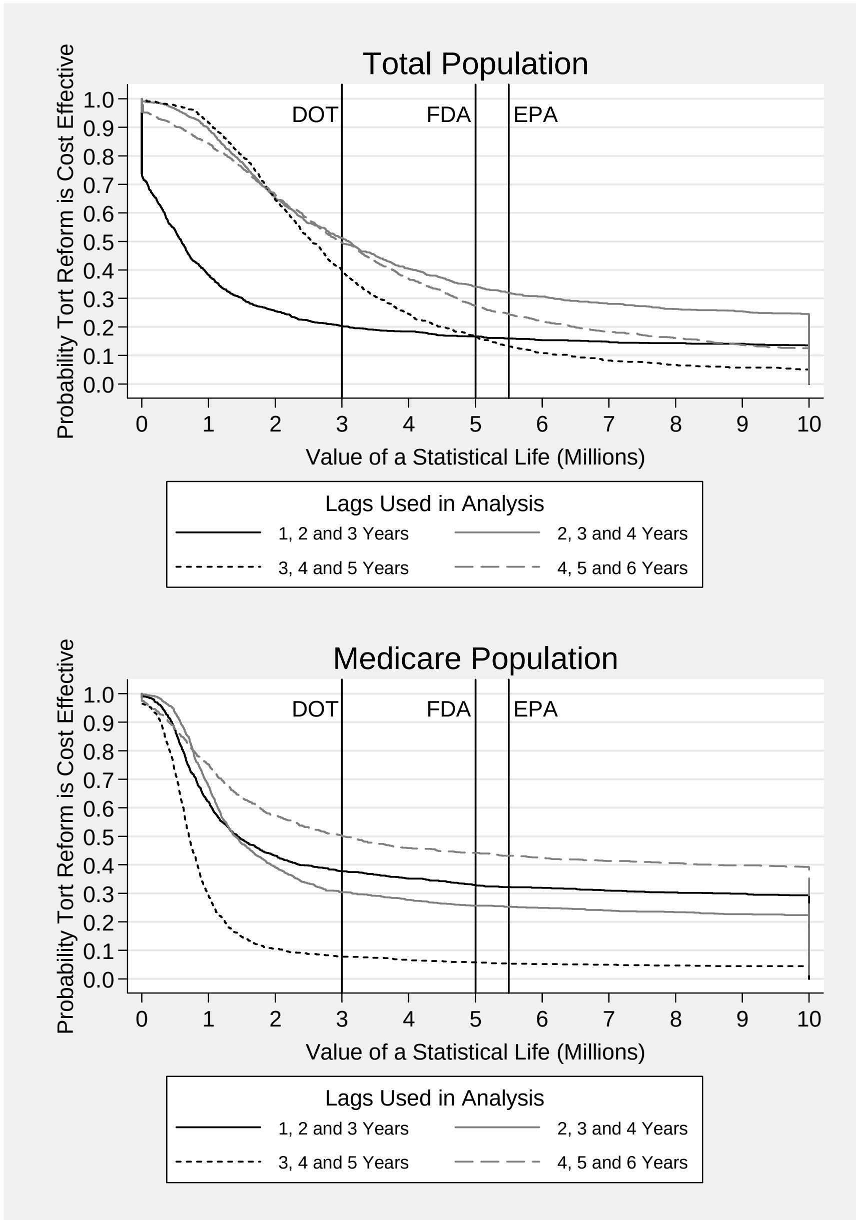
Appendix Figures

Appendix Figure 1: The welfare effects of reducing malpractice costs, correcting for over-sampling of large counties.



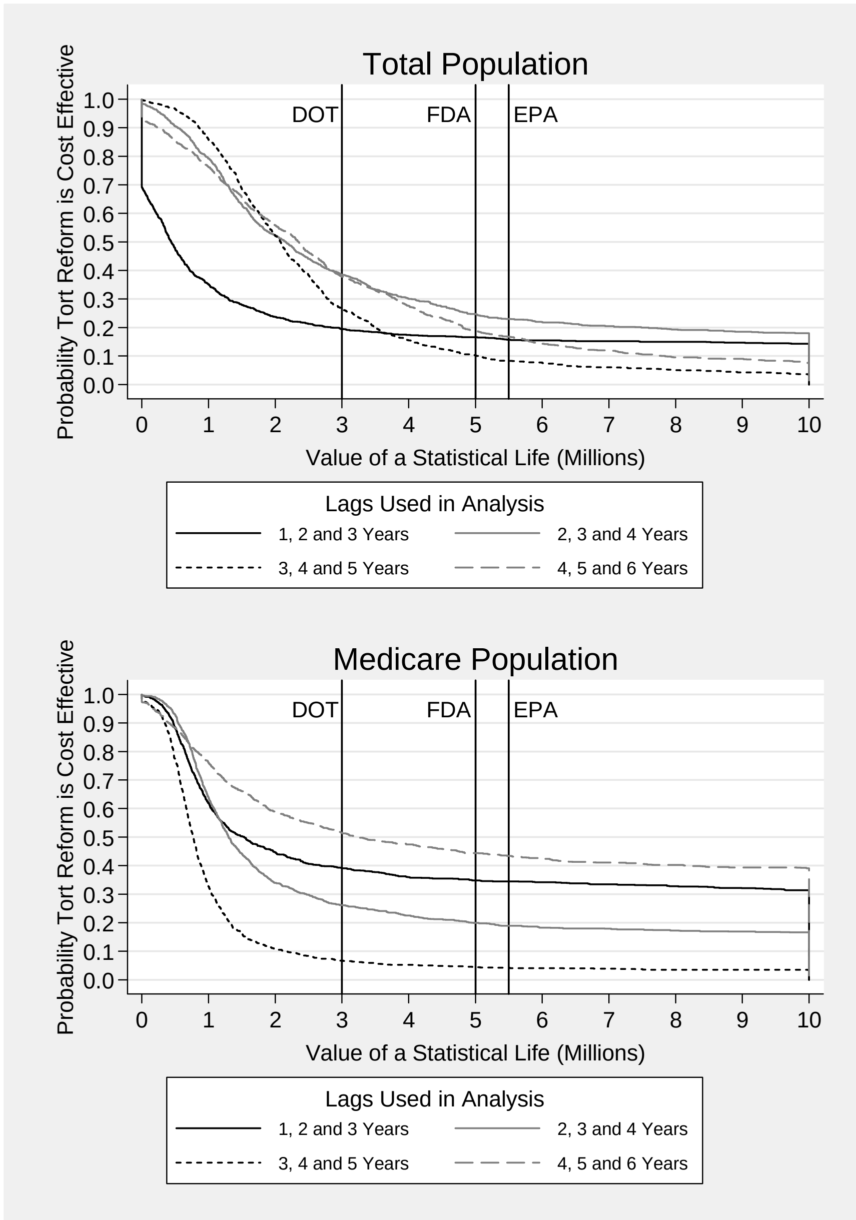
Notes: The curves depict the empirical probabilities that the estimated dollars saved per life lost exceed the given value of a statistical life. These are constructed identically as in Figure 3, except that in this the bootstrap is weighted with sampling weights that reflect the undersampling of small counties in our data. The weights function in such a way that the probability of inclusion in the bootstrap is proportional to the sampling weight (which is higher for the undersampled counties).

Appendix Figure 2: The Welfare Effects of Reducing Malpractice Costs Using the Full Set of Lags.



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 3, except that in this figure, we include the full set of lags for our analysis (as shown in the legend).

Appendix Figure 3: The Welfare Effects of Reducing Malpractice Costs 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 3, except that in this figure, we estimate the model to identify an effect net of the influence of California, which had a cap on noneconomic damages in place during our study period.