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Does Regulation of Physicians Reduce Health Care Spending?

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Abstract

The medical community often argues that physician fear of legal liability increases health care spending. Theoretically, though, the effect could be positive or negative, and empirical evidence has supported both cases. Previous studies, however, have ignored the fact that physicians face risk from industry oversight groups like state-level medical licensing boards in addition to civil litigation risk. This paper addresses this omission by incorporating previously unused data on punishments by oversight groups against physicians, known as adverse actions, along with malpractice payments data to study state-level health care spending. My analysis suggests that, contrary to conventional wisdom, health care spending does not rise in response to higher levels of risk. An increase in adverse actions equal to 16 (the mean, absolute value of year-to-year changes within a state) is found to be associated with statistically significant, annual decreases in state hospital care spending of more than $22 million, and in prescription drugs of nearly $10 million. Malpractice payments were generally estimated to have smaller, statistically insignificant effects.

Keywords: regulation; defensive medicine; medical malpractice; health care spending; medical licensing

JEL categories: K32, I11, I18, H75, K23

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1 Introduction

This paper investigates whether legal liability risk faced by physicians, which originates primarily through the civil litigation system and state-level medical licensing boards, induces changes in the overall cost of health care in the United States. The predominant belief in the medical field is that physicians are overly cautious in their treatment recommendations for patients out of fear of malpractice allegations (Studdert et al., 2005; Bishop et al., 2010; Sirovich et al., 2011; American Medical Association, 2012b). This phenomenon, often referred to as *defensive medicine*, is thought to cause increased spending on treatments not warranted by medical need. These views have led physicians to call for protection from legal liability in the practice of medicine (Bishop et al., 2010; American Medical Association, 2012a) – calls that have found a receptive audience in the public sector. As of 2006, 26 states had adopted laws limiting malpractice lawsuit damages (Mello, 2006a), and there have been efforts to pass national limits to damages.[1]

The public discourse on the subject of liability risk and defensive medicine, however, often ignores the fact that the theoretical response by physicians to malpractice risk is ambiguous. The staff of the congressional Office of Technology Assessment first formalized this ambiguity by defining *positive defensive medicine* as when physicians order extra medical services to avoid legal liability, and *negative defensive medicine* as when they avoid patients or services out of liability fears (U.S. Congress, Office of Technology Assessment, 1994). In the academic literature, both of these possibilities were incorporated into an economic model by Currie and MacLeod (2008). The uncertainty goes further, though, since even if we know for certain in which direction medical services provided moves in response to risk, the spending result would still be ambiguous. For example, physicians might respond to risk by performing more preventative services – which is positive defensive medicine – but which may result in lower spending if more expensive services are prevented.[2] Similarly, avoidance behavior by

[1] One recent attempt was the Protecting Access to Healthcare Act, H.R. 5, 112th Congress (2012). The bill was passed by the House of Representatives but not the senate.

[2] It is not clear such a scenario would be possible, though, as some research has questioned whether

physicians – negative defensive medicine – could actually increase spending if it leads to more expensive problems in enough of the avoided patients. The empirical literature on the subject of defensive medicine reflects these theoretical ambiguities. Kessler and McClellan (1996) found that laws restricting malpractice damages were associated with decreased spending in heart condition patients (positive defensive medicine), but Currie and MacLeod (2008) found they resulted in increased use of C-section procedures in births. Similarly, Dubay et al. (2001) found higher malpractice premiums were related to prenatal care delays. Further, Kessler et al. (2005) estimated that physician supply increased when malpractice damages were capped by tort reforms (all three examples of negative defensive medicine). Other authors generally found small effects or no statistically significant evidence for defensive medicine (Dubay et al., 1999; Baicker et al., 2007; Kim, 2007; Carrier et al., 2010; Thomas et al., 2010). Lakdawalla and Seabury (2012) found defensive medicine had moderate effects on spending growth, but argued these costs were justified by reduced mortality.[3]

My analysis introduces new evidence on the matter by exploiting a previously untapped measure of risk, the state-level frequency of sanctions taken against physicians by industry oversight groups, such as medical boards, Medicare, Medicaid, and hospitals. Known as *adverse actions*, these sanctions are punishments against physicians for professional misconduct, such as incompetent care or breaking certain types of laws, and are intended to motivate doctors to maintain high quality (Shryock, 1967). Actions taken by state medical boards generally involve restrictions on the physician's license (e.g. probation, suspension, revocation), and actions taken by other organizations are similar conceptually though smaller in scope (e.g. a hospital may suspend clinical privileges). From the perspective of physicians, adverse actions represent a threat that is similar to malpractice litigation risk. Both could result from an alleged medical malpractice incident, and both have potentially large monetary impacts (adverse actions through the inability to practice either temporarily or

preventative care is any more cost effective than ex-post treatment (e.g., Cohen et al., 2008).

[3]In an international context, Chen and Yang (2014) found evidence of positive defensive medicine in Taiwan.

permanently). Thus, the risk originating from adverse actions could potentially induce defensive medicine and affect spending in the same ways that have been argued for litigation risk.

This paper contributes to the literature by performing the first analysis combining and comparing both of these different types of malpractice risk. Previous work has all focused on the civil litigation risk, leaving a gap in our knowledge regarding the effects of adverse action risk. This is an important discrepancy since the financial risk posed by adverse actions is one that physicians do not insure against. Moreover, the risk that adverse actions pose is potentially severe, since, as shown in Table 1, they often limit physicians' ability to practice medicine freely. In contrast, nearly all physicians carry insurance against civil litigation risk (Mello, 2006b), rendering their medical treatment decisions irrelevant to their expected monetary malpractice litigation costs.[4] Some previous authors (e.g. Kessler and McClellan, 1996) have, therefore, suggested that the primary mechanism behind defensive medicine would be indirect or noneconomic costs associated with malpractice litigation, such as reputation effects, stress, or time spent defending against allegations. While this certainly could be the case, a physician facing an adverse action from the state medical board (for example) would face all these same costs *in addition* to potential financial consequences, since no insurance is available for adverse actions. Thus, if physicians do respond to malpractice litigation risk that they carry insurance for, it would then seem reasonable to expect that they would also respond to risk originating from adverse actions.

Measurement of both malpractice payments and adverse actions is possible through the use data made publicly available by the National Practitioner Data Bank (NPDB). Federal law mandates reporting of both adverse actions and malpractice payments to the NPDB, so this unique government database allows observation of the entire universe (or close to it) of

[4]Malpractice litigation insurance is unlike some other forms of insurance in that it is not heavily experience rated, meaning the claims experiences of individual physicians have minimal affect on their own insurance premiums. Instead, malpractice insurance is generally priced based on specialty and geography (Sloan, 1990; Fournier and McInnes, 2001; Mello, 2006b). From the perspective of individual physicians, therefore, malpractice insurance premiums are nearly fixed costs, as their own choices regarding services recommendations have negligible influence on their own insurance rates.

these variables. Using this data, I estimate the effect of changes in the frequency of malpractice payments and adverse actions over a 17 year period on health care expenditures at the state level. I find evidence suggesting that increases in adverse actions against physicians are associated with lower health care expenditures. An increase in adverse actions taken against physicians of 16 – the average of the absolute value of year-to-year changes within states – is estimated to decrease state hospital expenditures by more than $22 million each year. For spending on prescription drugs, the reduction is as much as $10 million. At the same time, I estimate that malpractice payments generally have much smaller, statistically insignificant effects, a result that is consistent with the argument that physician incentives for responding to malpractice litigation risk are muted due to insurance. To the extent that doctors do in fact respond to this type of risk, though, my results suggest that it also decreases spending since in most cases I estimate coefficients with negative signs. My estimates, therefore, are inconsistent with the conventional wisdom of physicians that legal liability risk is an important determinant of health care spending.

2 Empirical investigation

2.1 Econometric model

The primary focus of my empirical investigation is to estimate the influence of malpractice risk on state-level health care spending categories. In particular, I estimate linear panel data models of the form:

$$\ln(SPEND_{st}) = \alpha_s + \widetilde{\alpha}_s t + \beta_t + X'_{st}\gamma + NPDB'_{st}\delta + \epsilon_{st}. \tag{1}$$

Indexes s and t indicate the state and year, while $\ln(SPEND_{st})$ represents the natural log of state-level, real health care expenditures. Column vectors $NPDB_{st}$ and X_{st} contain measures of the malpractice risk environment (per the NPDB data) and other control variables,

respectively. State fixed-effects are represented by α_s, and $\widetilde{\alpha}_s t$ indicates state-specific, linear time-trends (accounting for long-term, state-specific growth-differences in spending). Year dummy variables are represented by β_t, while γ and δ are column vectors of parameters representing marginal effects on health spending.

The $NPDB_{st}$ vector is comprised of two yearly count variables: total physicians who received at least one adverse action and total physicians associated with at least one malpractice payout. These variables serve as proxies for the actual probabilities faced by physicians of adverse actions and malpractice payments, and are the primary variables of interest. I construct these measures using data from the NPDB, and so, for brevity, I will at times refer to these jointly as the "NPDB variables". The introduction of adverse actions and malpractice payments as two separate, linear terms is intended to reflect the argument that physicians view both as potentially affecting them via the same mechanism. The controls that are used to construct the X_{st} vector are described in detail in section 2.3. The actual controls used in a particular version of equation (1) vary during the empirical analysis to explore the robustness of the estimates.

I estimate equation (1) for each of four categories of expenditures (which are discussed in detail below): hospital care, physician and clinical services, prescription drugs, and other non-durable medical products (the last category being a placebo category). All models are estimated using the within estimator, with state-level, cluster-robust standard errors to account for potential serial correlation(Cameron et al., 2011).[5] Note that since the dependent variable is in logarithms, the estimates produced for equation (1), when multiplied by 100, are interpreted as percentage changes in spending due to one unit changes in the independent variables. For presentation purposes, though, all reported estimates are multiplied by 10,000, so are expressed as hundredths of a percent.

[5]All regression estimates, including standard errors, were implemented using the "xtreg" command of Stata/SE 12.1 for Windows StataCorp (2011).

2.2 Identification

One of the primary advantages of using the NPDB variables for this type of analysis is that they reflect input from the entire system that oversees physicians, including bureaucratic processes, behaviors of juries and judges, and the litigiousness of the population. The benefits of this as compared to an identification strategy based on changes in tort laws can be seen in a simple example. Suppose a state enacts a damages cap for malpractice lawsuits, but that before the law was passed, there were few allegations of malpractice made in the state in the first place. Upon observing little change in spending, one might conclude that changes in malpractice risk have little effect on spending. This conclusion would be faulty, however, because what really happened was there was no real change in the risk level faced by physicians. As there was little enforcement, the change in the law was rendered irrelevant. The NPDB variables, though, would reflect the minimal presence of regulation both before and after the law change, and therefore would more fully account for the risk levels in the state.

Despite this advantage, the use of the NPDB variables as measures of malpractice risk requires careful discussion of the basis for identification. Of first order concern is the threat of endogeneity, since the NPDB variables result from the interaction of the regulatory, litigation, and medical environments in a state. One step I took to address part of these concerns was to rely a frequency count of malpractice payments instead of a measure of the dollar value of payments or other dollar denominated measures like malpractice insurance premiums. This addresses the possibility of bias caused by trends in health care prices and utilization, which obviously play roles in determining health care spending, but that also contribute to the dollar value of malpractice payments and insurance premiums. For malpractice payments, this is because payments are often set to cover some or all of the medical bills of the accusing party. In the case of malpractice insurance premiums, the link comes from the fact that most malpractice payments are actually made by insurers, and premiums are set to help cover the cost of making these payments.

Avoiding dollar based measures of risk only addresses a portion of the endogeneity concerns, though. Another threat is that the NPDB variables may not only reflect pure enforcement, but may also, to some extent, reflect the rate that malpractice occurs in the state. For example, it may be the case that when there is more actual malpractice – perhaps due to generally worse doctors – there are more accusations and stronger cases against physicians, resulting in more observed payments or adverse actions. Basing estimation on measures of regulatory risk that are contaminated in this way could possibly cause misleading results. To reduce the severity of this threat, I include state-level fixed effects in my regressions to account for time-invariant, unobserved heterogeneity at the state-level, including state-specific malpractice rates. Furthermore, I also include state-specific, linear time trends to account for time-varying heterogeneity that occurs in a linear fashion. These steps would still, however, leave the possibility of non-linear time variation in malpractice rates influencing my results. The nature of the NPDB variables, however, provides a solution to this issue. Each malpractice payment and adverse action is the end result of a legal process that begins in year t_1 with the original incident or injury of the accusing patient. The original incident is possibly a reflection of the malpractice rate since the patient may have received substandard care resulting in injury, but my analysis does not count the incident until the end of the legal process in year t_2, when a payment is made or an action taken against a physician by an oversight group. These measures are then paired via regression with outcome measures of spending for the corresponding year, t_2. Thus, the NPDB variables used in my analysis are implicitly lag variables: to the extent that they measure the malpractice rate in the state, they measure it as of $t_2 - t_1$ years before the measurement of the dependent variable in my regressions, state health care spending. In the case of malpractice payments, the NPDB data shows that $t_2 - t_1$ is usually quite a long time, as the median payment occurred four years after the incident from which it originated, with the tenth percentile being two years after, and the ninetieth percentile, eight years after. The NPDB does not have the information necessary to determine the time lag for adverse actions, but statistics posted

to the website of the Medical Board of California (2010) indicate that the legal process for actions on a physician's license is also quite long, with the sum of the median number of days for the stages of the complaint process exceeding 600 days (and rising) since fiscal year 1999/2000. Category averages for each stage of the medical board legal process also exceed their medians, usually significantly so.

Given this lag aspect of the NPDB variables, the malpractice rate in a state could only influence my estimates if the malpractice rate in year t_1 has a direct influence on health spending several years after the fact, in year t_2. Thus, the validity of my estimates relies on the assumption that malpractice rates of years past do not influence spending in the current year. This assumption can be argued to be reasonable because previous research has suggested that the effect of *actual* malpractice – as in injuries to patients due to medical errors – on health spending is minimal, regardless of the year. A paper from a large study of medical injuries in Utah and Colorado by Thomas et al. (1999) estimated that "adverse events" – injuries caused by medical management, but not necessarily reflecting negligence – accounted for about 4% of national health expenditures in 1996. Only about half of these costs, though, were due to spending on health care (the rest was lost wages and home production). The amount of adverse events attributable to negligence (i.e. malpractice) was estimated by Brennan et al. (1991) and Thomas et al. (2000) to be on the order of 30%, bringing the overall estimate of the direct effect of malpractice on health spending to less than 1% of national health expenditures.

I have presented, therefore, several arguments suggesting that coefficient estimates based on the NPDB variables could be considered credible even if they partially reflect the malpractice rate of the state. These arguments, however, are only necessary in the first place if the NPDB variables do truly reflect the malpractice rate – a possibility that is *not* supported by previous research. In fact, two large studies of the connection between medical negligence (i.e. malpractice) in American hospitals and malpractice allegations – the first step of the relationship between actual malpractice and adverse actions or malpractice payments – have

shown that the link between the two is quite weak. The first study, performed in New York hospitals by Localio et al. (1991), found that a malpractice claim followed an actual example of malpractice only 1.5% of the time. The second study, by Studdert et al. (2000) and performed in Colorado and Utah hospitals, found a similar estimate of 2.5%. Furthermore, both studies found that claims of malpractice were frequently made in cases where it was likely no malpractice occurred (83% of claims in the New York sample and 78% in the Colorado and Utah sample). As a result, the authors of the Colorado and Utah study drew the following conclusions:

> "The poor correlation between medical negligence and malpractice claims that was present in New York in 1984 is also present in Utah and Colorado in 1992. Paradoxically, the incidence of negligent adverse events exceeds the incidence of malpractice claims but when a physician is sued, there is a high probability that it will be for rendering nonnegligent care." (p. 250)

Another study, national in scope, by Studdert et al. (2006), focused on the relationship between claims and outcomes of malpractice litigation – the second step of the relationship between malpractice and adverse actions or malpractice payments. Using data on closed claims from medical liability insurers, the authors found that 27% of malpractice claims regarding incidents in which it was judged that malpractice did actually occur resulted in no payment at all being made to the claimant. Conversely, in cases where no malpractice occurred, payment was still made 28% of the time. Thus, in cases where a claim is made, the wrong outcome – i.e. a payment made when it should not be or no payment made when it should be – is quite a common occurrence. Overall, once a malpractice accusation has been made, a payment is far from certain. Studdert et al. (2006) and Vidmar et al. (2004) both calculated that about 56% of malpractice claims resulted in a payment being made. Other studies, however, estimated this rate to be much lower at 22% (Jena et al., 2011) and 19% (Black et al., 2005). The current processes of accusations, negotiations, and legal mechanisms, therefore, imposes a strong filter between the actual occurrence of malpractice

and the observation of an outcome in the NPDB. Incidents of actual malpractice rarely result in malpractice claims, and even when claims are made, payment is estimated to take place as little as 19% of the time. Furthermore, claims and payments are commonly associated with incidents in which no malpractice occurred. These facts suggest that if the NPDB variables reflect the influence of the state's rate of malpractice, it is a very diluted effect.

In summary, clean identification of the effects of adverse actions and malpractice payments on health care spending hinges on the assumption that the NPDB variables are not correlated with unobserved factors like trends in health care prices and the true rate of malpractice in the state. As I have argued, there are several reasons to think this assumption is reasonable. I use counts of adverse actions and malpractice payments in measuring the risk of punishment physicians face, thereby avoiding dollar based measures that might be influenced by price trends. Additionally, by the nature of the legal processes through which adverse actions and malpractice payments arise, there is a long lag between the time of the underlying incident and the sanction. This lag acts as a natural filter of correlation, since the spread of knowledge about *past* sanctions is not likely to be related to *current* levels of malpractice and negligence that would affect *current* spending. Moreover, previous research has suggested that true incidents of malpractice and malpractice payments are poorly correlated, and that any effects of malpractice or negligence on spending are minimal. Nevertheless, I still incorporate several econometric modeling techniques in my analysis that are intended to address potential endogeneity, including state and time fixed effects, as well as state-specific, linear time trends. As discussed below, I evaluate the effectiveness of these techniques with a placebo category of spending and by performing robustness checks of my models that incorporate lead values of the NPDB variables.

2.3 Data

Equation (1) is estimated using a year-by-state (including Washington DC) panel data set that covers the years 1992 through 2008 and is compiled from several sources. The depen-

dent variable in my regressions is health expenditures by the state of the provider, estimated by the Centers for Medicare and Medicaid Services (2011) as part of their National Health Expenditure Accounts program. The Consumer Price Index[6] was used to express these expenditures in 2011 dollars. I use four expenditure categories in my analysis: hospital care, physician and clinical services, prescription drugs, and other non-durable medical products. Hospital care accounts for 37% of all expenditures and also reflects the highest risk setting[7]. Physician and clinical services reflects 25% of spending, and, broadly speaking, covers services provided in outpatient settings. The prescription drugs category includes 10% of total spending and tallies prescription drug sales in retail settings (drugs administered in hospitals or physician offices, and included in hospital or office revenue, are included in the hospital care or physician and clinical services categories). I use the final category, non-durable medical products, for a placebo analysis. This group is much smaller than the other categories at 2.5% of costs, and is unique in that it covers retail sales of medical products that do not require prescriptions or other physician input for purchase. Additionally, this category reflects almost entirely out-of-pocket spending (Medicare pays a small portion) that is at the consumer's discretion. Examples of products in this category include over-the-counter drugs like cough and allergy medicines and medical sundries such as bandages and thermometers. Legal risks faced by physicians, therefore, would be expected to have no influence on this category of spending.[8]

The NPDB variables are calculated using the Public Use Data File of the NPDB (Health Resources and Services Administration, Bureau of Health Professions, 2011). By law, the NPDB collects information on all adverse actions taken against and malpractice payments made by (or on behalf of) physicians in the country.[9] Full data collection began at the start

[6]More precisely, I used the Consumer Price Index for All Urban Consumers: All Items, with the index value for June taken as the value for the whole year (Bureau of Labor Statistics, 2012).

[7]A study of the NPDB by Bishop et al. (2011), using data no longer available to the public, found that roughly half of all malpractice payments originated in a hospital setting, despite that there are almost 30 times more outpatient visits than hospital discharges.

[8]See Centers for Medicare and Medicaid Services (2010) for detailed discussion of the categorization of the National Health Expenditure Accounts.

[9]The National Practitioner Data Bank was established during the presidency of Ronald Reagan by Title IV

of 1992, the first year I use in my analysis. I use the NPDB to count the number of unique physicians associated with malpractice payments and adverse actions by state and year.[10] This includes payments associated with both judgments and settlements, and all types of adverse actions, regardless of the reason given for why the payment was made or adverse action taken (even if it was not necessarily related to medical competence).[11] However, two important restrictions are made in order to avoid double counting. For malpractice payments, I exclude payments identified as made by "State Funds", since, according the the NPDB documentation, these payments are usually made only when a payment has also been made by an insurer. In the case of adverse actions, I do not count actions that are identified as modifying previous actions.

The NPDB collects several types of adverse actions, including state licensure actions (e.g. license probation, suspension, or revocation), exclusions from Medicare and Medicaid, and more localized actions such as hospital clinical privileges restrictions. Counts and shares for these types are presented in Table 1. Malpractice payments include those made after trials as part of judgments or jury awards as well as negotiated settlements. The inclusion of settlements is key to the representativeness of the NPDB since they make up a huge portion of the malpractice risk faced by physicians. Judgments and jury awards, in fact, represent less than 3% of the records in my sample of the NPDB. Thanks to this aspect of the NPDB and the public availability of portions of its data, numerous researchers have made use of it before (e.g., Baicker and Chandra, 2005; Baicker et al., 2007; Bishop et al., 2011).

A natural question to ask when using a dataset like the NPDB is whether it is truly

of Public Law 99-660 (known as the Health Care Quality Improvement Act of 1986), with final regulations found in 45 CFR Part 60 (Health Resources and Services Administration, Bureau of Health Professions, 2001).

[10] In both variables, a physician is only counted once per state and year, even if that physician receives more than one adverse action or malpractice payment. In some cases, a physician might have both an adverse action or malpractice payment in the same state and year (or even multiple cases of either), and in such situations the physician would be counted once in both variables.

[11] There are two justifications for not removing observations on the basis of reason given. First, many records do not have any reason given – 29% in the case of adverse actions, and 23% for malpractice payment records. Secondly, and more importantly, it is likely that all actions convey information about the regulatory regime regardless of the basis for the action (e.g. what types of actions lead to adverse actions or payments, and how costly the punishment or payment could be).

representative of the underlying incidence of malpractice payments and adverse actions. In theory, the law requiring the report of all such events implies that the data should reflect the entire universe, but this could only be true if the required reporting actually takes place. Jena et al. (2011) studied the issue by comparing the malpractice payments data of the NPDB over the period of 1991 to 2005 to data they obtained from a large, national malpractice liability insurer, finding that differences between the two sources were small. No study of the representativeness of the adverse actions data has been performed to my knowledge. However, more than 83% of records originate from state licensing boards or Medicare/Medicaid, groups which seem likely to report according to the requirements of the law.[12]

Regardless of the extent to which the NPDB reflects the true, underlying incidence of malpractice payments and adverse actions, the NPDB is the appropriate data for my analysis since it is the best available measure of physicians' perceptions of the risk they face. This is an important distinction because physicians can only be responsive to the threat of punishment or litigation to the extent that they are *aware* of the threat-level they face. If physicians were receiving adverse actions and settling malpractice claims in complete privacy, then there would not be any reason to expect these punishments to have any impact in the broader community. The NPDB, though, plays an important role in making malpractice payments and adverse actions public, both through data it makes publicly available and its use by health care organizations during credentialing and hiring processes (which results in more than 570,000 queries of the NPDB per month[13]). Thus, *the NPDB itself* – the *publicly known* set of adverse actions and malpractice payments – is the object that I need to measure for

[12]The United States General Accounting Office (2000) (GAO) criticized the NPDB for a number of issues, but most notably suggested that it might suffer from under-reporting. This critique was not based on any actual observation of under-reporting, and was undermined in the case of malpractice payments by the later-published work of Jena et al. (2011). For adverse actions, the critique was based on the fact that reporting by hospitals was far below the level *expected* before the creation of the NPDB, but these expectations were not based on any data source, and may have been unreasonable.

[13]As reported on the NPDB website (https://www.npdb.hrsa.gov/resources/whatIsTheNPDB.jsp, accessed 10/4/2016). It should be noted that this figure represents all queries of the NPDB, not all of which pertain to physicians.

my analysis, not the underlying true incidence of payments and adverse actions that the NPDB intends to measure. This renders the question of representativeness of the NPDB with respect to the true payment and adverse action rates to be somewhat less important in this case.

Besides the spending and NPDB variables, I also collect data from several other sources to create variables for use as regression model controls. Demographic measures include birth rates (Centers for Disease Control and Prevention, National Center for Health Statistics, 2016) and state population counts and shares (calculated by the author from U.S. Census Bureau tables). Shares are broken down by age group (50 to 64, 65 to 79, and 80 or older), race (black and other – that is, not white or black), and gender (U.S. Bureau of the Census, Population Estimates Branch, 2003; U.S. Census Bureau, Population Division, 2012). Health insurance coverage population shares by type (private, Medicare, Medicaid, military, and uninsured) and counts of medical and legal industry workers were calculated by the author using IPUMS CPS data for March (that is, the Annual Social and Economic Supplement) of each year (Flood et al., 2015).[14] The number of active medical doctors (excluding those employed by the federal government) comes from the Area Resource File (US Department of Health and Human Services, Health Resources and Services Administration, Bureau of Health Professions, 2012), while figures for the number of active attorneys in the state come from the American Bar Association (2012, 2009). Finally, state-environment controls include unemployment rates (U.S. Bureau of Labor Statistics, 2016) and indicators for democratic party governors and gubernatorial election years (Klarner, 2003, 2013).[15] Summary statistics for all the variables used in my analysis can be found in Table 2.

[14]Medical industry workers were taken as those in the "medical and other health services, except hospitals" and "hospitals" industries. Law industry workers were those in the "legal services" industry.

[15]Yearly unemployment rates are calculated as averages of monthly rates.

2.4 Results

Table 3 presents the results of estimation of variations of equation (1) on my sample of state health expenditures. Since the estimates, all of which have been multiplied by 10,000, are produced by log-linear models, they are interpreted as hundredths of a percentage. Each regression included state and year fixed-effects, plus linear, state-specific time trends. They otherwise differ, though, by the timing of control variables, which ones are included, or which sample is used. For each category of spending (across the top of the table) there are two columns, the first containing models where all control variables are measured coincidently with the outcome variable (that is, both are measured in time t) and the second where all model controls (including the NPDB variables) are measured at a one year lag (year $t-1$) relative to the measurement of the outcome variable (year t). The lag version of the model is intended as a check for reverse causality between health care spending and the coincidently timed control variables (especially the non-NPDB control variables, since they do not share the lag feature of the NPDB variables discussed in section 2.2).

The top panel of the table, labeled "Model 1" contains estimates where no control variables are included other than the NPDB variables. Focusing first on the hospital care spending category, the model with coincident control variables, column (1), estimates that, on average, one additional physician suffering an adverse action in a year results in a reduction of state hospital expenditures by 1.4 hundredths of a percent, a result that is statistically significant at the 5% significance level. Relative to the average annual state-level spending on hospital care over my sample period (approximately $11.5 billion), this estimate represents a decrease of about $1.6 million. For a change equal to the average of the absolute value of the year-to-year, within-state change of 16, this estimate represents a reduction in spending of more than 22 hundredths of a percent, or nearly $26 million. In contrast to this result, the estimated effect for malpractice payments is insignificant at conventional significance levels, and is quite small in comparison to the adverse actions estimate. In fact, the adverse actions effect is estimated about 33 times larger than the malpractice payments effect. Even

the upper bound of the 95% confidence interval for the malpractice payments coefficient is only 45% of the magnitude of the estimate for adverse actions. Given the conventional wisdom in the medical field that fear of malpractice lawsuits is an important determinant of cost, this result is surprising. That said, it is consistent with the view that broad use of malpractice insurance leaves little reason for physicians to alter their behavior out of fear of litigation. Estimation for the same model but with lagged NPDB variables (column(2)) produces results that are essentially the same (the largest difference being that the adverse action coefficient is statistically significant at the 1% level, not 5%).

Turning to my estimates for spending on physician and clinical services in columns (3) and (4), I find that adverse actions are again estimated to reduce spending, and to do so with magnitudes that (in percentage terms) are similar to those for hospital care spending. The estimate for the coincident model is -1.0 hundredths and is significant at the 10% level, while the lagged model is -1.6 hundredths and is significant at the 5% level. These coefficient estimates correspond to dollar valued effects of -$13 million and -$20 million, respectively, for an average-level increase of 16 in adverse actions and when evaluated at the sample average of the category's annual, state-level spending of $7.8 billion.

For malpractice payments, estimated coefficients for this category are notably larger in magnitude than the estimates for the other types of spending. The coincident model estimate of -0.8 hundredths is statistically significant at the 10% level, implying a decrease of almost 0.22% or $17 million in response to an increase of 26 malpractice payments, the average of the absolute value of the year-to-year, within-state change in payments. The lagged model estimate is somewhat smaller in magnitude at -0.5 hundredths, and thereby not statistically significant. While the magnitudes of the malpractice payments coefficients here are somewhat consistent with the argument that physicians are responsive directly to malpractice litigation risk, I will show below with regressions that include leads for the NPDB variables, there is reason to think that this outcome may be influenced by endogeneity bias. This may help explain the inconsistency of these estimates with those of the other categories.

The next set of regression estimates, for the prescription drug category, can be found in columns (5) and (6). The pattern of results in this case is similar to that of the first two spending categories: the adverse action coefficients have negative signs and are much larger in magnitude than those for malpractice payments. In this case, however, none of the estimates are statistically significant at conventional levels (a result driven by lower precision as compared to the other spending categories). For the coincident model, the adverse action estimate is -0.8. For an increase of 16 adverse actions, this result corresponds to a reduction of 12 hundredths of a percent ($3.8 million) relative to the spending category sample average of $3.2 billion annually. The malpractice payment coefficient is estimated at 0.3, which, for an increase of 26 payments, represents an increase of almost 9 hundredths ($2.7 million). In the lagged control model, the contrast between estimates is much larger, however, with the adverse action result, -1.3 hundredths, being more than 17 times larger in magnitude than the payments estimate of 0.07.

Results for the effects of the NPDB variables on combined spending (the sum of the first three categories) are presented in columns (7) and (8). Reflecting the pattern observed in all three categories, the estimate for adverse actions is negative and much larger in magnitude than that for malpractice payments. The coincident model produces an estimate of -1.2, significant at the 5% level, corresponding to a reduction of 19 hundredths of a percent in spending in response to an increase of 16 actions. This represents more than $43 million of the $22.5 billion in average spending across the three categories. The malpractice payment estimate of -0.2 implies nearly a 6 hundredths of a percent reduction ($12.6 million) in spending in response to an average sized increase of 26 payments. The estimates for the lagged model, at -1.6 for adverse actions and -0.1 for malpractice payments, tell the same, if more exaggerated, story as the coincident model.

The final two columns, (9) and (10), of Table 3 present estimates for a final spending category, other non-durable medical products. These results serve as a placebo analysis, since we should not expect to observe an effect of physician risk avoidance behavior on this

type of health care spending. If there is no endogeneity bias, we would expect to estimate coefficients for adverse actions and malpractice payments approximately equal to zero. On the other hand, if endogeneity is causing bias (and spurious statistically significant estimates in the main regressions) then we would expect to see significant, non-zero estimates when the placebo category is the dependent variable, as well.[16] This is a credible check of endogeneity to the extent that the dependent variables in my main regressions and the placebo regressions share the same unobserved determinants that may bias coefficient estimates. To that end, state-by-year correlations between other non-durable medical products and the other categories suggest that the groups share similar trends over time. They are 0.94, 0.97, 0.87, and 0.96 for hospital care, physician and clinical services, prescription drugs, and combined spending, respectively (0.17, 0.19, 0.33, and 0.26 for first-differences).

Despite these raw correlations, the effects of the NPDB variables observed in the main analyses do not appear to carry over in the non-durable medical products case. As columns (9) and (10) show, neither model estimates statistically significant effects for these variables, casting doubt on the possible influence of unobserved factors. Additionally, unlike the estimates for the other categories, which were generally negatively signed for the NPDB variable coefficients, the estimates in this case are positive, suggesting that if there are unobserved factors, they would seem to be working in the opposite direction of the results in the main analyses.

The next two panels of Table 3, labeled "Model 2" and "Model 3" present estimates for regression models including control variables. For Model 2, these included birthrates, population counts, demographic group and insurance coverage population shares, counts of lawyers and legal industry workers, the unemployment rate, and indicators for democrat governors and gubernatorial election years (due to these last two controls, Washington DC observations were not used for estimation). Model 3 included all these same controls, and added counts for non-federally employed medical doctors and all medical industry workers.

[16]Examples of previous uses of placebo analyses can be found in Hamersma and Kim (2009) and Bitler and Carpenter (2012).

Model 2 omitted these medical industry variables to make sure their inclusion did not result in over controlling for the spending outcome.

Inspecting the results produced by these two models, it is clear that the addition of the control variables has little impact on the overall pattern seen in the estimates produced by Model 1. Both point estimates and sign patterns are substantially similar across all three models. Still, there are a couple of differences that are worth noticing. First, the adverse action coefficient estimates are larger in magnitude for physician & clinical services, prescription drugs, and combined spending when controls are added to the model. Second, for the placebo category, both adverse actions and malpractice payments have smaller estimates (which includes the payments coefficients becoming negative). Lastly, estimates become more precise once controls are added in almost all cases. The implication of these differences is that the relationships suggested in Model 1 are seen even more strongly in Models 2 and 3.

Given that Model 3 includes all available control variables, it is the most credible of the variants of equation 1, producing estimates that hold particular interest. For hospital care, both versions of Model 3 suggest that spending would decline by more than $22 million in response to an average increase (16) in adverse actions, while malpractice payment estimates are both small and not significant. For physician and clinical services, the estimates are consistent with the same average increase in adverse actions resulting in a decrease of $20 million or more. Malpractice payments, in this case, also imply a decrease in spending amounting to more than $11 million per average-level increase (26) of payments. For prescription drugs, the effect of adverse actions appears to be stronger after a year, suggesting that an average increase (16) in payments results in almost $10 million less spending the following year, with no significant effect for payments. For all three categories combined, Model 3 implies that a change in payments of 16 will result in at least $50 million less spending than had the change not occurred, with the effects of payments not being statistically significantly different than zero.

The final panel of the table, labeled "Model 4" contains estimates obtained using the same control variables as in Model 3, but after excluding observations for large states. The purpose of this is to see what extent the estimates are driven by the environments of big states. The excluded states were California, New York, Texas, Florida, Illinois, Pennsylvania, and Ohio, all of which had average populations over the period of my sample of more than 11 million. Each state's average for annual, nominal health care spending also exceeded $52 billion. Here we see that while the estimates have, in most cases, the same characteristics as the estimates for the previous models, there are some notable changes. Most drastically, the adverse actions estimate for the coincident model in column (1) is very near zero (and, hence, not significant) at -0.1. Thus, the estimate is smaller in magnitude, but larger (less negative) in an absolute sense. At the same time, the estimate for malpractice payments is a larger, positive number in this case, at almost 0.5 (it is not significant, though). These estimates, therefore, suggest that the larger states are the primary drivers of the estimated negative effects between the (coincident) NPDB variables and the spending in hospitals. That said, this seems to only be an issue for this model, because in when lagged controls are used, as in column (2), the estimate for adverse actions again becomes large in magnitude and negative at nearly -1.5 hundredths of a percent.

Other notable changes when dropping the large states are seen in the prescription drugs and placebo categories. For prescription drugs, the estimates for both NPDB variables are much more negative than when the large states are included, implying that for this category of spending (and in contrast to hospital care) the negative association between the NPDB variables and spending is stronger among the smaller states. In the placebo category, other non-durable medical products, the estimated effects change signs as compared to when all states are used, implying that the NPDB variables and spending have correlations with opposite signs between the larger and smaller states. Overall, then, these results show that there is some heterogeneity in effects between big and small states. That said, the patterns observed here end up telling essentially the same story as when all states are included.

The estimated effects of adverse actions always negative, and usually large in magnitude. Comparatively speaking, estimates for malpractice payments are much smaller. Additionally, coefficients for the placebo category are usually smaller, and are particularly so when comparing models with lagged controls.

2.4.1 Checking for endogeneity of the NPDB variables

Table 4 presents results for the estimation of a version of equation (1) that includes one- and two-period-forward leads, as well as one- and two-period-back lags for the NPDB variables (in addition to the coincidently timed versions of those variables). For each category of spending, the table presents two regressions. The first, "Model 1" includes only the listed variables and the year and state fixed effects and state time trends. The second, "Model 2", includes all the additional (coincidently timed) control variables that were included in Models 3 and 4 of Table 3. These regressions are intended to address the possibility that the malpractice risk environment itself is a response to unobserved conditions in the state. If the NPDB variables are *not* endogenous, then one would expect that their lead values would estimate as insignificantly different from zero, and estimates of the coincident and lag versions would be similar to when the models are estimated without leads.[17] Large and statistically significant estimates, on the other hand, would be indications that the estimates are influenced by unobserved factors or trends.

Reviewing the results of these regressions, the estimates for adverse actions stand out. For nearly every spending category, the coefficients for the coincident (t) and lag periods ($t-1$ and $t-2$) are relatively large in magnitude and statistically significant. Additionally, all of these estimates are negative and have magnitudes that are similar to those seen for adverse actions in Table 3 (especially for the lag periods). For the lead periods ($t+1$ and $t+2$), however, the estimates are relatively small in magnitude and not statistically significant. At the same time, the estimates for other non-durable medical products are all

[17] Two notable previous uses of this type of strategy to address endogeneity can be found in Gruber and Hanratty (1995) and Friedberg (1998).

comparatively small in magnitude, not significant, and (in contrast to the other categories) positively signed. Thus, these results are consistent with an environment where endogeneity is not an issue: there is no indication of effects flowing backwards in time, and the placebo category has no indication of effects (or, if they are there, they are not consistent with a negative bias and change in size at period $t+1$ that would explain the results for the other categories).

Turning next to the malpractice payments coefficients, we see results that are dramatically different. Estimates here are nearly all small and not significant, and both positive and negative signs are present. These results hold for all spending categories, including the placebo. This overall pattern is consistent with the results for the models without leads and lags. However, there are two important issues. One is that for prescription drugs, the two-period-lead coefficient in the first model (column 5) is statistically significant at the 10% level and large in magnitude. While this result would be consistent with an endogeneity problem, it does not survive the inclusion of control variables, as seen in column (6). The second issue, however, seen in the physician and clinical services category, does not resolve via the inclusion of additional controls. As columns (3) and (4) show, large and significant estimates for the one-period-lead coefficient are present in both models. Additionally, though it is not significant, the two-period-lead coefficient for the second model is similar in magnitude to the $t+1$ period estimate (both of which are much larger than the lag period estimates). This issue is even seen, to a lesser degree, in the estimates for adverse actions in this spending category: the (insignificant) lead period estimates in column (4) are similar in magnitude to the (significant) $t+1$ period malpractice payment estimates.

Considering the results of Table 4 as a whole, I find no sign of an endogeneity problem for the hospital care and prescription drug categories. However, for physician and clinical services, there is some evidence suggestive of endogeneity that has not eliminated by the econometric models. As already discussed above, this endogeneity might be due to the unobserved quality of the health care system in the state. Said another way, it might be the

unobserved true malpractice rate that is correlated with the NPDB variables. The analysis contained in the next table attempts to address this possibility more directly.

Table 5 contains similar leads-lags regression model estimates that include an additional NPDB variable, "MDs with both", which counts the number of physicians in a state and year who had both an adverse action and a malpractice payment.[18] These models also include all other control variables.[19] The rationale for adding MDs with both is that if a physician received both types of punishment, it probably stems from poor care. That is, these are the cases that are most likely to reflect malpractice. MDs with both, therefore, is intended as a proxy for the malpractice environment. If its inclusion in the regression were to cause significant changes to the results, it would suggest that they are spurious.

The estimated effects for the MDs with both variable (and its leads and lags) are found at the bottom of Table 5. In nearly all cases these coefficients are large in magnitude, particularly for the physician and clinical services and other non-durable medical products categories. The estimates are imprecise, though, as only three are statistically significant (and only at the 10% level). Nevertheless, the size of the estimates would suggest that the added MDs with both controls indeed absorb some portion of the unobserved variation in spending. Despite this, the adverse actions and payments results are largely the same as before MDs with both was included in the model. For hospital care and prescription drugs, we see that adverse actions estimates for periods t, $t-1$, and $t-2$ are large, negative, and statistically significant (except for one case), and the lead periods are small and not significant. Meanwhile, the malpractice payments estimates for these categories are much smaller and insignificant. In both cases, these patterns mimic what was seen in Table 4.

The results for the physician and clinical services category are also similar to those from Table 4. As before, the $t+1$ period estimate for malpractice payments is statistically significant,[20] but in this case, the $t+1$ adverse action coefficient is as well (at the 10% level).

[18] As in the case of the adverse action and malpractice payment variables, each physician is counted at most one time per state and year in the MDs with both variable.

[19] That is, the same controls as the "Model 2" regressions from Table 4.

[20] This result is also reflected in the statistically significant (at the 10% level) estimate for the $t+1$ version

The fact that this is seen despite the inclusion of MDs with both adds to the suspicion that this category is affected by unobserved factors, and that the estimates it produces are biased. That said, the hospital care and prescription drugs categories do not exhibit these symptoms of endogeneity, suggesting estimates for those categories at credible.

3 Conclusion

I have presented evidence suggesting that regulation of physicians results in decreases in overall average health care spending at the state-level. In particular, I find that rising frequencies of adverse actions against physicians are associated with statistically significant spending decreases in the hospital care and prescription drug categories. My estimates for effects of increasing frequencies of malpractice payments are smaller in comparison to the adverse action estimates and, except for one spending category, are statistically insignificant at conventional levels. For the category where I find a significant effect for payments, physician and clinical services, the sign of the estimate is also negative, but there is some evidence of endogeneity affecting the estimates for this category.

Though my estimates for effects of adverse actions are generally statistically significant, their magnitudes are small in comparison to the amount of spending that takes place in each category. For an increase in adverse actions that is the size of the average year-to-year change, the corresponding decrease in spending would only amount to about a quarter of a percent of total category spending. Similarly, for malpractice payments, my estimates are either statistically indistinguishable from zero or negative and small, with physician and clinical services decreasing by a fifth of a percent in response to an average, year-to-year-change-sized increase in payments. Moreover, my estimates are precise enough that large changes in spending do not lie in 95% confidence intervals around them. My results, therefore, contradict the conventional wisdom of the medical field that legal liability risk is an important driver of the high cost of health care. Moreover, to the extent the threats of

of malpractice payments in the combined spending category.

adverse actions and malpractice payments play a role in maintaining quality in the delivery of health care, the regulation of physicians and the malpractice litigation system may actually be net positive contributors to societal welfare, given that I estimate small costs in terms of health care spending.

Another implication of these results arises from the fact that physicians are asymmetrically insured with respect to the direct financial impacts of malpractice payments and adverse actions, but symmetrically *un*insured against the indirect impacts (like reputation effects, stress, and time spent in defense of accusations). Since physicians seem to respond more strongly to adverse action risk, implying that they perceive their total exposure to be greater in the case of adverse actions, either the expected indirect costs of adverse actions are greater than or equal to those of malpractice payments, or the expected direct financial costs of adverse actions are large enough to outweigh the gap in indirect costs when those are higher for malpractice payments. In either case, it raises a question: why have industry oversight groups like state medical boards not received as much ire from physicians as have plaintiff side medical malpractice attorneys?

While there have been notable examples in the literature that have found that legal liability risk increases costs (e.g., Kessler and McClellan, 1996), my results are consistent with other authors who have found small effects and, in particular, Dubay et al. (2001), Kessler et al. (2005), Baicker et al. (2007), and Currie and MacLeod (2008), who found some evidence of negative effects to malpractice risk. Currie and MacLeod (2008) found that caps on malpractice damages were associated with higher rates of C-section procedures, while Kessler et al. (2005) estimated that they resulted in increased physician supply. Measuring risk with malpractice insurance premiums, Dubay et al. (2001) found that risk increases were associated with delays in prenatal care. These findings suggest that physicians avoided riskier patients or practices when risk levels rose. The measures of risk in these papers – damages caps and malpractice premiums – though, are very different from my measures. Baicker et al. (2007), however, do perform an analysis using counts of malpractice payments to measure

risk. Using this to explain use of major medical procedures, they obtain negatively signed estimates in four of nine procedure categories. Of the nine, only one category estimate was statistically significant, and that was back surgery, which was negative. My estimates, therefore, are consistent with these results of these previous papers, but also provide additional evidence regarding the prevalence of this type of effect of risk, as I find it most strongly emanating from adverse actions, a new measure of risk, and find effects on broad measures of health care spending.

There are several mechanisms through which greater fear of punishment or litigation for physicians could result in lower spending levels. Doctors could increase their use of preventative services, leading to fewer cases of more serious (and expensive) illnesses. Alternatively, increased risk could induce physicians to take greater precaution when providing services, leading to fewer errors that require costly additional care. This explanation would be consistent with some findings of Currie and MacLeod (2008), since they found that law changes that increased perceived risk were associated with fewer preventable complications in labor and delivery. Yet another possibility is doctors may reduce their use of risky services that have low marginal value to the health of the patient. This is consistent with both Dubay et al. (2001) and Currie and MacLeod (2008), neither of which found measures of risk to be associated with measures of infant health. More cynically, it is also possible physicians reduce their use of risky, costly procedures even when they have high value to the health of the patient, or simply restrict their supply of all services, as in Kessler et al. (2005). The true explanation could even be a combination of these mechanisms and others. Given the data available for this study, it is not possible for me to distinguish between these mechanisms, but there is clearly room for additional research on this point.

Finally, it is helpful to reiterate that the validity of my analysis in this study rests on several assumptions. One of the most important of these is that the variables of interest, the counts of adverse actions and malpractice payments, accurately measure the risk levels perceived by physicians. Given the federal rules on reporting, the findings of previous

researchers that malpractice payments reports were similar to those of a national insurer (Jena et al., 2011), and the importance of the NPDB in the health care industry, implying it is a quick disseminator of information, I argue that this assumption is reasonable. Another critical presumption of my analysis is that there are no relevant determinants of health care spending that are correlated with my regression control variables and have not been accounted for in my regression models. The unobserved malpractice rate in a state, in particular, is of primary concern here. Like the case of measurement, however, I view this assumption as sensible for the hospital care and prescription drug spending categories, given the above described efforts to confront the issue, like econometric approaches (state and time fixed effects, state-specific linear time trends, numerous time varying control variables), robustness checks (placebo outcome, leads and lags analyses), and arguments about the nature of the NPDB variables. Nevertheless, for both of these assumptions, to the extent that they are, in fact, not correct, then the causal interpretation of my estimates would be undermined. This concern is palpable especially in the face of the evidence uncovered by my analysis in the case of the physician and clinical services category of spending, which suggests potential influence of omitted factors. That said, even if *all* the results I find are biased by such factors, it seems extremely unlikely that the bias is so great such as to undermine the conclusion given above of a small effect on spending of legal liability (either regulatory or litigation based) for physicians. Hence, the conventional wisdom in the medical field, that fear of liability is an important driver of rising health care spending, certainly appears to be wrong.

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Table 1: Types of adverse actions taken against physicians

Adverse Action Type	Record Count	Record Share
Clinical Privileges/Panel Membership: Denial	1,398	2.1%
Clinical Privileges/Panel Membership: Limitation or Restriction	2,555	3.8%
Clinical Privileges/Panel Membership: Revocation	2,826	4.2%
Clinical Privileges/Panel Membership: Surrender	2,615	3.9%
Clinical Privileges/Panel Membership: Suspension	3,749	5.5%
Exclusion from Government Health Care Programs	5,721	8.4%
License: Denial	749	1.1%
License: Fine	1,499	2.2%
License: Limitation or Restriction	2,230	3.3%
License: Other	3,455	5.1%
License: Probation	10,945	16.1%
License: Public Negative Action or Finding	105	0.2%
License: Reprimand or Censure	7,943	11.7%
License: Reprimand, Censure, Surrender of License	6,531	9.6%
License: Revocation	5,526	8.1%
License: Surrender	3,246	4.8%
License: Suspension	10,019	14.8%
Professional Society Membership: Denial	51	0.1%
Professional Society Membership: Limitation or Restriction	131	0.2%
Professional Society Membership: Revocation	270	0.4%
Professional Society Membership: Suspension	121	0.2%
Total Adverse Action Records	67,888	100.0%

Note: Based on the NPDB AACLASS1 through AACLASS5 variables. Some adverse action records have more than one reported type (that is, the physician received more than one punishment), so the sum of the percentages exceeds 100%. Counts exclude records that modify previous actions (such as license reinstatements).

Table 2: Data summary statistics

Dependent variables	Mean	Std.Dev.	Min.	Max.
Real spending (millions of June 2011 dollars)				
Hospital care	11,523	12,556	620	75,931
Physician & clinical services	7,818	9,671	297	67,725
Prescription drugs	3,159	3,745	123	24,588
All three categories above	22,500	25,548	1,040	167,629
Other non-durable medical products	795	925	49	5,044
Log of real spending (log of millions of June 2011 dollars)				
Hospital care	8.8556	1.0191	6.4301	11.2376
Physician & clinical services	8.3948	1.0933	5.6934	11.1232
Prescription drugs	7.4512	1.1617	4.8112	10.1100
All three categories above	9.4958	1.0523	6.9470	12.0295
Other non-durable medical products	6.1145	1.1074	3.8851	8.5259
Independent variables	**Mean**	**Std.Dev.**	**Min.**	**Max.**
Physicians suffering adverse actions	65.993	77.317	0	632
Physicians with malpractice payments	239.82	338.17	10	1,838
Population ('000s)	5,512.9	6,125.8	466.25	36,604
Population share				
Under 50 years old (*omitted from regressions*)	.71949	.032731	.62777	.85862
Age 50 to 64	.15435	.022395	.098393	.21847
Age 65 to 79	.092934	.012898	.036337	.14166
Age 80 or older	.033224	.007173	.006257	.048541
Black race	.11236	.11746	.002811	.66142
Other race (not white or black)	.063266	.098368	.00549	.72429
Male	.49126	.008356	.46803	.52637
Covered by private health insurance	.7175	.065307	.53316	.88503
Covered by Medicare	.13538	.022622	.041999	.20606
Covered by Medicaid	.11262	.035812	.036296	.23282
Covered by military insurance	.041628	.025805	.005982	.21496
Uninsured	.12643	.041681	.040423	.28176
Birth rate (per 1,000)	14.101	1.735	10.1	21.2
Practicing lawyers ('000s)	19.512	26.124	1.093	150.54
Medical industry workers ('000s)	258.51	272.96	13.478	1,600.4
Legal industry workers ('000s)	29.194	38.524	.5507	248.94
State unemployment rate	5.0713	1.3709	2.3	11.308
State gubernatorial election year indicator	.25176	.43428	0	1
State governor is a democrat indicator	.44588	.49736	0	1
Non-Federally Employed M.D. Physicians ('000s)	13.654	16.454	.64	95.906

Note: All variables have 867 observations except the indicators for the state governor party and election years, which both have 850 observations since Washington DC does not have a governor.

Table 3: Results for estimation of variations of equation (1). Dependent variable for each regression is the natural log of the category of spending indicated. All raw estimates were multiplied by 10,000 (so are expressed as hundredths of a percent)

	Hospital Care		Physician & Clinical Services		Prescription Drugs		Combined Spending		Other Non-Durable Medical Products (Placebo Category)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Control variable timing	*Coincident*	*Lagged*	*Coincident*	*Lagged*	*Coincident*	*Lagged*	*Coincident*	*Lagged*	*Coincident*	*Lagged*
Model 1	$n = 867$	$n = 816$	$n = 867$	$n = 816$	$n = 867$	$n = 816$	$n = 867$	$n = 816$	$n = 867$	$n = 816$
Adverse Actions	-1.398**	-1.477***	-1.043*	-1.625**	-0.750	-1.290	-1.199**	-1.577***	0.822	0.654
	(0.694)	(0.480)	(0.549)	(0.682)	(1.428)	(1.127)	(0.570)	(0.506)	(0.864)	(0.805)
Malpractice Payments	0.0424	0.0882	-0.838*	-0.522	0.328	0.0724	-0.216	-0.0918	0.274	0.193
	(0.301)	(0.306)	(0.432)	(0.435)	(0.838)	(0.701)	(0.182)	(0.190)	(1.019)	(0.880)
Model 2	$n = 850$	$n = 800$	$n = 850$	$n = 800$	$n = 850$	$n = 800$	$n = 850$	$n = 800$	$n = 850$	$n = 800$
Adverse Actions	-1.213**	-1.063**	-1.598**	-2.112**	-1.212	-1.608	-1.327**	-1.569***	0.719	0.513
	(0.556)	(0.419)	(0.700)	(0.914)	(1.360)	(1.125)	(0.573)	(0.540)	(0.794)	(0.733)
Malpractice Payments	0.222	0.264	-0.825***	-0.485*	0.249	0.0538	-0.154	-0.0243	-0.127	-0.263
	(0.334)	(0.312)	(0.294)	(0.271)	(0.716)	(0.649)	(0.215)	(0.210)	(1.007)	(0.910)
Model 3	$n = 850$	$n = 800$	$n = 850$	$n = 800$	$n = 850$	$n = 800$	$n = 850$	$n = 800$	$n = 850$	$n = 800$
Adverse Actions	-1.358***	-1.211***	-1.575**	-2.158**	-1.615	-1.930**	-1.426**	-1.695***	0.712	0.524
	(0.483)	(0.404)	(0.757)	(0.949)	(1.077)	(0.852)	(0.542)	(0.469)	(0.855)	(0.813)
Malpractice Payments	0.0286	0.111	-0.816**	-0.564*	-0.237	-0.247	-0.286	-0.162	-0.115	-0.251
	(0.302)	(0.267)	(0.330)	(0.295)	(0.648)	(0.554)	(0.238)	(0.209)	(0.939)	(0.831)
Model 4	$n = 731$	$n = 688$	$n = 731$	$n = 688$	$n = 731$	$n = 688$	$n = 731$	$n = 688$	$n = 731$	$n = 688$
Adverse Actions	-0.0843	-1.459	-0.998	-2.196**	-1.858	-3.710**	-0.557	-1.973***	-0.816	-0.862
	(0.946)	(0.877)	(1.219)	(0.843)	(1.157)	(1.576)	(0.754)	(0.689)	(1.387)	(1.347)
Malpractice Payments	0.457	0.488	-0.575	-0.329	-0.690	-1.032	0.0408	0.0831	0.679	0.0532
	(0.445)	(0.483)	(0.653)	(0.595)	(0.734)	(0.874)	(0.405)	(0.386)	(0.755)	(0.779)

Notes: Standard errors (clustered at state-level) in parentheses. Levels of statistical significance (for two-tailed tests) indicated as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions included year and state fixed effects and state-specific, linear time-trends. Model 1 regressions contained no other control variables. Model 2 included birthrates, population counts, population shares for demographic groups (ages 50 - 64, 65 - 79, and 80 plus; black and other – not black and not white – race; male gender), insurance coverage population shares (private, Medicare, Medicaid, military, none) counts of lawyers and legal industry workers, the unemployment rate, and indicators for democrat governors and gubernatorial election years. Washington DC observations were dropped because of the inclusion of the governor and election year variables. Model 3 added counts for non-federally employed medical doctors and all medical industry workers. Model 4 had the same controls as Model 3 but excluded observations for large states (California, New York, Texas, Florida, Illinois, Pennsylvania, and Ohio). Combined spending is the sum of the first three categories, and excludes the placebo category.

Table 4: Results for estimation of variations of equation (1) that include leads ($t+1$, $t+2$) and lags ($t-1$, $t-2$) of NPDB variables. Dependent variable for each regression is the natural log of the category of spending indicated. All raw estimates were multiplied by 10,000 (so are expressed as hundredths of a percent)

	Hospital Care		Physician & Clinical Services		Prescription Drugs		Combined Spending		Other Non-Durable Medical Products (Placebo Category)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Model 1	*Model 2*	*Model 1*	*Model 2*	*Model 1*	*Model 2*	*Model 1*	*Model 2*	*Model 1*	*Model 2*
Adverse Actions ($t+2$)	-0.391	-0.504	0.0563	-0.501	0.162	-0.294	-0.224	-0.526	0.325	0.636
	(0.366)	(0.432)	(0.547)	(0.637)	(0.571)	(0.666)	(0.258)	(0.314)	(0.400)	(0.560)
Adverse Actions ($t+1$)	-0.332	-0.231	-0.120	-0.767	-0.125	-0.152	-0.235	-0.386	0.0500	0.549
	(0.470)	(0.434)	(0.937)	(0.852)	(0.827)	(0.810)	(0.354)	(0.318)	(0.440)	(0.558)
Adverse Actions (t)	-0.875**	-0.791**	-0.343	-0.795	-0.730	-0.903*	-0.646**	-0.759**	0.308	0.349
	(0.393)	(0.383)	(0.521)	(0.505)	(0.541)	(0.463)	(0.294)	(0.328)	(0.621)	(0.752)
Adverse Actions ($t-1$)	-1.014***	-1.172***	-0.789	-0.908*	-1.602***	-1.973***	-1.059***	-1.180***	0.353	0.415
	(0.369)	(0.354)	(0.548)	(0.517)	(0.546)	(0.522)	(0.294)	(0.268)	(0.701)	(0.724)
Adverse Actions ($t-2$)	-0.907*	-1.320***	-1.169*	-1.144**	-1.519**	-1.960**	-1.186***	-1.355***	0.534	0.704
	(0.472)	(0.488)	(0.621)	(0.441)	(0.719)	(0.842)	(0.277)	(0.267)	(0.677)	(0.702)
Malpractice Payments ($t+2$)	0.330	0.242	-0.297	-0.502	0.949*	0.562	0.189	0.0303	0.257	0.183
	(0.335)	(0.396)	(0.353)	(0.326)	(0.480)	(0.469)	(0.257)	(0.280)	(0.757)	(0.914)
Malpractice Payments ($t+1$)	-0.0892	-0.142	-0.525**	-0.631**	0.283	-0.132	-0.188	-0.323	-0.0673	-0.0985
	(0.240)	(0.324)	(0.223)	(0.290)	(0.398)	(0.372)	(0.174)	(0.210)	(0.515)	(0.640)
Malpractice Payments (t)	0.0563	0.147	-0.343	-0.372	0.00825	-0.164	-0.0789	-0.0739	-0.0237	-0.0873
	(0.234)	(0.268)	(0.292)	(0.321)	(0.382)	(0.368)	(0.144)	(0.183)	(0.430)	(0.569)
Malpractice Payments ($t-1$)	-0.121	-0.0959	-0.177	-0.0721	0.170	0.278	-0.0652	0.00969	0.0655	0.184
	(0.267)	(0.279)	(0.256)	(0.254)	(0.414)	(0.386)	(0.148)	(0.196)	(0.409)	(0.367)
Malpractice Payments ($t-2$)	-0.0143	0.0112	-0.344	-0.198	0.0736	0.145	-0.0973	-0.00324	-0.294	-0.279
	(0.282)	(0.299)	(0.408)	(0.392)	(0.357)	(0.321)	(0.173)	(0.176)	(0.414)	(0.417)
Observations	663	650	663	650	663	650	663	650	663	650

Notes: Standard errors (clustered at state-level) in parentheses. Levels of statistical significance (for two-tailed tests) indicated as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions included year and state fixed effects and state-specific, linear time-trends. Model 1 regressions contained no other control variables. Model 2 included birthrates, population counts, population shares for demographic groups (ages 50 - 64, 65 - 79, and 80 plus; black and other – not black and not white – race; male gender), insurance coverage population shares (private, Medicare, Medicaid, military, none) counts of legal and medical industry workers (lawyers, all legal industry workers, non-federally employed medical doctors, and all medical industry workers), the unemployment rate, and indicators for democrat governors and gubernatorial election years. Washington DC observations were dropped because of the inclusion of the governor and election year variables. Combined spending is the sum of the first three categories, and excludes the placebo category.

Table 5: Results for lead/lag models including controls for the number of MDs with *both* adverse actions and malpractice payments. Dependent variable for each regression is the natural log of the category of spending indicated. All raw estimates were multiplied by 10,000 (so are expressed as hundredths of a percent)

	Hospital Care	Physician & Clinical Services	Prescription Drugs	Combined Spending	Other Non-Durable Medical Products (Placebo Category)
	(1)	(2)	(3)	(4)	(5)
Adverse Actions $(t+2)$	-0.244	-0.994	-0.114	-0.502	0.793
	(0.485)	(0.615)	(0.735)	(0.345)	(0.733)
Adverse Actions $(t+1)$	-0.236	-1.665*	-0.0775	-0.659	1.007
	(0.595)	(0.983)	(1.011)	(0.425)	(0.767)
Adverse Actions (t)	-0.988*	-1.713**	-0.899	-1.153**	0.929
	(0.573)	(0.693)	(0.692)	(0.465)	(1.068)
Adverse Actions $(t-1)$	-1.437***	-1.375**	-2.036**	-1.475***	0.766
	(0.516)	(0.539)	(0.795)	(0.322)	(1.065)
Adverse Actions $(t-2)$	-1.323**	-1.315***	-2.037**	-1.413***	0.931
	(0.524)	(0.442)	(0.951)	(0.278)	(0.890)
Malpractice Payments $(t+2)$	0.320	-0.476	0.595	0.0872	0.140
	(0.369)	(0.314)	(0.511)	(0.248)	(0.928)
Malpractice Payments $(t+1)$	-0.126	-0.750**	-0.112	-0.349*	-0.0586
	(0.316)	(0.314)	(0.423)	(0.198)	(0.675)
Malpractice Payments (t)	0.178	-0.609*	-0.122	-0.123	0.0311
	(0.272)	(0.360)	(0.428)	(0.182)	(0.613)
Malpractice Payments $(t-1)$	-0.136	-0.265	0.284	-0.0696	0.287
	(0.304)	(0.275)	(0.407)	(0.201)	(0.393)
Malpractice Payments $(t-2)$	0.00218	-0.343	0.113	-0.0571	-0.151
	(0.334)	(0.375)	(0.342)	(0.189)	(0.475)
MDs with Both $(t+2)$	-7.305	12.31*	-5.772	-1.348	-4.055
	(5.084)	(6.370)	(7.666)	(3.766)	(9.769)
MDs with Both $(t+1)$	2.294	13.90*	-0.0842	5.562	-6.209
	(5.444)	(7.920)	(9.435)	(4.275)	(9.947)
MDs with Both (t)	2.435	16.61*	1.545	6.894	-13.76
	(6.380)	(8.958)	(11.01)	(4.520)	(10.81)
MDs with Both $(t-1)$	4.676	8.019	-2.576	4.410	-1.759
	(5.938)	(6.638)	(11.15)	(4.380)	(10.29)
MDs with Both $(t-2)$	-0.477	9.535	3.385	3.077	-10.13
	(6.506)	(7.303)	(9.075)	(4.173)	(9.176)

Notes: Standard errors (clustered at state-level) in parentheses. Levels of statistical significance (for two-tailed tests) indicated as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions were run on 650 observations and included all the same unreported fixed effects and control variables as those in Model 3 of Table 3. Combined spending is the sum of the first three categories, and excludes the placebo category.