Do Medical Malpractice Reforms Affect Health Care Costs and Outcomes?

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ABSTRACT

The impact of medical malpractice reforms on the cost and quality of health care is of great interest to policy makers. This study examines national data on malpractice reforms implemented and health care provided to Medicare beneficiaries between 1995 and 2004. State-level reforms' effect on health care expenditures and outcomes is determined in four disease-based populations. Reforms are shown not to have any meaningful impact on six-month expenditures or outcome measures in the majority of cases. Policymakers considering tort reforms to restrain the growth of health care expenditures are advised to concentrate on alternative measures.

I. INTRODUCTION

The prevalence and even existence of defensive medicine[1], where physicians order tests or procedures primarily due to malpractice liability, has been controversial for well over two decades. To the extent doctors do practice defensively, increased expenditures on defensive care are socially wasteful only if they fail to appreciably improve patient health outcomes. Although malpractice liability is often blamed for rising health care costs and altering physician treatment decisions, existing empirical literature is far from consensus on the extent of the effect, if it exists at all.

Estimates of the amount of health care costs attributable to malpractice liability have ranged from $20 billion to $50 billion (Reynolds et al., 1987; Kessler and McClellan, 1996), while others have argued that malpractice liability has no significant impact on physician behavior (Baldwin et al., 1995; Sloan et al., 1997). This study expands on the work of Kessler and McClellan (KM) and others by examining new patient groups and refining the methodology used to evaluate the impact of malpractice liability on health care expenditures and outcomes. In doing so, the paper addresses questions relevant to formulating health policy, improving the quality of care and examining the consequences of medical malpractice liability reform.

Does malpractice liability induce physicians to practice defensive medicine, as proponents of tort reform[2] allege? More generally, what impact do reforms have on the

[1] In the context of this paper, "defensive medicine" refers to the overuse of medical treatments and procedures in response to the threat of malpractice liability, more specifically referred to as "positive defensive medicine" or "assurance behaviors". "Negative defensive medicine" or "avoidance behaviors," in which physicians refuse to treat patients or perform procedures that carry a high risk of malpractice liability, is not considered here.

[2] Tort refers to general legal processes in which an injured party seeks damages from a defendant alleged to be responsible for the injury. Tort reform generally refers to changes in legal rules governing medical malpractice cases, and such reforms almost always benefit physicians by reducing the size and frequency of payments made to patients filing malpractice claims.

cost and quality of care provided to patients? By examining the impact of legal changes in medical malpractice, this paper examines whether legal rules governing malpractice cases function as intended, motivating physicians to take appropriate measures that improve patient outcomes at a reasonable cost, or lead to overuse of tests and procedures with financial and medical benefits that do not justify their cost. This judgment is made by analyzing ten years of data on changes in malpractice laws and the cost and outcomes of health care, while controlling for patient characteristics and state and time fixed effects. If those who support malpractice reforms in hopes of reducing wasteful defensive practices are correct, states which implement reforms should see their health care expenditures grow less quickly than nonreforming states while experiencing similar health outcomes.

Several arguments have been made downplaying the impact of malpractice liability on health care expenditures and outcomes. A lack of experience rating[3] among medical malpractice insurers means that physicians who generate large amounts of malpractice claims do not pay more for malpractice insurance than their peers who generate fewer claims (Sloan and Chepke, 2008).When malpractice claims do result in payments to plaintiffs, awards are almost always paid exclusively by a physician's malpractice insurance company and are effectively capped by the size of the physician's policy limit; doctors rarely pay awards from malpractice claims out-of-pocket (Zeiler et al., 2007).

Danzon (2000) estimated that malpractice insurance premiums represent just one percent of the cost of providing health care to patients. *Prima facie*, this figure suggest

[3] Experience rating refers to insurers taking one's previous claims history into account when setting premiums. Car insurance is typically experience rated; premiums are increased following an accident.

that malpractice-related expenses are a relatively unimportant part of the health care cost structure and do not significantly impact patients or physicians. However, looking only at this one percent rate understates the true influence of the malpractice liability system by on medical decision-making by failing to consider several factors.

First, physicians bear and are unable to insure against significant nonmonetary costs arising from malpractice lawsuits filed against them. In addition to the opportunity costs of time-consuming litigation, physicians also suffer emotionally from the stress and humiliation associated with a malpractice suit (Wenokur and Campbell, 1991; Martin et al., 1991). Thus, while the monetary costs of insuring against malpractice may be small compared to total health care costs, the non-financial costs of malpractice suits may well influence physician behavior.

Second, physicians in high-risk specialties such as obstetrics, orthopedics and neurosurgery have been shown to overestimate the probability of being sued by more than 150 percent and the probability of a given negligent adverse event[4] resulting in a malpractice claim by nearly 35 times (Lawthers et al., 1992). When high-risk specialty physicians in New York State were asked "In your opinion, for every 100 physicians in your Specialty in New York State, how many do you think will be sued at least once this year?" the average response was 34.3, compared to an actual suit rate of 20.8. When asked what percentage of negligent adverse events lead to malpractice suits, the average response of 69 percent was dramatically higher than the actual rate of two percent.

[4] Brennan et al. (1996) define an adverse event as "an injury resulting from medical treatment, as opposed to the underlying disease process, that prolonged a patient's hospitalization, caused disability at the time of discharge, or both" A negligent adverse event is "an injury judged to be due to medical care that failed to meet the standards expected of a typical medical practitioner."

Thus, while one may argue that the low rate of negligent adverse events resulting in malpractice claims means physicians are not significantly influenced by the tort system's deterrent signal, perceptions among physicians suggest an exaggerated response to the threat of malpractice lawsuits and, by extension, changes to that threat in the form of tort reforms.

Third, fee-for-service reimbursement systems, in which physicians are compensated based on the number of procedures they perform, encourage defensive medicine since a third party (e.g. Medicare), rather than patients or physicians, bears the financial costs of precautionary measures.

This paper examines the extent to which the health care patients receive is affected by changes in malpractice law, using measures relevant to patients and policymakers: cost and outcomes. The methodology used is similar to that of Kessler and McClellan (1996), who examined the impact of state-level tort reforms on health expenditures, mortality and readmission rates of elderly cardiac patients enrolled in Medicare. The current study extends KM's work by examining cardiac and non-cardiac patients and isolating the impact of individual types of reform (i.e. caps on damages) in addition to grouping reforms based on the extent to which they reduce awards paid to claimants by malpractice insurance companies. KM found that reforms which directly reduced awards could deliver five to nine percent reductions in a state's medical expenditures without affecting mortality or readmission rates. The current study largely confirms KM's findings with respect to outcomes but fails to replicate the previously reported cost reductions in states implementing reforms.

Section II reviews existing literature on defensive medicine and the effect of tort reforms on health care delivery. Section III describes the empirical framework and analysis used in this paper. Section IV details the sources of data, while section V presents the results. Section VI discusses the findings and limitations of this study. Section VII concludes with policy implications.

II. LITERATURE REVIEW

The empirical literature on defensive medicine can be grouped into two main categories: surveys asking physicians about defensive practices and empirical studies examining the relationship between measures of malpractice risk, cost of care and health outcomes at the state level.

Surveys of Physicians

Studdert et al. (2005) surveyed 824 Pennsylvania physicians in six high-risk specialties (determined by levels of malpractice insurance premiums) in May 2003, when the state was experiencing a crisis of rapidly rising premiums. Respondents were asked to rate on a scale of 1 to 4 how often concerns about malpractice risk influenced their clinical decision-making. Doctors were also asked about behaviors consistent with defensive medicine, which the researchers divided into two groups: assurance behaviors (commonly known as positive defensive medicine) and avoidance behaviors (negative defensive medicine). Assurance behaviors included ordering unnecessary tests, overprescribing medications and suggesting invasive procedures, such as biopsies, to confirm test results. Avoidance behaviors included refusal to performance procedures and/or treat patients thought to carry a high risk of malpractice liability. Self-reported rates of assurance and avoidance behaviors each exceeded 90 percent, with excessive use of imaging studies (e.g. X-ray, MRI) being the most commonly reported defensive practice.

In addition to selection bias[5], physician surveys are inherently susceptible to over-reporting of defensive medicine by self-interested doctors seeking passage of malpractice reforms, which almost always benefit physicians rather than potential claimants. Many professional medical societies have lobbied for malpractice forms, and physicians may consciously or unconsciously attempt to aid in those efforts (Mello, 2006). Some studies (Klingman et al., 1996; Liang, 1999) attempt to mitigate this effect by presenting physicians with clinical "scenarios" describing characteristics and medical indications of high-risk patients. Doctors are then asked to make hypothetical treatment decisions and choose from a list of factors impacting each decision, with malpractice liability being one of the given choices. Since such surveys do not question physicians about health outcomes and whether the reported "defensive" medicine actually benefits patients, they fail to distinguish defensive practices which nonetheless benefit patients from those which are truly wasteful. Thus, survey findings should be treated with caution and interpreted in conjunction with empirical methodology that examines actual, rather than reported, shifts in procedure use and how such shifts affect health outcomes.

Empirical Studies of Defensive Medicine

Measuring Malpractice Risk

The multi-factorial, complex nature of clinical decisions makes identifying care provided primarily to reduce the risk of malpractice liability a formidable task. The empirical methodology typically involves regressing some aspect or consequence of physicians'

[5] Selection bias occurs when recipients of the survey are given the option of whether or not to participate. Recipients who choose to complete and return a survey concerning malpractice may have stronger opinions than nonrespondents; respondents are perhaps more likely to have been named as a defendant in a malpractice lawsuit. In these cases, the sample population is not random, which can bias the results.

clinical behavior (procedures used, fees billed, complications recorded in patient charts or administrative databases) on a measure of malpractice liability risk while controlling for state and patient-specific characteristics. Liability risk is proxied in three ways:

(1) Malpractice insurance premiums paid by physicians.[6]

(2) The frequency (number) and/or severity (average award size) of malpractice claims in a physician's county or state.[7]

(3) The presence of state-level medical malpractice reforms which reduce the size and/or frequency of awards given to plaintiffs.[8]

Although one might expect that rising malpractice insurance premiums are caused by increased liability exposure and an excessive number of malpractice suits, substantial empirical evidence demonstrates that other factors, such as interest rates and competition among malpractice insurers, significantly impact premiums, reducing their utility as a measure of malpractice risk. Specifically, Baker (2005) described the "long tail" of medical malpractice insurance claims, whereby insurers are not fully aware of all losses stemming from a policy until up to 10 years after the policy was written, as the underlying cause of much of the volatility in malpractice insurance premiums.

Measuring malpractice risk using frequency and severity of claims at the state level has its own issues, chief among them data availability. Most state governments, with the exceptions of Texas and Florida, do not maintain centralized database of open or closed (resolved by settlement or trial) malpractice claims, nor do they require that malpractice insurers release such information. Consequently, studies matching variations in

[6] See Tussing et al. (1992), Localio et al. (1993), Dubay et al. (1999) and Baicker et al. (2002).
[7] See Tussing et al. (1992), Localio et al. (1993), Baldwin et al. (1996), Sloan et al. (1997), Baicker et al. (2002) and Kessler and McClellan (2002).
[8] See Kessler and McClellan (1996) and Currie and MacLeod (2008).

malpractice claims frequency or severity to differences in health care procedures, costs and outcomes across geographic areas are often limited to a single state[9] and/or single year[10] in which sufficient data was available, raising serious concerns about generalizability and omitted variable bias.

While single-state studies can examine the effect of geographic differences in malpractice liability risk within a state, they do not account for the impact of differences across states and may suffer from omitted variable bias, since a given geographic area's level of malpractice claims can be influenced by the quality of its health care providers, the general "sickness" or "healthiness" level of its population or differences in preferences for litigation. Cross-sectional studies also fail to measure the impact of time trends in malpractice risk, such as the passage of tort reforms.

A third method, employed in this study, uses longitudinal data detailing patient care and state-level legal changes to compare differences in health care expenditure growth and health care outcomes over time for reforming and nonreforming states. Measuring changes in malpractice risk using a longitudinal tort reforms database allows not only for examination of interstate differences and time trends in liability risk, but also provides a means of controlling for unobserved variables via state and time fixed effects. Importantly, the tort reforms methodology assumes that reforms are uncorrelated with unobserved differences across patients and states. Given that acute crises in specific states often receive nationwide attention and drive the lobbying efforts of national medical professional societies, this assumption is not *prima facie* unreasonable. Section

[9] Single-state studies surveyed include Tussing et al. (1992), Localio et al. (1993), Baldwin et al. (1995) and Sloan et al. (1997).
[10] Single-year studies surveyed include Tussing et al. (1992), Localio et al. (1993) and Sloan et al. (1997).

III provides a detailed description of the tort reforms methodology as it is implemented in this paper.

Defensive Medicine in Obstetrics

Due to the availability of rich datasets detailing natal care, much of the empirical literature on defensive medicine is focused on obstetricians and Caesarean sections. Kravitz et al. (1991) reviewed detailed records of 333 malpractice claims filed against obstetricians covered by a New Jersey malpractice insurer and found that failure to perform a Caesarean section was cited as reason for a malpractice claim 10 times more often than unnecessary performance of a Caesarean. Kim (2007) posited that obstetricians fearful of malpractice liability may prefer C-sections because they can exert more control over the situation and better manage potential complications.

Empirical studies of defensive medicine in obstetrics typically regress rates of Caesarean section, antenatal testing or some other measure of treatment intensity on proxies of malpractice risk, such as premiums or claims, at the physician, county or hospital level. While a positive relationship between malpractice liability and Caesarean section rates has been established in some cases (Localio et al., 1993; Dubay et al., 1999), most studies do not consider how increased rates impact patient health outcomes. In general, the empirical literature regarding the behavioral impact of malpractice risk on obstetricians' treatment decisions draws mixed conclusions.

Single-State Studies of Malpractice Risk and Obstetrician Behavior

Sloan et al. (1997) examine the impact of malpractice risk on obstetricians' rates of C-section and antenatal testing, both which were thought to increase with increased risk of malpractice liability. Malpractice risk was measured by individual obstetricians' claims history as well as the frequency and size of payments arising from obstetrics malpractice claims. The impact of malpractice liability risk on the decision to perform a Caesarean section was found to be insignificant at the five percent level using all proxies of malpractice risk. Results for antenatal testing were mixed and generally inconclusive.

Using cross-sectional data from New York state to construct eight measures of malpractice risk at the physician, hospital and county levels, Localio et al. (1993) find that five measures, including malpractice insurance premiums and hospital-level malpractice claims rates, are positively associated with increased rates of Caesarean section, after controlling for clinical risk of Caesarean delivery, patient socioeconomic status and physician and hospital characteristics. Baldwin et al. (1995) examine the effect of Washington state obstetricians' exposure to malpractice claims – measured by individual physicians' claims history and county-level claims rates – on practice decisions, including Caesarean section. In agreement with Localio et al. (1993), Baldwin et al. (1995) find physicians' individual claims history and county-level claims rates do not significantly impact the practice of obstetrics.

The Sloan, Localio and Baldwin studies offer limited evidence that malpractice concerns influence physician decisions in some cases. However, the utility of these studies is limited, since each is confined to observations in a single state. As intra-state differences in malpractice risk across geographic areas are largely driven by unobserved

idiosyncratic characteristics (quality of health care system or propensity of residents to sue), single-state studies are more susceptible to endogeneity problems than national, longitudinal studies which can better control for geographic idiosyncrasies.

National Studies of Malpractice Risk and Obstetrician Behavior

Dubay et al. (1999) study the impact of malpractice insurance premiums on rates of Caesarean section and measures of infant health while controlling for underlying medical risk for Caesarean section and maternal and natal health indicators. The researchers find small but statistically significant increases in C-section rates for mothers in areas of high malpractice liability risk, and the effect was more pronounced for mothers of low socioeconomic status, who were posited to be more litigious and thus more likely to receive defensive care. No impact on birth outcomes, as measured by APGAR[11] scores, was found.

Tussing and Wojtowycz (1992) examine the impact of a variety of economic incentives, including the threat of malpractice, on Caesarean section rates in New York State. They find that increases in malpractice claims risk actually decrease the rate of Caesarean sections, contrary to popular belief. However, the single-state, single-year nature of the study raises serious endogeneity concerns.

Taken in sum, the obstetrics studies present mixed results. When malpractice liability is found to have an effect on physician decision-making, it is generally small. While several studies use detailed measures of health outcomes and malpractice risk, the labor involved in constructing these measures means that most studies examine data over

[11] The APGAR score, which is used to measure the health of an infant immediately after birth, is calculated on a 1-10 scale and awards 0, 1 or 2 points in each of the following categories: muscle tone, response to stimulus, pulse, breathing and skin color (Apgar, 1953).

relatively short time periods in a single state and may have issues with endogeneity and generalizability. The current study avoids these issues by examining nationwide, longitudinal data spanning 10 years of health care costs and outcomes in Medicare.

National Studies of Non-Obstetric Defensive Medicine

Baicker et al. (2007) examine the impact of malpractice liability on procedure choice and cost of care in Medicare patients treated nationwide from 1993 to 2001. Malpractice risk was measured in two ways: state-level average malpractice awards to claimants on a per physician basis and malpractice insurance premiums. Dependent variables included Medicare spending per beneficiary as well as utilization rates for imaging procedures. A 10.0 percent increase in average malpractice payments per physician in a given state was associated with a 1.0 percent increase in overall Medicare spending and a 2.2 percent increase in spending on imaging.

In a national study of elderly cardiac patients, Kessler and McClellan (2002) regressed Medicare expenditures and outcomes for cardiac patients on various measures of malpractice pressure. Using their results to evaluate the impact of hypothetical tort reforms, KM report that for each dollar reduction in average Allocated Loss Adjustment Expense (sum of awards paid to claimants and legal costs incurred as a result of cardiac malpractice cases), $4.76 would be saved in Medicare expenditures with no significant impact on health outcomes. Taken together, the Baicker and KM studies support the assertion that health care cost savings can be achieved by reducing physicians' exposure to malpractice liability.

Tort Reforms and Health Outcomes

Summarizing empirical studies on the effects of tort reforms, the most consistent findings are that caps on damage awards reduce the number of malpractice lawsuits, the aggregate amount paid out in awards to claimants and the malpractice insurance premiums paid by physicians (Holtz-Eakin, 2004). Conclusions regarding reforms' impact on the cost and quality of health care – the focus of this study – are less numerous, and the empirical literature has not yet reached a consensus.

Currie and MacLeod (2008) examine the effect of tort reforms on birth outcomes nationwide from 1989 to 2001 and find that different types of reform have opposing effects. Reforms analyzed included caps on noneconomic damages, abolishment of punitive damages, joint and several liability (JSL) reform and collateral source reform (CSR).[12] Currie and MacLeod's strongest finding is that JSL reform reduces rates of preventable complications and Caesarean sections, possibly by more closely aligning a physician's risk of malpractice liability with his own behavior. Contrary to popular belief, caps on noneconomic damages[13] were shown to increase the rate of Caesarean sections. However, noneconomic damage caps had no impact on birth outcomes, and abolishing punitive damages did not impact C-section or complication rates.

Sloan and Shadle (2009) examine the impact of direct and indirect reforms on Medicare payments and survival rates using data collected by the National Long-Term Care Survey and Medicare between 1985 and 2000. Disease-based populations analyzed include acute myocardial infarction (heart attack), breast cancer, diabetes mellitus and

[12] See Table I for descriptions of various tort reforms, as well as KM's classification of direct and indirect reforms.

[13] Awards in malpractice cases are typically composed of economic damages (actual monetary loss including lost wages) and noneconomic damages (compensation for pain and suffering or punitive damages, if allowed under state law).

stroke. Neither direct nor indirect reforms were shown to significantly impact Medicare payments or survival rates. The current study extends this analysis by using a different data set (Medicare Current Beneficiary Survey) over a different time period (1995 to 2004), covering additional disease-based populations (chronic obstructive pulmonary disease and pneumonia) and incorporating an additional outcome measure (readmission within six months of diagnosis). In addition to analyzing the impact of reforms when placed in groups (e.g. direct and indirect), the current study also isolates the effect of individual types of reforms (e.g. caps, JSL, etc.).

In a landmark study of defensive medicine, Kessler and McClellan (1996) examined the care of Medicare beneficiaries hospitalized with a first-time diagnosis of acute myocardial infarction or ischemic heart disease in 1984, 1987 and 1990. KM quantified the impact of tort reforms on cost of care and rates of mortality or rehospitalization due to cardiac illness within one year following initial treatment. By comparing differences in costs and outcomes across reforming and nonreforming states, the marginal impact of changes in malpractice pressure (through legal reform) was isolated. Expenditures and outcomes were modeled as functions of individual patient demographic characteristics, state-specific political and legal characteristics (including the implementation of tort reforms) and state and time fixed effects.

KM found that direct malpractice reforms, those that reduced awards to claimants by "truncating the awards distribution or reducing its mean" (e.g. caps) rather than indirectly (e.g. limits on attorney fees), lead to cost reductions of up to nine percent without significantly impacting outcome measures. Somewhat paradoxically, indirect

reforms were shown to increase heart disease patient expenditures by three percent, also without impacting outcomes.

The KM study has several strengths. It is nationwide in scope and examines important outcome measures for the treatment of cardiac disease, a leading cause of death and significant source of health care expenditures. Its definition of defensive medicine as increasing health expenditures without impacting patient outcomes distinguishes defensive but beneficial treatments from those which are simply wasteful. Measuring malpractice risk using changes in legal rules over time, rather than malpractice insurance premiums or the frequency and severity of claims, reduces the risk of contamination by geographically-specific factors (quality of health care system, litigiousness of population) that could distort the true impact of the malpractice liability system.

On the other hand, the study's sample population (covering only elderly Medicare patients with cardiac disease) means generalizability a serious concern. As KM point out, generalizing the cost savings of direct reforms (under the assumption all states had such reforms in place for the duration of the study) to all cardiac care provided in the United States yields annual savings estimates of $500 million with no significant impact on health outcomes. Expanding the estimate to include care given to patients of all ages with various types of disease produces savings of over $50 billion. Both of these estimates, particularly the latter, require significant leaps from the observed results. Nonetheless, as a longitudinal study that uses a large and well-specified sample population while controlling for various confounding variables, KM provides strong evidence of defensive medicine in the treatment of elderly cardiac patients. Although this paper examines a different data set over a more recent time frame and measures the impact of specific

reforms in addition to KM's groupings of direct and indirect reforms, the basic methodology employed in this paper is most similar to that of KM in their seminal 1996 work.

TABLE I. KESSLER AND MCCLELLAN'S DIRECT AND INDIRECT REFORMS

Reform	**Description**	***Type***
Caps on damages	Statutorily imposed limit on amount that can be awarded for economic or (more typically) noneconomic damages	Direct
Collateral source reform	Judgments awarded to plaintiffs are reduced by amount received from other sources (i.e. health insurance, other lawsuit)	Direct
Abolishment of punitive damages	Awards limited solely to economic damages	Direct
Contingency fee caps	Statutorily imposed limits on the fees plaintiffs' attorneys charge as a percentage of amount awarded	Indirect
Mandatory periodic payments	Awards given to plaintiffs as annuities rather than lump-sum payments	Indirect
Joint and several liability	Joint and several liability (whereby plaintiffs found partially responsible for damages can be held responsible for full amount of damages) abolished	Indirect

TABLE II. SUMMARY OF REVIEWED LITERATURE

Pub. Year	1st Author	Malpractice Risk Measure	Impact Measure	Scope	Period	Result of Incr. Liability Risk
1992	Tussing	-Premiums -Claims per MD	-C-section	-New York State -Analyzed at county level	1986	-Reduce C-section
1993	Localio	-Premiums -Survey of perceived risk -Claims per 100 MD's -Claims per 1000 hospital discharges -Indiv. claims hist.	-C-section	-New York -Analyzed at physician, hospital and geographic region level (NY divided into 4 regions)	1984	-All risk measures incr. C-section. except indiv. claims history
1995	Baldwin	-Indiv. claims hist. -Claims rate	-C-section -Ultrasound -Prenatal care resources	-Washington state -Analyzed at county level	Sep. 1988 – Aug. 1989	-All insig.
1997	Sloan	-Indiv. claims hist. -F/S of claims	-C-section -Prenatal testing	-Florida -Analyzed at county level	1987	-C-section insig. -Testing mixed -Satisfaction insig.
1996	Kessler	-Tort reforms -Direct: Caps, CSR, no pun. -Indirect: Cont. fees, MPP, JSL	-Mortality or rehospitalization within 1 year	-Nationwide -Analyzed at county level	1984, 1987, 1990	-Direct reduces costs 5 to 9 percent -Indirect reduces costs 1.8 percent -Neither type sig. impacts outcomes
1999	Dubay	-Premiums	-C-section -APGAR	-Nationwide -Analyzed at county level	1990-1992	-Pos. for c-section, increasing effect with low socioeconomic status -Insig. for APGAR
2002	Kessler	-Freq./severity -Duration -Tort reforms	-Cost and outcomes (rehospitalization) -Services ordered	-Nationwide -Analyzed at state level	1984-1994	-Pos. for all measures on cost, insig. for all measures on outcomes -Pos. for diagnostic expenditure
2002	Baicker	-Premiums -Severity	-Costs -Procedures -Mortality	-Nationwide -Analyzed at state level	1993-2001	-Pos. for severity on costs -Pos. for severity on imaging -Insig. for both on mortality
2008	Currie	-Tort reforms (caps, JSL, CSR)	-C-section -Complications	-Nationwide -Analyzed at county level	1989-2001	-Pos. for Caps on both -Neg. for JSL on both -CSR insig.

Table II summarizes the variables, scope and results of existing literature examining the link between malpractice risk (measured using premiums, claims, or reforms) and health care costs or outcomes. Taken in sum, the literature offers few definite conclusions. With the exception of Currie and MacLeod (2008), no study reports that changes in malpractice liability risk affect health outcomes.

The literature is still far from consensus when it comes to the relationship between costs, outcomes and shifts in a state's malpractice liability climate. Some studies find no significant evidence of defensive practices in obstetrics (Baldwin et al. 1995; Sloan et al. 1997), while others examining cardiac and general health care show significant cost and/or outcome shifts in response to changes in malpractice liability (Kessler et al. 1996, 2002; Baicker et al. 2002). Still others draw mixed conclusions themselves, finding significant results only using certain measures of malpractice risk or outcome metrics (Dubay et al. 1999; Currie et al. 2008).

The resulting ambiguity concerning the existence of defensive medicine, as well as the limited scope (single-state, obstetrics only) of several studies, calls for further examination of this important issue in a way that can provide sensible policy recommendations.

III. EMPIRICAL FRAMEWORK

The current study's methodology is modeled after that of Kessler and McClellan (1996) in examining levels of defensive medicine in cardiology. However, several important changes separate this paper from the work of KM and previous literature in the field. First, health outcomes are measured over the 10-year period spanning 1995-2004, while KM used outcomes data from 1984, 1987 and 1990. Second, in addition to grouping reforms into "direct" and "indirect" groups like KM, this paper examines the impact of individual types of reform (caps, joint and several liability and collateral source reform) on health care costs and outcomes. Third, the current study extends KM's work in heart disease patients to include patients afflicted with other potentially life-threatening medical conditions.

The extent to which time trends in health expenditures and outcomes in a given state can be explained by changes in the state's malpractice risk climate, as indicated by tort reforms, is quantified. Analysis is conducted at the state level, since tort reforms have historically been enacted on a state-by-state basis. In addition, the panel nature of the data set allows for the use of state and time fixed effects to control for unobserved state-specific characteristics and time trends.

Two separate but similar sets of regressions are run:

$$Outcome_{ist} = \varphi REFORM_{ist} + \lambda PATIENT_{ist} + \delta_t + \omega_s + \varphi_{ist} \quad (1)$$

$$Cost_{ist} = \pi REFORM_{ist} + \mu PATIENT_{ist} + \alpha_t + \beta_s + \varepsilon_{ist} \quad (2)$$

In the first set of regressions, the health outcome of patient *i* receiving care in state *s* during year *t* is modeled as a function of:

- $REFORM_{ist}$, a binary variable indicating the presence or absence of a specific reform at the time the patient was diagnosed.
- $PATIENT_{ist}$, a vector of patient-specific characteristics: age, gender, race, income, education and Charlson comorbidity score.[14]

State and time fixed effects (ω_s and δ_t for outcomes, β_s and α_t for cost respectively) control for geographic differences (e.g. quality of local health care system) and time-varying, national health care trends (e.g. improved technology). The heteroskedasticity-robust error terms φ_{ist} and ε_{ist} have an expected value of zero. Two binary outcome indicators — death or readmission with same diagnosis within six months of initial diagnosis — are used for $OUTCOME_{ist}$, with separate regressions run for each indicator. The cost regressions use identical covariates, with the dependent variable being a logged sum of all Medicare expenditures on behalf of patient *i* during the six month period following a first-time (index) diagnosis for a potentially lethal medical condition. Similar to the interpretation provided by Kessler and McClellan (1996), the evidence is consistent with defensive medicine when $\pi < 0$ (reforms reduce expenditures) and $\varphi = 0$ (reforms do not impact outcomes).

The decision to use KM's reforms-focused methodology was motivated by several factors. First, measuring risk using legal changes avoids several of the confounding determinants (e.g. interest rates, level of competition among insurers) of malpractice insurance premiums that limit their utility as a proxy of malpractice risk

[14] The Charlson comorbidity score quantifies a patient's risk for death based on pre-existing medical conditions and is used here as a measure of overall health. See Quan et al. (2002) for a study assessing the validity of Charlson comorbidity scores compiled using ICD-9 administrative data, as is used in this study.

(Baker, 2005). Second, using a nationwide database of tort reforms implemented over a 10-year period, rather than claims data limited to certain insurance companies or states, allows for a larger and more representative sample (reforming states) and control (nonreforming states) groups while mitigating concerns about generalizing results from one state to the rest of the nation. Third, analyzing the effect of tort reforms on the cost and quality of care provided to patients is of great policy significance. Better understanding how patients are affected by reforms can allow policymakers to evaluate proposed legal changes, or perhaps even design new reforms, on the basis of the medical and financial ramifications. However, a potential weakness of the reforms methodology is that it assumes physicians are cognizant of changes in malpractice law. Given that reforms are often adopted during "times of crisis" and rising malpractice insurance premiums (Mello, 2006), when physicians are acutely aware of malpractice liability, such an assumption is not *prima facie* unreasonable.

The diagnoses used to sample the Medicare population were four of the five leading medically-related causes of death among persons over the age of 65: heart disease, stroke, chronic obstructive pulmonary disorder (COPD) and pneumonia ("Data Watch," 2006).[15] For each of the four disease-based sample populations, five cost and 10 outcome (five mortality and five readmission) regressions were run, each differing in the construction of the $REFORM_{ist}$ indicator variable (DIRECT, INDIRECT, CAPS, JSL or CSR). First, the impact of the presence of direct and indirect reform measures on states' costs and outcomes is examined. In addition to the direct and indirect regressions, three additional regressions analyze the impact of each type of reform (caps, joint & several

[15] Cancer patients were not examined due to lack of data regarding stage of cancer progression at time of diagnosis, an important outcome predictor for cancer patients.

liability and collateral source offset reform) individually. Thus, for each of the three dependent variables – expenditures, six-month mortality and six-month readmission, five separate regressions are run, differing only in which reform indicator variable is used (DIRECT, INDIRECT, CAPS, JSL or CSR).

Reform indicator variables are binary and equal to one if the state in which the patient received an index diagnosis had the reform of interest (i.e. any direct reform or a cap on damages) in place at the time of index diagnosis and zero otherwise. For example, a patient diagnosed with ischemic heart disease in a hypothetical state X in 1997 would be coded as CAP=1, JSL=0 and CSR=0 since state X had damage caps in place in 1997 but did not have joint and several liability nor collateral source reform in place. Since damage caps are a direct reform and state X had no other reforms in place at the time of index diagnosis, the patient would be coded as DIRECT=1 and INDIRECT=0 . Had that patient been diagnoses with IHD in 1998, after state X had repealed its cap on damages and thus had no reform measures in place, the patient would have received a zero for all five reform indicator variables.

A separate regression is run using each reform indicator variable to avoid issues arising from multicollinearity among the reform indicator variables. For instance, a patient receiving care in a state with a damage cap in place at the time of diagnosis is coded as $CAP_{ist} = 1$ and $DIRECT_{ist=} 1$, since damage caps are a type of direct reform. Since states often implement multiple types of reform in a single piece of legislation, running separate regressions indicates whether reforms as a group (e.g. DIRECT) have an impact as well as if any particular type of reform is driving that impact (e.g. CAPS). The analysis is repeated for each of the four patient populations (heart disease, stroke, COPD

and pneumonia), yielding a total of sixty regressions. Figure I provides a visual representation of the regression strategy using the heart disease population as an example.

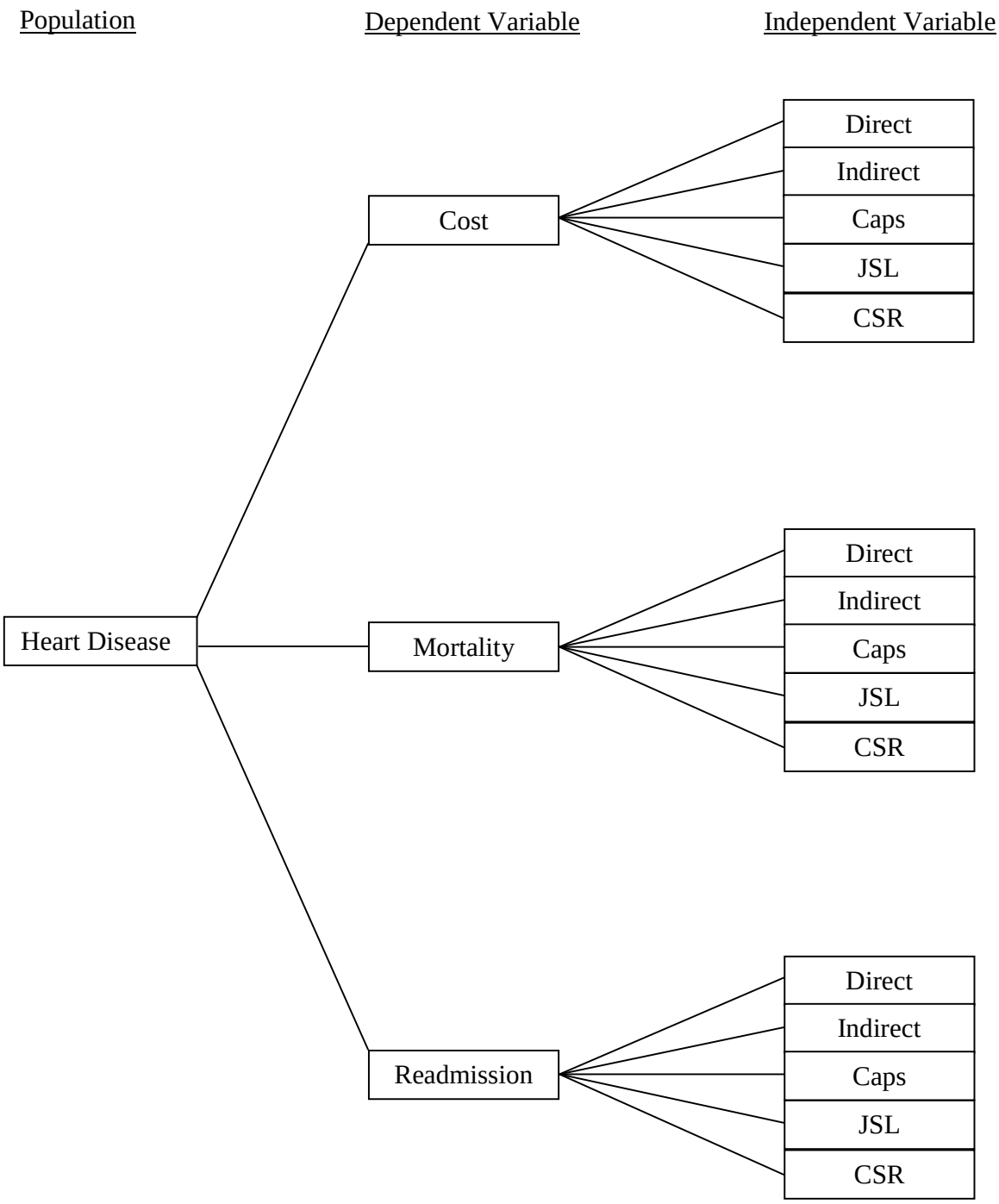
Reforms are modeled as first being in place on the day they go into effect (not necessarily the day they are signed into law). Reform measures, especially caps on damage awards, often face constitutional challenge and have been subsequently invalidated by the courts (Kelly and Mello, 2005). In these cases, the reforms are coded as being no longer in place starting with the day on which the relevant judgment is issued. Since malpractice reform measures are often subject to extensive public debate, with both medical and legal professional associations informing their membership of the timing and implications of upcoming legal changes, no lag measures are introduced when coding reforms.

In addition to standard demographic covariate controls, such as age, gender, race, education and income, a Charlson comorbidity score is also calculated for each patient in the sample based on diagnoses attached to Medicare claims filed in the six months preceding the patient's index diagnosis. The Charlson algorithm assigns varying weights to 22 conditions known to increase the likelihood of death and sums the number of (weighted) conditions to obtain a score. The intent in using the Charlson index is to control for the patient's general level of "sickness," or pre-existing risk for death or readmission, at the time of index diagnosis.

An important assumption of this model, along with those of KM and Currie and MacLeod, is that unobservable characteristics of patients which affect medical decision-making and health outcomes are homogenous when patients are grouped at the state level and are also uncorrelated with the implementation of tort reforms. Several exclusion

criteria ensured members of the sample population were not unusually prone to poor health care outcomes and could reasonably stand to benefit based on the quality of care provided to them. Beneficiaries under the age of 65, who qualify for Medicare coverage due to disability, were excluded, as were those with end-stage renal disease, who are particularly prone to poor health care outcomes. As a given beneficiary participates in the MCBS for a three-year period, patients diagnosed in the first six months or last six months of their survey rotation were excluded, since insufficient data was available on pre-existing conditions and expenditures following the index diagnosis. A small number of patients (less than two percent of the otherwise eligible population) whose demographic data, such as education or race, were missing or inconsistently coded were also dropped from the analysis to ensure integrity of the data.

FIGURE I. REGRESSION FLOW CHART



IV. DATA

Data on patients' health outcomes are taken from the Medicare Current Beneficiary Survey (MCBS), conducted annually by the Center for Medicare & Medicaid Services. The MCBS Cost & Use files analyzed for this study include data on services provided, diagnoses recorded and fees billed for tens of thousands of elderly Medicare patients treated nationwide from 1995 to 2004. Self-reported information on patients' physical and mental health status, health conditions and socio-demographic characteristics are also included.

Table III provides sample sizes for each of the four disease-based populations (heart disease, stroke, COPD and pneumonia) as well as mean values for variables measuring demographics, reforms, costs and outcomes. As expected, Medicare beneficiaries diagnosed with potentially life-threatening conditions were generally of advanced age (in their late 70s on average). The lower proportion of males reflects the general composition of the Medicare beneficiary population, which is 44 percent male according to a recent report (Murgolo, 2008).

TABLE III. MEAN DESCRIPTIVE STATISTICS OF SAMPLE POPULATIONS

	Heart Disease	***Stroke***	***COPD***	***Pneumonia***
Population Size	4116	2710	3901	2445
Demographics & Controls				
Age	78.6	80.2	78.0	80.7
Gender (male=1)	0.44	0.41	0.40	0.42
Annual Income (USD)	25,287	22,581	24,118	21,902
Charlson Comorbidity	0.97	1.47	0.93	1.21
Education (years)	10.8	10.6	10.7	9.9
Explanatory Variables				
DIRECT	0.78	0.76	0.75	0.75
INDIRECT	0.76	0.74	0.72	0.73
CAP	0.60	0.60	0.60	0.57
JSL	0.56	0.54	0.53	0.53
CSR	0.61	0.57	0.56	0.56
Outcome Measures				
Six-Month Mortality	0.048	0.087	0.041	0.167
Six-Month Readmission	0.201	0.131	0.145	0.095
Six-Month Expenditures (USD)	21,090	22,471	14,523	22,670

The pneumonia-afflicted population had lower levels of education and income compared to the other populations, but as its size is 50 percent smaller than that of the largest population (heart disease), it is also most subject to chance fluctuations. Importantly, approximately 75 percent of persons selected for the study were first diagnosed with a potentially fatal condition at a time in which their state had imposed at least one direct (or indirect) tort reform. Average coverage for individual types of reforms is considerably lower, in the 60 percent range.

Surveying the dependent variables in the four disease-based populations, pneumonia had the highest six-month rate of mortality (16.7 percent), while chronic obstructive pulmonary disease had the lowest (4.1 percent). Patients diagnosed with heart disease were most likely to be readmitted with the same diagnosis within six months (20.1 percent), while those diagnosed with pneumonia were least prone to readmission (9.5 percent). Six-month expenditures for patients with heart disease, stroke and pneumonia were in the 21,000 to 23,000 USD range, while costs for COPD patients were dramatically lower at approximately 14,500 USD, perhaps reflecting higher levels of outpatient management (e.g. inhalers, home breathing assistance) among those affected and less intensive hospital therapy.

Data on tort reforms are taken from a publicly available database compiled by Currie and MacLeod (2008) for their paper on tort reforms and birth outcomes. The data include a comprehensive list of the timing and nature of various reforms implemented nationwide between January 1, 1985 and December 31, 2005. Reforms covered include caps on noneconomic damages, caps on punitive damages, abolishment of joint and several liability and collateral source offset. Federal Consumer Price Index (CPI) data

was used to adjust Medicare expenditures for inflation. All expenditures are reported in 2004 U.S. dollars.

A modified system of state fixed effects was used to control for geographic, state-specific factors (e.g. quality of medical system, number of academic teaching hospitals, etc.). For the purpose of state fixed effects (but not reform indicators), patients from states with a small number of patients (generally less than 15) in the sample were coded as living in a larger, neighboring state.[16] This strategy resolved the issue of state dummy variables perfectly predicting success or failure for certain binary outcome variables in underrepresented states. For example, all Rhode Island patients were alive six months after diagnosis and being in Rhode Island perfectly predicted survival, confounding the analysis. ICD diagnosis codes[17] were used to select for patients receiving target diagnoses during the study period.

[16] Patients from the following nine states and the District of Columbia were reclassified as residing in the states listed in parentheses: Connecticut (Massachusetts), Delaware (Maryland), Utah (Colorado), Oregon (Idaho), Mississippi (Louisiana), New Hampshire (Maine), Rhode Island (Massachusetts), District of Columbia (Virginia), North Dakota (Nebraska) and South Dakota (Nebraska).

[17] The International Classification of Disease (ICD) coding system is a standard published by the World Health Organization to identify medical procedures, diseases, symptoms and causes of injury or disease. The Medical Current Beneficiary Survey claims files used in this study are required to contain an ICD diagnosis code indicating the primary disease or condition for which the patient was treated.

TABLE IV. THE EFFECTS OF TORT REFORMS ON SIX-MONTH MEDICARE EXPENDITURES

	Heart Disease	***Stroke***	***COPD***	***Pneumonia***
Primary Specification				
Direct	-0.137 (0.476)	0.747 (0.445)	-0.096 (0.438)	-0.185 (0.534)
Charlson	0.328 (0.021)**	0.310 (0.024)**	0.384 (0.021)**	0.300 (0.022) **
Education	0.010 (0.007)	-0.001 (0.008)	-0.007 (0.007)	0.002 (0.008)
Income	0.004 (0.007)	0.001 (0.004)	0.005 (0.006)	-0.007 (0.001)
Male	-0.086 (0.062)	0.141 (0.072)*	0.073 (0.063)	0.097 (0.075)
Black	-0.023 (0.107)	0.108 (0.125)	0.131 (0.131)	0.236 (0.138)
Age	-0.020 (0.004)**	-0.007 (0.004)	0.010 (0.004)**	-0.016 (0.005)**
Addl. Specifications				
Indirect	-0.650 (0.533)	0.619 (0.477)	0.075 (0.439)	-0.420 (0.666)
Caps	0.024 (0.178)	0.033 (0.179)	0.209 (0.174)	-0.189 (0.182)
JSL	0.301 (0.164)	-0.234 (0.189)	0.080 (0.175)	0.270 (0.181)
CSR	-0.111 (0.221)	0.111 (0.218)	0.019 (0.185)	-0.230 (0.220)

OLS regression coefficients reported with standard errors in parentheses. Dependent variable is logged total Medicare expenditures on patient's care during six months following index diagnosis. Due to space considerations, coefficients for state and year dummies and control variables for additional reform specifications not shown. Magnitude and statistical significance of control variables did not materially differ among the various reform specifications. Dummy variable indicating whether stroke was ischemic or hemorrhagic not shown. Income is annual and measured in thousands of U.S. Dollars.
*5% significance.
**1% significance.

TABLE V. THE EFFECTS OF TORT REFORMS ON SIX -MONTH MORTALITY

	Heart Disease	***Stroke***	***COPD***	***Pneumonia***
Primary Specification				
Direct	1.192 [0.149, 9.512]	1.321 [0.267, 6.547]	0.998 [0.062, 15.995]	0.517 [0.094, 2.860]
Charlson	1.305 [1.208, 1.409]**	1.231 [1.132, 1.338]**	1.298 [1.201, 2.707]**	1.106 [1.042, 1.435]**
Education	0.936 [0.903, 0.967]**	0.971 [0.937, 1.005]	0.949 [1.201, 1.403]*	0.991 [0.966, 1.017]
Income	1.000 [1.000, 1.000]	1.000 [1.000, 1.000]	1.000 [1.000, 1.000]	1.000 [1.000, 1.000]
Male	1.306 [0.944, 1.808]	1.441 [1.069, 1.942]*	1.510 [1.080, 2.112]*	1.668 [1.325, 2.101]**
Black	0.866 [0.518, 1.448]	1.507 [0.985, 2.308]	1.491 [0.839, 2.650]	1.123 [0.773, 1.630]
Age	1.077 [1.053, 1.102]**	1.074 [1.053, 1.095]**	1.053 [1.030, 1.076]**	1.055 [1.040, 1.070]**
Addl. Specifications				
Indirect	1.886 [0.360, 9.891]	0.221 [0.030, 1.613]	0.479 [0.008, 30.501]	0.233 [0.044, 1.228]
Caps	0.748 [0.361, 1.551]	1.122 [0.584, 2.158]	0.800 [0.372, 1.722]	0.458 [0.270, 0.778]**
JSL	1.140 [0.547, 2.377]	1.135 [0.561, 2.298]	1.248 [0.605, 2.573]	0.608 [0.351, 1.052]
CSR	2.260 [0.976, 5.236]	0.462 [0.165, 1.291]	0.994 [0.364, 2.716]	0.856 [0.440, 1.663]

Logistic regression odds ratios reported with 95 percent confidence intervals in brackets. Dependent variable is binary and equal to one if patient died within six months of index diagnosis. Due to space considerations, coefficients for state and year dummies and control variables for additional reform specifications not shown. Magnitude and statistical significance of control variables did not materially differ among the various reform specifications. Dummy variable indicating whether stroke was ischemic or hemorrhagic not shown.

*5% significance.

**1% significance.

TABLE VI. THE EFFECTS OF TORT REFORMS ON SIX -MONTH READMISSION

	Heart Disease	***Stroke***	***COPD***	***Pneumonia***
Primary Specification				
Direct	2.440 [0.704, 8.452]	1.091 [0.239, 4.989]	4.707 [0.580, 38.192]	0.980 [0.192, 5.005]
Charlson	1.004 [0.949, 1.063]	1.048 [0.978, 1.122]	1.030 [0.969, 1.095]	1.117 [1.033, 1.207]**
Education	0.995 [0.976, 1.016]	1.007 [0.979, 1.037]	0.995 [0.974, 1.108]	0.982 [0.953, 1.011]
Income	1.000 [1.000, 1.000]	1.000 [1.000, 1.000]	1.000 [1.000, 1.000]	1.000 [1.000, 1.000]
Male	1.193 [1.106, 1.402]*	0.961 [0.759, 1.217]	1.014 [0.840, 1.223]	0.846 [0.636, 1.125]
Black	0.903 [0.670, 1.217]	1.108 [0.738, 1.665]	1.055 [0.725, 1.535]	0.916 [0.546, 1.537]
Age	1.010 [1.000, 1.022]	1.004 [0.990, 1.019]	0.995 [0.982, 1.007]	0.993 [0.975, 1.011]
Additional Specifications				
Indirect	4.177 [0.762, 22.892]	2.191 [0.490, 9.800]	1.944 [0.476, 7.939]	0.339 [0.060, 1.901]
Caps	0.726 [0.493, 0.069]	0.751 [0.439, 1.286]	1.070 [0.679, 1.685]	1.267 [0.661, 2.431]
JSL	0.676 [0.447, 1.022]	0.567 [0.305, 1.054]	0.730 [0.433, 1.231]	1.073 [0.581, 1.980]
CSR	1.362 [0.830, 2.233]	0.770 [0.410, 1.446]	1.026 [0.613, 1.717]	0.281 [0.120, 0.659]**

Logistic regression odds ratios reported with 95 percent confidence intervals in brackets. Dependent variable is binary and equal to one if patient died within six months of index diagnosis. Due to space considerations, coefficients for state and year dummies and control variables for additional reform specifications not shown. Magnitude and statistical significance of control variables did not materially differ among the various reform specifications. Dummy variable indicating whether stroke was ischemic or hemorrhagic not shown.
*5% significance.
**1% significance

V. RESULTS

Table IV presents the impact of tort reforms on patients' Medicare expenditures during the six-month period following an index diagnosis (heart disease, stroke, COPD or pneumonia). Importantly, each coefficient reported for a "reform variables" came from a separate regression in which that reform indicator variable was the only reform indicator included in the regression. Control variables were included in all regressions, but only the results from the first regression using the direct reforms indicator variable are reported, as changing the reforms indicator did not significantly impact the magnitude or statistical significance of any of the control variables' coefficients.

Looking at costs in the heart disease population, which is most similar to Kessler and McClellan's (1996) analysis, we see that none of the reforms indicator variables have a statistically significant impact on six-month health expenditures. However, in three of the four populations (stroke being the exception), the signs and magnitudes of the "direct reforms" coefficients generally agree with KM's finding that direct reforms are associated with a five percent reduction in costs. Turning to the indirect and specific (caps, JSL, CSR) reforms indicators, the signs and magnitudes of the coefficients are mixed, with no coefficient coming close to statistical significance.

As expected, patients with higher Charlson scores, who were generally more "sick" at the time of index diagnosis, are responsible for greater Medicare expenditures in the six-month period following an index diagnosis. Increased age is associated with reduced expenditures in three of the four patient populations at a one percent confidence level, perhaps reflecting the increased likelihood of death and the higher prevalence of do not resuscitate orders in more elderly populations. With the exception of males in the

stroke population − whose expenditures were 14.1 percent higher than that of females − education, income, gender generally do not impact expenditure levels.

Table V presents the effect of tort reforms on six-month mortality rates. Among various reform specifications, only caps in pneumonia patients are statistically significant at the five percent level. Pneumonia patients in states with some type of damage cap in place at the time of admission were about half as likely to die as pneumonia patients in states without caps. In obstetrics, Currie and MacLeod (2008) showed through theoretical modeling and empirical results that joint and several liability reform reduces complication rates (and thus improves health outcomes) by better aligning physicians' incentives with respect to the care they provide. The reduction in mortality rates resulting from the presence of damage caps is unprecedented, as no previous study has examined the impact of a specific type of reform on mortality rates in the pneumonia population. Although this particular finding is somewhat at odds with KM's conclusion that direct and indirect reforms do not affect mortality rates in heart disease patients, the vast majority of Table V is in agreement with KM, finding no statistically significant impact for 19 of the 20 patient type/reform indicator combinations.

Turning to the general plausibility and validity of the regressions, several controlling covariates have the expected impact on mortality rates at strong levels of statistical significance. Higher Charlson comorbidity scores are strongly associated with increased risks of mortality, ranging from a 10 percent higher risk for pneumonia patients to 30 percent for heart disease. Higher levels of education are associated with lower risk of death in two of the four patient populations and have no impact in the two remaining groups. Males afflicted with stroke, COPD and pneumonia are significantly more likely

to die within six month than females (about 1.5 times on average), and each additional year of age is associated with an approximate five percent increase in mortality risk for these same three populations. Interestingly, gender and age have no significant impact on heart disease patients' mortality risk.

Table VI presents the impact of reforms on six-month readmission rates. In line with the results of KM, reforms are generally not associated with any effect on readmission rates (with the exception of decreased readmission rates for pneumonia patients in states implementing collateral source reform). In fact, there are only a few statistically significant results for readmission. Clearly, moving beyond heart disease patients and examining the impact of individual types of reforms complicates KM's picture of reforms' potential to reduce costs without affecting health outcomes.

VI. DISCUSSION

The potential impact of medical malpractice reforms on the cost and quality of care delivered to patients is of great interest to policymakers. Health care costs continue to climb, and the evidence presented by KM in 1996 suggested that so-called "direct reforms" have the potential to restrain growth in health care expenditures without affecting two relevant and important health care outcomes – mortality and readmission. KM's results, however, dealt only with a single diagnosis-based subgroup: cardiac disease. Their findings in heart disease patients, although weakly replicated in this study, do not seem to translate to patients afflicted with other forms of disease. The current study, which runs from 1995 to 2004, picks up nearly five years after the conclusion of KM, which examined patients treated in 1984, 1987 and 1990.

Rather than confirming KM's implicit assertion that tort reforms reduce defensive medicine, restraining growth in health care costs without affecting outcomes, the current study argues that reforms, whether analyzed in groups or individually, have no meaningful impact on medical decision-making (as evidenced by expenditures, mortality and readmission). To the extent wasteful expenditure on defensive medical care exists, changes to legal rules governing medical malpractice cases do not seem to affect the volume of such expenditures, after controlling for patient demographics, state-specific factors and time trends. Neither does lowering physicians' risk of malpractice lawsuits, however, generally place patients at greater risk of mortality or readmission for the same medical condition during the six months following first-time diagnosis with a potentially lethal disease.

In terms of health care delivery and reimbursement, the landscape of the late 1990s and early 2000s is different from that of the late 1980s in several respects. The rise of managed care, which seeks to control health care costs by negotiating agreements with providers in the managed care organization's approved "network," has lessened doctors' influence in determining what treatments are provided and how much they cost. This transfer of decision-making power from physicians to payers (private and public health insurance programs which decide what care to reimburse) may help explain the failure to replicate KM's cost results, as physicians are less able to alter their practice and billing in response to changes in states' malpractice climates.

That reforms can significantly impact health outcomes has already been shown by Currie and MacLeod (2008), who found that JSL improves outcomes (as measured by complication rates) while damage caps tend to worsen outcomes. Again, however, CM's findings are limited to a specific patient subgroup: pregnant women undergoing labor and delivery. Inside their respective patient populations, the findings of KM and CM are self-contained and consistent. As results are compared across various types of patients and physicians, however, discord starts to emerge. The current study, examining four types of patient subgroups, makes clear that findings dealing with one area of medical practice may not readily translate outside their limited scope.

This study has several important limitations which qualify the results. The analysis was confined to health care received by Medicare beneficiaries who were at least 65 years of age at the time of diagnosis and were diagnosed with specific medical conditions. Tort reforms may significantly impact the cost and outcome of care provided to younger patients or patients diagnosed with diseases other than the four examined in

this study. Data availability necessitated that the size of the disease-based populations (3300 on average), while adequate for statistical purposes, was far smaller than the hundreds of thousands of individuals covered by Kessler and McClellan (1996). Further research could examine reforms' impact on the cost and quality of care provided to younger patients using datasets covering a larger number of individuals and afflicted with other medical conditions.

VII. Conclusion

In practice, these findings suggest that policymakers concerned with the impact of tort reforms on health care delivery would do better to focus their attention elsewhere, perhaps considering issues of fairness or the efficiency with which the current legal system compensates patients affected by medical malpractice. Tort reform is a complex issue involving many stakeholders who are significantly impacted by legal changes. Indeed, it would be a mistake to read the results of this study as suggesting that tort reforms are unimportant or inconsequential.

From decreasing physicians' malpractice insurance premiums (Nelson et al., 2007) to influencing a states' supply of physicians (Kessler et al., 2005), reforms have proven effects that should be carefully considered before implementation. However, arguing for reforms on the basis of cost reductions or against reforms as increasing patients' mortality and readmission risk would be misguided, as would implementing such reforms in hopes of influencing any of these measures.

In most cases, reforms seem to have little impact on costs or outcomes, and in cases where reforms do matter, the direction of the effect is difficult to systematically predict. Many factors – from insurance reimbursement to managed care organizations to the increasing specialization of physicians – undoubtedly impact the quality and expenditures of the health care system. The presence or absence of tort reform measures in a given state, however, does not appear to be a significant factor for most types of reform nor for most types of patient subgroups.

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