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Will Tort Reform Bend the Cost Curve? Evidence from Texas

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Will tort reform "bend the cost curve?" Health-care providers and tort reform advocates insist the answer is "yes." They claim that defensive medicine is responsible for hundreds of billions of dollars in health-care spending every year. If providers and reform advocates are right, once damages are capped and lawsuits are otherwise restricted, defensive medicine, and thus overall health-care spending, will fall substantially. We study how Medicare spending changed after Texas adopted comprehensive tort reform in 2003, including a strict damages cap. We compare Medicare spending in Texas counties with high claim rates (high risk) to spending in Texas counties with low claim rates (low risk), since tort reform should have a greater impact on physician incentives in high-risk counties. Pre-reform, Medicare spending levels and trends were similar in high- and low-risk counties. Post-reform, we find no evidence that spending levels or trends in high-risk counties declined relative to low-risk counties and some evidence of increased physician spending in high-risk counties. We also compare spending trends in Texas to national trends, and find no evidence of reduced spending in Texas post-reform, and some evidence that physician spending rose in Texas relative to control states. In sum, we find no evidence that Texas's tort reforms bent the cost curve downward.

I. Introduction

Health-care professionals believe that fear of medical malpractice ("med mal") litigation motivates widespread overtreatment (defensive medicine).[1] For example, in a recent survey of 1,231 physicians, "[a]n overwhelming majority of respondents (91.0%) reported believing that physicians order more tests and procedures than needed to protect themselves

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[1]See, for example, Kachalia and Mello (2011).

from malpractice suits."[2] Physicians also believe that tort reform has great potential to lower health-care spending, by reducing defensive medicine. In the same survey, 90.7 percent of the responding physicians thought that "protections against unwarranted malpractice suits are needed to decrease the unnecessary use of diagnostic tests."[3]

The true extent of defensive medicine is unclear, but estimates in the $100–$400 billion range (4–15 percent of total health-care spending) are common and higher estimates are sometimes made.[4] Even in Washington, this is real money—so calls for tort reform are frequently heard. The House of Representatives passed a cap on non-economic damages ("non-econ cap") in 2002, 2003, and 2005, only to see them die each time in the Senate. Minority Leader John Boehner included a variety of tort reforms, including a strict cap on non-economic damages, in his 2009 alternative to the Democrats' health reform bill. In the Senate, Senator John Cornyn of Texas introduced the Medical Liability Reform Act of 2009, which contained similar provisions.[5] Medical societies lined up in support of these proposals and, for a time, seemed to have convinced the president. In a fall 2009 speech, President Obama stated, "I don't believe malpractice reform is a silver bullet, but I have talked to enough doctors to know that defensive medicine may be contributing to unnecessary costs."[6] In the end, the 2010 health-care reform law contained no substantive tort reforms.[7] The House of Representatives again passed a non-econ cap in 2012.[8]

Did Congress miss an opportunity to save hundreds of billions of dollars? Can states significantly reduce health-care costs by adopting state-level reforms? Most prior studies of whether malpractice reform affects health-care spending use state-level reform as an (assumed) exogenous shock to malpractice risk, and estimate the impact on spending using state-level data. In contrast, we rely principally on variation in malpractice risk *within Texas*. Our approach provides a different "instrument" for assessing whether and how malpractice reform impacts spending. It also provides a larger sample size and less potential for unobserved confounding variables, and thus potentially a stronger basis for causal inference, albeit only for a single (but large and diverse) state.

We study Texas because of a fortunate confluence of events. First, Texas adopted tort reform in 2003, which had a dramatic impact on malpractice risk. The Texas reform produced an estimated 60 percent drop in claim rates and a one-third drop in payouts per claim, for a combined drop of over 70 percent in total payouts. Second, Texas is one of only

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[2]Bishop et al. (2010); see also Studdert et al. (2005).

[3]Bishop et al. (2010). Id.

[4]Jackson Healthcare (2010) ($650-850 billion/year); Pricewaterhouse Coopers' Health Research Institute (undated, approx. 2007) ($210 billion/year).

[5]See <http://www.govtrack.us/congress/billtext.xpd?bill=s111-1734>.

[6]Obama (2009).

[7]Patient Protection and Affordable Care Act § 10607 (2010).

[8]See H.R. 5, Protecting Access to Healthcare Act <http://www.govtrack.us/congress/bills/112/hr5>.

two states (Florida is the other one) in which detailed intrastate data on malpractice claims are publicly available.

Our core causal inference assumption is that physicians are sensitive to the *local* risk of a malpractice claim. If defensive medicine is an important spur to spending, then spending within Texas should decline more in counties that had high pre-reform claim rates (high-risk counties), and therefore could expect a larger post-reform drop in claim rates, relative to counties with low pre-reform claim rates (low-risk counties).

Medicare uses an administered pricing system, with prices largely set on a national basis. These prices are not, so far as we know, adjusted in response to local tort reform. Thus, we effectively study whether tort reform changed the *quantity* of medical services provided. Over the long run in a well-functioning market, providers' savings from lower malpractice premiums should be reflected in lower health-care *prices* and thus health insurance rates. We cannot assess with our data whether any such change took place in private insurance markets in Texas. However, for our research question, which is how tort reform affects physician decisions, the *quantity* of medical services, rather than prices, is what we want to measure. Happily, Medicare spending provides such a measure.

We find no evidence that prior to reform, Medicare spending or spending trends were higher in high-risk counties, nor evidence that tort reform reduced Medicare spending or trends in high-risk counties, in each case relative to low-risk counties. Moreover, we can place fairly tight confidence bounds around this null result. In our principal county fixed effects specification (Table 2, regression 2), a one standard deviation increase in malpractice risk predicts a nearly zero (–0.04%) change in Medicare spending per beneficiary ($t = 0.08$), with a 95 percent confidence interval (CI) of [–1.0%, +1.0%]. We also find no significant effect of tort reform on spending for imaging and laboratory services, which is widely considered to be the area of medical practice that is most sensitive to liability risk (predicted change in Table 4, regression 4, of –0.067%; $t = 0.85$; 95% CI = [–2.2%, + 0.9%]). These null results are robust to an array of checks with alternative specifications.

Turning from spending levels to spending *trends*, we find no evidence of differing pre-reform trends between high- and low-risk counties, and no evidence that reform reduced spending growth rates in high-risk counties. On the contrary, we find some evidence that Medicare Part B spending (physician spending) trends *rose* in high-risk counties relative to low-risk counties.

In a secondary analysis, we examine whether spending in Texas as a whole changes relative to states that did not undergo tort reform at around the same time. We find no evidence that the 2003 reforms reduced either Medicare spending levels or spending trends in Texas. On the contrary, we find some evidence that physician spending *rose* in Texas after reform, relative to other states.

In sum, no matter how we slice the data, we find no evidence that the Texas 2003 tort reforms "bent the cost curve" downward, and some evidence of higher post-reform spending by Texas physicians who practice in high-risk counties. Our null result is broadly consistent with other studies of changes in health-care spending after tort reform, most of which rely on state-level data. Some recent state-level studies find no significant effect of tort reform on health-care spending; others find a modest 1–3 percent drop in health-care spending. One early study finds a 4–5 percent drop for heart attack patients. This accumulation of recent evidence finding zero or small effects suggests that it is time for policymakers to abandon the hope that tort reform can be a major element in health-care cost control.

Section II reviews the literature on the impact of tort reform on health-care spending and defensive medicine. Section III describes our data sets, hypotheses, and empirical methodology. Section IV studies the impact of Texas 2003 tort reform on medical spending *levels*. Section V examines the impact of the reforms on spending *trends*. Section VI discusses our findings. Section VII concludes.

II. Literature Review

A. *What is Defensive Medicine?*

Tort reform can affect health-care spending both directly and, potentially, indirectly. The direct costs of liability include "the cost of malpractice awards, all legal costs associated with the claims, and the administrative costs borne by medical malpractice insurers."[9] Several studies find that tort reforms lead to lower direct costs (approximated by liability insurance premiums).[10] However, because direct costs account for a small fraction (likely under 1 percent) of health-care spending, a drop in direct costs has limited potential to make health-care cheaper.[11] Even a 50 percent drop in direct costs will reduce spending by less than 1 percent—and the states that have already adopted tort reform have captured most of these savings.

Indirect costs are incurred when providers take steps that do not benefit patients (or lack sufficient benefit to justify their costs) to reduce the likelihood that the providers will be sued. These extra steps are commonly called "defensive medicine." Because tort reforms make lawsuits less likely and less expensive, they may reduce defensive medicine and thereby reduce health-care spending. Tort reform advocates and health-care providers often claim that these potential savings are large.

Whether tort reform would actually lead to large reductions in indirect costs is unclear. First, as described below, most prior studies find either small effects, or no statistically significant effect on spending from tort reform. These weak results could arise from several sources. First, if costs attributable to defensive medicine are small, then any savings from tort reform must be small as well. Second, providers may practice defensive medicine (assuming they do) for reasons tort reform does not address. For example, physicians may fear second-guessing of their professional judgment—whether through lawsuits or peer assessment—and practice defensively to avoid such assessments, without much regard to the probability of a negative assessment.[12] Concerns about reputation and

[9]Hellinger and Encinosa (2006).

[10]Zuckerman et al. (1990); Sloan et al. (1989); Danzon (1984, 1986).

[11]Mello et al. (2010) estimate direct costs in 2010 at $10 billion, or 0.4 percent of total health-care spending.

[12]Sclar and Housman (2003).

professional discipline may also motivate defensive practices. Other factors (local practice patterns; the desire to be thorough; economic incentives to run more tests; patient preferences for a definitive answer) can also contribute to unnecessary spending. If most physicians would be inclined to "do more" for multiple reasons, tort reform could have only a modest impact on spending.

Third, tort reform might cause health-care costs to rise. Some defensive medicine takes the form of omission (also called avoidance behavior). Legal risk may cause providers to refrain from performing risky procedures. Tort reform could encourage greater use of such procedures. Montanera develops a model in which physicians do more for some patients, but avoid others, leading to a nonlinear effect of malpractice pressure on health-care spending.[13] When asked in surveys, many physicians report limiting risky procedures due to fear of malpractice liability.[14] Similarly, if physicians have economic incentives to provide services, legal risk could provide a counterweight against this "physician-induced demand." Tort reform weakens this counterweight. These higher spending channels could offset any savings from performing fewer defensive procedures in response to med mal risk.[15] This "freeing" effect could explain our finding some evidence that physician spending rose after reform in high-risk counties.

Finally, some studies find evidence that malpractice liability leads to improved quality.[16] Tort reform could cause care quality to decline, which could, in turn, cause spending to rise.

Thus, the overall impact of tort reform on spending cannot be answered on theoretical grounds. Both the sign and magnitude of any effect must be determined through empirical study.

B. How Much Defensive Medicine is There, and How Much Does it Cost?

1. Studies Exploiting Plausibly Exogenous Variation in Risk

Kessler and McClellan performed the first rigorous studies of the impact of tort reforms on health-care spending.[17] Using longitudinal data on Medicare beneficiaries who received cardiac treatments in hospitals for acute myocardial infarction (heart attack) or ischemic heart disease in three years (1984, 1987, and 1990), they found that damages caps and other reforms that directly limited liability reduced posttreatment medical spending by 5–9

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[13]Montanera (2011).

[14]In a 2010 survey of Illinois physicians, 89 percent reported that malpractice fears caused them to order "more tests than medically needed," but 66 percent also reported that they "reduced or eliminated high-risk services or procedures," and 77 percent either did so or planned to. ISMIE Mutual Insurance Company and Illinois State Medical Society (2010).

[15]Currie and MacLeod (2008).

[16]See, e.g., Dhankhar et al. (2007); Lakdawalla and Seabury (2009); Zabinski and Black (2012).

[17]Kessler and McClellan (1996).

percent, without adverse health effects. Other tort reforms did not produce statistically significant spending reductions. In response to criticisms that their study had not controlled for managed care penetration, Kessler and McClellan reanalyzed their data with this control and found a 4–5 percent decline.[18]

In their original article, Kessler and McClellan observed that "*if our results are generalizable to other medical expenditures outside the hospital, to other illnesses, and to younger patients,* then direct [tort] reforms could lead to expenditure reductions of well over $50 billion per year without serious adverse consequences for health outcomes" (emphasis added). Tort reform advocates played up Kessler and McClellan's number and played down their qualifications. One stated flatly that nationwide tort reform would save "well over $50 billion a year."[19] In 2002, the Department of Health and Human Services issued a report that relied on Kessler and McClellan's first article and concluded that tort reform "would save $60–$108 billion in health care costs each year."[20] President Bush gave several speeches that relied on this figure.

More recent studies cast doubt on the generalizability of Kessler and McClellan's results. The Congressional Budget Office applied their methods to a broader range of medical conditions, and "found no evidence that restrictions on tort liability reduce medical spending."[21] A follow-up CBO study finds that states that implemented tort reforms in the 1980s had above-average health-care pricing before the 1983 implementation of the Medicare Prospective Payment System, which disproportionately affected states with higher pricing. When CBO controls for this (which Kessler and McClellan did not), its estimate of the effect of a cap on non-economic damages on Medicare spending is a statistically insignificant-1.6 percent.[22] A study by Sloan and Shadle that covers more conditions and more years than Kessler and McClellan also found insignificant results.[23] Lakdawalla and Seabury used county-level non-economic damage awards as an instrument for med mal risk. They found that lower risk predicts modestly lower health-care prices, no significant change in health-care quantity, and somewhat higher mortality.[24]

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[18]Kessler and McClellan (2002).

[19]Huber (1997).

[20]U.S. Department of Health and Human Services (2002).

[21]CBO (2004).

[22]CBO (2006). In 2009, the CBO estimated, based on a survey that included more recent studies, that a package of tort reforms, including caps on non-economic and punitive damages, would reduce health-care spending by 0.5 percent, comprised of a 0.2 percent decline due to lower direct med mal costs and a 0.3 percent decline from "slightly less utilization of health care services." CBO (2009).

[23]Sloan and Shadle (2009).

[24]Lakdawalla and Seabury (2009) (10 percent increase in malpractice awards predicts 1.2 percent higher spending, due to higher prices, no change in quantity, and 0.2 percent lower mortality). The Lakdawalla and Seabury identification strategy is similar to ours in relying on county-level rather than state-level measures, and thus assuming that physicians know about and are sensitive to local risk. Their strategy assumes either (implausibly, in our view) that

Studies of childbirth treatment and outcomes have also produced mixed results. Currie and MacLeod found that non-econ caps increased cesarean section rates (thus increasing costs), while joint and several liability reform decreased cesarean section rates (thereby reducing costs).[25] Frakes examined the impact of non-econ caps on cesarean rates, episiotomies, and bed days post-delivery.[26] Non-econ caps predict roughly 5 percent fewer episiotomies (depending on specification) and perhaps also a 3 percent or so decline in hospital stays, but did not affect cesarean rates. In contrast, Yang et al. find that non-econ caps predict lower primary and total cesarean rates, and higher VBAC (vaginal birth after cesarean) rates.[27] Finally, recent studies by Dranove and Watanabe and by Gimm assess whether Florida obstetricians' practice patterns changed after they were sued.[28] Dranove and Watanabe found lagged increases in cesarean section rates following hospitals' and physicians' involvement in claims, but the effects were small and temporary. Gimm finds insignificant effects. Overall, these studies suggest that tort reforms have limited potential to contain health-care costs by reducing defensive medicine among obstetricians.

Avraham, Dafny, and Schanzenbach measured spending in terms of premiums collected by employer-funded health insurance plans representing over 10 million Americans annually from 1998 and 2006.[29] They found that each of three reforms (non-econ caps, abrogation of the collateral source rule, and reform of joint and several liability) reduced premiums for self-funded health plans by 1–2 percent, but had no effect on premiums for fully insured plans.

2. Studies Without Exogenous Variation in Risk

The studies discussed below lack exogenous variation in risk, so provide a weaker basis for causal inference. We review them briefly for completeness.

Hellinger and Encinosa rely on state tort reform as a measure of med mal risk, but have only cross-sectional data. They estimate that a state cap on non-economic damages predicts 3–4 percent lower Medicare spending.[30] Morrissey, Kilgore, and Nelson have panel

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physicians are informed about changes in county-level non-economic damage awards or (more plausibly) that (1) higher awards lead to more suits, and (2) physicians are aware of the local risk of being sued. In contrast, we directly measure the impact of the Texas reforms on claim and payout rates; thus, we do not need to assume an indirect effect on claim rates through the channel of non-economic damage awards.

[25]Currie and McLeod (2008).

[26]Frakes (2010a, 2010b).

[27]Yang et al. (2009).

[28]Dranove and Watanabe (2010); Gimm (2010). An earlier study of Florida obstetricians by Grant and McInnes (2004) found a 1 percent increase in cesarean rates for obstetricians who experienced a large payout claim during 1992–1995, but no significant change for obstetricians who experienced smaller claims.

[29]Avraham et al. (2012).

[30]Hellinger and Encinosa (2006).

data, and find no effect of damage caps on private employer premia using a state fixed effects specification, but their time period is short (1999–2004) and effectively predates most of the caps adopted in the 2000s.[31]

Another plausible measure of med mal risk is med mal insurance premia. In two studies, Baicker and co-authors find no overall association between insurance premia and Medicare spending, but do find an association for the Medicare Part B spending subcategory for "diagnostic, laboratory, and x-ray services."[32] They hypothesize that diagnostic testing is more likely to reflect defensive medicine than medical practice more generally, and thus is more sensitive to liability risk. Thomas, Ziller, and Thayer compare medical spending for 35 clinical specialties in regions with high and low med mal insurance premiums. They conclude that even a large (30 percent) reduction in malpractice premiums would predict only a 0.4 percent decline in health-care spending.[33]

For childbirth, Dubay, Kaestner, and Waidmann use county fixed effects and find a statistically significant but small association between obstetrician malpractice premiums and cesarean section rates.[34] Localio and co-authors found an association between malpractice risk and cesarean rates in New York State.[35] However, Baldwin and co-authors found no relationship between cesarean rates and either personal malpractice experience or county-level malpractice risk within Washington State.[36]

Another potential risk measure is rates for physicians' paid claims, reported to the National Practitioner Data Bank. Dhankhar, Khan, and Bagga find that higher claim rates are associated with improved post-heart-attack outcomes, and predict lower treatment costs for less severely ill patients.[37]

C. Physician Perception and Malpractice Risk

The logic of tort reform is straightforward: if physicians respond to liability risk by practicing defensive medicine, tort reform that lowers liability risk will also reduce defensive medicine. It is the perception of liability risk (and not actual risk) that influences physicians' propensity to practice defensive medicine. Thus, tort reform may not reduce defensive medicine if it does not change physicians' perception of the liability climate—even if reform does, in fact, reduce liability risk. Several studies indicate that physicians dramatically overestimate their liability risk. For example, a study of physicians in New York in 1984

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[31]Morrissey et al. (2008).

[32]Baicker and Chandra (2005); Baicker et al. (2007).

[33]Thomas et al. (2010). The authors do not report the statistical significance of their results.

[34]Dubay et al. (1999).

[35]Localio et al. (1993).

[36]Baldwin et al. (1995).

[37]Dhankhar et al. (2007). This study includes reasonably detailed covariates for hospitals and patients but, oddly, none for states.

indicated that they overestimated their risk of being sued by a factor of three.[38] Physician risk perceptions do vary with the level of med mal risk, but not nearly as much as they likely should.[39]

III. Data, Hypotheses, and Methodology

A. Intrastate Causal Inference Strategy

Most prior studies that estimate the causal effect of tort reform on health-care spending or outcomes do so using state med mal reforms as an exogenous shock to med mal risk, with state-level spending as the dependent variable. One problem with this approach is that state reforms differ substantially. For example, damage caps vary in what type of damages they apply to (total vs. non-economic vs. punitive), the cap level, whether the cap varies with the type and number of defendants, and whether the cap level is indexed for inflation. Even if one studies only one type of damages cap, there are obvious difficulties with treating all caps as identical, as all previous studies do. Second, reforms are often bundled together, which complicates any attempt to estimate the impact of one particular type of reform, such as a non-econ cap. Third, there are only so many state-level tort reforms, so sample sizes are small.

The principal methodological innovation in this study is to rely on a large tort reform shock to a single state (Texas), posit (and verify) that this reform reduced local med mal risk more strongly in areas that had higher pre-reform risk levels, and look for *intrastate* differences in how that shock to local risk affected local spending. This approach is attractive because Texas experienced a uniquely large med mal shock, it has detailed county-level data on med mal risk, and that risk varies substantially across counties. We can study roughly 200 Texas counties, all subject to the same reform, instead of a much smaller number of states, each subject to a different package of reforms.

To estimate the impact of Texas tort reform on health-care spending, we need to make two core causal inference (identification) assumptions, which we cannot directly test. First, we assume that providers are sensitive to *local* med mal risk, and providers in high-risk counties will perceive a larger reduction in med mal risk than providers in low-risk counties. One channel for this differential impact is the absolute level of malpractice insurance premiums. Suppose that tort reform reduces the likelihood of a med mal claim by 50 percent from its pre-reform level. This might imply a $50,000 drop in annual premiums in a high-risk county, say from $100,000 to $50,000, but only a $15,000 drop in a low-risk county, from $30,000 to $15,000. A second channel is conversations between doctors about personal experiences in being sued. Here, too, a similar percentage drop in claim frequency should affect personal experience more in high-risk than low-risk counties.

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[38]Lawthers et al. (1992).

[39]Carrier et al. (2010).

Second, we assume that any other statewide factors that influence health-care spending affect high-risk and low-risk counties similarly. The low-risk counties thus serve as a control group for the high-risk counties, similar to a difference-in-differences (DiD) research design.[40]

We also assume that any impact of med mal reform on health-care spending will appear within a reasonable period of time. The longer the lag, of course, the less confidence one can have that there are not other, unobserved differences that emerge over time between high-risk and low-risk counties, which would affect the validity of our DiD-like research design. We study lags of up to four years.

We asked physicians, including health outcomes researchers, and the proponents of the Texas reforms whether these assumptions seemed reasonable. They generally concurred that physicians are likely to be aware of and respond to local risk. Their principal concern was a lag between reform and response, due to the "stickiness" of local practice patterns.[41]

B. Interstate Analysis

We also assess whether tort reform predicts any change in Texas spending levels, or spending trends, relative to other states. We use three principal control groups: (1) the 41 other states that did not undergo major tort reform during our principal 1999–2009 sample period; (2) the 19 other states that do not have caps on non-economic or total damages at all (no-cap states); and (3) a subset of nine no-cap states that are geographically and culturally relatively similar to Texas. The core causal inference assumption for this analysis is that tort reform is the only factor that causes Texas spending to change, relative to the control states. Because this assumption is untestable and could easily be wrong, we consider this to be a secondary analysis, akin to a robustness check on our primary intrastate analysis.

If Texas spending had fallen relative to control states, this would suggest a possible statewide impact of reform not captured by our analysis of intrastate variation in risk. Conversely, if Texas spending remains roughly constant or rises relative to control states, this is consistent with our main intrastate finding that a larger med mal risk shock does not predict lower post-reform spending.

C. Data Sources and Variables

1. Medicare Spending Data and Variables

Our Medicare spending data cover 1999–2009, and come from the Centers for Medicare and Medicaid Services.[42] This source provides annual spending at the county level; there are

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[40]Compare Atanasov et al. (2010), who assess the impact of Bulgarian legal reforms that limit self-dealing on firm market values by comparing firms with high self-dealing risk to low-risk firms.

[41]For example, one of us (Black) presented the research design to the Colorado Cardiovascular Outcomes Research Group <http://www.coloradooutcomes.org/> (May 2011).

[42]Currently, data from 2001 to 2009 are available at <http://www.cms.gov/MedicareAdvtgSpecRateStats/05_FFS_Data.asp>. We have data from this source from 1999 (downloaded before it rolled off). We thank Leemore Dafny and

254 counties in Texas. It reports total Medicare spending, Part A spending, and Part B spending, but not spending within subcategories of Part B. We therefore use another data set from the Dartmouth Atlas of Health Care, which provides annual Medicare spending through 2007, including subcategories of Part B, at the Hospital Service Area (HSA) level; there are 205 HSAs in Texas. An HSA is a local health-care market, usually centered around a single hospital.[43] We use the fraction of overall HSA-level Part B spending in each Part B subcategory to estimate spending in each Part B subcategory by county. We map HSAs onto counties using a zip-code-county-HSA cross-walk and population weights. Appendix A provides details on our estimation procedure.

Our principal dependent variable is LN(RELATIVE MEDICARE SPENDING), defined as ln(Texas spending per Medicare enrollee/mean state spending per enrollee in control states). The principal control group is 41 states that did not adopt a cap on non-economic or total damages (a damages cap) during our sample period.[44] In robustness checks we also use a "non-reformed" control group of 19 states that have never adopted a damages cap, and a "narrow" control group of nine non-reformed states that excludes northeastern and eastern states, leaving states that are likely to be culturally closer to Texas.[45]

2. Medical Malpractice Data

Our data on med mal claims come from the Texas Closed Claims Database (TCCD), a publicly accessible database maintained by the TDI.[46] From the larger database we extracted claims that closed from 1990–2009 with payouts of at least $10,000 involving medical

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Seth Seabury for sharing their data for earlier years with us. We use Medicare data back to 1988 (when our malpractice data begin) in unreported robustness checks, with results consistent with those we report. We report results using only data from 1999 forward due to large, implausible changes in Texas Medicare spending in some earlier years (e.g., a 70 percent jump in Part B spending per enrollee in 1996 vs. 1995, and then an 18 percent drop in 1998).

[43]Data from 1992 to 2007 are available at <http://www.dartmouthatlas.org/data/download.shtm>. After 2007, Dartmouth Atlas provides a new data series, which does not include the Medicare Part B subcategories we study here.

[44]The 41 "states" without recent reforms are 50 states plus District of Columbia, less Texas and eight other states that adopted damage caps over 2003–2006 (Florida, Georgia, Illinois, Mississippi, Nevada, Ohio, Oklahoma, and South Carolina), and Oregon, in which a cap was invalidated in 2000. No other states adopted or repealed caps on total or non-economic damages during our sample period. Alaska lowered its non-econ cap from $400k to $250k in 2006; Wisconsin raised its non-econ cap from $450k to $750k in 2006. Our definition of a "damages cap" excludes caps on punitive damages, on the grounds that these caps likely have only a small impact on med mal risk.

[45]The 19 non-reformed states are Alabama, Arizona, Arkansas, Connecticut, Delaware, District of Columbia, Iowa, Kentucky, Maine, Minnesota, New Hampshire, New York, North Carolina, Pennsylvania, Rhode Island, Tennessee, Vermont, Washington, and Wyoming. The nine similar non-reform states are Alabama, Arizona, Arkansas, Iowa, Kentucky, Minnesota, Tennessee, Washington, and Wyoming.

[46]This database contains individual reports of all personal injury claims closed from 1988 on, covered by five lines of commercial insurance—monoline general liability, auto, multiperil, medical professional liability, and other professional liability—involving payouts by all defendants of more than $10,000 in nominal dollars. Data are currently available through 2009. TDI checks the reports for internal consistency and reconciles them against aggregate annual reports filed by each insurer. For a fuller discussion of the TCCD, the med mal data set, and data set limitations, see Black et al. (2008). The Texas Department of Insurance (TDI) Closed Claim Reporting Guide (2004) (containing

malpractice by physicians or hospitals.[47] Our sample includes 18,981 nonduplicate cases involving total payouts over 1988–2008 of $4.9 billion. The TCCD only includes "insured" claims. Most physicians carry malpractice insurance, but we lack data on claims against physicians employed by the University of Texas hospital system, which is self-insured, and on self-insured hospitals. We lack data on cases with zero or small payout.

3. Med Mal Risk Variables

Our principal med mal risk measure is LONG-TERM MED-MAL RISK, defined for each county as the mean over 1999–2003 of ln(1 + (no. of claims/100,000 population)), normalized to mean = 0 and standard deviation = 1. We take a five-year average to capture the notion that physicians are likely to have a general sense for the level of risk they face, but are not likely to change their risk perceptions because of short-term fluctuations in claim rates. We add 1 before taking logs to avoid having to drop counties with zero claims. Before normalization, this measure has mean = 0.86 and standard deviation = 0.71, so there is reasonable variation between counties. We use a number of alternate measures in robustness checks (see Table 5).

4. Control Variables

We obtain Texas population by county and year from the U.S. Census Bureau.[48] When mapping counties onto HSAs or vice-versa, we use population weights based on exact Census 2000 zip-code-level data. We define a county as urban or rural based on the USDA definition.[49]

5. Texas Med Mal Reform

In 2003, Texas capped non-economic damages in med mal cases against physicians and other individual health-care providers at $250,000 nominal ($161,000 in the 1988 dollars we use in this article), with an additional $250,000 nominal possible if a hospital or other

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reporting instructions), the long and short forms, summary "Closed Claim Annual Reports," and the data on which we rely are available at <http://www.tdi.state.tx.us>.

[47]The claims in our med mal data set meet at least two of the following criteria: (1) payment under medical professional liability insurance; (2) physician or hospital defendant; (3) injuries caused by "complications or misadventures of medical or surgical care." Claims with payout of $10,000–$25,000 are reported on a "Short Form"; claims with payout of at least $25,000 are reported on a "Long Form." Only the Long Form contains the nature of the injury. We require that Short Form claims meet criteria (1) and (2). The reporting thresholds are not inflation adjusted. Thus, some claims that were reported in later years would not have been reported in earlier years if the thresholds had been inflation adjusted. The larger claims we study account for over 99 percent of total payout on all paid claims. We convert payouts to 1988 dollars using the Consumer Price Index for All Urban Consumers (CPI). Source: <www.bls.gov/cpi/>.

[48]The Census provides estimated county population by year, based on interpolation between decennial censuses. The zip-code-level data are available at <http://www.census.gov/epcd/www/zipstats.html#AFF>.

[49]U.S. Department of Agriculture, 2004 County Typology Codes, available at <http://www.ers.usda.gov/data/TypologyCodes/>.

health-care institution is also liable, up to a maximum of two institutions, for a maximum overall cap of $750,000 nominal. Other components of the 2003 reforms include making the death cap apply per claim, rather than per defendant, higher proof standards for emergency care, and a requirement that plaintiffs file an expert report within 120 days of suit with regard to each defendant's negligence (by a practicing physician, if the defendant is a physician).[50]

As Figures 1 and 3 reflect, these reforms dramatically reduced both claim rates and payouts per claim; this reduction in claim rate was larger in counties with higher pre-reform med mal risk. Thus, these reforms provide a large exogenous shock to the tort system, which we can use to assess whether this large decrease in malpractice risk affects health-care spending.

D. Hypotheses

Our hypotheses, based on the defensive medicine literature and on popular views about how physicians respond to med mal risk, are as follows.

Hypothesis 1: Med mal risk and local spending. If med mal risk significantly affects health-care spending, then (subject to endogeneity concerns) there should be a positive association between county-level med mal risk and health-care spending.

Hypothesis 2: Med mal reform and statewide spending trends. If med mal risk significantly affects health-care spending, then the 2003 tort reform will result in lower health-care spending in Texas, compared to other states.

Hypothesis 3: Med mal reform and local spending. Med mal reform will have a greater impact on spending in Texas counties with high pre-reform med mal risk than in low-risk counties because the reforms will cause a larger drop in risk in high-risk counties.

Hypothesis 4: Med mal reform and spending on imaging and laboratory services. Med mal reform will have a greater impact on services that are more discretionary (especially laboratory tests and imaging studies) than on services that are less discretionary (hospitalizations).[51]

To preview our conclusions, we find no support for any of these hypotheses. Instead, we consistently find null results, with reasonably tight confidence bounds. Where we do find statistically significant results, they have the opposite sign from that predicted. In particular, we find non-robust evidence that Texas physician spending *rises* after reform (contra to

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[50]Damages in death cases were limited to $975,000 total; this limit previously applied separately to each defendant. The standard of liability for emergency medical care in a hospital emergency room or obstetrical unit (or in surgery immediately after admission to the emergency room) was raised from ordinary negligence to "willful and wanton negligence." The 2003 reforms included several less significant changes, including the following. In general, defendants who are less than 50 percent at fault are liable for damages multiplied by their percentage of fault, with damages reduced by any prior settlements; med mal defendants may choose to have damages reduced by either the amount of prior settlements or the percentage of fault assigned to the settling defendants. Defendants can claim that part or most of the fault belongs to a nondefendant third party, and the jury must allocate fault to that person. And there is a 10-year "statute of repose," so if parents do not sue for a child's birth injuries by age 10, the child cannot do so when he or she becomes an adult.

[51]See Baicker et al. (2007), discussed in the literature review.

Hypothesis 3), with higher spending coming from high-risk counties (again contra to Hypothesis 3). We also find non-robust evidence that imaging and lab spending *rises* in high-risk counties after reform (contra to Hypothesis 4).

E. Empirical Methodology

We test these hypotheses using a variety of regression specifications. Our preferred specification for our intrastate hypotheses 3 and 4 uses county fixed effects to control for unobserved local factors that might predict both med mal risk and Medicare spending:

$$\ln(relative\ spending)_{it} = \alpha + C_i + T_t + \beta * PR_t + \gamma * Risk_i + \lambda * (PR_t * Risk_i) + \omega * \boldsymbol{controls}_{it} + \varepsilon_{it}. \quad (1)$$

Here, 1 indexes counties, *t* is time, C_i and T_t are county and (year-and-after) fixed effects, PR_t is a post-reform dummy (= 1 in 2004 or after; 0 otherwise), and $Risk_i$ is med mal risk. Other controls include ln(population) and urban dummy.[52] The coefficient of principal interest is on the interaction term. In robustness checks, we obtain similar results if we include county-specific time trends, and also with county random effects and pooled OLS models. We address the potential correlation among observations of the same county across years by using standard errors clustered on county (in HSA-level analyses, we cluster on HSA). In robustness checks, we obtain similar, indeed somewhat smaller, standard errors for our OLS results with two-way clustering on year and county. We cannot combine two-way clustering with fixed or random effects. Medicare spending is spending *per enrollee*.[53] We also map our med mal risk measures onto HSAs and study spending *at the HSA level* (with HSA fixed effects).

The independent variables of principal interest are: various measures of med mal risk, a med mal reform dummy (= 1 in Texas in 2004 and after; 0 for other states and for earlier years), and an interaction between med mal risk and reform dummy. Other controls include ln(population), and either a year trend variable or year dummy variables.

Some important limitations of our study: we study only one state, albeit a large and diverse one, which experienced a very large reform shock. We have detailed data only on claims with payout over $10,000 (nominal). For other claims, including those that close without payout, we have only statewide aggregate counts. There is puzzling year-to-year variation in Medicare spending data in the mid-1990s, which leads us to start our pre-reform period only in 1999. Our HSA-level data (and thus our data on subcategories of physician spending) extends only through 2007.

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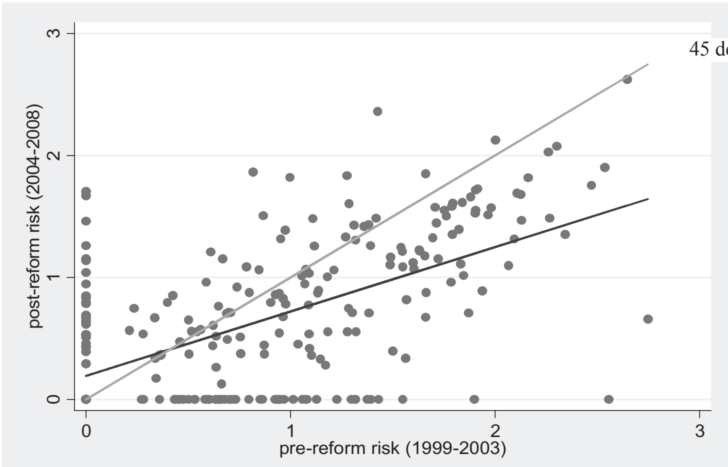
[52]Post-reform dummy, our risk measure variable, and urban dummy drop out with county and time fixed effects. We include them in Equation (1) because they are included in some of the alternate regression specifications we use below (e.g., random instead of fixed effects, time-varying risk).

[53]In robustness checks, we obtain similar results with *ln* (Medicare spending per enrollee) as the dependent variable instead of ln(relative spending), and U.S. spending either included as a control variable or omitted, and with nonlogged relative Medicare spending per enrollee as the dependent variable.

Figure 1: Pre-reform versus post-reform med mal risk.



NOTE: Scatter plots of 45 degree line and regression line from regression of non-normalized post-reform long-term med mal risk (2004–2008) on non-normalized pre-reform long-term med mal risk (1999–2003) and constant. Regression coefficient = 0.53 (t = 9.67 for null hypothesis that coefficient = 1).

F. Exploring the Causal Inference Assumptions

1. Does Pre-reform Risk Predict Post-reform Drop in Risk?

Our empirical strategy assumes that (1) high-med-mal-risk regions will experience a larger drop in med mal risk after tort reform than low-risk regions; and (2) physicians are aware of local med mal risk. The first assumption is testable. In Figure 1, we plot non-normalized *pre-reform* long-term med mal risk (over 1999–2003) on the x-axis, and *post-reform* long-term med mal risk (over 2004–2008) on the y-axis, together with a regression line showing the relationship between the two, and a 45-degree line. If med mal risk declines more in high-risk counties, we expect the regression line to have a slope less than 1; the actual slope is substantially less than 1 (slope = 0.53; t = 9.67).[54]

2. Geographic Variation in Med Mal Risk

One might also question our causal inference strategy if particular county (or HSA) factors (e.g., population, urban vs. rural, percent Hispanic, or a particular region within Texas) were dominant predictors of med mal risk. Our preferred fixed effects specification controls for time-invariant factors, but such a factor could still be associated with changes in

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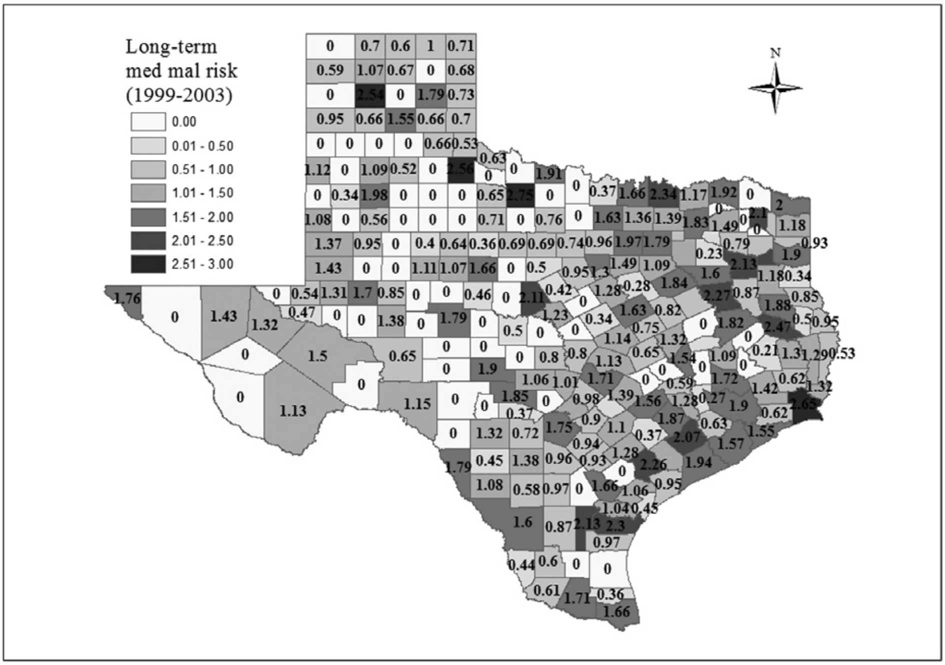
[54]A partial cause for the slope of less than 1 in Figure 1 is regression to the mean (or median). However, we would not expect doctors to be sophisticated enough to adjust their behavior taking into account the potential for an already long-term risk measure to regress to the mean. Also, the crossing point between the two lines is well below the median (mean) pre-reform risk of 0.79 (0.86), so regression to the mean (or median) is only a partial explanation for the observed results.

Figure 2: Geographic distribution of long-term med mal risk.



NOTE: Figure shows county-level long-term med mal risk, defined as (mean over 1999–2003 of $ln(1 + (\text{no. of claims})/100{,}000 \text{ population})_t$). Darker shading indicates higher risk.

Medicare spending.[55] Fortunately, this is not the case. Figure 2 shows the geographic distribution of long-term med mal risk by county. Darker shading shows higher risk. The high-risk counties are well distributed—all over the map, as it were. In regression analyses, larger population predicts higher risk, but there is substantial scatter that is not explained by population or other demographic controls.[56]

IV. Med Mal Reform and Spending Levels

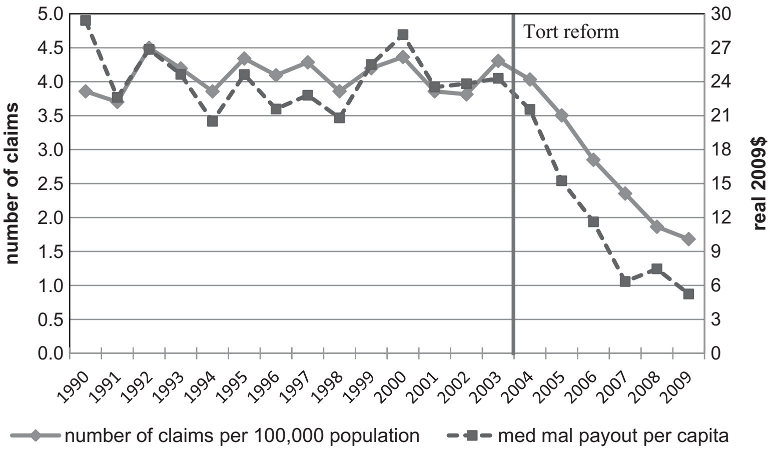
A. Aggregate Trends

By any measure, Texas 2003 tort reforms transformed the med mal liability environment. As Figure 3 reflects, from 1990 to 2003, claim frequency (the number of paid claims per

[55]To give a concrete example, suppose that high-med-mal-risk counties were concentrated in Southeast Texas, near Houston. One would worry that some unobserved change, affecting the Houston area around the time of med mal reform, caused spending to rise, and offset a decrease caused by med mal reform.

[56]For example, in the smallest fifth of counties by population, long-term med mal risk has mean = 0.42, $\sigma = 0.67$, while the largest fifth have mean risk = 1.60, $\sigma = 0.50$.

Figure 3: Medical malpractice claim rates and payouts in Texas.



NOTE: Number of claims per 100,000 population by year for all claimants (left scale), and payouts per capita (right scale), for 14,995 nonduplicate, non nursing home, med mal cases closed from 1990–2009 with payout >$25,000 in 1988 dollars. Texas tort reform in 2003 is depicted by vertical line. Amounts in 2008 dollars.

100,000 Texas residents) and claim severity (defined as payout per capita for all paid claims that closed in a given year) were generally stable. Post-reform, both frequency and severity trended sharply downward. Over 2003–2009, paid claims/100,000 population fell by 57 percent and payout per "large" paid claim (over $25,000 in 1988 dollars) dropped by 35 percent.[57]

Because the TCCD contains only paid claims and claims typically close several years after being brought, Figure 3 tells only part of the story. One would also like to assess the impact of Texas 2003 tort reforms on the rate at which new claims were filed and on the value of claims currently in the pipeline. Various sources, including the University of Texas Medical System, indicate that new filings declined sharply.[58] In 2009, claims intake at the Texas Medical Liability Trust (TMLT), the state's largest malpractice carrier, was "approximately half of the amount [TMLT] experienced in the years prior to tort reform," even though the number of insured physicians increased. Open claims also appear to be worth less than similar claims pending in prior years. In 2009, TMLT's "[t]otal trial losses . . . amounted to less than $1 million, one of the lowest trial loss years ever recorded."

[57]Paik et al. (2012). On the post-reform drop in claim frequency and payouts, see also Carter (2006) and Daniels and Martin (2009). On declining med mal premia, see also Guardado (2009) and Slavin (2010). The drop in all claims (including claims with zero or small payout) is similar to the drop in large paid claims shown in Figure 3.

[58]Stewart et al. (2011) report an 80 percent drop in lawsuits following surgery, from 40 suits/100,000 procedures before reform to 8 suits/100,000 procedures after reform.

Insurance premiums have dropped as well. TMLT reports that the 2003 reforms "dropped the cost of medical liability insurance by 50%" for its policyholders.[59] One should also note the scale of the right-hand y-axis in Figure 3. By 2007, total med mal payouts were only $7/capita—a tiny amount. Even if defense costs and insurer overhead add 50 percent to this number, direct med mal costs are only $10/capita. Manifestly, direct med mal spending is a small contributor to health-care costs.

Given the large post-reform drop in med mal risk, Hypothesis 2 predicts a drop in statewide health-care spending. However, we find no evidence of a drop. Figure 4 presents the Texas "spending gap" (Texas Medicare spending per enrollee – Medicare spending per enrollee in control states, separately for Part A (loosely, hospital spending), Part B (loosely, physician spending), and total Medicare spending (A + B) from 1999–2009. In Panel A, we compare Texas with the 41 U.S. states that did not adopt or repeal damages caps during our sample period. In Panel B, we restrict the control group to the 19 non-reformed states. The Texas spending trend relative to the controls is similar in both panels, and is also similar (figure not shown) if we use the narrow control group of nine similar non-reformed states. The Texas Part A spending gap is small and declines somewhat in 2008 and 2009. The Texas Part B spending gap rises over 2002–2007, with no downward "bend" after reform. This is not the pattern predicted by Hypothesis 1.

This is, to be sure, a weak test. Other factors affect statewide spending trends—indeed, the Texas spending gap is not flat prior to reform. Still, these aggregate data suggest that tort reform likely did not have a large impact on health-care spending. We return in Section V to the question of whether Texas spending levels or trends rose or fell after reform.

B. Overall Association Between Medicare Spending and Med Mal Risk

We turn next to Hypothesis 1—medical spending will be higher in counties with higher med mal risk. We begin with graphical analysis. Both per-capita health-care spending and med mal risk are higher in urban areas. Both effects could be related to population, rather than a causal effect of med mal risk on spending or vice-versa. We therefore regress *ln* (relative spending) on *ln* (population), urban dummy, year dummies, and constant, and compute residuals from this regression for each county and year. We similarly regress ANNUAL MED MAL RISK, defined as normalized [*ln* (1 + number of claims/100,000 population)], on the same control variables, and compute residuals.

In Figure 5, Panel A, we plot the spending residuals against the risk residuals, and show a regression line from regressing residual spending on residual risk plus constant term. Panel B is similar, except we drop county-year observations with zero med mal claims. In both figures, the slope of the regression line is effectively zero and is not close to being statistically significant. We obtain similar results if we weight observations by ln (population)

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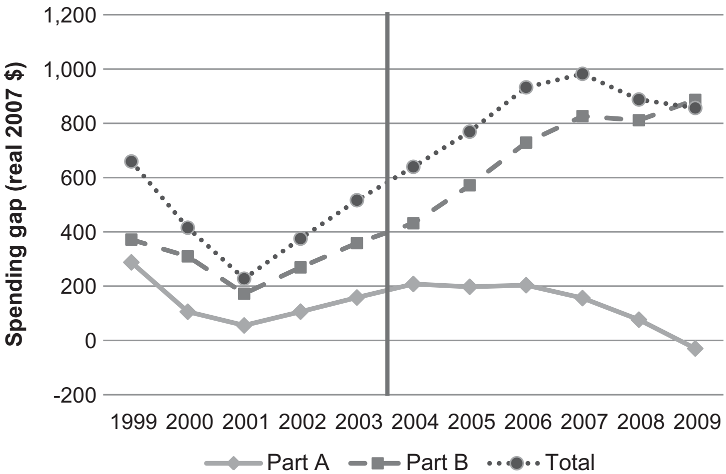
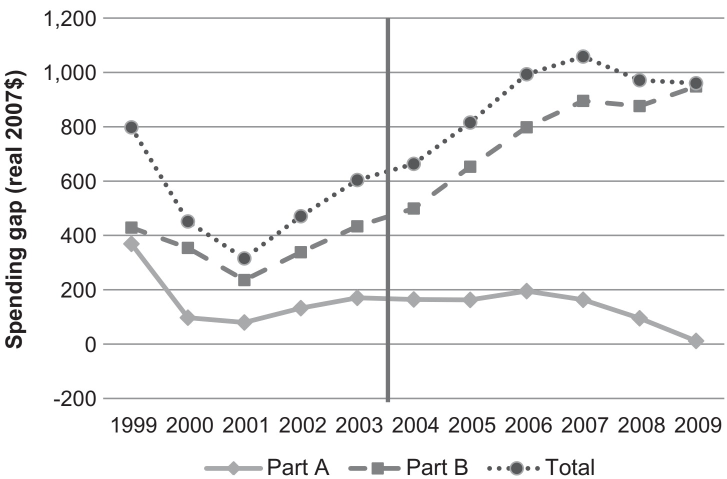
[59]Texas Medical Liability Trust, 2009 Annual Report, p. 4. This is in nominal dollars; the decline would be larger if adjusted for inflation.

Figure 4: Texas spending gap, 1999–2009.

Panel A. Texas vs. 41 states without recent reforms

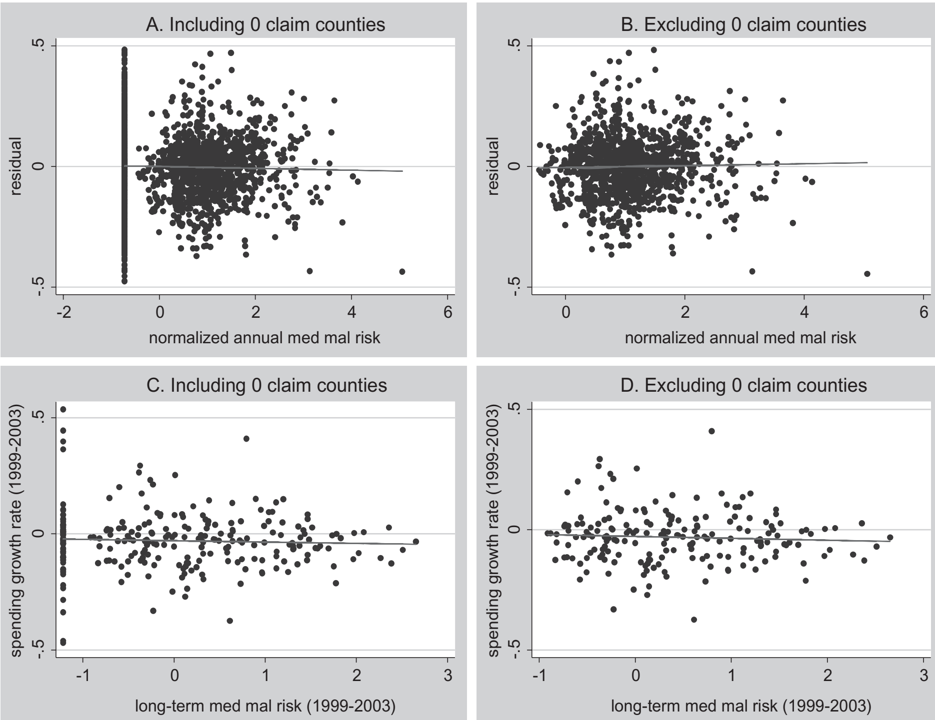


Panel B. Texas vs. 19 non-reformed states



NOTE: Graphs of Texas spending gap (Texas – control state spending per enrollee) for Medicare Part A, Part B, and total Medicare spending. Medicare Part A covers inpatient care in hospitals and hospice care services. Medicare Part B covers physician services and outpatient care, including home health, imaging, and clinical laboratory testing. Control groups are specified in the text. Texas tort reform in 2003 is depicted by vertical line. Amounts in 2007 dollars.

Figure 5: Association between med mal risk and Medicare spending.



Panels A and B: Annual Med Mal Risk and Spending Levels
Panel A. Scatter plot of residuals from a regression of ln(Medicare spending per enrollee/U.S. average spending per enrollee) ("relative spending") on population, urban and year dummies over 1999–2008, against *annual med mal risk*, defined as normalized [*ln*(1 + (number of claims/100,000 population)), and regression line for regression of first-stage residuals on normalized annual med mal risk and constant term, for 254 Texas counties over 1999–2008 ($n = 2{,}540$). Slope = –0.003 ($t = 0.78$). Amounts in 2007$. **Panel B.** Similar scatter plot and regression, after dropping county-year observations with 0 claims ($n = 1{,}040$ for 215 counties). Slope = +0.005 ($t = 0.50$).
Panels C and D: Long-Term Med Mal Risk and Spending Trends
Panel C. Scatter plot of relative Medicare spending growth rate over 1999–2003, calculated as ln(relative spending)$_{2003}$—*ln*(relative spending)$_{1999}$, and long-term med mal risk, defined as normalized [mean over 1999–2003 of *ln*(1 + (no. of claims)/100,000 population)], and regression line for regression of relative Medicare spending growth rate on long-term med mal risk and constant term, for 254 Texas counties. Slope is –0.006 ($t = 0.75$). Amounts in 2007$. **Panel D.** Similar scatter plot and regression line, after dropping 65 counties with 0 claims ($n = 189$). Slope = –0.008 ($t = 0.99$).

(not reported). There is thus no evidence that higher med mal claim rates are associated with higher spending *levels*.

In Panels C and D of Figure 5 we assess spending *trends*. Panels C and D plot relative (Texas minus U.S.) Medicare spending growth for each county over 1999–2003 against long-term med mal risk, and show a regression line from regressing spending growth on long-term med mal risk plus constant term. There is no significant association between med mal risk and pre-reform spending trends, whether we include zero-claim counties (Panel C) or exclude them (Panel D). The point slope estimates are negative–spending grows more slowly in high-risk counties.[60]

In Table 1, we turn to regression analysis of the association between Medicare spending and annual med mal risk. Regressions (1) and (2) provide OLS results, with the same control variables used to generate the residuals in Figure 5. Regression (1) includes a year variable.[61] In regression (2), we replace this trend variable with year dummies. Regression (1) indicates that Texas spending is increasing by about 0.5 percent per year over this period, relative to U.S. spending. In both regressions, we find a small, negative, and insignificant coefficient on annual med mal risk. In unreported regressions, we replace annual med mal risk with long-term med mal risk in OLS regressions (1) and (2). The coefficient on long-term med mal risk is negative and insignificant; in the analogue to regression (2), the coefficient is –0.0107 ($t = 1.34$).

In regressions (3) and (4) in Table 1, we switch to county fixed effects, which will absorb any time-invariant factors that predict county-level spending. The coefficients on annual med mal risk are smaller, but remain negative and insignificant. In regressions (5) and (6), we switch to HSA-level data and fixed effects. This introduces some measurement error due to the need to estimate HSA-level med mal risk from county data. The coefficient on annual med mal risk becomes positive, but remains small and statistically insignificant.

Taken together, Figure 5 and Table 1 provide no support for Hypothesis 1. There is no significant association between long-term or annual med mal risk and Medicare spending, with tight confidence bounds—for example, in regression (3), the 95 percent confidence interval for the change in spending from a one standard deviation change in med mal risk is [–0.6%, +0.3%]. The coefficients in the county-level regressions have the opposite sign from that predicted by Hypothesis 1.

To be sure, causation could run in both directions. Still, if higher med mal risk strongly caused higher spending, we would likely observe an association, because the potential for more medical spending to lead to more malpractice claims would only partially offset the main channel running from med mal risk to spending.

C. Estimating the Causal Effect of Tort Reform on Medical Spending

To address the potential endogeneity between health-care spending and med mal risk, we turn next to the heart of this study. We use the 2003 Texas tort reforms as an exogenous shock to med mal risk, which is larger in high-risk counties, and assess whether spending changes in high-risk counties relative to low-risk counties. Our principal risk measure is long-term med mal risk measured over the five years preceding the reforms (1999–2003); our principal time period is 1999–2009; our principal specification uses county fixed effects. We use other risk measures, time periods, and specifications in robustness checks.

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[60]In unreported analyses, we confirm that there is no significant association between residual spending *trends* and residual med mal risk (based on residuals from regressing trends and med mal risk on ln(population), urban dummy, and constant term).

[61]We define this variable to equal (actual year – 1999), so that this variable takes a value of 0 in the first year of our study (1999).

Table 1: Medicare Spending v. Med Mal Claim Rates

Dependent variable	*Ln(relative spending)*					
	[1]	*[2]*	*[3]*	*[4]*	*[5]*	*[6]*
Regression type	*OLS (County)*		*Fixed effects (County)*		*Fixed effects (HSA)*	
annual med mal risk	–0.0051	–0.0047	–0.0017	–0.0012	0.0007	0.0019
	[1.29]	[1.21]	[0.73]	[0.54]	[0.10]	[0.26]
95% confidence interval	*[–0.013, 0.003]*	*[–0.012, 0.003]*	*[–0.006, 0.003]*	*[–0.006, 0.003]*	*[–0.013, 0.015]*	*[–0.012, 0.016]*
Year	**0.0042**		**0.0043**		**0.0104**	
	[3.68]*		**[3.65]***		**[4.16]***	
Year dummies	No	Yes	No	Yes	No	Yes
Controls, constant	Yes	Yes	n.a.	n.a.	n.a.	n.a.
Observations	2,794	2,794	2,794	2,794	1,720	1,720
No. of counties or HSAs	254	254	254	254	194	194
R^2	0.062	0.074	0.626	0.637	0.474	0.479

NOTE: Ordinary least squares and county (HSA) fixed effects regressions of *ln* (Medicare spending per enrollee/average U.S. spending per enrollee) over 1999–2008 (1999–2007 for HSA) on annual med mal or, defined as normalized [(*ln* (1 + (no. of claims)/100,000 population)], year, and other control variables. County-level controls in OLS regressions are *ln*(population in 2000) and urban dummy. *t* statistics, with standard errors clustered on county (HSA) are in parentheses. Amounts in 2007$. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels (omitted for constant term). Significant results (at 5 percent or better) are in **boldface**.

In Table 2, we examine total Medicare spending. The dependent variable is ln(relative Medicare spending). The principal independent variables are a post-reform dummy, long-term med mal risk, and the variable of principal interest: the interaction between post-reform dummy and long-term med mal risk. All regressions include ln(population) and a constant term. The regressions in Table 2 assume that observations of different counties within Texas are independent. This is plausible for the interaction term, although we need to assume that any statewide factors affect high-risk and low-risk counties similarly, so that low-risk counties are a proper control group for the high-risk counties. It is less plausible for the coefficient on post-reform dummy.

Regressions (1) and (2) in Table 2 both use county fixed effects, but specify year differently. Regression (1) includes a year variable, while regression (2) uses year dummies. In both, the coefficient on the interaction variable is small and insignificant, with reasonably tight confidence bounds. The 95 percent CI for one standard deviation change in med mal risk on spending is [−1.0%, +1.0%].

We obtain similar results using county random effects in regressions (4) and (5) in Table 2, where we also include an urban dummy; and using HSA-level data and HSA fixed effects in regressions (7) and (8), although standard errors are substantially larger in the HSA analysis. The coefficient for the interaction term is slightly negative in the county-level regressions and slightly positive in the HSA regressions, but is not close to being statistically significant in any of the regressions. If Hypothesis 3 were correct, we would expect the interaction term to be negative and significant.

We address the possibility that high-risk counties had higher pre-reform spending trends than low-risk counties, as an explanation for our null result, in two ways. First, as Figure 5 shows, med mal risk does not predict spending trends. Second, in regressions (3) and (6) of Table 2, we add county-specific trends (county dummy * year) to regressions (2) and (4). Very little changes—the coefficients on the interaction terms are now positive but are still not close to significant.

In the regressions with a year trend variable (regressions (1), (4), and (7) in Table 2), we find evidence that Medicare spending grew faster in Texas than in control states during the post-reform period. This evidence is only suggestive because the regression design assumes independent observations across counties, so the standard errors will be too low if correlation is present, but is consistent with the visual evidence in Figure 3. We provide additional evidence on statewide trends below, using a stronger specification. The regressions using individual "year-or-after" dummies show that the rise in relative spending is concentrated in the two years after reform, with no trend thereafter.

In Table 3, we report a similar analysis for Medicare Part A (hospital) and Part B (physicians) spending. We expected Part B spending would be more strongly affected by tort reform, since physicians often have more discretion in ordering medical tests and exams than in the decision whether to hospitalize a patient. We find no evidence of a spending drop in high-risk counties, relative to low-risk counties, for either Part A or Part B spending. The coefficients on the interaction term are small and not close to being statistically significant, with reasonably tight confidence intervals. The 95 percent CI for our preferred fixed-effects specification is [−0.015, +0.015] for Part A spending (regression (1)), and [−0.012, +0.010] for Part B spending (regressions (1) and (4)). The coefficients on the interaction term

Table 2: Effects of Med Mal Risk and Tort Reform on Medicare Spending

Dependent Variable	*Ln(relative spending)*							
	County						*HSA*	
Geographic Area	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Regression Type	*Fixed Effects (County)*			*Random Effects (County)*			*Fixed Effects (HSA)*	
Post-reform dummy	**0.0377**			**0.0388**			0.0394	
	[6.60]***			**[7.22]*****			[1.80]*	
Long-term med mal risk				–0.0012	–0.0013	–0.0155		
				[0.13]	[0.14]	[1.41]		
Post-reform dummy * long-term med mal risk	–0.0002	–0.0004	0.0019	–0.0053	–0.0053	0.0018	0.0030	0.0029
	[0.04]	[0.08]	[0.32]	[0.90]	[0.90]	[0.32]	[0.20]	[0.19]
95% confidence interval	*[–0.010, 0.010]*	*[–0.010, 0.010]*	*[–0.010, 0.014]*	*[–0.017, 0.006]*	*[–0.017, 0.006]*	*[–0.009, 0.013]*	*[–0.027, 0.033]*	*[–0.027, 0.033]*
ln(county population)	–0.1617	–0.1546	0.2296	**0.0211**	**0.0213**	**0.0391**		
	[1.93]*	[1.81]*	[0.88]	**[3.10]*****	**[3.11]*****	**[2.69]*****		
Urban				–0.0100	–0.0103	–0.0108		
				[0.51]	[0.52]	[0.39]		
Year	–0.0002			–0.0010			0.0037	
	[0.10]			[0.72]			[0.87]	
2004 or after		**0.0129**	–0.0046		**0.0126**	0.0108		0.0165
		[2.28]**	[0.60]		**[2.35]****	[1.44]		[0.77]
2005 or after		**0.0261**	0.0080		**0.0255**	**0.0237**		**0.0526**
		[3.41]***	[1.16]		**[3.58]*****	**[3.11]*****		**[2.95]*****
2006 or after		0.0082	–0.0101		0.0075	0.0056		–0.0197
		[1.06]	[1.08]		[1.01]	[0.64]		[1.15]
2007 or after		–0.0001	**–0.0184**		–0.0008	–0.0027		0.0232
		[0.02]	**[2.20]****		[0.11]	[0.33]		[1.45]
2008 or after		–0.0037	**–0.0221**		–0.0044	–0.0063		
		[0.44]	**[2.91]*****		[0.55]	[0.75]		
2009 or after		–0.0091	**–0.0284**		–0.0102	–0.0121		
		[1.30]	**[3.17]*****		[1.52]	[1.49]		
Other year-or-after dummies	No	Yes	Yes	No	Yes	Yes	No	Yes
County trends	No	No	Yes	No	No	Yes	No	No
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,794	2,794	2,794	2,794	2,794	2,794	1,720	1,720
No. of counties or HSAs	254	254	254	254	254	254	194	194
R^2	0.6328	0.6403	0.7627	0.0630	0.0707	0.5290	0.4753	0.4794

NOTE: County fixed and random effects and HSA fixed effects regressions of *ln*(relative Medicare spending) (defined in Table 1) on post-reform dummy (= 1 for 2004 and later), long-term med mal risk, defined as normalized [mean over 1999–2003 of *ln*(1 + (no. of claims)/100,000 population)], interaction between these variables, *ln*(population), and constant term. Random effects regressions include urban dummy. "2004 or after" = 1 for year = 2004 or later, 0 otherwise, and similarly for other "year-or-after" variables. Amounts in 2007$. *t* statistics, with standard errors clustered on state (HSA), in parentheses. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels. Significant results (at 5 percent or better) are in **boldface**.

Table 3: Effects of Med Mal Risk on Medicare Part A and Part B Spending

Dependent Variable	*Ln(relative spending)*							
Geographic Area	*County Level*						*HSA Level*	
	Part A			*Part B*			*Part A*	*Part B*
Medicare Spending Area	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Regression Type	*Fixed*	*Fixed*	*Random*	*Fixed*	*Fixed*	*Random*	*Fixed*	*Fixed*
Long-term med mal risk			–0.0005 [0.05]			–0.0047 [0.46]		
Post-reform dummy * long-term med mal risk	–0.0001 [0.02]	0.0057 [0.76]	–0.0080 [0.92]	–0.0012 [0.22]	–0.0063 [1.03]	–0.0038 [0.61]	–0.0024 [0.13]	0.0199 [1.24]
95% confidence interval	*[–0.015, 0.015]*	*[–0.009, 0.021]*	*[–0.025, 0.009]*	*[–0.012, 0.010]*	*[–0.018, 0.006]*	*[–0.016, 0.008]*	*[–0.040, 0.036]*	*[–0.012, 0.052]*
ln (county population)	**–0.2608** **[2.52]******	0.3181 [0.81]	**0.0204** **[2.61]***	–0.0643 [0.57]	0.1226 [1.03]	**0.0305** **[3.87]***		
Urban dummy			–0.0072 [0.31]			–0.0247 [1.25]		
Year-or-after dummies, constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County trends	No	Yes	No	No	Yes	No	No	No
Observations	2,794	2,794	2,794	2,794	2,794	2,794	1,040	1,720
No. of counties or HSAs	254	254	254	254	254	254	126	194
R^2	0.4951	0.6189	0.0353	0.7159	0.8549	0.1171	0.5299	0.5593

NOTE: County fixed and random effects and HSA fixed effects regressions for Medicare Parts A and B of *ln*(relative Medicare spending) over 1999–2009 (1999–2007 for HSA regressions) on post-reform dummy, normalized long-term med mal risk, interaction between these variables, and other control variables as shown. Variables are defined in Tables 1 and 2. Amounts in 2007$. *t* statistics, with standard errors clustered on county (HSA), in parentheses. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels. Significant results (at 5 percent or better) are in **boldface**.

remain insignificant if we add county trends (regressions (2) and (5)), use random instead of fixed effects (regressions (3) and (6)), or switch to HSA-level analysis (regressions (7) and (8)). Taken together, Tables 2 and 3 provide no support for Hypothesis 3 (spending will decline after reform in high-risk counties, relative to low-risk counties).

We turn in Table 4 to spending within Part B subcategories, using regression specifications similar to Table 3. We have Medicare spending for Part B subcategories only at the HSA level, so we use HSA data to estimate county-level spending. This limits our sample period to 1999–2007.

Hypothesis 4 predicts that imaging and lab spending will be especially sensitive to tort reform. The interaction term is positive for medical and surgical services (regressions (1)–(3) in Table 4) and negative for imaging and lab (regressions (4)–(6)), but is not close to being significant for either category. In the HSA-level regressions (7) and (8), the coefficient for imaging and lab is positive and statistically significant—opposite to Hypothesis 4. There is thus no support for Hypothesis 4.

D. Robustness: Alternate Measures of Med Mal Risk

So far, we have used the long-term frequency of paid claims over 1999–2003 as our measure of pre-reform liability risk, and thus the size of the reform shock. However, other measures might better reflect physician risk perceptions. In Table 5, we therefore assess the robustness of our results using a variety of alternate risk measures. As in Tables 2–4, the interaction term is the independent variable of principal interest. We report coefficients only for this term.

Each panel of Table 5 reports results for the interaction between the post-reform dummy and a different measure of med-mal risk, for fixed effects regressions with ln(relative Medicare spending) as the dependent variable, for total Medicare spending, Part A spending, Part B spending, and Part B subcategories, all at the county level; plus HSA-level results for imaging and lab spending, for which we found a significant positive coefficient in Table 4. Panel A collects results from Tables 2–4, for comparison with the other panels. The risk measures in the remaining panels are as follows. All measures are normalized to mean = 0 and standard deviation = 1. Appendix B provides correlation coefficients between the measures.

Panel B: Long-term med mal risk with counties weighted by population, to give more weights to larger counties.[62]

Panel C: Liability measure based on *payouts* rather than number of claims, defined as the normalized mean over 1999–2003 of ln(1 + payout per capita).

Panel D: Long-term risk measure based on the number of claims *opened* in a given year, rather than the number *closed*, defined as the mean over 1999–2003 of $\ln(1 + (\text{number of claims}/100{,}000\ \text{population})_t)$.[63]

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[62]In robustness checks for this and other tables, we obtain similar results if we use Medicare enrollments instead of population as weights.

[63]Our data set is based on closed paid claims, so this measure omits open claims that have not closed by 2009. We have no reason to expect bias across counties in the number of opened claims that are not closed by 2009.

Table 4: Effects of Med Mal Risk on Medicare Spending: Part B Subcategories

Dependent Variable	*Ln(relative spending)*							
Geographic Area	*County-Level*						*HSA-Level*	
Medicare Spending Area	*Medical and Surgical*			*Imaging and Lab*			*Medical and Surgical*	*Imaging and Lab*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Regression Type	*Fixed*	*Fixed*	*Random*	*Fixed*	*Fixed*	*Random*	*Fixed*	*Fixed*
Long-term med mal risk			**–0.0283** **[2.39]****			–0.0111 [0.79]		
Post-reform dummy * long-term med mal risk	0.0026 [0.32]	0.0057 [0.50]	0.0026 [0.34]	–0.0067 [0.85]	–0.0055 [0.48]	–0.0066 [0.86]	0.0277 [1.65]	**0.0386** **[2.73]*****
95% confidence interval	*[–0.013, 0.018]*	*[–0.017, 0.028]*	*[–0.013, 0.018]*	*[–0.022, 0.009]*	*[–0.028, 0.017]*	*[–0.022, 0.008]*	*[–0.005, 0.061]*	***[0.011, 0.066]***
ln (county population)	0.0562 [0.68]	–0.0012 [0.01]	**0.0526** **[5.28]*****	0.0495 [0.47]	–0.0329 [0.15]	**0.0463** **[4.63]*****		
Urban dummy			0.0187 [0.87]			0.0519 [1.90]*		
Year-or-after dummies, constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County trends	No	Yes	No	No	Yes	No	No	No
Observations	2,231	2,231	2,231	2,231	2,231	2,231	1,705	1,693
No. of counties or HSAs	249	249	249	249	249	249	194	194
R^2	0.6932	0.8006	0.1707	0.7388	0.8327	0.1432	0.5879	0.7766

NOTE: County and HSA fixed effects regressions with variables as shown, over 1999–2007. Variables are defined in Tables 1 and 2. Amounts in 2007$. *t* statistics, with standard errors clustered on county (HSA), in parentheses. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels. Significant results (at 5 percent or better) are in **boldface**.

Table 5: Robustness: Alternate Measures of Malpractice Risk

Dependent Variable	*Ln(relative spending)*					
	County					*HSA*
Geographic Area	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Medicare Spending Area	*Total*	*Part A*	*Part B*	*Medical and Surgical*	*Imaging and Lab*	*Imaging and Lab*
Panel A: Long-Term Med Mal Risk (from Tables 2–4)						
Post-reform dummy * Long-term med mal risk	–0.0004	–0.0001	–0.0012	0.0026	–0.0067	**0.0386**
	[0.08]	[0.02]	[0.22]	[0.32]	[0.85]	**[2.73]*** **
Panel B: Long-Term Med Mal Risk, with Population Weights						
Post-reform dummy * Long-term med mal risk	–0.0021	–0.0062	0.0021	–0.0057	0.0130	0.0159
	[0.23]	[0.70]	[0.21]	[0.38]	[1.58]	[1.07]
Panel C: Long-Term Payout Risk						
Post-reform dummy * Payout risk	–0.0002	–0.0013	0.0007	0.0008	–0.0034	**0.0310**
	[0.05]	[0.19]	[0.12]	[0.11]	[0.49]	**[2.42]** **
Panel D: Open Claim-Based Long-Term Med Mal Risk						
Post-reform dummy * Open claim risk	–0.0072	–0.0089	–0.0051	–0.0066	–0.0099	0.0201
	[1.34]	[1.22]	[0.86]	[0.85]	[1.21]	[1.53]
Panel E: Time-Varying Med Mal Risk						
E.1. 5-year, normalized rish						
Post-reform dummy * Med mal risk	–0.0035	–0.0067	–0.0010	–0.0038	–0.0109	**0.0297**
	[0.58]	[0.82]	[0.17]	[0.50]	[1.34]	**[2.32]** **
E.2. annual, normalized risk						
Post-reform dummy * Med mal risk	–0.0013	–0.0020	–0.0010	–0.0080	–0.0053	0.0100
	[0.32]	[0.35]	[0.25]	[1.19]	[0.77]	[0.93]
Panel F: Long-Term Elderly Med Mal Risk						
Post-reform dummy * Med mal risk	–0.0013	–0.0036	0.0006	0.0033	0.0131	**0.0401**
	[0.26]	[0.60]	[0.11]	[0.50]	[1.68]*	**[2.81]*** **
Panel G: Ex-Post Med Mal Risk Drop						
Post-reform dummy * Ex-post med mal risk drop	0.0009	0.0030	–0.0017	0.0048	0.0004	**0.0437**
	[0.18]	[0.44]	[0.29]	[0.57]	[0.05]	**[3.05]*** **
All Panels						
Fixed effects	county	county	county	county	county	HSA
Control variables	yes	yes	yes	yes	yes	yes
Observations	2,794	2,794	2,794	2,231	2,231	1,693
No of Counties or HSAs	254	254	254	249	249	194

NOTE: **All Panels:** County or HSA fixed effects regressions of ln(relative Medicare spending) over 1999–2009 (1999–2007 for Part B subcategories) on post-reform dummy, med mal risk measure, interaction between these variables, and control variables. Table shows only coefficients on interaction term. Control variables are year-and-after dummies, ln(population), and constant term. Variables are defined either below or in Tables 1 and 2. Amounts in 2007$. *t* statistics, with standard errors clustered on county or HSA, in parentheses. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels. Significant results (at 5 percent or better) are in **boldface**. Risk measures are: **Panel A:** long-term med mal risk (this panel reproduces selected results from Tables 2–4). **Panel B:** same, but adds county population weights. **Panel C:** normalized [mean over 1999–2003 of ln(1 + payout per capita)]. **Panel D:** normalized [mean over 1999–2003 of ln(1 + (no. of opened claims which lead to large paid claims)/100,000 pop.)$_t$]. **Panel E1:** time-varying long-term risk, defined as normalized [mean over most recent five years of ln(1 + (no. of claims)/100,000 pop.)]. **Panel E2:** annual med mal risk, defined as in Figure 5. **Panel F:** Long-term med mal risk, measured based on claims by elderly (age 65+). **Panel G:** normalized [(long-term med mal risk for 1999–2003 (not normalized)) – (long-term risk for 2004–2008, based on post-reform claims, not normalized)].

Panel E1: Time-varying med mal risk (five-year average), based on number of claims, defined as (ln(1 + (claims per 100,000 population)$_t$).

Panel E2: Annual med mal risk, defined as ln(1 + (claims per 100,000 population).

Panel F: Long-term med mal risk, based only on claims by the elderly, to addresses the possibility that Medicare spending responds to elderly-specific risk, rather than total med mal risk.

Panel G: Actual post-reform drop in med mal risk, defined as non-normalized [long-term med mal risk for 1999–2003] − non-normalized [(long-term med mal risk for 2004–2008, based only on claims affected by the 2003 reforms], with this difference then normalized).[64]

In the county-level regressions, we obtain consistent null results across all risk measures.[65] The coefficient on the interaction between med mal risk and the post-reform dummy is insignificant, with varying sign, for total spending, Part A spending, Part B spending, medical and surgical spending, and imaging and lab spending. For HSA-level imaging and lab spending, the coefficient is consistently positive and sometimes significant, but not reliably so. Thus, these alternate analyses confirm the results above—there is no support for Hypotheses 3 and 4, and some non-robust evidence from the HSA-level regressions of higher post-reform imaging and lab spending.

E. Lagged Results

Tort reform might have a delayed, rather than immediate, impact on medical spending. If so, our regressions above might understate that impact by combining early and later post-reform years. We did not find evidence of a lagged decline in statewide spending (see Figure 4 and the regressions in Table 2 using year-and-after dummies). Here, we examine whether there was a lagged effect in the relative response of high- and low-risk counties. We report results for long-term med mal risk, but find consistent results in robustness checks with other risk measures.

The format of Table 6 is similar to Table 5. Each panel shows the coefficients on the interaction between post-reform dummy and long-term med-mal risk, from fixed effects regressions with *ln* (relative Medicare spending) as the dependent variable. Panel A is copied from Table 5, Panel A, and shows nonlagged results. In Panels B–E, we show lags from one to four years. Almost nothing changes when we add lags. For longer lags, the significance of the positive coefficient on imaging and lab spending in the HSA-level regressions weakens. This is expected because, for these regressions, our data ends with 2007. There is thus no support for a post-reform relative drop in spending in high-risk counties, with lags of from one to four years.

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[64]Many post-reform claims will not have closed by 2009, so prior to normalization, our post-reform measure will underestimate post-reform risk. We have no reason to expect this to introduce bias across counties.

[65]We also obtain null results in additional unreported robustness checks, including: three-year time-varying risk (as an alternative to annual or five-year risk) and weighting by population or ln(population) for all risk measures.

Table 6: Robustness Check: Lagged Effects of Tort Reform

Dependent Variable	*Ln(relative spending)*					
Geographic Area	*County*					*HSA*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Medicare Spending Area	*Total*	*Part A*	*Part B*	*Medical and Surgical*	*Imaging and Lab*	*Imaging and Lab (HSA)*
Panel A: Nonlagged Results (from Tables 2–4)						
Post-reform dummy * Long-term med mal risk	–0.0004	–0.0001	–0.0012	0.0026	–0.0067	**0.0386**
	[0.08]	[0.02]	[0.22]	[0.32]	[0.85]	**[2.73]***
Observations	2,794	2,794	2,794	2,231	2,231	1,693
Panel B: One-Year Lag (Drop 2004)						
Post-reform dummy * Med mal risk	–0.0008	–0.0005	–0.0014	0.0014	–0.0092	**0.0355**
	[0.15]	[0.07]	[0.24]	[0.15]	[0.98]	**[2.30]**
Observations	2,540	2,540	2,540	1,982	1,982	1,501
Panel C: Two-Year Lag (Drop 2004–2005)						
Post-reform dummy * Med mal risk	–0.0004	0.0005	–0.0016	0.0002	–0.0152	0.0307
	[0.08]	[0.06]	[0.24]	[0.02]	[1.40]	[1.69]*
Observations	2,286	2,286	2,286	1,734	1,734	1,311
Panel D: Three-Year Lag (Drop 2004–2006)						
Post-reform dummy * Med mal risk	–0.0001	–0.0006	0.0000	–0.0008	–0.0158	0.0249
	[0.02]	[0.06]	[0.00]	[0.07]	[1.17]	[1.25]
Observations	2,032	2,032	2,032	1,487	1,487	1,123
Panel E: Four-Year Lag (Drop 2004–2007)						
Post-reform dummy * Med mal risk	–0.0007	–0.0027	0.0009	n.a.	n.a.	n.a.
	[0.09]	[0.25]	[0.11]			
Observations	1,778	1,778	1,778			
Control variables	yes	yes	yes	yes	yes	yes
No. of counties or HSAs	254	254	254	249	249	194

NOTE: Table shows coefficients on interaction between post-reform dummy and long-term med mal risk from county or HSA fixed effects regressions of *ln* (relative Medicare spending) over 1999–2009 (1999–2007 for Part B subcategories) on post-reform dummy, long-term med mal risk, interaction between these variables, year-and-after dummies, ln(population), and constant term. Variables are defined in Tables 1 and 2. Amounts in 2007$. Panel A shows nonlagged results (same as Table 5, Panel A). Panels B–E show results with one- to four-year lags (dropping early post-reform years as indicated). *t* statistics, with standard errors clustered on county or HSA, in parentheses. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels (omitted for constant term). Significant results (at 5 percent or better) are in **boldface**.

F. Difference-in-Differences Analysis

1. High- Versus Low-Risk Counties

Thus far, we have treated med mal risk as a continuous variable, and assessed whether reform affects spending in high-risk counties more than in low-risk counties. We use here an alternate approach, difference-in-difference (DiD) analysis, in which we consider high-risk counties to be "treated" by the reforms, and low-risk counties to be controls. We place the top 25 percent of counties, by risk level, in the treatment group. The control group is the 65 "bottom-risk" counties (25.6 percent of all counties) with zero paid claims over the five years preceding reform (1999–2003); we drop middle-risk counties. In robustness checks, we obtain similar results using the top 33 percent (or top half) of counties as the treatment group and the bottom 33 percent (half) as the control group, and similar results if we allow for a lagged impact of tort reform.

We report results for different spending categories in Table 7. The regression specification is similar to that in Table 2, regression (2), except we replace long-term med mal risk with a high-risk dummy. The coefficient of principal interest is on the interaction between post-reform dummy and high-risk dummy. This coefficient is never close to significant, and is positive for all categories except imaging and lab spending. Standard errors are substantially higher than in prior tables because we drop the middle 50 percent of counties and coarsen the risk measure, but the overall picture is unchanged: there is no evidence of a spending drop in high-risk counties, relative to low-risk ones.

2. Texas Versus Control States

We turn next to our state-level analysis. In Table 8, we report results from a DiD analysis using Texas counties as the treatment group and counties in control states as the control group. We regress *ln* (relative Medicare spending) on county and year-and-after dummies, county demographic controls, post-reform dummy, post-reform dummy * Texas dummy, and a constant term. The coefficient of principal interest is on the interaction between the post-reform and Texas dummies. Standard errors are clustered on state. The control groups are: in Panel A, 41 states without contemporaneous reforms; in Panel B, 19 non-reformed states; in Panel C, nine similar non-reformed states; all three groups are defined above. For all panels, we present both equally-weighted and population-weighted results.[66]

Consider first the unweighted results. There is no evidence of a post-reform change in total Medicare spending in Texas. For physician spending, results are mixed: Texas physician spending rises after reform using the broad control group, but this result weakens with the narrow group. Hospital spending declines using the narrow control group, but this result weakens with the broad control group. The population-weighted results provide evidence, across all three control groups, of a post-reform rise in physician spending. The results for hospital spending and total spending remain mixed.

Overall, these results are consistent with, and modestly strengthen, the hints in Figure 4 and Table 2 of higher post-reform physician spending. The stronger rise in physician spending using population weights implies that the rise is driven by larger counties, which also tend to be high-risk counties. At the same time, for the narrower control groups, there is evidence of a post-reform *drop* in hospital spending, which roughly offsets higher physician spending. One possible explanation for these results is that overall spending did not respond to med mal reform, but there was another Texas-specific change that moved health-care spending from Part A to Part B.

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[66]We conducted a similar DiD analysis using only border counties in Texas and its neighboring states (we exclude Oklahoma, which adopted a non-economic cap around the same time as Texas). The sample includes 26 Texas counties neighboring New Mexico, Arkansas, or Louisiana and 17 counties in these three other states. None of the differences were significant, which is not surprising given the small sample size.

Table 7: DiD Results: Top-Versus Bottom-Risk Counties

Dependent Variable	*Ln(relative spending)*				
Geographic Area	*County*				
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Medicare Spending Area	*Total*	*Part A*	*Part B*	*Medical and Surgical*	*Imaging and Lab*
Post-reform dummy * High-risk dummy	0.0146	0.0202	0.0086	0.0133	–0.0120
	[0.98]	[0.91]	[0.53]	[0.53]	[0.50]
95% confidence interval	*[–0.015, –0.044]*	*[–0.023, –0.064]*	*[–0.023, –0.041]*	*[–0.037, –0.063]*	*[–0.060, –0.036]*
Year-or-after dummies, Ln(population), constant	yes	yes	yes	yes	yes
Observations	1,430	1,430	1,430	1,138	1,138
No. of counties	130	130	130	127	127
R^2	0.5947	0.4191	0.7000	0.7238	0.7417

NOTE: Difference-in-differences regressions, with county fixed effects, of *ln* (relative Medicare spending) over 1999–2009 on post-reform dummy (= 1 for 2004 and later), high-risk dummy (= 1 for 25 percent of Texas counties with highest long-term med mal risk), interaction between these variables, year-and-after dummies, ln(population), and constant term. Variables are defined in Table 2. Control group is 65 "bottom-risk" counties with zero paid claims over 1999–2003. Middle-risk counties are dropped. Amounts in 2007$. *t* statistics, with standard errors clustered on county, in parentheses. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels. Significant results (at 5 percent or better) are in **boldface**.

Table 8: DiD Results: Texas Versus Other States

Dependent Variable	*Ln(relative spending)*					
Geographic Area	*County*					
	Unweighted			*Population Weighted*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Medicare Spending Area	*Total*	*Part A*	*Part B*	*Total*	*Part A*	*Part B*
Panel A. Texas vs. 41 States Without Recent Reform						
Post-reform dummy * Texas dummy	0.0104	–0.0046	**0.0233**	0.0241	–0.0194	**0.0684**
	[1.50]	[0.56]	**[2.98]*****	[1.85]*	[1.46]	**[4.67]*****
Observations	27,005	27,003	27,005	27,005	27,003	27,005
Panel B. Texas vs. 19 "Non-reformed" States						
Post-reform dummy * Texas dummy	0.0003	**–0.0249**	0.0249	0.0123	–0.0314	**0.0566**
	[0.03]	**[2.73]****	[1.67]	[0.95]	[1.78]*	**[6.06]*****
Observations	12,760	12,760	12,760	12,760	12,760	12,760
Panel C. Texas vs. 9 Similar "Non-reformed" States						
Post-reform dummy * Texas dummy	–0.0091	**–0.0318**	0.0122	–0.0044	**–0.0504**	**0.0429**
	[0.98]	**[4.14]*****	[0.64]	[0.31]	**[2.46]****	**[3.00]****
Observations	9,614	9,614	9,614	9,614	9,614	9,614

NOTE: Difference-in-differences regressions, with county fixed effects, of *ln* (relative Medicare spending) over 1999–2009 on post-reform dummy, Texas dummy (= 1 for Texas counties), interaction term, year-and-after dummies, ln(population), and constant term. Variables are defined in Table 2. Amounts in 2007$. Control groups for each panel are defined in the text. *t* statistics, with standard errors clustered on state, in parentheses. Regressions (4)–(6) use population weights. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels. Significant results (at 5 percent or better) are in **boldface**.

V. Med Mal Reform and Spending Trends

A. *Spending Trends and Med Mal Risk*

In Section IV, we studied the effect of tort reform on Medicare spending *levels*. In this section, we examine spending *trends*. Tort reform might affect cost trends, instead of, or as well as, cost levels. Or, more likely, it might affect levels gradually, with the effect showing up as a post-reform change in trend that persists for a number of years after reform.

Table 9 assesses whether Texas spending trends change after reform. Regression (1) reports results from a regression of ln(relative Part B spending) on year, post-reform dummy, long-term med mal risk, two-way interactions among these variables, the triple-interaction variable of principal interest (post-reform dummy * long-term med mal risk * year), county dummies, ln(population), and a constant term. A negative coefficient on the triple-interaction term implies that spending trends declined after reform in high-risk counties relative to low-risk counties (the "trends" version of Hypothesis 3). In regression (1), this coefficient is negative but insignificant, consistent with no relative post-reform change in spending trends.

Table 9: Changes in Spending Trends After Tort Reform

Dependent Variable	*Ln(relative spending)*					
Geographic Area	*County*					
Risk Measure	*Continuous (Long-Term Risk)*			*Discrete (High-Risk Dummy)*		
Weights	*None*	*Population Weighted*		*None*	*Population Weighted*	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Medicare Spending Area	*Part B*	*Part B*	*Total*	*Part B*	*Part B*	*Total*
Trend (year – 1999)	**–0.0103**	**–0.0117**	**–0.0072**	**–0.0157**	–0.0111	–0.0062
	[4.83]***	**[3.07]*****	**[2.21]****	**[2.55]****	[1.86]*	[1.46]
Post-reform dummy	**–0.1747**	**–0.1367**	–0.0550	**–0.2321**	–0.0787	0.0050
	[5.39]***	**[3.32]*****	[1.31]	**[2.32]****	[1.19]	[0.10]
Trend * Post-reform dummy	**0.0193**	**0.0163**	**0.0082**	**0.0249**	0.0112	0.0033
	[6.59]***	**[4.28]*****	**[2.05]****	**[2.76]*****	[1.81]*	[0.74]
Trend * Risk measure	0.0027	–0.0032	–0.0034	0.0092	–0.0067	–0.0070
	[1.11]	[1.12]	[1.56]	[1.07]	[0.81]	[1.19]
Post-reform dummy * Risk measure	0.0230	**–0.0869**	**–0.0685**	0.0515	**–0.1979**	**–0.1624**
	[0.67]	**[2.71]*****	**[2.48]****	[0.46]	**[2.34]****	**[2.43]****
Trend * Post-reform dummy * Risk measure	–0.0027	**0.0073**	**0.0058**	–0.0063	**0.0171**	**0.0137**
	[0.85]	**[2.50]****	**[2.24]****	[0.62]	**[2.19]****	**[2.27]****
Ln(population), constant	yes	yes	yes	yes	yes	yes
Observations	2,794	2,794	2,794	1,430	1,430	1,430
No. of counties	254	254	254	130	130	130
R^2	0.7071	0.8073	0.8053	0.6924	0.8144	0.8217

NOTE: Columns (1)–(3): county fixed effects regressions of *ln* (relative Medicare spending) over 1999–2009 on year trend (year – 1999), post-reform dummy, long-term med mal risk, double interactions, and triple interaction between these variables, ln(population) and constant term. Variables are defined in Tables 1 and 2. Columns (4)–(6) show difference-in-difference regressions, with county fixed effects, of *ln* (relative Medicare spending) over 1999–2009 on post-reform dummy, high-risk dummy (= 1 for top 25 percent of Texas counties based on long-term med mal risk), double interactions, and triple interaction between these variables, ln(population), and constant term. Amounts in 2007$. Control group is 65 "bottom-risk" counties with zero paid claims over 1999–2003. Middle-risk counties are dropped. Regressions (2), (3), (5), and (6) use population weights. The noninteracted risk measure (long-term med mal risk or high-risk dummy) is absorbed by the county fixed effects. *t* statistics, with standard errors clustered on county, in parentheses. *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent levels. Significant results (at 5 percent or better) are in **boldface**.

In regression (2), we use population weights. The coefficient on the triple interaction becomes positive and significant. In regression (3), we study relative trends in *total* spending using population weights. The coefficient on the triple interaction is positive and statistically significant. In similar unreported regressions for Part A spending, the triple interaction is always economically small and statistically in significant. Regressions (2) and (3) provide evidence of a relative risk in spending in high-risk counties, driven by physician spending in larger, urban counties.

We next switch to a DiD framework, in which we replace the continuous med mal risk measure with a high-risk dummy. As we did above, we compare the top 25 percent of counties to the bottom 25 percent, and drop middle-risk counties. Table 9, regression (4) reports equal-weighted results for Part B spending, regression (5) reports population-weighted results for Part B spending, and regression (6) reports population-weighted results for total spending. The coefficient of interest is again the triple-interaction term. The population weighted coefficients on both Part B and total spending are positive and significant. We obtain similar results with top-versus-bottom 33 percent or top-versus-bottom half sample splits.

Taken as a whole, these trend results are consistent with the "levels" results in Section IV. They provide no support for Hypothesis 3, and mild evidence of a relative rise in physician spending in larger, high-risk counties.

B. McAllen Versus El Paso: Can Med Mal Liability Explain Divergent Trends?

A highly publicized 2009 article by Dr. Atul Gawande in the *New Yorker* stressed the role of physician culture in generating high health-care costs. The article focused on McAllen, Texas, a border town in the Rio Grande Valley.[67] It compared McAllen, which had per-capita Medicare spending almost twice the national average, with another border area, El Paso, which had similar demographics but Medicare spending similar to national and Texas averages. In 1992, McAllen and El Paso had similar per-capita Medicare spending, but since then, spending in McAllen has grown far faster (8.3 percent vs. 3.4 percent per year). Physicians in McAllen blamed malpractice, and said practicing in McAllen was "legal hell."[68] Can higher med mal risk help explain why spending in McAllen grew so much faster than in El Paso?

Figure 6 presents some evidence. Panel A shows physician (Part B) Medicare spending in the McAllen and El Paso HSAs, and Texas as a whole, in each case as a ratio with U.S. mean spending in the denominator. There is a striking increase in spending in McAllen compared to the United States, Texas, and El Paso. In 1992, McAllen spending on physicians is close to the national norm. By 2007, it is over twice the national norm. In unreported results, spending patterns for Medicare Part A were similar in McAllen and El Paso. Thus, McAllen physicians changed their behavior; McAllen hospitals did not.

Panel B of Figure 6 shows annual, non-normalized med mal risk. Med mal risk in McAllen was, if anything, falling during the period when Part B spending soared, and was not materially different than El Paso or Texas as a whole. Thus, differences in liability risk cannot explain McAllen's sharply rising physician spending.

McAllen versus El Paso is, of course, an anecdote, not data. Still, it suggests that physician culture, including the profit-seeking culture that Gawande describes, is a first-order explanation for health-care spending. The impact of malpractice risk on spending, in contrast, is second order at best.

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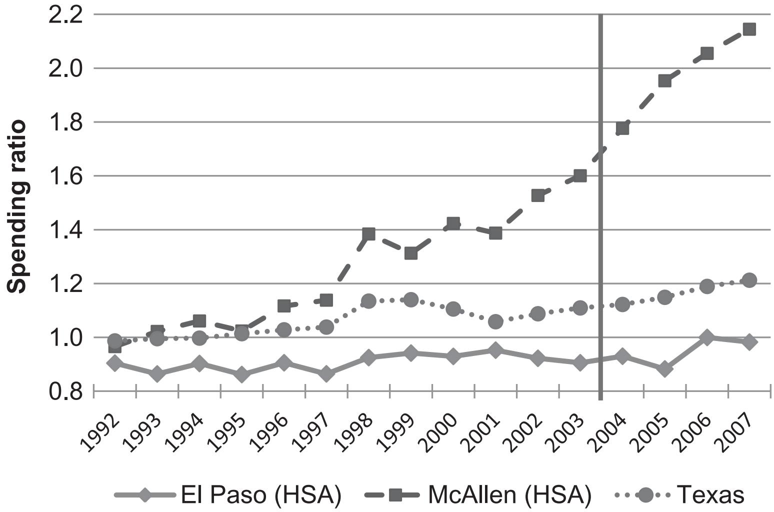
[67]Gawande (2009). Gawande discussed data for McAllen and El Paso, Texas based on hospital referral regions (HRRs). The HRR for El Paso includes zip codes in New Mexico, for which we lack information on med mal risk, so we focus on the slightly smaller McAllen and El Paso HSAs.

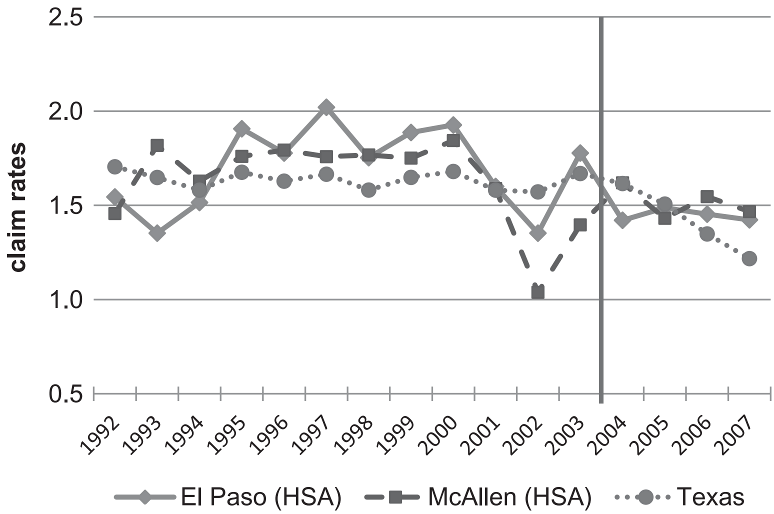
[68]Id.

Figure 6: Medicare Part B spending and med mal risk: El Paso and McAllen HSAs.

Panel A. Medicare Part B spending ratio



Panel B. Med mal claim rates



Panel A. Ratio of Medicare Part B spending per enrollee in El Paso and McAllen HSAs, and Texas as a whole, to U.S. Part B spending per enrollee. **Panel B.** Non-normalized annual med mal risk in El Paso and McAllen HSAs, and Texas as a whole, defined as $ln(1 + \text{number of claims}/100{,}000 \text{ population})$. Texas tort reform in 2003 is depicted by a vertical line.

VI. Discussion

A. Texas Tort Reform and Medicare Spending

We find no evidence that Texas's 2003 tort reforms reduced health-care spending or spending trends. A major exogenous shock to med mal risk from the reforms had no material impact on Medicare spending (in effect, health-care *quantity*), no matter how we slice the data. We find no evidence that overall health-care spending, physician spending, or imaging and lab spending declined more in counties with higher med mal risk. We also find no overall decline in Texas Medicare spending relative to control states, nor an overall association between spending (or spending trends) and med mal risk. If anything, we find some evidence, well short of definitive, that physician spending *rose* after reform in larger, high-risk counties. Our data are limited to Medicare, but med mal reform seems even less likely to influence treatment intensity for the privately insured, since most private insurers exercise greater oversight over treatment decisions than does Medicare.

Med mal premiums fell by half for the state's largest insurer and likely by similar percentages for other carriers. This drop in direct costs, without more, would not directly affect Medicare spending, but might lead to modestly lower prices paid by private insurers. But this would be a small effect, if only because med mal premiums are a small fraction of health-care costs. It also would not suggest a decline in defensive medicine. If one seeks to estimate how tort reform affects defensive medicine, the quantity measure reflected in Medicare spending is the appropriate measure.

We have six years of data on post-reform health-care spending, but only four years for the subcategories of Part B spending. We cannot rule out a longer-term impact of tort reform on Medicare spending. Perhaps defensive medicine is both substantial and sensitive to liability risk, but physicians are slow to change their practice patterns. That longer-term impact, if it exists, would be hard to estimate, given the other factors that affect health-care spending. The further one gets from the time of reform, the less reliable will be any effort to have confidence in a causal link between tort reform and health-care spending.

B. A "Credible Interval" for the Impact of Tort Reform on Spending

The political debate over defensive medicine has focused on how much the United States might save on healthcare if tort reform was enacted. Our results, combined with those from other studies, let us place some bounds on the likely impact of tort reform on spending. We believe a "credible interval" for the most likely effect of major tort reform on health-care spending runs from 0 percent to about a 2 percent decline for states that currently lack caps on non-econ or total damages. *Higher* spending cannot be ruled out; indeed, our study finds some evidence suggesting higher spending after reform. The only study with both a strong research design and a central estimate above this range is the early study of cardiac care by Kessler and McClellan, but there is reason to doubt whether this estimate is generalizable. Zero to one percent of health-care spending is $0 to $30 billion per year. The upper end of this range is more than small change, but we believe that claims that tort reform can meaningfully bend the health-care cost curve, or save hundreds of billions of dollars in annual spending, are not plausible, based on the available research.

C. Explaining a "Null" or Nearly Null Result

If even a large shock to med mal risk does not affect health-care spending, what are the implications? One possibility is that there may not be much "pure" defensive medicine—medical treatments driven solely by liability risk. If liability is only one of a number of factors that influence clinical decisions, even a large reduction in med mal risk might have little impact on health-care spending.

There could be offsetting effects. Lower med mal risk could lead some doctors to practice less defensive medicine, yet make other doctors more willing to offer aggressive medical treatment that is profitable to the doctor but of doubtful value to the patient. There could be savings in some areas of medical practice (cardiac care, perhaps), yet higher costs in other areas. The physician tendency toward more aggressive treatment as med mal risk declines might be stronger in urban areas, with more sophisticated physicians. This could explain the hints we find of higher physician spending in these areas.

Alternatively, the level of defensive medicine may be insensitive to actual liability risk. As noted previously, doctors substantially overestimate the probability of being sued, and their level of concern with malpractice risk responds only weakly to tort reform. Perhaps they are underestimating the significance of the Texas 2003 reforms—even though claim rates and malpractice premiums have dropped dramatically. One survey of Texas physicians reports that since tort reform, 31 percent report practicing less defensive medicine, with 64 percent reporting no change and 5 percent reporting an increase.[69] Still, if the major, highly publicized Texas reforms, followed by a major drop in insurance premiums, did little to persuade doctors to practice less defensively, it is unclear what would do so, other than complete abolition of med mal liability. To date, no one has proposed going that far.

D. Implications for Health-Care Cost Control

There is no shortage of plausible "first-order" explanations for the high cost of U.S. healthcare. One is physician incentives to provide profitable services, as suggested by Gawande's comparison of McAllen and El Paso, and by the Dartmouth Atlas work on spending disparities.

A second is a political system that has thus far been unwilling to impose, for the publicly financed portion of health-care spending, the types of limits on spending that are routine in many other countries. Little about healthcare generates bipartisan agreement, but common ground to date is agreement that the elderly have a right to all the healthcare their physician wants to prescribe, almost entirely paid for with other people's (i.e., taxpayer) funds.

Politically convenient myths are hard to kill. The myth that defensive medicine is an important driver of health-care costs is convenient to politicians who claim to want to

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[69]Robeznieks (2010).

control costs, but are unwilling to take the unpopular (with physicians or the elderly) steps needed to do so. It is convenient for health-care providers, who prefer lower liability risk. It is also convenient for members of the public, who find it easy to blame lawyers and the legal system for problems that have more complex and difficult roots, and call for stronger responses.

E. Heterogeneous Causal Effects?

Most prior research on whether med mal reform affects health-care spending relies on state-level data. For these studies, each state offers one data point. In contrast, our primary analysis relies on intrastate data. An obvious limitation of our study is that we study only one state, albeit a large and diverse one, which experienced a large shock to med mal risk. But our approach has several strengths, which make it a valuable complement to the state-level studies.

First, we benefit from a far larger sample size, permitting us to obtain tight confidence bounds around our results. Second, we can use a continuous measure of med mal risk, rather than just discrete "reform or not" dummy variables, which treat all non-econ caps as equivalent, and all states as having equivalent background levels of med mal risk.

Third, our continuous measure lets us ask a question that state-level studies cannot address: Does the impact of reform vary with background charactistics—for us, county characteristics? We find suggestive evidence that physician spending increases after reform in high-risk urban counties. That evidence opens up an area of fruitful inquiry. In econometrics-speak, there could be "heterogeneous causal effects" of med mal reforms. Reforms might (modestly) reduce spending in some geographic areas, and raise it in others. That possibility deserves investigation in future studies.

VII. Conclusion

Our findings do not indicate that there is no defensive medicine. They do provide evidence that tort reform is not a potent way to reduce health-care spending. Even a major shock to Texas med mal risk produced no apparent decline in health-care utilization, at lags up to four years. Indeed, we find some evidence of higher physician-directed spending in high-risk urban areas. Our findings are robust to multiple alternative specifications. Those interested in a magic bullet that will limit the growth of health-care spending should look elsewhere.

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Appendix A: Mapping Between Counties and HSAs

We analyze the impact of malpractice risk on Medicare spending principally at the county level because malpractice claim data are available at the county level. Medicare spending data are available at the county level from CMS, but only for total spending, Part A spending, and Part B spending, not for subcategories of Part B. Spending for subcategories of Part B is available from Dartmouth Atlas, though only through 2007. For county-level analysis of Part B subcategories, we use HSA-level spending to estimate county-level spending following the procedure in Section A.1. For robustness checks using HSA-level spending, we use county-level med mal risk to estimate HSA-level risk following the procedure in Section A.2.

A1. Mapping HSA-Level Spending on Part B Subcategories to County-Level Spending

To convert HSA-level spending for Part B subcategories to county-level spending, we first compute what percentage of overall Part B spending in each HSA is in each subcategory. The subcategories we study are (medical and surgical) and (imaging and laboratory = "lab"). We apply the percentages from two or more HSAs that overlap with a given county based on the fraction of country population located in each HSA. We use zip-code-level data on population (from the 2000 Census), together with a zip-code-county-HSA cross-walk (we merge a zip code to county cross-walk from the Census Bureau and a zip code to HSA cross-walk from Dartmouth Atlas) to assign weights to each HSA, for each county. The example below illustrates.

Computation for County Z Associated with HSA_1 and HSA_2

Suppose that:

Two HSAs (1 and 2) overlap with County Z
(HSA_1 Lab spending)/(HSA_1 Part B total spending) = 20%

(HSA_2 Lab spending) / (HSA_2 Part B total spending) = 40%
Zip codes within HSA_1 comprise 70% of County Z population
Zip codes within HSA_2 comprise 30% of County Z population

(Each HSA may also have other zip codes, located in other counties.)

We assume that for County Z, the ratio of lab spending/Part B total spending is:

$$70\% * 20\% \text{ (from } HSA_1) + 30\% * 40\% \text{ (from } HSA_2) = \mathbf{26}\%$$

Example: Anderson County

Anderson County is associated with two HSAs, Palestine and Tyler. Palestine zip codes comprise 90 percent of Anderson County population; Tyler zip codes comprise the remaining 10 percent. In 2007, 31 percent of Palestine Part B spending is for medical and surgical services, and 36 percent of Tyler Part B spending is for medical and surgical services. Thus, we estimate the medical and surgical services ratio for Anderson County in 2007 as (90% * 31%) + (10% * 36%) = 31.5%. Medicare Part B spending per enrollee in Anderson County in 2007 is $4,384, so we estimate spending per enrollee on medical and surgical services as $4,384 * 31.5% = $1,380. If data had been missing for Tyler HSA, we would use Palestine data, and estimate Anderson County medical and surgical spending as 31% * $4,384 = $1,358.[70]

A2. Mapping County-Level Med Mal Risk to HSAs

For HSA-level regressions, we have spending data at the HSA level, and need to map counties to HSAs in order to convert our county-level med mal risk measures to HSA-based measures. We use the zip-county-HSA cross-walk described above. We compute HSA population based on the zip codes included in each HSA. Suppose that: (1) HSA_1 is located partly in County Y and partly in County Z; (2) zip codes within County Y comprise 70 percent of the HSA_1 population, and zip codes within County Z comprise the remaining 30 percent of HSA_1 population. Each county may have other zip codes, located in other HSAs. We compute med mal risk in HSA_1 as (70% * County Y risk) + (30% * County Z risk).

[70]More generally, if two or more HSAs overlap with a single county, but data are missing for one or more HSAs, we use the nonmissing HSA data to estimate county-level spending.

Appendix B: Correlations Between Med Mal Risk Measures

We use multiple measures of med mal risk in this study. Table B1 reports the correlation coefficients among the different measures we employ.

Table B1: Correlations Between County-Level Med Mal Risk Measures

	Annual Med Mal Risk	*Long-Term Med Mal Risk*	*Long-Term Payout Risk*	*Long-Term Open Claim Risk*	*Long-Term Elderly Claim Risk*	*Time-Varying 5-Year Risk*
Annual med mal risk	1					
Long-term med mal risk	**0.5206*****	1				
Long-term payout risk	**0.5079*****	**0.9392*****	1			
Long-term open claim risk	**0.5228*****	**0.8290*****	**0.8350*****	1		
Long-term elderly claim risk	**0.4086*****	**0.6s914*****	**0.6959*****	**0.6630*****	1	
Time-varying 5-year risk	**0.6515*****	**0.8080*****	**0.7810*****	**0.7850*****	**0.6327*****	**1**
Ex-post med mal risk drop	**0.3438*****	**0.8505*****	**0.7709*****	**0.5932*****	**0.5028*****	**0.5758*****

Note: Risk measures are defined in Table 5. *** indicates significance at the 1 percent level. Significant results are in **boldface**.