THE EFFECT OF MEDICAL MALPRACTICE LIABILITY ON THE DELIVERY OF HEALTH CARE

By

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To my ladies, Jackie and Ava

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This study examines the impact of medical malpractice liability on the delivery of health care in the United States. I examine how liability affects three facets of the health care system: physician workforce, physician income and patients' access to care. Chapter 2, "The Effect of Medical Malpractice on Physician Supply," investigates the claim that increased medical malpractice liability has caused shortages of physicians in some states. I use long-difference and panel models to extend the current literature along a number of dimensions. I find evidence that state-level Family/General and Hospital-Based physician workforces respond negatively to increases in the frequency of medical malpractice settlement payments. A one-standard-deviation increase in the number of payments per physician causes a 0.6% to 3.9% decrease in these physician workforces.

Chapter 3, entitled "The Effect of Medical Malpractice Liability on Physicians' Incomes," presents evidence of a compensating wage differential for physicians who face more malpractice risk, which is measured by the frequency of malpractice settlement payments per physician and the median size of those payments. I find that physicians in areas with more frequent lawsuits have incomes that are 1% to 2% higher. Also, larger median settlements increase incomes by 2.5% to 3%. In addition, there is evidence that physicians' incomes net of

malpractice insurance premiums respond negatively to increases in premiums; income falls by 28% to 31% of the increase in premium. This result is consistent with anecdotal evidence that it is difficult for physicians to increase their fees in response to increases in overhead.

In Chapter 4, which is entitled "The Effect of Medical Malpractice Liability on Access to Care," I investigate the hypothesis that individuals' access to health care is diminished in response to increased medical malpractice liability. I find that the median size of medical malpractice settlement payments results in diminished access for the uninsured. Specifically, larger settlement payments increase emergency department utilization, decrease routine care, decrease mammography, increase the likelihood of postponing needed care, and increase travel times.

CHAPTER 1
INTRODUCTION

Many observers and researchers have suggested that the United States is in the midst of a medical malpractice liability crisis, characterized by an increasing number of malpractice lawsuits, larger settlement payments, decreased availability of malpractice liability insurance, deterioration of malpractice insurers' financial positions, and greater premiums for liability insurance. Several important questions emerge when considering how liability affects the delivery of health care: Does medical malpractice liability cause defensive medicine and/or affect physician workforce, physician compensation and access to care? This work adds to the current literature, which examines some of these questions.

Chapter 2 investigates the impact of changes in the size and frequency of medical malpractice settlement payments on state-level physician workforce per capita. Chapter 3 tests the hypothesis that physicians are compensated for facing greater liability risk. Chapter 4 examines whether increased malpractice liability results in diminished access to care. There is almost no existing literature examining the effect of liability on physician compensation and access to care. Chapter 5 concludes.

On the whole, there is evidence that changes in medical malpractice liability affect the delivery of health care in the United States. For some physician types, the frequency of settlement payments per physician attenuates physician workforce per capita. Also, physicians appear to be compensated for bearing more liability risk; at the same time, however, their incomes net of malpractice premiums suffer in response to increases in premiums. Finally, I find that increases in the size of malpractice payments diminish some measures of access to care.

CHAPTER 2
THE EFFECT OF MEDICAL MALPRACTICE LIABILITY ON PHYSICIAN SUPPLY

Introduction

According to the American Medical Association (AMA) and a number of popular press outlets, the United States is currently experiencing a medical liability crisis that has been caused by increasing numbers of malpractice liability cases, coupled with larger malpractice settlements. It is suggested that this increased litigiousness has resulted in an inhospitable climate for physicians: malpractice lawsuits impose costs on physicians; thus, since physicians are now more likely to be sued for malpractice, their costs are higher. Do the higher costs incurred in high liability states lead some doctors to migrate to less litigious states?[1] Another way to consider this question is to ask whether physicians consider states' liability situations in their decisions of where to practice medicine. Alternatively, physicians may choose to retire early or do more administrative work rather than patient care. I find evidence that physician workforce falls in response to increased liability.

For physicians, there are two main costs of increased liability: (1) Direct costs include considerations such as higher malpractice insurance rates in the future, lost work time, and the fact that some patients would choose to avoid physicians whom have been sued for malpractice; and (2) psychic costs, which include suffering the stress of dealing with a lawsuit. Under free entry and exit, the theory of compensating wage differentials predicts that, in the long run, there will be no differences in physicians' profits across states, holding locational amenities fixed. As malpractice costs increase, firms (physicians) will exit the industry until prices increase enough to cover the increases in costs due to increased lawsuits. Since the opportunity cost of exiting the

[1] These issues are discussed in a number of news articles: Dorschner (2007), Solomont (2007),PR Newswire (2007), Editorial Staff, Investor's Business Daily (2006).

industry is high for physicians (equal to their foregone income), it is likely that rather than exiting the industry entirely, physicians might move to a more legally hospitable state.

In considering the research question posed above, it is important to consider cross-state differences that might affect physician supply. One of these issues is differences in state licensing requirements. The Federation of State Medical Boards (2007) explains that the Tenth Amendment of the United States Constitution empowers states to protect the health and safety of their constituents. In response, all 50 states have established agencies that monitor and license physicians and other health care professionals. Licensing requirements are generally similar across states[2]. There are some inconsequential differences among states: some perform a criminal background check or, on occasion, require an in-person interview at the discretion of the board. Since there are no state-specific exams (as there are in the legal professions, for example) and licensing requirements are similar across states (the USMLE (United States Medical Licensing Exam) simply reports pass or fail; state licensing boards do not consider percentile rank), it is unlikely that state variations in licensing processes draw physicians into some states and away from others. However, once a physician is established in a particular state, the need to obtain a license in a new state may deter movement. Although there are no significant institutional barriers to entry for established physicians, moving across state lines will, almost always, cause a physician in private practice to lose her entire clientele. This is a concern only for physicians in private practice, since full-time hospital employees receive fixed salaries.

The existing literature on the response of physician supply to medical malpractice liability is composed of two types of studies. Most identify the effect of liability through changes in

[2] Prospective licensees must have graduated from an accredited medical school, passed the (non-state-specific) United States Medical Licensing Exam (USMLE), completed post-graduate training and attend continuing medical education courses. There is also a processing fee (approximately $1,000 in total) and an application must be filled out.

liability legislation[3], while two studies use variables that more directly measure the liability environment[4] (the papers employing each strategy are discussed in more detail below). While the first type of research, which employs an event study strategy, finds that the enactment of measures such as damage caps results in increases in physician workforce, only one of the two papers that use the second strategy finds an effect, and this effect holds only for a specific subset of physicians, those at the beginning or end of their careers.

There are advantages and disadvantages to each of the two approaches. While the cap studies are able to take advantage of a well-defined, discrete event (the enactment of damage caps or other tort reforms), there is also a concern about policy endogeneity. For example, the AMA actively lobbies for tort reform at the state level and the strength of such a lobby is probably positively related to the per capita size of a state's physician workforce. This would generate a positive bias in the coefficient of interest, since one would expect to observe tort reform in states with large physician workforces and, thus, stronger lobbies. The studies discussed below take measures to deal with this policy endogeneity problem.

In the studies using direct measures of liability (i.e., size and frequency of malpractice award payments), the primary concern is endogeneity of the variables of interest. For bias to occur, it would have to be the case that both the size of the physician workforce and the liability measures react to a third omitted variable. In order to avoid omitted variables bias, I control for a variety of factors that affect physician workforce, including demand for health services, desirability of location, and previous trend in workforce growth. Also, I control for any state-specific, time invariant unobservables as well as unobservable variables that affect all states in a particular year.

[3] See Matsa (2007), Encinosa and Hellinger (2005), Klick and Stratmann (2007), Kessler, Sage and Becker (2005).
[4] See Danzon, Pauly and Kington (1990) and Baicker and Chandra (2005).

An advantage to using direct liability measures rather than tort reform legislation involves interpretation of the estimated effects. In models where tort reform legislation is the variable of interest, the estimates give the effect of the *legislation* on workforce. How the legislation achieves the result is not entirely clear. While it is plausible to assume that tort reform reduces liability and that this reduction causes physician workforce to increase, it might also be the case that part of the effect of tort reform on physician supply works through a different channel. For example, physicians might perceive a state with damage caps as more doctor-friendly in general, or as favoring physicians over lawyers. Estimates produced by models using direct measures of liability, however, give estimates of how changes in *liability* affect workforce. While it is important to analyze whether a particular *policy* affects workforce (tort reform), it is also important to know how a more proximate variable (size and frequency of settlements) affects physician supply, since policymakers are not restricted to tort reform in their efforts to maintain appropriate numbers of doctors per capita. The question of malpractice liability's effects on physician workforce is very important because if malpractice liability does reduce the number of physicians in a state, then it might be appropriate for state governments to enact policies that reduce the size and/or frequency of medical malpractice lawsuits in order to avoid the diminution of the physician workforce. If, however, it is not the number or size of payments that has driven physician shortages, then these policies will likely be ineffective. Whether the relevant question of interest is "Does state X have noneconomic damage caps?" or "How frequently are doctors sued in state X and how big are the settlements?" is debatable. If it is the latter, then the studies using direct liability measures may be more appropriate than the event study approach.

The present research uses direct measures of liability (size and frequency of malpractice settlement payments) and thus falls into the second category of literature. I find evidence that

workforce responds to liability. My results are very similar to those presented in the event studies, but tend to be more statistically significant than the results found in the other work that employs direct measures of liability. My empirical analysis improves upon the research done in the other two papers employing direct measures of liability in several ways. I use more complete data on physician workforce. Additionally, I employ both panel models and long-differences (Danzon, Pauly and Kington use a much shorter panel and Baicker and Chandra use only long-difference models). I also improve upon Baicker and Chandra's use of long-differences: Rather than using the first and last (or averages of the first three and last three) observations to calculate growth rates of the variables, I construct better, less noisy measures of long-term growth that use all data points in the sample period. Finally, I use expert opinions to allocate different allegation types to the appropriate physician categories.

The advantage of long-difference models is that they explain long-term changes using trends in the independent variables and incorporate information from the entire sample period into each observation; thus, they capture equilibrium reactions. However, using long-differences results in the loss of many data point - all years between the endpoints. This loss of information is not necessary with panel models. The disadvantage of using panel models in this context, however, is that they might place too much precision on the physician location decision process. That is, the panel models force us to specify which time period is relevant in explaining physician workforce in a particular year. Long-differences, however, explain the long-term change in workforce using the long-term changes in the independent variables. The findings in this study that liability measures have a significant impact on physician supply are qualitatively consistent across these two complementary strategies. The statistically significant results

produced by both models are within the range of estimates generated by the previous event study literature.

Previous Literature

Four of the event studies discussed below (Matsa 2007; Encinosa and Hellinger 2005; and Kessler, Sage and Becker 2005; Klick and Stratmann 2005) use difference-in-differences (DD) while one (Klick and Stratmann 2007) uses a triple-differences (DDD) design. The DD studies use the physician workforces in states that never have caps during the sample period as a control group for the workforces in states that do pass cap legislation over the sample period. The results from the DD estimates can be interpreted causally as long as the (regression adjusted) workforces of the two groups behave similarly before the introduction of the caps. Stated differently, there must be no omitted variables that are correlated with both workforce growth and the passage of cap legislation (i.e., passage of tort reform must not be endogenous). The DDD study improves upon the DD investigations by using low-risk specialty physician workforces as a control for high-risk specialty physician workforces within the same state. This technique relaxes the DD assumption that the trajectories of workforces across states must be the same.

Matsa (2007) uses a difference-in-differences strategy and employs state- and county-level data to estimate the effect of damage caps on physician workforce from 1970-2000. He finds that caps increase the workforce of specialist physicians in rural (but not more densely-populated) areas by more than 10%. Similarly, Encinosa and Hellinger (2005) employ difference-in-differences and use county-level data from 1985-2000. The authors find that noneconomic damage caps increase counties' per capita physician workforce by approximately 2.2%. The effect is stronger for rural counties (3.2%). Additionally, Kessler, Sage and Becker (2005) use difference-in-differences and state-level data from 1985-2001. They group tort

reforms into direct (those affecting how much a defendant will have to pay in the event of a judgment) and indirect (affecting whom and when a plaintiff can sue) measures. The authors find that direct reforms increase physician supply per capita by approximately 3%. Klick and Stratmann (2005) combine a difference-in-differences approach with instrumental variables designed to remove the possibility of policy endogeneity. The IV results suggest that, after removing policy endogeneity, the noneconomic damage cap is the only tort reform that has a statistically significant impact on physician workforce per capita. The IV results suggest that caps increase physician workforce by between 10% and 37%, depending on the specification. This is much stronger than the non-IV effect, which is around 2%. Klick and Stratmann's (2007) later paper uses a triple differences design and state-level data from 1980 through 2001 to estimate the effect of tort reform on physician supply. The authors assume that high-risk specialty physicians are treated while low-risk specialties form a contemporaneous within-state control group. The authors find that noneconomic damage caps increase the number of physicians per capita by 6% to 7%, with the effect concentrated among the riskiest specialties. Taken together, the event study research provides evidence that tort reform, particularly caps on noneconomic damages, tend to increase physician supply.

Two papers use direct measures of malpractice liability to examine the effect of lawsuits on physician workforce. Danzon, Pauly and Kington (1990) estimate the effects of the malpractice environment on physicians' fees and workforce using state-level data on claim frequency, average claim size and the malpractice insurance rate charged by the state's largest insurer from 1976, 1978 and 1983. They find that physicians' net incomes do not suffer as a result of increased liability; rather, doctors increase their fees by more than enough to offset

higher liability costs. The authors do not find evidence that physician workforce is affected by changes in the liability environment.

Baicker and Chandra (2005), henceforth BC, test the hypothesis that higher levels of malpractice liability decrease the physician workforce by using a long-run growth approach. BC use the change in the natural logarithms of all variables from 1993 to 2001; thus, their specification uses eight-year growth rates. To calculate the growth rates of their variables over the time period, BC use the change in the natural logs of the variables. For the liability measures, they calculate differences based on three-year averages for 1992-1994 and 2000-2002. For physician workforce growth rates, they use data from 1993 and 2001, where the 1993 data is interpolated using 1989 and 1995 observations. Because the variables are differenced, this long-difference estimation approach is robust to time-invariant state-level unobservables that might otherwise bias coefficient estimates. The evidence in their paper suggests that the workforces of physicians younger than 35 and older than 55 react negatively to increased frequency of payments. The negative effect is also present for older internists and younger obstetrician-gynecologists. When physicians of all ages are grouped together, however, there is no statistically significant effect of liability on workforce per capita. Also, the null hypothesis that all coefficients in the models are jointly zero could not be rejected.

Empirical Approach

Long-Difference Specifications

As I note above, the previous literature has used growth rates calculated by differencing the natural logarithms of the first and last observations (or two- or three-year averages at each endpoint) of the stock variables. Figure 2-1 plots the natural logarithms of the mean and median payment size for all physicians involved in patient care in the five states with the largest populations in 2004 (these plots are typical of most states; other graphs are available upon

request). From the plot, it is apparent that the conventional differencing strategy can introduce noise into the growth rate variables (i.e., simply subtracting the first value from the last value could produce a misleading growth rate). Also, long-differences produced by subtracting endpoints are not robust to changes in the sample period. In order to remedy this problem, for all variables in the long-difference specifications, I use the slope parameter of a regression of the natural log of the variable on year. This strategy uses all years of data to calculate the average annual growth rate over the sample period, rather than just depending upon the endpoints for an accurate depiction of the growth rate of the variable.

I first estimate the following long-differences model:

$$\Delta\log(MDs)_i = \beta_0 + \beta_1 * \Delta\log(\#\ payments/MD)_i + \beta_2 * \Delta\log(payment\ size)_i + \beta_3 * \Delta\log(neighbor\ \#\ payments/MD)_i + \beta_4 * \Delta\log(neighbor\ payment\ size)_i + \beta_5 * \Delta\log(income)_i + \beta_6 * \Delta\log(elderly)_i + \beta_7 * \Delta\log(population)_i + \beta_8 * \Delta\log(insured)_i + \beta_9 * \Delta\log(HMO)_i + \beta_{10} * \Delta\log(pupil/teacher)_i + \beta_{11} * \Delta\log(prior\ physician\ workforce)_i + \varepsilon_i \quad (2\text{-}1)$$

In the long-differences model (Equation 2-1), *# payments/MD* is the number of settlement payments per physician, *payment size* is either real mean or real median malpractice payment size, *neighbor # payments/MD* is the number of payments per physician for the composite neighbor of state *i*, *neighbor payment size* is the real mean or real median payment size for the composite neighbor of state *i*, *income* is real per capita personal income, *elderly* is the proportion of the state population 65 years of age or older, *population* is lagged[5] total population, *insured* is the proportion of individuals in the state who have private health insurance, *HMO* is the HMO (health maintenance organization) penetration rate, *pupil/teacher* is the pupil-teacher ratio, and *prior physician workforce* is the extrapolated value for *MDs* based upon data from 1978, 1985

[5] Population is lagged to avoid endogeneity concerns. The rationale for having population in the model is discussed later.

and 1991 (so that Δlog(*prior physician workforce*) is the growth rate of *MDs* based upon 1978, 1985, and 1991 data).

I expect the variables capturing a state's malpractice liability environment (*# payments/MD* and *payment size*) to have negative coefficients, since theory suggests that some physicians would flee the state in response to an increase in malpractice litigation and awards. These are the primary hypotheses of this work.

The composite neighbor variables are defined as population-weighted averages of state *i*'s contiguous neighbors' payments per physician and payment sizes. Physicians are likely to consider neighboring states in their decisions of where to practice. Thus, it is important that the neighbor variables be included in the model. The hypothesized signs for the neighbor variables are positive: As a measure of neighbors' litigiousness decreases, the workforce in the state under study should also decrease as physicians choose to move into nearby states where they are less likely to be sued for malpractice; low liability states are thus hypothesized to "steal" physicians from nearby high liability states. However, it is possible that the neighbor variables could have negative signs. If states in the same area tend to move together in terms of liability measures, the neighbor variables could act as proxies for the number and size of payments variables; in this case, the neighbor liability variables could appear to have negative effects on physician workforce because of collinearity.[6]

Per capita income, elderly population, and proportion insured privately control for demand for medical services. I expect income to have a positive coefficient, since health services are

[6] There is some evidence that collinearity exists: the correlation between state *i*'s payments per physician and *i*'s composite neighbor's payments per physician is 0.6143; the analogous correlations for mean and median size of payments are smaller, 0.2273 and 0.4120. These correlation coefficients are 60% to 90% larger than the correlations produced when state *i* is randomly matched (within years and physician types) with some composite neighbor; the correlation coefficients produced by random matching are 0.3850, 0.1241 and 0.2163 for number of payment, mean size and median size, respectively.

normal goods. Also, the proportion of the state population over 65 years should have a positive effect on physician workforce, since older individuals consume more health services. The proportion insured privately should also have a positive coefficient, since individuals with medical insurance will likely demand more medical services than those without insurance. I expect the pupil-teacher ratio to have a negative effect on physician workforce, since an increase in the ratio signals lower school quality, and physicians are likely to consider school quality in deciding where to settle. The lagged state population is included in the model because physicians will likely respond to population change with a lag. I expect the HMO penetration rate to have a negative sign, since higher HMO penetration may be associated with reduced workforce for some physician categories (Escarce et al. 1999). The prior physician growth variable is designed to control for the pre-existing trend in physician workforce growth.

Because of data constraints, I am not able to include the load factor (which is equal to malpractice premium divided by payouts by malpractice insurance companies). This variable serves to relate total payments by insurance companies to insurance premiums. The omission of the load factor is likely not very serious since the main component of other income for insurance companies is investment income, which is derived from investing premiums in (mostly) conservative financial instruments. Since all insurance companies, regardless of where in the United States they operate, invest in the same financial market, the time dummy variables I use in the panel specifications control for common shocks to investment income and thus help to control for changes in insurance premiums. Similarly, in the long-difference specifications, these state-invariant shocks are captured by the constant term. Consistent with this explanation, Baicker and Chandra found that the load factor coefficient was not statistically significant.

The long-differences model (Equation 2-1) is estimated separately for each of the six physician workforce categories. In addition to estimating separate regressions for each physician type, I also "stack" the data, which produces five observations (one for each included physician category) per state per year. I use this data structure to estimate a version of the long-differences model (Equation 2-1), which also includes dummy variables for physician category and interactions of physician category with the liability variables. Analogous to the "unstacked" model, the stacked long-difference model is robust to state and physician type time-invariant unobservables.

Panel Specifications

The data set I use in this study enables me to use panel as well as long-difference specifications. The physician workforce data are available for all states in all years from 1992-2004, except for 1994.[7] I estimate the following state fixed-effects model:

$$MDs_{it} = \beta_i + \beta_t + \beta_1 *(\#\ payments/MD)_{it} + \beta_2 *payment\ size_{it} + \beta_3 *(neighbor\ \#\ payments/MD)_{it} + \beta_4 *neighbor\ payment\ size_{it} + \beta_5 *income_{it} + \beta_6 *elderly_{it} + \beta_7 *population_{it-1} + \beta_8 *insured_{it} + \beta_9 *HMO_{it} + \beta_{10} *(pupil/teacher)_{it} + \beta_{11} *(prior\ physician\ workforce)_{it} + \varepsilon_{it} \quad (2\text{-}2)$$

In the panel model (Equation 2-2), β_i and β_t are state and year fixed-effects, respectively, and the other variables are defined as before, for state *i* and year *t*. Standard errors are clustered by state to control for correlation in error terms within states over time.

Similar to the long-difference models, in addition to estimating the panel model (Equation 2-2) separately for the 5 physician workforce categories plus Total physicians in patient care, I also stack the panel data so that 5 different physician categories are observed in each year, in each state (again, Total physician workforce is excluded since it equals the sum of the other five categories). I then estimate the fixed-effects model (Equation 2-2) with state-

[7] The results presented are based on data for which 1994 workforce was linearly interpolated, but non-interpolated results are very similar.

specialty fixed-effects rather than state fixed-effects. In the stacked model, standard errors are clustered by state-specialty.

Data

All data are state-level and cover the period 1993-2004.

Physician Workforce

Physician workforce data were collected from the American Medical Association's Physician Characteristics and Distribution in the US, which (American Medical Association 2007)

> is the most accurate and complete source for statistical data about the physician supply in the United States...All data are derived from the American Medical Association Physician Masterfile, which obtains data from primary sources only. Primary sources include medical schools, hospitals, medical societies, the National Board of Medical Examiners, state licensing agencies and many others. The stringent verification process is unique and one of the most thorough in the industry.

The AMA tracks physician movement both through physicians' reporting their new addresses as well as through the postal service's address correction system. Many authors, including Baicker and Chandra, have used workforce data are from the Area Resource File (ARF), which is also derived from the AMA's physician Masterfile, but is missing several years of data.

The physician workforce categories I use in this work are (1) Total physicians in patient care (this category includes office- and hospital-based physicians, but not those exclusively involved in administration, teaching or research), (2) Family/General practice (including family and general practitioners, geriatricians and sports physicians), (3) Medical Specialties (including allergy and immunology, cardiovascular disease, dermatology, gastroenterology, internal medicine, pediatrics, pediatric cardiology, and pulmonary disease), (4) Surgical Specialties (colon/rectal surgery, general surgery, neurological surgery, obstetrics/gynecology,

ophthalmology, orthopedic surgery, otolaryngology, plastic surgery, thoracic surgery, and urological surgery), (5) Other Specialties (aerospace medicine, anesthesiology, child psychiatry, diagnostic radiology, emergency medicine, forensic pathology, general preventive medicine, medical genetics, neurology, nuclear medicine, occupational medicine, psychiatry, public health, physical medicine and rehabilitation, anatomic/clinical pathology, radiology, radiation oncology, other unspecified categories), and (6) Hospital-Based physicians (physicians in residency training (including clinical fellows) and full-time members of hospital staff).

Physician workforce per capita is equal to the AMA physician supply figures divided by state population, which is reported by the U.S. Census Bureau. Descriptive statistics for the physician workforce data are presented in Table 2-1. The physician workforce per capita grew over the sample period for all physician categories.

Malpractice Payments

Malpractice payment data are taken from the National Practitioner Data Bank (NPDB) Public Use File. The NPDB contains data on all disclosable reports regarding malpractice payments and adverse actions (e.g., loss of clinical privileges, professional association membership revocation) against licensed physicians, dentists, and other health care professionals. One criticism of the NPDB is that malpractice settlements that include the dismissal by a hospital or other corporation of at least one health care provider need not be reported. Nevertheless, the NPDB is the most comprehensive database of medical malpractice actions and enables researchers to construct measures of liability at the state level. The version (June 2007) of the NPDB public use file I use in the present study reports information on 419,660 malpractice cases from September 1, 1990 through June 30, 2007. I keep observations associated with medical doctors (MDs) and doctors of osteopathic medicine (DOs) (i.e., settlements involving only nurses, psychologists, pharmacists, and other health care professions

are dropped). Also, in order to use the contiguous composite neighbor variables, I limit the sample to the continental 48 states. If a payment is listed as covering more than one physician, the average payment is used for that record.

The NPDB does not report the specialty of the physician on whose behalf a malpractice payment was made. That is, it is impossible to know, for example, whether a particular settlement was the result of a lawsuit against a surgeon, psychiatrist, internist, etc. However, the NPDB does report the nature of the allegation. Malpractice payments are categorized into eleven possible allegation natures: Diagnosis Related, Anesthesia Related, Surgery Related, Medication Related, IV & Blood Products Related, Obstetrics Related, Treatment Related, Monitoring Related, Equipment/Product Related, Other Miscellaneous, and Behavioral Health Related. Rather than attempt to allocate these types of allegations to the six physician workforce categories, I administered a short questionnaire to 22 physicians. All respondents are attending physicians, and their mean number of years since graduation from medical school is 21.4 years. Respondents matched the eleven allegation natures to each of the physician workforce categories according to what types of allegations they thought were most likely to be leveled against a particular physician type. I then ranked the allegation natures by the frequency with which they were chosen for a particular physician category, and then I allocated the most popular allegation natures accounting for 75% of responses to each physician type. For example, if the four top-ranking allegation natures *a*, *b*, *c* and *d* were matched with hospital-based practitioners by all 22 physicians surveyed (thus accounting for 88 responses), and if there were 118 total responses for hospital-based physicians (so that allegations *a*, *b*, *c* and *d* accounted for 88/118 = 74.5% of responses), then I would allocate only allegations *a*, *b*, *c* and *d* to hospital based-practitioners. The allocations produced by this method are listed in Table 2-9. Although it would be ideal to

match the frequency and size of lawsuits to each particular physician type, data constraints make this impossible. The advantage of surveying physicians is that I am not arbitrarily allocating liability measures to physician types. The allocations I use are intended to capture the liability experiences of the various physician types. Considering the allocations listed in Table 2-9, Family/General Practice physicians and Medical Specialists face the same allegation types. This means that the physicians I surveyed believe that Family doctors and Medical Specialists face similar types of risks. Thus, in the models I estimate, Family physicians and Medical Specialists both have the same liability measures, and differences in their workforce levels generate specialty-specific coefficients. Summary statistics for malpractice payments are displayed in Table 2-1. While the frequency of malpractice payments generally declined over the sample period (except for the category of Other Specialists), the size of payments, as measured by both the mean and median, grew. It is clear that the distribution of payment size is skewed since the median payment is always less than the mean. On the one hand, this might suggest that the median is a better measure of payment size since it is less noisy. However, it is also plausible that physicians' decisions are particularly affected by large payments (those that skew the payment distribution), since those are the payments that are most likely to gain notoriety. I adjust all payments for inflation using the personal consumption expenditures deflator, published by the Bureau of Economic Analysis.

Other Covariates

The HMO penetration rate was calculated using data from the Centers for Medicare and Medicaid Services (CMS). Following Laurence Baker (1997), I proxy for HMO penetration rate using the penetration rate from the Medicare Advantage Program, where enrollees are members of "Medicare HMOs." Data on actual market share of HMOs are very limited, and Baker shows

that the Medicare HMO penetration rate is highly correlated with the overall market penetration rate, and serves as a sufficient proxy for HMO activity.

Per capita income is available from the Bureau of Economic Analysis. The proportion of the state population aged 65 or over, and the proportion of the state population insured privately are available from the Bureau of the Census, and the pupil-teacher ratio is available from the National Center for Education Statistics (NCES) Common Core of Data.

Results

Long-Difference Specifications

I investigate the effect of malpractice liability on physician supply using the models and data described above. The results for the long-difference models (Equation 2-1) are presented in Tables 2-2 and 2-3; a summary of the magnitudes of the statistically significant effects for the variables of interest is presented in Table 2-8. The joint hypothesis that all coefficients are not statistically distinguishable from zero is rejected for all regressions other than for one Medical Specialties regression (see Table 2-2). Also, the models presented here explain the data well relative to models estimated in previous research: the null hypothesis that all coefficients were equal to zero could not be rejected in the models estimated by Baicker and Chandra.

The frequency of malpractice payments has a statistically significantly negative effect on the Family/General and Hospital-Based workforces in both the mean and median specifications. Also, median payment size has a negative effect on Medical Specialist workforce. The results for the long-difference model in which the data are stacked are presented in Table 2-4. The number of malpractice payments per physician has a negative effect on physician workforce for both the excluded category of Family/General and Hospital-Based physicians. Also, median payment size negatively affects Medical Specialist workforce. The coefficients from the long-differences models presented above suggest that one-standard-deviation increases in the number

of settlement payments per physician decrease Family/General physician workforce per capita by 3.2% to 3.8% and Hospital-Based physician workforce by 3.7% to 3.9%. The evidence presented above contrasts with previous findings by Danzon, Pauly and Kington, where neither the frequency nor the size of payments had an effect on the physician workforce. Also, Baicker and Chandra only find evidence that the liability variables affect the workforces of physicians who are younger than 35 or older than 55.

In addition to the primary explanatory variables of interest discussed above, a number of other covariates were statistically significant with the expected signs. Proportion of the population aged 65 or older and income per capita have positive effects on physician workforce in several regressions, and the coefficient for the pupil-teacher ratio is significantly negative in two models. Two counterintuitive results are observed: The coefficient for the proportion of the state population insured privately is consistently negative, and the HMO penetration rate has a positive effect on workforce in two Medical Specialty models.

Panel Specifications

I further investigate the research questions using panel specifications, which increase the sample size by twelve fold and produce more precise estimates. Tables 2-5 and 2-6 display results from estimating the panel models (Equation 2-3) and a summary of the magnitudes of the statistically significant effects is presented in Table 2-8[8]. All regressions are highly statistically significant, and the joint hypothesis that all coefficients are zero is rejected for all panel models. As in the long-differences model, the frequency of malpractice payments has a negative effect on the Family/General and Hospital-Based physician workforces in both the mean and median specifications. Also, both the mean and median payment sizes have negative effects on the

[8] The relevant comparisons for the magnitudes of the long-difference and panel specifications are displayed in Table 2-8.

Surgical Specialist workforce, and the median payment size has a negative effect on the Hospital-Based workforce. Table 2-7 displays results from the models where the data are stacked. For both the mean and median specifications, payment frequency has a negative effect on Medical Specialist and Hospital-Based workforces. Also, the mean and median of payment size negatively affect Surgical Specialty and Hospital-Based workforces while only the median payment size has a negative effect on Family/General Physician workforce. Surprisingly, the size of malpractice payments has an estimated *positive* effect on the number of Medical Specialists per capita. The point estimates from the panel models presented above imply that a one-standard-deviation increase in the number of lawsuits per physician causes a decrease of 1.2% in the 2004 Family/General physician workforce per capita and a decrease of 0.6% to 1.2% in the Hospital-Based workforce. Also, a one standard deviation increase in the size of malpractice payments causes a decrease in the 2004 Surgical Specialist workforce of 0.4% to 1.5%, and a decrease of 0.7% to 1.8% in the Hospital-Based physician workforce. These effects produced by the panel models are smaller than those generated by the long-difference models.

As in the case of the long-difference models, a number of other covariates are statistically significant. Again, as predicted, the coefficients for proportion of the population over 65 and income per capita are frequently positive, and the effect of pupil-teacher ratio is negative in two models. Again, the puzzling results that the coefficient for the proportion privately insured is negative, and that the coefficient for the HMO penetration rate is positive for two Medical Specialist regressions are present.

The larger sample size in the panel models also enables more precise coefficient estimates for the composite neighbor variables. The coefficient for the neighbor's number of malpractice payments is negative for Family/General Practitioners in both the mean and median non-stacked

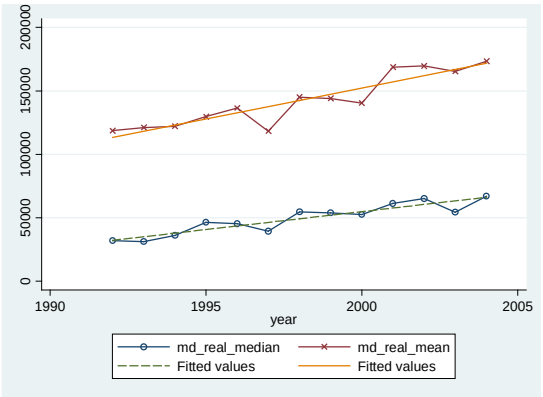
specifications. Also, there is evidence from the stacked models that the neighbor's payment frequency negatively impacts Medical Specialist, Other Specialist, and Hospital-Based workforces. The negative signs on the neighbor variables run counter to the hypothesized signs; this inconsistency may be due to high correlation in the neighbor and own-state liability variables.

Conclusions

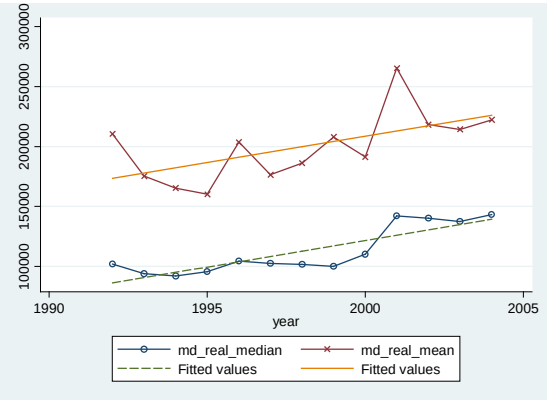
This study tests the hypothesis that higher malpractice liability costs negatively affect the size of the physician workforce. I have presented evidence suggesting that this hypothesis is true for the frequency of malpractice settlements for two physician specialty categories. In a variety of specifications, including long-differences and panel models with fixed-effects, both of which include variables to control for pre-existing trends in the growth of the physician workforce in each state, the frequency of malpractice award payments has a negative effect on the Family/General and Hospital-Based physician workforces per capita. Estimates suggest that an increase of one standard deviation in the number of payments per physician causes a decrease of 0.6% to 3.9% in these physician workforce categories, depending on the model's specification. The estimates of the effect of payment size on physician workforce are less robust. The coefficients sometimes indicate that median payment size affects the Medical Specialist, Surgical Specialist, and Hospital-Based workforces, but payment size does not have a consistently statistically significant negative effect on workforce.

In recent years, some have voiced concern over the availability of primary care physician services resulting from a declining physician workforce. (Lambert 2008; Boston Globe Editorial Staff 2007) The evidence presented in this work and elsewhere suggests that medical malpractice liability, particularly the frequency of settlement payments, may be one factor contributing to this perceived shortage at the state level. State policymakers whose goals include

increasing the number of Family/General practitioners in their state might consider policies intended to reduce the frequency of malpractice payments. Such policies might include tort reform such as noneconomic damage caps, adjustments to the way in which attorneys are reimbursed for malpractice litigation (e.g., contingency versus hourly fees), and policies that will reduce the frequency of non-meritorious negligence cases.



A



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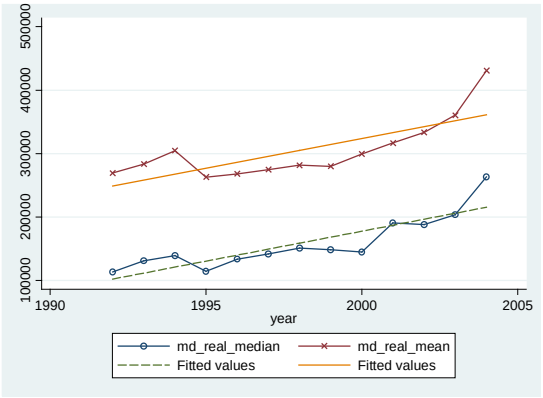

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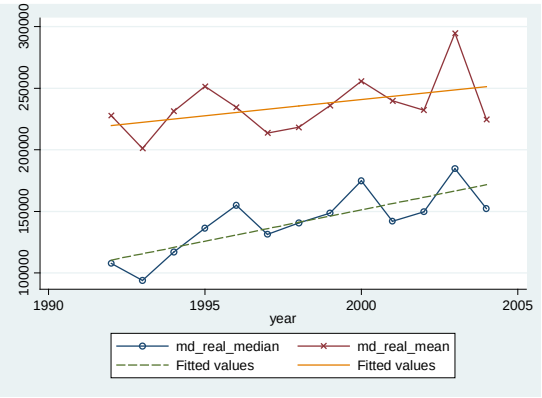
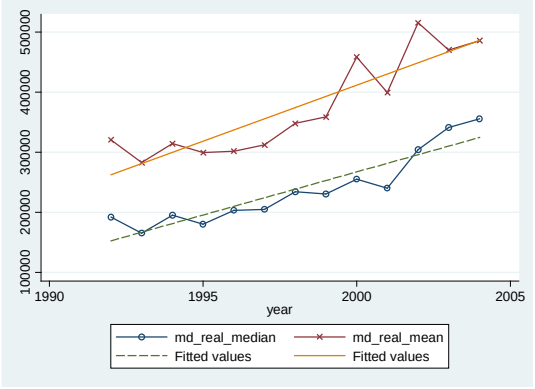


D



E

Figure 2-1. Median and mean malpractice settlement payment size. A) California. B) Texas. C) New York. D) Florida. E) Illinois.

Table 2-1. Descriptive statistics

	Mean	Std. Dev.	Min	Max	Growth rate, 1992-2004
Workforce per 10,000 population					
Total	19.0	4.3	12.4	31.3	17.7
Family/General Practice	2.4	0.7	1.3	3.8	14.2
Medical Specialties	4.5	1.5	2.4	8.4	34.4
Surgical Specialists	4.0	0.6	3.0	5.4	6.9
Other Specialists	3.7	0.8	2.2	6.0	20.0
Hospital-based Physicians	4.4	2.2	1.0	11.3	9.4
Number of payments per 100 physicians					
Total	2.5	1.0	0.7	5.4	-21.9
Family/General Practice	12.8	9.1	1.9	44.1	-15.2
Medical Specialties	6.2	3.0	1.0	16.5	-35.4
Surgical Specialists	10.6	4.6	2.5	26.2	-10.9
Other Specialists	13.0	5.5	4.0	29.8	30.3
Hospital-based Physicians	12.4	9.3	3.0	62.0	-24.2
Average payment size (real dollars)					
Total	194020	79034	65404	425332	38.3
Family/General Practice	195941	78938	69565	402254	32.1
Medical Specialties	195941	78938	69565	402254	32.1
Surgical Specialists	180923	71273	65122	376379	37.5
Other Specialists	194020	79034	65404	425332	38.3
Hospital-based Physicians	192371	77371	68097	431516	37.1
Median payment size (real dollars)					
Total	80340	34862	26223	192307	65.3
Family/General Practice	85348	40594	32051	227272	57.2
Medical Specialties	85348	40594	32051	227272	57.2
Surgical Specialists	73997	32024	17482	168997	65.5
Other Specialists	80340	34862	26223	192307	65.3
Hospital-based Physicians	81585	36107	26223	203962	65.3

Mean, standard deviation, minimum and maximum are based on 1992 levels. Growth rates are equal to the growth rates from the regressions of the natural logarithm of each variable on year multiplied by 12, since there are 12 years in the sample.

Table 2-2. Long-differences, mean payment size

	Total	Family/ general	Medical specialties	Surgical specialties	Other specialties	Hospital-based
# of payments	0.0163 (0.6551)	-0.0927 (0.0024)	0.0051 (0.8971)	0.0263 (0.5667)	0.0117 (0.7759)	-0.1245 (0.0801)
payment size	-0.0108 (0.7608)	-0.0558 (0.1051)	-0.0227 (0.5874)	-0.0227 (0.5883)	-0.0057 (0.8847)	0.0378 (0.6415)
Neighbor # of pmts	-0.077 (0.2326)	-0.0637 (0.2049)	-0.0356 (0.6251)	-0.0436 (0.5755)	-0.0752 (0.3121)	-0.0007 (0.9951)
Neighbor payment size	-0.064 (0.4382)	-0.0748 (0.2878)	-0.0699 (0.4199)	0.0116 (0.8992)	-0.0684 (0.4649)	-0.04 (0.8211)
Proportion Insured	-0.594 (0.0235)	-0.5217 (0.0462)	-0.281 (0.3692)	-0.5788 (0.0741)	-0.6726 (0.0218)	-0.8822 (0.1173)
HMO penetration	0.014 (0.1773)	0.0058 (0.5850)	0.0256 (0.0553)	0.0174 (0.1827)	0.0117 (0.3106)	0.0185 (0.3952)
Proportion over 65	0.4718 (0.0679)	0.398 (0.1206)	0.5238 (0.0959)	0.3643 (0.2525)	0.3226 (0.2565)	0.9345 (0.0883)
Income per Capita	0.4192 (0.1820)	0.2593 (0.3923)	0.0699 (0.8557)	0.8616 (0.0314)	0.4259 (0.2213)	0.2346 (0.7257)
Population	-0.0753 (0.4866)	-0.0496 (0.6556)	-0.0157 (0.9078)	-0.2203 (0.1112)	-0.179 (0.1435)	-0.1596 (0.4869)
Pupil-teach ratio	-0.1706 (0.4028)	-0.3591 (0.0837)	-0.0766 (0.7580)	-0.1059 (0.6757)	-0.0618 (0.7828)	-0.0249 (0.9535)
Prior phys growth	0.0114 (0.5215)	-0.0135 (0.3381)	0.0112 (0.7177)	0.0355 (0.1603)	0.0123 (0.5177)	0.0268 (0.4345)
Constant	0.0038 (0.6463)	0.0031 (0.6969)	0.0264 (0.0157)	-0.0141 (0.1657)	0.0074 (0.4174)	0.0007 (0.9681)
Adjusted R^2	0.2657	0.4874	0.0136	0.2442	0.175	0.1632
F	2.5457 (0.0170)	5.063 (0.0001)	1.0591 (0.4192)	2.3809 (0.0247)	1.9066 (0.0716)	1.8331 (0.0843)
N	48	48	48	48	48	48

P-values in parentheses.

Table 2-3. Long-differences, median payment size

	Total	Family/ general	Medical specialties	Surgical specialties	Other specialties	Hospital-based
# of payments	0.0138 (0.6992)	-0.1023 (0.0012)	-0.0020 (0.9550)	0.0219 (0.6330)	0.0099 (0.8077)	-0.1211 (0.0908)
payment size	-0.0301 (0.4350)	-0.0260 (0.4370)	-0.1000 (0.0099)	0.0001 (0.9972)	-0.0099 (0.8194)	-0.0249 (0.7629)
Neighbor # of pmts	-0.0754 (0.2220)	-0.0640 (0.2135)	-0.0607 (0.3646)	-0.0444 (0.5607)	-0.0673 (0.3444)	-0.0123 (0.9131)
Neighbor payment size	-0.0508 (0.5203)	-0.0814 (0.3113)	-0.0250 (0.7782)	-0.0446 (0.6323)	-0.0417 (0.6410)	-0.0327 (0.8327)
Proportion Insured	-0.6863 (0.0133)	-0.6023 (0.0328)	-0.4824 (0.1141)	-0.5932 (0.0774)	-0.7185 (0.0209)	-1.0245 (0.0913)
HMO penetration	0.0132 (0.2045)	0.0076 (0.4790)	0.0232 (0.0535)	0.0177 (0.1771)	0.0110 (0.3477)	0.0161 (0.4629)
Proportion over 65	0.4681 (0.0670)	0.4157 (0.1217)	0.3598 (0.2216)	0.4055 (0.2049)	0.3303 (0.2453)	0.8850 (0.1053)
Income per Capita	0.4240 (0.1732)	0.2786 (0.3687)	0.1527 (0.6645)	0.9012 (0.0255)	0.4125 (0.2365)	0.2491 (0.7128)
Population	-0.0403 (0.7184)	-0.0448 (0.7057)	0.1201 (0.3614)	-0.2220 (0.1219)	-0.1624 (0.2056)	-0.1104 (0.6430)
Pupil-teach ratio	-0.1664 (0.4048)	-0.3372 (0.1086)	-0.1677 (0.4622)	-0.0716 (0.7752)	-0.0661 (0.7663)	-0.0510 (0.9049)
Prior phys growth	0.0098 (0.5781)	-0.0123 (0.3912)	0.0111 (0.6969)	0.0346 (0.1735)	0.0111 (0.5612)	0.0245 (0.4734)
Constant	0.0052 (0.5443)	0.0042 (0.6223)	0.0240 (0.0239)	-0.0127 (0.2274)	0.0081 (0.3994)	0.0022 (0.8979)
Adjusted R^2	0.2762	0.4686	0.1792	0.2425	0.1695	0.1601
F	2.6308 (.0141)	4.7680 (.0002)	1.9330 (.0675)	2.3677 (.0254)	1.8718 (.0773)	1.8147 (.0878)
N	48	48	48	48	48	48

P-values in parentheses.

Table 2-4. Long-differences, stacked

	Mean	Median
# of payments	-0.1080	-0.1124
	(.0018)	(.0007)
# of payments, med spec	0.1260	0.1135
	(.0022)	(.0016)
	[.0180]	[.0011]
# of payments, surg spec	0.1237	0.1239
	(.0002)	(.0001)
	[.0157]	[.0115]
# of payments, other spec	0.1157	0.1220
	(.0004)	(.0001)
	[.0078]	[.0095]
# of payments, hosp-based	-0.0263	-0.0202
	(.6957)	(.7685)
	[-.1342]*	[-.13267]
Payment size	-0.0371	-0.0230
	(.1964)	(.4510)
Payment size, med spec	0.0008	-0.0523
	(.9866)	(.2507)
	[-.0362]	[-.0753]**
Payment size, surg spec	0.0067	0.0053
	(.8630)	(.9187)
	[-.0304]	[-.0177]
Payment size, other spec	0.0363	0.0140
	(.3010)	(.7372)
	[-.0008]	[-.0090]
Payment size, hosp-based	0.0737	0.0076
	(.2395)	(.9135)
	[.0366]	[-.0154]
Neighbor # of payments	-0.0395	-0.0397
	(.3780)	(.3784)
Neighbor # of payments, med spec	0.0240	0.0010
	(.7299)	(.9888)
	[-.0155]	[-.0387]
Neighbor # of payments, Surg spec	-0.0135	-0.0198
	(.8039)	(.7287)
	[-.0530]	[-.0595]
Neighbor # of payments, Other spec	-0.0465	-0.0416
	(.4790)	(.5097)
	[-.0860]	[-.0813]
Neighbor # of payments, Hosp-based	0.0082	0.0014
	(.9243)	(.9873)
	[-.03125]	[-.0383]
Adjusted R^2	0.6060	0.6123
F	89.3778	90.9423
	(.0000)	(.0000)
N	240	240

	Mean	Median
Neighbor payment size	-0.0608	-0.0648
	(.3500)	(.4255)
Neighbor payment size, med spec	-0.0376	-0.0459
	(.6951)	(.6273)
	[-.0985]	[-.1107]
Neighbor payment size, surg Spec	0.0859	0.0080
	(.4826)	(.9343)
	[.0250]	[-.0569]
Neighbor payment size, other spec	0.0072	0.0247
	(.9475)	(.7892)
	[-.0537]	[-.0401]
Neighbor payment size, hosp-based	-0.0044	0.0223
	(.9773)	(.8527)
	[-.0652]	[-.0426]
Income per capita	0.3492	0.3836
	(.3210)	(.2958)
Proportion insured	-0.5795	-0.6842
	(.0088)	(.0040)
HMO penetration	0.0161	0.0150
	(.1688)	(.1726)
Proportion over 65	0.5062	0.4930
	(.0350)	(.0447)
Pupil-teacher ratio	-0.1195	-0.1233
	(.5119)	(.4831)
Med spec	0.0196	0.0223
	(.0000)	(.0001)
Surg spec	-0.0069	-0.0049
	(.0431)	(.3824)
Other spec	0.0040	0.0035
	(.2178)	(.4983)
Hosp-based	-0.0058	-0.0052
	(.1988)	(.4407)
Population	-0.1259	-0.0899
	(.2654)	(.4518)
Prior phys growth	0.0120	0.0109
	(.2625)	(.2960)
Constant	0.0032	0.0034
	(.7034)	(.7161)

Omitted category is Family/general. Figures in parentheses are p-values. Figures in square brackets are the sum of the main effect and the interaction. *, **, ***: Sum of the main effect and interaction is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively.

Table 2-5. Panel, mean payment size

	Total	Family/ general	Medical specialties	Surgical specialties	Other specialties	Hospital-based
# of payments	-2.93E-04	-4.26E-05	5.02E-05	-3.50E-05	-2.95E-05	-7.70E-05
	(0.7284)	(0.0028)	(0.3679)	(0.2765)	(0.3817)	(0.0158)
payment size	-5.87E-11	2.22E-12	-9.44E-12	-2.42E-11	-1.50E-11	-8.38E-12
	(0.1306)	(0.7229)	(0.3111)	(0.0122)	(0.1762)	(0.6139)
Neighbor # of pmts	7.63E-04	-8.89E-05	3.35E-04	1.30E-05	-2.66E-05	-1.10E-04
	(0.6323)	(0.0215)	(0.1274)	(0.8113)	(0.7359)	(0.3589)
Neighbor payment size	1.78E-11	-1.35E-11	-6.43E-12	-8.98E-12	1.51E-12	3.00E-11
	(0.8905)	(0.4559)	(0.7868)	(0.6994)	(0.9614)	(0.5632)
Proportion Insured	7.02E-03	1.19E-03	3.20E-04	1.51E-03	1.29E-03	2.24E-03
	(0.0353)	(0.0137)	(0.7472)	(0.0446)	(0.0824)	(0.0685)
HMO penetration	9.87E-09	1.66E-09	5.79E-09	1.84E-09	3.61E-09	-2.56E-09
	(0.1089)	(0.1516)	(0.0449)	(0.3600)	(0.0076)	(0.4207)
Proportion over 65	-4.73E-11	-5.67E-12	-6.59E-12	-1.22E-11	-9.39E-12	-9.94E-12
	(0.0002)	(0.0003)	(0.1096)	(0.0000)	(0.0001)	(0.0316)
Income per Capita	-2.53E-06	-6.74E-07	2.67E-07	-4.87E-07	-1.01E-06	-4.19E-07
	(0.1696)	(0.0291)	(0.6650)	(0.1068)	(0.0267)	(0.6096)
Population	4.75E-07	-5.12E-08	1.21E-06	-3.99E-07	1.14E-07	-6.79E-08
	(0.8273)	(0.7885)	(0.0624)	(0.2159)	(0.7838)	(0.9285)
Pupil-teach ratio	-5.64E-06	-2.70E-06	-6.47E-08	-2.27E-07	7.82E-07	-3.27E-06
	(0.5608)	(0.0883)	(0.9855)	(0.8996)	(0.6708)	(0.3054)
Prior phys growth	-6.70E-03	1.96E-01	6.22E-02	9.36E-02	-1.36E-02	-2.24E-02
	(0.8694)	(0.0225)	(0.6420)	(0.1025)	(0.6108)	(0.5392)
Adjusted R^2	0.8402	0.7421	0.9088	0.5383	0.801	0.5809
F	106.9744	24.8843	68.3948	82.9386	54.0055	39.2262
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)
N	576	576	576	576	576	576

P-values in parentheses.

Table 2-6. Panel, median payment size

	Total	Family/ general	Medical specialties	Surgical specialties	Other specialties	Hospital-based
# of payments	-1.31E-04	-4.35E-05	5.07E-05	-3.38E-05	-2.56E-05	-7.55E-05
	(0.8143)	(0.0020)	(0.3607)	(0.3054)	(0.4351)	(0.0168)
payment size	-1.22E-10	2.26E-13	-2.59E-11	-4.08E-11	-2.41E-11	-5.80E-11
	(0.1370)	(0.9836)	(0.2919)	(0.0291)	(0.2382)	(0.0997)
Neighbor # of pmts	7.10E-04	-8.93E-05	3.30E-04	1.67E-05	-3.17E-05	-1.16E-04
	(0.6419)	(0.0163)	(0.1240)	(0.7494)	(0.6731)	(0.3271)
Neighbor payment size	6.03E-11	-1.63E-11	-6.85E-12	-6.74E-11	5.47E-11	1.57E-11
	(0.8480)	(0.6639)	(0.9056)	(0.2562)	(0.3765)	(0.8845)
Proportion Insured	7.02E-03	1.18E-03	2.85E-04	1.50E-03	1.31E-03	2.17E-03
	(0.0373)	(0.0150)	(0.7720)	(0.0451)	(0.0795)	(0.0825)
HMO penetration	9.92E-09	1.69E-09	5.88E-09	2.21E-09	3.37E-09	-2.30E-09
	(0.1073)	(0.1397)	(0.0427)	(0.2824)	(0.0114)	(0.4536)
Proportion over 65	-4.70E-11	-5.65E-12	-6.65E-12	-1.25E-11	-9.08E-12	-1.00E-11
	(0.0001)	(0.0003)	(0.1036)	0.0000	(0.0003)	(0.0246)
Income per Capita	-2.67E-06	-6.68E-07	2.52E-07	-5.59E-07	-1.03E-06	-4.76E-07
	(0.1412)	(0.0295)	(0.6791)	(0.0519)	(0.0239)	(0.5597)
Population	4.63E-07	-5.60E-08	1.20E-06	-3.87E-07	9.30E-08	-4.23E-08
	(0.8290)	(0.7667)	(0.0623)	(0.2370)	(0.8162)	(0.9554)
Pupil-teach ratio	-5.26E-06	-2.72E-06	-2.00E-08	-2.79E-07	9.67E-07	-3.17E-06
	(0.5782)	(0.0841)	(0.9954)	(0.8726)	(0.5975)	(0.3126)
Prior phys growth	-7.09E-03	1.98E-01	6.27E-02	8.83E-02	-1.25E-02	-2.40E-02
	(0.8612)	(0.0215)	(0.6404)	(0.1117)	(0.6438)	(0.4948)
Adjusted R^2	0.8404	0.7415	0.909	0.5383	0.8017	0.5828
F	65.9284	22.3464	63.0203	56.7355	43.3378	38.3517
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)
N	576	576	576	576	576	576

P-values in parentheses.

Table 2-7. Panel, stacked

	Mean	Median
# of payments	-0.0000	-0.0000
	(.4326)	(.1771)
# of payments, med spec	-0.0003	-0.0001
	(.0285)	(.1460)
	[-.0003]**	[-.0002]*
# of payments, surg spec	-7.87E-06	-0.0000
	(.8671)	(.8096)
	[-.0000]	[-.0000]
# of payments, other spec	-0.0000	-0.0000
	(.2961)	(.5910)
	[-.0000]	[-.0000]
# of payments, hosp-based	-0.0001	-0.0001
	(.0102)	(.0208)
	[-.0002]***	[-.0002]***
Payment size	-1.33E-11	-4.41E-11
	(.3419)	(.0138)
Payment size, med spec	6.45E-11	1.59E-10
	(.0492)	(.0000)
	[5.15e-11]*	[1.14e-10]***
Payment size, surg spec	-4.78E-11	-7.75E-11
	(.0170)	(.0100)
	[-6.11e-11]***	[-1.22e-10]***
Payment size, other spec	5.84E-12	1.56E-11
	(.7343)	(.5626)
	[-7.56e-12]	[-2.88e-11]
Payment size, hosp-based	-5.19E-11	-1.11E-10
	(.1453)	(.0305)
	[-6.51e-11]**	[-1.55e-10]***
Neighbor # of payments	1.92E-06	-0.0000
	(.9709)	(.7872)
Neighbor # of payments, med spec	-0.0017	-0.0009
	(.0000)	(.0000)
	[-.0017]***	[-.0009]***
Neighbor # of payments, Surg spec	-0.0001	-0.0001
	(.4552)	(.4261)
	[-.0001]	[-.0001]
Neighbor # of payments, Other spec	-0.0002	-0.0001
	(.0272)	(.0732)
	[-.0002]**	[-.0002]**
Neighbor # of payments, Hosp-based	-0.0006	-0.0006
	(.0001)	(.0002)
	[-.0006]***	[-.0006]***
Adjusted R^2	0.6080	0.6457
F	39.96	28.76
	(.0000)	(.0000)
N	2880	2880

	Mean	Median
Neighbor payment size	-9.26E-11	-2.15E-10
	(.3500)	(.0024)
Neighbor payment size, med spec	3.75E-10	8.17E-10
	(.0001)	(.0000)
	[2.82e-10]***	[6.01e-10]***
Neighbor payment size, surg Spec	-8.17E-11	-1.45E-10
	(.0946)	(.0338)
	[-1.74e-10]***	[-3.59e-10]***
Neighbor payment size, other spec	1.19E-10	2.36E-10
	(.0086)	(.0018)
	[2.62e-11]	[2.07e-11]
Neighbor payment size, hosp-based	-1.39E-10	-1.14E-10
	(.0980)	(.3154)
	[-2.31e-10]***	[-3.28e-10]***
Income per capita	2.97E-09	3.12E-09
	(.0546)	(.0219)
Proportion insured	-2.61E-07	-3.24E-07
	(.3910)	(.2801)
HMO penetration	3.50E-07	3.14E-07
	(.2583)	(.2616)
Proportion over 65	0.0011	0.0011
	(.0261)	(.0139)
Pupil-teacher ratio	-1.93E-06	-1.74E-06
	(.1396)	(.1657)
Population	-6.87E-12	-7.37E-12
	(.0044)	(.0004)
Prior phys growth	0.0972	0.0723
	(.0940)	(.1571)

Omitted category is Family/general. Figures in parentheses are p-values. Figures in square brackets are the sum of the main effect and the interaction. *, **, ***: Sum of the main effect and interaction is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively.

Table 2-8. Interpretation of coefficient magnitudes for statistically significant variables of interest

Workforce Category	Liability Variable	Effect of a one-standard-deviation increase in the liability variable on the physician workforce per 10,000 people	Effect of a one-standard-deviation increase in the liability variable on the physician workforce per 10,000 people as percent of 2004 workforce
Long-difference specifications			
Data not stacked			
Family/General	Pmts per physician[1]	-0.09	-3.2%
Family/General	Pmts per physician[2]	-0.10	-3.5%
Hospital-Based	Pmts per physician[1]	-0.19	-3.9%
Hospital-Based	Pmts per physician[2]	-0.18	-3.8%
Family/General	Mean pmt size	-0.04	-1.6%
Med Specialties	Median pmt size	-0.17	-2.7%
Data Stacked			
Family/General	Pmts per physician[1]	-0.10	-3.7%
Family/General	Pmts per physician[2]	-0.11	-3.8%
Hospital-Based	Pmts per physician[1]	-0.20	-4.2%
Med Specialties	Median pmt size	-0.13	-2.1%
Panel specifications			
Data not stacked			
Family/General	Pmts per physician[1]	-0.03	-1.2%
Famly/General	Pmts per physician[2]	-0.03	-1.2%
Hospital-Based	Pmts per physician[1]	-0.03	-0.6%
Hospital-Based	Pmts per physician[2]	-0.03	-0.6%
Surgical Specialties	Mean pmt size	-0.02	-0.4%
Surgical Specialties	Median pmt size	-0.02	-0.5%
Hospital-Based	Median pmt size	-0.03	-0.7%

Table 2-8. Continued

Workforce Category	Liability Variable	Effect of a one-standard-deviation increase in the liability variable on the physician workforce per 10,000 people	Effect of a one-standard-deviation increase in the liability variable on the physician workforce per 10,000 people as percent of 2004 workforce
Data Stacked			
Medical Specialties	Pmts per physician [1]	-0.05	-0.8%
Medical Specialties	Pmts per physician [2]	-0.03	-0.4%
Hospital-Based	Pmts per physician [1]	-0.06	-1.2%
Hospital-Based	Pmts per physician [2]	-0.06	-1.2%
Family/General	Median pmt size	-0.03	-1.0%
Surgical Specialties	Mean pmt size	-0.05	-1.1%
Surgical Specialties	Median pmt size	-0.06	-1.5%
Hospital-Based	Mean pmt size	-0.05	-1.0%
Hospital-Based	Median pmt size	-0.09	-1.8%

[1] Mean payment size specification. [2] Median payment size specification.

Table 2-9. Physician categories and allegation natures

Total physicians	Family/general practice	Medical specialties	Surgical specialties	Other specialties	Hospital-based
All allegation natures	Diagnosis	Diagnosis	Diagnosis	All allegation natures	Diagnosis
	Treatment	Treatment	Anesthesia		Surgery
	Medication	Medication	Surgery		Medication
	IV and blood products	IV and blood products	Medication		IV and blood products
	Monitoring	Monitoring	IV and blood products		Obstetrics
	Behavioral health	Behavioral health	Treatment		Treatment
			Equipment/ product		Monitoring

CHAPTER 3
THE EFFECT OF MEDICAL MALPRACTICE LIABILITY ON PHYSICIANS' INCOMES

Introduction

By some accounts, the United States has been experiencing a medical malpractice liability crisis characterized by increasingly large (primarily non-economic) damage awards and higher medical malpractice insurance premiums.[1] These facets of the liability crisis constitute two kinds of costs that physicians face as a result of malpractice liability. Direct costs include higher insurance premiums resulting from increased liability, while psychic costs result from the stress or disutility of being sued for malpractice. This study examines the effects of malpractice liability on physicians' incomes net of malpractice insurance premiums. This is an important question for a number of reasons. If higher malpractice insurance premiums have a negative effect on physicians' incomes net of premiums, then increased malpractice liability reduces the return to the many years of education required to become a physician; this will tend to reduce the physician workforce per capita and would serve to exacerbate the physician shortage to which some in the media have alluded. It might also reduce the overall quality of physicians, as more talented individuals choose other careers with higher rates of return to human capital.

In addition to examining the effect of malpractice insurance premiums on physician income, I also investigate how non-premium measures of liability (i.e., the frequency and size of malpractice awards payments) impact physicians' incomes. This is an important question, since it adds to our understanding of how litigiousness affects costs in our health care system: If physicians receive a compensating differential for malpractice liability, then excessive litigation results in higher health care costs. Other studies have considered defensive medicine (where

[1] See, for example, Dorschner (2007), Solomont (2007), PR Newswire (2007), Editorial Staff, Investor's Business Daily (2006).

physicians supply an inefficiently large amount of medical services in an attempt to avoid the possibility of being sued) as one way in which excessive litigation might introduce higher costs into the health care system. For example, Kessler and McClellan (1996) as well as O'Neill and Hennesy (2005) find that liability-reducing malpractice reforms decrease health care expenditures without compromising quality of care. Also, Dubay, Kaestner and Waidmann (1999) find that increased liability risk causes defensive medicine practices in obstetrics. The present study considers another way in which malpractice liability might affect health care costs. To the extent that physicians are monetarily compensated for bearing increased liability risk, this compensation is another channel through which liability introduces costs into our health care system.

As Rosen (1986) explains, "the theory of equalizing differences refers to observed wage differentials required to equalize the total monetary and nonmonetary advantages or disadvantages among work activities." (p. 641) That is, under free entry and exit, and holding locational and employment amenities (or disamenities) fixed, there will be no differences in physicians' profits across geographical areas in the long run. If the increased risk of being sued for a larger award is a disamenity associated with practicing medicine in a particular area, then theory predicts that physicians will be compensated for bearing this risk. The evidence presented in this work supports this hypothesis; physicians' incomes tend to be higher in labor markets (metropolitan statistical areas) with more liability risk (i.e., larger median awards and a higher number of awards per physician).

Theory also predicts that, all else equal, higher malpractice insurance rates will increase physicians' incomes *gross of malpractice premium expenses*. This compensation will result in no differences across geographical areas in physicians' incomes *net of premiums*. Premiums are

a cost to physicians and thus reduce their profits; if doctors' *gross* incomes are not higher in areas where premiums are higher, then doctors' migratory response to premiums will eventually increase *gross* wages and result in equalization of *net* wages. However, anecdotal evidence suggests that physicians' incomes *net of premiums* have suffered from higher malpractice insurance premiums, since reimbursements by third-party payers are sticky.[2] This stickiness precludes physicians from increasing their fees in response to higher premiums. Krishnan (2006) quotes a Raleigh, NC physician: "It really doesn't matter what we charge; it's a matter of how much we will be reimbursed." Krishnan further explains that even as doctors' overhead increases, their "reimbursements are stagnant or getting smaller." Medicare reimbursement policies are important, since private health insurance companies tend to follow Medicare's reimbursement rates.

I find evidence that higher malpractice insurance premiums have a negative effect on physicians' net of premium incomes. However, the other malpractice liability variables I use, the frequency of lawsuits per physician and the median settlement size, which capture the uninsurable costs of being sued (e.g., disutility or negative effects on reputation), are accompanied by a positive compensating differential.

There is much empirical evidence that compensating wage differentials occur in response to factors such as weather, crime, cost of living, job- or location-related health risks, and other locational and work-related conditions. However, there is only one study examining the impact of medical malpractice liability on health professionals' incomes. Danzon, Pauly and Kington (1990), hereafter DPK, use data from 1976, 1978, and 1983 to estimate the effect of state-level average malpractice claim size, frequency of claims per physician, and malpractice insurance

[2] See Krishnan (2006), Washburn (1998), and Appleby (2000).

premium on physicians' fees per visit. In cross-sectional models where regressions for each year are run separately, malpractice insurance premium, average claim size, and frequency of claims each have a positive effect on physicians' fees per visit. In a panel model in which there are 72 observations (one for each insurance rating territory) for each of the three years of data, malpractice premium has a positive effect on fee per visit, while the other liability variables' coefficients are not statistically distinguishable from zero. The authors state that "the analysis controls for relevant market area characteristics that are expected to affect physicians' fees and incomes," (p. 125) but these variables are not specified in the paper. It appears that physician-level variables, such as experience, are not included in the models. Assuming that factors correlated with both the liability measures and physicians' fees are adequately controlled for, the cross-section and panel models suggest that physicians were able to pass on higher malpractice premium rates to patients and/or insurance companies in the form of higher fees. It is also noteworthy that the sample period employed by DPK predates the introduction of the Resource-Based Relative Value Scale (RBRVS) reimbursement method, which was implemented in 1992. Prior to RBRVS, Medicare (and, consequently, other third-party payers) reimbursed physicians according to physicians' billing (up to the "usual and customary" amount). Thus, pre-RBRVS reimbursement tended to be more flexible in the short-run, enabling physicians to more readily pass on liability costs to third-party payers: Providers billed Medicare, which paid the amount requested by the provider, up to the usual and customary amount. In contrast, the RBRVS bases physician reimbursement on the resource costs necessary to provide a particular service. Resource costs differ across geographical areas and are based upon physician work (accounting for 52% of the total relative value for a particular service), practice expense (44% of relative value), and (since 2000) professional liability insurance expense (4% of relative value). Updates

to the relative value scale are based upon recommendations from the American Medical Association and other professional societies. (American Medical Association 2009)

The present research extends the extant literature along a number of dimensions. As mentioned earlier, there is only one study that seeks to estimate the effects of malpractice liability risk on physicians' wages. This study uses old data that pre-dates the rise of RBRVS. Additionally, the physician data I use enables me to control for many micro-level variables that might otherwise bias the estimated effects of the variables of interest. Also, my liability data enable me to conduct within-state analyses and to control for state-level, unobserved, time variant factors. Finally, the evidence presented in this study provides useful policymaking information regarding the response of physicians' wages to malpractice liability.

Empirical Model

I first describe the two empirical models for which I present detailed regression results later in the paper, followed by an exploration of the sources of variation available in my data and the pros and cons of each model.

My preferred specification is a state-year fixed-effects model that controls for state-level unobservables that may change over the sample period. Identification of the variables of interest (the liability measures) thus comes from inter-MSA variation within state-years. I estimate the following state-year fixed-effects model (standard errors are clustered by state-year):

$$\ln(Y_{izpsmt}) = \beta_1 COUNT_{zsmt} + \beta_2 MEDIAN_{zsmt} + \beta_3 PREMIUM_{zsmt} + \beta_4 INDEX_{sm} + \beta_5 X_{izsmt} + \alpha_z + \delta_p + \gamma_{st} \quad (3\text{-}1)$$

In the state-year fixed-effects model (Equation 3-1), the subscripts and variables are as follows (expected effect of variable, if applicable, is in parentheses):

- i. Individual
- z. Specialty, z = internist, general surgeon, obstetrician-gynecologist

- p. Practice type[3], p = 1, ..., 21
- s. State
- m. Metropolitan statistical area
- t. Year, t = 1995, 1997, 1999, 2003
- $LN(Y_{IZPSMT})$. Natural logarithm of real annual income, net of all expenses
- $COUNT_{ZSMT}$. Number of malpractice payments per physician in MSA (+)
- $MEDIAN_{ZSMT}$. Real median size of malpractice payments in MSA (+)
- $PREMIUM_{ZSMT}$. Real mean malpractice insurance premium in MSA (-)
- $INDEX_{SM}$. Metropolitan area wage index for physicians (+)
- X_{IZSMT} . vector of exogenous individual-level variables:
    - Dummy for doctor of osteopathic (as opposed to allopathic) medicine (?)
    - Dummy for graduation from a medical school outside the United States (?)
    - Dummy for female (?)
    - Years of experience practicing medicine (+)
    - Years of experience squared (-)
    - Dummy for whether the physician has passed the board exam in his/her specialty[4] (+)
    - Natural logarithm of the number of weeks worked in the previous year (+)
    - Natural logarithm of the number of hours spent in medically-related activities during the last complete week of work[5] (+)
    - Proportions of patient care practice revenue coming from Medicare, Medicaid and managed care (-)
    - Number of contracts the practice has with managed care plans (+)
    - Dummy variable for salaried physicians (-)
    - Dummy variable for the possibility of salary adjustments depending on the performance of the physician and/or practice (+)

---

[3] Practice types identified by the CTS survey include Solo practice, two physician practice, group practice, group model HMO, staff model HMO, medical school/university, private hospital-owned, state/local government hospital, state/local government clinic, state/local government other, other insurance, integrated health system, free-standing clinic, physician practice management (PPM), community health center, management services organization (MSO), physician-hospital organization (PHO), locum tenens (temporary positions), independent contractor, employer-based clinic, other.

[4] Physicians need not be boarded to practice medicine legally. However, boarded physicians may have higher incomes, in part because a physician who is not boarded may have difficulty contracting with insurance companies.

[5] I use the natural logarithm of weeks of work and hours of work, rather than the untransformed variables, since Y = (weeks/year)*(hours/week)*(wage/hour) => ln(Y) = ln(weeks/year) + ln(hours/week) + ln(wage/hour).

- α_z . Specialty fixed-effect (+ for general surgeons and obstetrician-gynecologists)
- δ_p . Practice type fixed-effect
- γ_{st} . State-year fixed effect

As discussed in the introduction, the main variables of interest are *COUNT*, *MEDIAN*, and *PREMIUM*. I expect *COUNT* and *MEDIAN* to have positive coefficients, since both the frequency of malpractice payments per physician and the size of those payments are disamenities for which a physician would be compensated. I expect *PREMIUM* to have a negative effect on physician income net of premiums since reimbursements are sticky and thus physicians cannot easily increase fees in response to changes in costs.

According to the MedLinePlus Medical Encyclopedia, doctors of osteopathy (D.O.s) emphasize holistic treatment and manual manipulation of the body. There are no differences in the training of allopathic physicians (M.D.s) and D.O.s during residency, and in recent years, the philosophical and practical gaps between M.D.s and D.O.s have narrowed. The effect of being a D.O. is thus ambiguous: It is possible that patients prefer the better-known allopathic approach and the "M.D." designation, but it is also plausible that these two types of physicians are very close or even perfect substitutes. The expected sign for the coefficient on the foreign graduate dummy variable is also ambiguous: It is possible that foreign graduates have higher salaries if only the most talented foreign graduates train and become licensed in the United States. However, if patients prefer American physicians and/or perceive foreign medical schools as inferior, foreign graduates would tend to have lower incomes. The hypothesized sign for the female indicator is ambiguous; it may be zero or negative. Sasser (2005) finds that female physicians' incomes are lower because of choices (e.g., raising children) that reduce hours worked, while Hoff (2004) finds that female hospitalists are paid less than their male counterparts, despite similar work schedules. Roter et al. (1991) and Langwell (1982) find that

office visits are longer for female physicians than for male physicians; thus, in an environment where physicians are paid according to the number of patients they see, females may earn less even after controlling for work schedule. The models I estimate control for hours and weeks worked. Thus, Sasser's finding suggests that the coefficient for the female dummy variable will be equal to zero while Hoff's evidence implies that the coefficient will be negative. The anticipated effects for proportions of practice revenue coming from Medicare, Medicaid and managed care are all negative, since these payers tend to be less generous in terms of reimbursement; the excluded category includes non-managed care private insurers which tend to be more generous. After controlling for the proportion of income from managed care, I expect the number of managed care contracts to have a positive effect on income since having more managed care contracts means that the physician is a member of more approved provider networks and would thus attract more patients.[6] I expect surgeons and obstetrician-gynecologists to have higher incomes since they have longer residencies (obstetrician-gynecologists train for a minimum of 4 years after graduation from medical school; general surgeons train for 5).

In addition to the state-year fixed-effects model, I also estimate a model with state-year-specialty fixed-effects. Like the state-year model discussed above, the state-year-specialty model controls for state-level unobservables that vary over time. The advantage of the state-year-specialty model is that it allows these unobservables to differentially affect the three specialties I examine. Since the state-year-specialty model is more highly parameterized and restricts identification to within state-year-specialty cells, estimates tend to be less precise.

[6] Without controlling for proportion of revenue from managed care, the number of managed care contracts would likely proxy for the importance of managed care to patient care revenue and thus would likely have a negative coefficient.

$$\ln(Y_{izpsmt}) = \beta_1 COUNT_{zsmt} + \beta_2 MEDIAN_{zsmt} + \beta_3 PREMIUM_{zsmt} + \beta_4 INDEX_{sm} + \beta_5 X_{izsmt} + \delta_p + \varphi_{stz} \quad (3\text{-}2)$$

In the state-year-specialty model (Equation 3-2), φ_{stz} is a state-year-specialty effect and other variables and subscripts are defined above.

The data sources I employ enable me to take advantage of within-state variation in the malpractice liability measures. This is important for a number of reasons. Most obviously, there are likely state-level unobservables, which if not controlled for, might bias the coefficients of interest. Using liability measures at the metropolitan area level enables me to use state-by-year fixed-effects rather than state fixed-effects. I can thus control for state-level unobservables that *are not* time invariant. My identification therefore comes from differences within states and across MSAs in a particular year, holding any legislative, cultural, etc. variables constant. Using state-level liability variables would only allow me to control for state-level unobservables that are time-invariant and would preclude identification within state-years and across MSAs. In addition, there is substantial within-state variation in the frequency and size of malpractice settlement payments, as well as malpractice premiums. My data enable me to take advantage of this variation.

Figures 3-1, 3-2 and 3-3 help to demonstrate the source and importance of within-state variation. The bar labeled "Max payments per physician" ("Min payments per physician") in Figure 3-1 displays the four-year average number of malpractice payments per physician in the MSAs with the maximum (minimum) number of payments for each of the five states with the largest 2000 populations. The bar labeled "Mean payments per physician" uses the four-year average of the mean number of payments per physician in each state. Figures 3-2 and 3-3 do the same for median size of malpractice payments and the malpractice insurance premium, respectively. From these figures, it is evident that using state-level liability and premium data

would result in the loss of important variation that could help to explain the relationship between income and malpractice liability. For example, in Figure 3-1, Texas and Florida have similar levels for "Mean payments per physician," but their minimum and maximum payments per physician are quite different. Similarly, considering Texas and New York in Figure 3-2, using the state's average malpractice payment size would mask significant within-state variation that might otherwise be used to explain physicians' incomes. A similar phenomenon is evident in Figure 3-3: The average malpractice insurance premiums in New York and Florida are similar, when compared to the differences in their maximum and minimum values.

In addition to the state-year fixed-effects model, I also estimate a model where I control for state-year-physician specialty unobservables. In this model, identification comes from variation within state-year-specialty cells. This is important, for example, because in a particular year, a state's legislative environment might affect specialties differentially. For example, if a state's legislature passes a cap limiting damages, it is likely that the size and/or frequency of malpractice settlement payments on behalf of obstetrician-gynecologists will decrease by a larger amount than those of internists; this is simply because obstetrician-gynecologists generally suffer larger and more frequent malpractice awards payments than do internists. The state-year-specialty model controls for this type of unobserved effect. Figure 3-4A presents the average number of payments per physician in California for each sample year (Figure 3-4B does the same for Texas; Figures 3-4C through 3-4F do the same for settlement payment size and malpractice insurance premium, respectively). Figures 3-4A through 3-4F demonstrate that the malpractice variables do indeed behave differently for different specialties for the two largest states by 2000 population. Examining Figure 3-4A, California's ob-gyn payment frequency increased from 1995 to 1997, decreased from 1997 to 1999, and increased again from 1999 to

2003. This pattern is strikingly different from those of internists and general surgeons in the same state over the same period. Internists' payment frequency decreased over each interval; surgeons' payments increased from 1995 to 1997, and then decreased for the rest of the sample period. There are also obvious differences in the size of settlement payments (Figures 3-4C and 3-4D) across the three specialties, within state-year combinations. Figures 3-4E and 3-4F illustrate that there are also differences across specialties in the behavior of malpractice insurance premiums, though they are not as pronounced as the inter-specialty differences in the other two liability measures.

As Figures 3-4A through 3-4F demonstrate, internists, general surgeons, and obstetrician-gynecologists have distinct malpractice liability experiences. This might suggest that their incomes also *respond* differently to given changes in the malpractice variables. It is for this reason that I interact the variables of interest (frequency of payments, median payment size, and malpractice insurance premium) with physician specialty. This allows the estimated coefficients to vary according to specialty.[7]

This study seeks to estimate the effect of malpractice liability on physicians' incomes. Figures 3-1 through 3-3 provide some suggestive evidence that doctors are compensated for bearing more malpractice risk. In Figure 3-1, the line labeled "Income in MSA with max payments per physician" ("Income in MSA with min payments per physician") plots the four-year average incomes in the MSAs with the maximum (minimum) number of payments per physician. Figures 3-2 and 3-3 do the same for the median size of malpractice payments and the

[7] I present interacted regression results for the state-year fixed-effects model. I estimated the interacted model for the state-year-specialty fixed-effects model, but the model is too highly parameterized to detect statistically significant effects.

malpractice insurance premium[8], respectively. In Figures 3-1 and 3-2, it appears that incomes in the MSAs with more liability (i.e., maximum frequency and size of payments) tend to be greater. There is no obvious pattern, however, relating income to malpractice premium. In the fixed-effects models described herein, after controlling for myriad covariates that may be correlated with both income and the variables of interest, I find that the size and frequency of lawsuits do indeed have positive effects on physicians' incomes net of premiums, while the size of the malpractice premium has a negative effect on income net of premiums.

An important identification issue to consider is the exogeneity of the variables of interest. In order for endogeneity bias to occur, it would have to be the case that both physicians' incomes and the liability measures are jointly determined or are otherwise correlated with an omitted variable. In order to avoid omitted variables bias, I control for a variety of individual- and practice-level factors that affect physician income (these variables are discussed above). Also, in my preferred specification, I include state-year fixed-effects, which control for state-level unobservables that vary over time. Thus, an omitted variable must be correlated with both physicians' incomes and the liability measures, and must vary within a state and within a year. For example, omitted variable μ would cause biased results if μ has a different effect on internists in Alabama in 1995 than on surgeons in Alabama in 1995. Bias might also occur if μ affects Birmingham, Alabama in 1995 differently from Montgomery, Alabama in 1995. In the state-year-specialty model, for omitted variable η to cause bias, it must be correlated with both physician income and the variables of interest, and η's effect must vary within a state, in a particular year, in a particular specialty. For example, η's omission would result in bias if η

[8] The income numbers in Figure 3-3 are adjusted for the number and size of malpractice payments. Unadjusted income values produce a similar pattern. In Figures 3-1 and 3-2, income is not adjusted.

affects Birmingham, Alabama internists in 1995 differently from Montgomery, Alabama internists in 1995.

Data

The present study examines the effect of medical malpractice liability on physician income using a number of data sources. Data on physicians' incomes net of all expenses (including malpractice insurance premiums), demographics and practice characteristics are from the restricted versions of four rounds of the Community Tracking Study (CTS) Physician Survey (Center for Studying Health System Change, various years), covering the years 1995, 1997, 1999 and 2003[9]. Unlike the unrestricted version of the data, the restricted version includes information on physicians' locations, which may enable identification of individual physicians. An application (including a data confidentiality agreement, data protection plan, and approval of intended use) is required to obtain the restricted data.

I eliminate any observations where it appears that the attachment to the labor force is weak. This includes observations where income is less than $10,000, weeks worked in the last year are less than 26, and hours worked in the last week are less than 20. Additionally, I eliminate observations where hours worked are greater than 84. Finally, I drop observations from the state of New Jersey since New Jersey is an outlier in terms of the difference between malpractice insurance premiums and expected malpractice payouts; results are very similar when I keep New Jersey in the sample. From Table 3-1, mean income of all physicians in the sample is over $200,000 (2000 dollars) with an average of 13.5 years of experience. Slightly more than half of the sample consists of salaried physicians and 19% of the sample's salaries are adjustable within the current contract period. Only 5% of the physicians are osteopathic physicians, 18% are

[9] There is a fifth round of the CTS Physician Survey, but these data are not yet available.

foreign graduates, 17% are female, and 84% are boarded in their specialties. Physicians in solo or group practices account for approximately two-thirds of the sample, followed by physicians employed by medical schools (10.8%), hospitals (10.3%), and HMOs (4.4%); the remainder are employed by "other." The majority of physicians in the sample are internists (68.2%), followed by general surgeons (23.8%) and obstetrician-gynecologists (8%).

Variables capturing malpractice liability come from the National Practitioner Data Bank (NPDB) and include the number of settlement payments per physician and the median size of payments. The NPDB was created by the Health Care Quality Improvement Act of 1986 in an attempt to improve the quality of health care by enabling (National Practitioner Data Bank 2009)

> state licensing boards, hospitals and other health care entities, and professional societies to identify and discipline those who engage in unprofessional behavior; and to restrict the ability of incompetent physicians, dentists, and other health care practitioners to move from state to state without disclosure or discovery of previous medical malpractice payment and adverse action history.

Certain entities (state licensing boards, hospitals, practitioners requesting information about themselves, boards of medical examiners, and others) can query the NPDB about particular physicians' malpractice histories, but only state-level information is freely available to the public. By law, certain entities (such as malpractice settlement payers, state licensing boards, hospitals, etc.) are required to report adverse actions against providers. Thus, the NPDB contains data on all disclosable reports regarding malpractice payments and adverse actions (e.g., loss of clinical privileges, professional association membership revocation) against licensed physicians, dentists, and other health care professionals. One criticism of the NPDB is that malpractice settlements that include the dismissal by a hospital or other corporation of at least one health care provider need not be reported. Nevertheless, the NPDB is recognized as one of the most comprehensive databases of medical malpractice actions and enables researchers to construct measures of liability at the state level. Because of confidentiality concerns, data at geographical

units finer than the state require a special request; state-level data, however, are available in the NPDB public use file. For the purpose of this study, I obtained MSA-level data from the Division of Practitioner Data Banks at the Health Resources and Services Administration.

The NPDB does not report the type of physician on whose behalf a malpractice payment was made. That is, it is impossible to know, for example, whether a particular settlement was the result of a lawsuit against a surgeon, psychiatrist, internist, etc. However, the NPDB does report the nature of the allegation. Malpractice payments are categorized into eleven possible allegation natures: Diagnosis Related, Anesthesia Related, Surgery Related, Medication Related, IV & Blood Products Related, Obstetrics Related, Treatment Related, Monitoring Related, Equipment/Product Related, Other Miscellaneous, and Behavioral Health Related. To create a means of relating allegations to physician categories, I administered a short questionnaire to 22 physicians. All respondents are attending physicians, and their mean number of years since graduation from medical school is 21.4 years. Respondents matched the eleven allegation natures to each of the physician workforce categories according to which types of allegations they thought were most likely to be leveled against a particular physician type. I then ranked the allegation natures by the frequency with which they were chosen for a particular physician category, and then I allocated the most popular allegation natures accounting for 75% of responses to each physician type. For example, if the four top-ranking allegation natures *a*, *b*, *c* and *d* were matched with hospital-based practitioners by all 22 physicians surveyed (thus accounting for 88 responses), and if there were 118 total responses for hospital-based physicians (so that allegations *a*, *b*, *c* and *d* accounted for 88/118 = 74.5% of responses), then I would allocate only allegations *a*, *b*, *c* and *d* to hospital based-practitioners. The allocations produced by this method are listed in Table 3-8. For the purpose of this study, I use only internists,

surgeons and obstetrician-gynecologists because of malpractice insurance premium data constraints. Although it would be ideal to match the frequency and size of lawsuits to each particular physician type, data constraints make this impossible. The advantage of surveying physicians is that I am not arbitrarily allocating liability measures to physician types. Applying the results of the survey to the NPDB data produces the summary figures in Table 3-1. The average median malpractice payment size is over $160,000 (2000 dollars), and there are 0.22 malpractice payments per physician. Obstetrician-gynecologists tend to have the largest and most frequent malpractice settlement payments, followed by internists and general surgeons. Generally, the insurance premium (which is invested in a conservative portfolio of securities after its collection at the beginning of the policy year) reflects the expected payout by the insurer and administrative expenses. This implies that, unless insurers' investment revenues are particularly strong, the observed malpractice insurance premium should, on average, be greater than the expected payout per physician. For all physician types together and general surgeons, this is indeed the case: For all physician specialties, the median expected payout is $16,778 and the median insurance premium is $18,813.[10] For general surgeons, expected payout is $12,479 and the premium is $26,729. However, this pattern reverses for internists and obstetrician-gynecologists: Internists' expected payout is $14,676 but their premium is only $8,046, and obstetrician-gynecologists' payout and premium are $50,827 and $41,966, respectively. The causes for this pattern may be related to the current malpractice insurance crisis, which started around 2000, that Thorpe (2004) and Mello (2006) describe. Both authors explain that insuring

[10] The premiums reported here are the median premium figures from Table 3-1, multiplied by 1+r, where r is equal to half the annualized yield on 6-month Treasury bills (assuming that the insurance company is able to collect interest on the premiums for half of the policy year). The interest rates are 2.795%, 2.59%, 2.38% and 0.53% for 1995, 1997, 1999, and 2003, respectively. These data are from the Economic Report of the President, Table B-73, available from http://www.gpoaccess.gov/eop/tables08.html (accessed 9/5/08).

medical malpractice involves a large amount of uncertainty, making it difficult for actuaries to set premiums appropriately (i.e., commensurate with payouts). One reason for this uncertainty is the long "tail" inherent in medical malpractice, where the average time between alleged harm and claim settlement is four to five years. This makes it difficult for insurers to accurately estimate their liability in any particular year, resulting in weak relationship between premiums and payouts. Indeed, the "statistical relationship between insurers' claim payments and malpractice premiums is *weakly* positive." (emphasis added) (Mello 2006) Also, in times of stable claims payments by insurers; it is likely that premiums would more closely track payouts. However, both Mello and Thorpe note that over my sample period, claim frequency and severity were increasing. It is possible that these changes further weakened the premium-payout relationship. Related to this uncertainty is the fact that there were lower than expected claims payments in the early-1990s. The unused reserves that insurers put aside to cover claims in these years were thus carried over into subsequent years. Since adding to reserves is an expense, and since insurers did not have to augment reserves as much in the mid- and late-1990s, profits increased. This had a slowing effect on premium increases. Another explanation for expected payouts being greater than premiums is changes in insurers' investment income over the sample period. Higher returns from the mid-1990s through 2000 had a decreasing effect on premiums, while lower returns after 2000 likely resulted in higher premiums. My sample consists of observations from four years (1995, 1997, 1999 and 2003), three of which are in the high-return period.

Physician workforce (which is used to calculate the number of lawsuits per physician) was collected from the American Medical Association's (AMA) publication, Physician Characteristics and Distribution in the US, which (American Medical Association 2007)

> is the most accurate and complete source for statistical data about the physician supply in the United States...All data are derived from the American Medical Association Physician Masterfile, which obtains data from primary sources only. Primary sources include medical schools, hospitals, medical societies, the National Board of Medical Examiners, state licensing agencies and many others. The stringent verification process is unique and one of the most thorough in the industry.

The AMA tracks physician movement both through physicians' reporting their new addresses as well as through the postal service's address correction system.

Medical malpractice insurance rates were collected from the Medical Liability Monitor (MLM). Although the MLM lists insurance rates at the county, metropolitan, or regional level within some states, much of the premium data are only available at the state level. I calculated the average premium for the insurers listed in the MLM. The MLM collects data only for internists, general surgeons, and obstetrician-gynecologists, thus limiting my sample to these specialties. The MLM reports the premium paid by a typical physician in the state or area specified. Although the large majority of the rates reported are for claims-made policies with $1 million/$3 million coverage limits, occasionally different rates are reported. For example, if the most common policy in a particular state is for $1 million/$1 million limits, the MLM reports rates for those limits. Similarly, if a state has a patient compensation fund that covers damages above a certain threshold, the MLM reports the premium for the physician's base coverage, as well as the surcharge used to pay for the compensation fund. In any case, the MLM aims to report representative malpractice insurance premiums.[11] The heterogeneity in the type of malpractice insurance coverage provided will probably not bias the results from the state-year fixed-effects model (Equation 3-1) nor the state-year-specialty model (Equation 3-2). This is because I am controlling for unobservables within each state-year combination. Even if the

[11] Estimating the models after dropping states with premiums other than for simple $1 million/$3 million claims-made coverage without a patient compensation fund reduces precision and, thus, the significance of the results.

compensation fund laws or the most common insurance coverage limits change over time, the state-year fixed-effects model will produce unbiased estimates. From Table 3-1, the average premium across all specialties is $24,538, with obstetrician-gynecologists having the largest average premium ($47,862), followed by general surgeons ($31,056) and internists ($9,756). An index to control for wage differences across metropolitan areas was provided by Dewey and is based upon the work by Dewey and Rojas (2008). This index improves upon existing measures of inter-city wage differentials because it accounts for the fact that occupations located in denser areas within MSAs tend to have higher wages. All data expressed in dollar amounts are deflated to 2000 dollars using the consumer price index.

Results

Table 3-2 presents regression results for estimation of the state-year and state-year-specialty fixed-effects models (Equations 3-1 and 3-2, respectively). The first column displays results from the state-year fixed-effects model while the second column shows results from the state-year-specialty fixed-effects model. In both models, the estimated coefficient for the median payment size variable is positive and significant, implying that an increase of $141,545 (the standard deviation of median payment size) in the median malpractice award results in an increase in income of 2.9%. There is also evidence from the state-year fixed-effects model that the number of malpractice payments has a positive impact on income net of premium. A one-standard deviation increase in the number of payments per physician (0.67) increases income by 0.9%. Also, both models suggest that income net of premium responds negatively to premium; a one-standard deviation increase in the malpractice premium ($22,199) is associated with a decrease in income of 3.1% or $6,242; thus, physicians' incomes net of premiums decrease by approximately 28% of the change in the premium. If incomes displayed full stickiness, then premiums would reduce net income dollar-for-dollar; if incomes were not at all sticky and

adjusted perfectly, then there would be no effect of premium on income.[12] The results presented here provide evidence for the intermediate case, where physicians' net incomes are "moderately" sticky in response to changes in premiums. Perhaps stickiness is mitigated by the fact that physicians are able to forecast, to some extent, changes in premiums before the changes occur; they may be able to incorporate this information into fee negotiations with payers. Also, larger physician groups and institutions such as hospitals, universities, medical schools, and managed care corporations are able to shift revenues across operations and may therefore be able to insulate their physicians' incomes from changes in overhead. Finally, there is anecdotal evidence that physicians who face especially high premiums sometimes "go bare" and choose not to buy malpractice insurance, or self-insure using bonds (see Miille (2002), Clarke (2004), Skidmore (2002), and Boulton (2004)). Nevertheless, on average, physicians in areas with higher malpractice insurance rates tend to have lower net incomes, ceteris paribus.

Table 3-3 presents results for the state-year fixed-effects model (Equation 3-1) where the liability and malpractice insurance premium variables are interacted with physician specialty. This specification permits the effects of the variables of interest to differ across specialties; the flexibility afforded by the interactions is important since different physician types face different levels of risk and may have distinct attitudes on the subject. For example, the incidence of lawsuits resulting in settlement is much higher for obstetrician-gynecologists than for the other two specialties (0.56 lawsuits per ob-gyn, compared to 0.13 for surgeons and 0.14 for internists). Perhaps riskier specialties accept the idea that being sued is something that will most likely happen sometime in the course of a career, and thus require less compensation for bearing an

[12] In the long-run, there is evidence that physicians' incomes adjust fully to changes in the malpractice premium: In models where the premium is lagged by one (e.g., 1995 premium is used to explain 1997 income; 1997 premium explains 1999 income; and 1999 income explains 2003 income) or two periods, the coefficient for the premium is never statistically significantly negative. In fact, it is only statistically distinguishable from zero once, in the case of one lag for internists, and the coefficient is *positive*.

extra “unit” of liability risk. Figure 3-4 (discussed in Section III) illustrate graphically the differences in the behavior of the liability measures for each of the three physician specialties in the sample.

The excluded physician type is internist; the results presented for general surgeons and obstetrician-gynecologists equal the sum of the internist’s coefficient plus the relevant specialty’s estimated coefficient. For example, the figure presented for “Payments per physician (general surgeons)” in for the state-year fixed-effects model is 0.0383, which is equal to the sum of the “Payments per physician” coefficient (0.1293) and the coefficient for number of payments per physician interacted with the general surgeon indicator when it is equal to unity (-0.0911).

The results in Table 3-3 show that the number of payments per physician has a positive effect on income for internists and general surgeons. An increase of 0.15 payments per internist (the standard deviation of the payments per internist variable) results in an increase in income of approximately 2%; the corresponding figure for general surgeons is 1.2%. There is also evidence that internists are compensated for bearing the risk of a larger malpractice settlement: An increase of one standard deviation in payment size causes an increase in internists’ income of 2.7%. The results also show that general surgeons’ incomes suffer in response to increases in insurance premiums; an increase of one standard deviation in general surgeons’ insurance premiums ($18,086) is associated with a 2.2% ($5,637) decrease in income, which is 31% of the change in the premium.

All of the regressions are highly significant, and many of the results for the other variables in the regressions are as predicted. Graduates of foreign medical schools earn 2% to 3% less than graduates of American schools, and female physicians earn 23% less than male physicians, even when I control for amount worked. Salaries peak at about 19 years of practice experience

after graduation from medical school. Boarded physicians are paid a premium of approximately 12% while salaried physicians tend to make 5% to 6% less. The hours worked in the previous week has a positive coefficient, but the number of weeks worked in the previous year has no effect on income.[13] The proportions of patient care income derived from Medicare, Medicaid, and managed care sources negatively impact income. After controlling for the amount of practice income derived from managed care, the number of contracts the physician's practice has with managed care insurers has a positive effect on income. Finally, the two non-internist specialties earn more than internists; surgeons earn approximately 29% to 34% more, while obstetrician-gynecologists earn an extra 26% to 28%.

Further Investigation

In addition to the models described above, I make a number of changes to the specification and the sample to investigate the robustness of my results. Already I have shown in Table 3-2 that preferred state-year results are robust to the inclusion of state-year-specialty fixed-effects in addition to state-year effects.

I also estimate a state fixed-effects model in which I include separate year effects. In addition to variation within state-year and state-year-specialty cells, there is substantial variation in the variables of interest within states over time (this is evident in Figure 3-4). I estimate the following state fixed-effects model, to take advantage of this variation:

$$\ln(Y_{izpsmt}) = \beta_1 COUNT_{zsmt} + \beta_2 MEDIAN_{zsmt} + \beta_3 PREMIUM_{zsmt} + \mu CAP_{st} + \beta_4 INDEX_{sm} + \beta_5 X_{izsmt} + \alpha_z + \delta_p + \gamma_s + \tau_t \quad (3\text{-}3)$$

In the state fixed-effects model (Equation 3-3), CAP_{st} is an indicator variable equal to unity if state *s* had economic, non-economic, or total damage cap legislation in place in year *t*. The cap

[13] Perhaps this unexpected result can be explained by the lack of variation in the weeks worked variable. This is evident in Figure 3-4.

variable is an important control for a state's legislative environment vis-à-vis medical malpractice liability. I expect the coefficient on the cap variable to be negative since, even after controlling for frequency and size of settlements, physicians might perceive protection from high malpractice settlements as an amenity. I also include year fixed-effects, τ_t, to control for unobservables affecting all physicians' incomes in all areas in a particular year. Standard errors are clustered by state. Data on economic, non-economic and total awards caps are from Piette (2007); one quarter of the states in the sample experience a change in damage cap laws over the sample period. The advantage of this model is that it allows identification of the effects of the variables of interest both over time and across MSAs in the same state, rather than just across MSAs within a state in a particular year (as was the case with the state-year fixed-effects model). The results for the variables of interest produced by the state fixed-effects model (Equation 3-3) are presented in column A of Tables 3-4 (non-interacted) and 3-5 (variables of interest interacted with physician specialty). Table 3-4 shows that median payment size has a positive effect on physicians' income while malpractice premium has a negative effect. Also, from Table 3-5, physicians of all three specialties are compensated for higher frequency of lawsuits and while internists are the only specialists compensated for larger settlement magnitude. Internists and obstetrician-gynecologists' incomes suffer in response to higher malpractice insurance premiums. The coefficient on the cap indicator is positive for general surgeons and obstetrician-gynecologists, suggesting that in states with malpractice award caps, surgeons and obstetrician-gynecologists' incomes are, respectively, 5.5% and 6.5% higher than in states without caps. This finding is contrary to my hypothesis that the effect of the cap on income should be negative since having a cap on damage awards is an amenity. Perhaps a selection issue explains this finding: States with caps may have those caps because of their highly litigious environments, which is a

disamenity for physicians. In this scenario, the cap indicator is actually serving as a proxy for any litigiousness not already captured by the other liability variables. Although I cannot interpret the coefficient for the cap variable causally, it is still an important control variable.

Included in the full sample are physicians working in a variety of settings, ranging from solo and group practices to hospitals, universities, or other institutions. It is plausible that the incomes of physicians working in the more traditional solo/group practice setting respond differently to liability risk and malpractice premium costs than do physicians employed by institutions. In order to investigate this possibility, I estimate the state-year and state-year-specialty models for the subsample of physicians in more traditional solo or group practices; the results of these estimations are presented in Columns B and C of Table 3-4 and Column B of Table 3-5. Some differences among the results produced by the full and subsample estimations are evident. For the non-interacted state-year model (Column B, Table 3-4), only the malpractice insurance premium negatively affects physician income; unlike the full sample results, the subsample of group practice physicians shows that neither the frequency of payments nor the size of payments affects income. Also, for the state-year-specialty model, only the median payment size coefficient is (marginally) statistically significantly positive. For the interacted state-year model, however, the coefficient for frequency of payments is statistically significantly positive for two of the three specialties, median payment size has a positive effect for one specialty, and malpractice premium has a negative effect on income for two of the three specialties. Considering the results of the interacted and non-interacted state-year models for the subsample, it appears that the non-interacted results are more sensitive to exclusion of nontraditional practices. This may suggest that the effects of the variables of interest on income may be more heterogeneous across specialties within this subsample.

In my base specifications, I include two indicator variables describing each physician's compensation structure. *SALARIED* is equal to unity if the physician receives a salary, and *ADJUSTABLE_SALARY* is equal to unity if the physician is salaried and if the salary is adjustable within the current contract period based upon the performance of the physician and/or the practice. It is possible that these salary structure variables influence both income and at least one of the liability variables. For example, perhaps the possibility of a bonus causes a physician to induce care (e.g., ordering unnecessary diagnostics) in an effort to increase revenues. This behavior might also have the effect of reducing the likelihood of being sued if, for example, a test that would not have otherwise been ordered happens to detect a disease that would otherwise not have been appreciated. In such a scenario, failing to control for physician compensation structure would result in omitted variables bias. Suppose, however, that, in order to compensate their physicians for bearing greater liability risk, a practice offers the possibility of a bonus. Then the salary structure variables ought not be included in the model. Columns D and E of Table 3-4 and column C of Table 3-5 display results from estimations of the state-year (Equation 3-1) and state-year-specialty (Equation 3-2) models without the variables *SALARIED* and *ADJUSTABLE_SALARY*. The results are essentially unchanged when the salary structure variables are excluded from the model.

A final analysis involves estimating the models with different combinations of the variables of interest. This serves to test the robustness of the results to exclusion of the variables of interest. There is not a high level of correlation among the variables of interest: The correlation between median and frequency is 0.014 and the corresponding figures are 0.465 for premium and frequency, and 0.055 for premium and median. This is consistent with Mello's (2006) observation that size and frequency of payments do not explain much of the variation in

premiums. Tables 3-6 (non-interacted models) and 3-7 (interacted) suggest that the pattern of statistical significance for the coefficients of interest is similar when I drop the variables of interest, one or two at a time. Of the 18 models estimated, only three (columns B, E, and K in Table 3-6) show any differences in the significance of the variables of interest.

Conclusions

This research examines the effects of medical malpractice liability on physicians' incomes. The evidence suggests that physicians in areas experiencing higher median settlement payments have incomes that are approximately 2.5% to 3% higher. Also, physician incomes are approximately 1% to 2% higher for physicians practicing in areas where lawsuits are more frequent. These findings suggest that physicians practicing in higher liability areas are compensated for bearing the risk of higher malpractice awards and a higher likelihood of being sued. In accord with anecdotes about the effect of malpractice insurance premiums on physicians' incomes, I find that higher premiums have a negative effect on physicians' incomes, which fall by approximately 28% to 31% of the amount of a premium increase. This result is consistent with sticky reimbursement rates, which may be caused, at least in part, by the shift away from usual and customary reimbursement and toward RBRVS reimbursement. The results are robust to a number of changes in specification.

This study augments the existing literature in a number of ways. I improve upon the only other study that investigates the response of physicians' incomes to malpractice risk. Additionally, the data employed herein enables MSA-level analyses, thus allowing me to look within states. In addition to controlling for several physician-level factors that may affect income, I control for state-level unobservables that are not necessarily time invariant (e.g., changes in unobserved legislation or insurance regulations). I also investigate how different physician specialties respond to their differing levels of malpractice liability risk.

The evidence presented in this paper sheds light on another mechanism through which medical malpractice litigation can affect the delivery of health care in the United States. Liability generates two countervailing forces that act upon physicians' incomes: Higher malpractice insurance premiums tend to have a decreasing effect on income while greater non-premium liability measures have an increasing effect on income. The reduction in income associated with greater premiums will reduce the return to education for physicians and may result in the deterioration of the physician workforce in both physician quality and number of physicians per capita. Policymakers concerned with these effects might consider enacting policies that maintain the return to education for physicians. This may include, for example, reducing the cost of medical education through subsidies, adjusting physician reimbursement, and/or enacting policies intended to limit the growth of malpractice insurance premiums. The notion that physicians are compensated for bearing liability risk (i.e., the uninsurable costs such as disutility of lawsuits) is a channel through which liability may increase health care costs. Policymakers whose goals include cost containment might consider tort reform policies, such as noneconomic damage caps, aimed at limiting excessive litigation.

Table 3-1. Summary statistics

	Mean	Median	Max	Min	Std. Dev.
Real Income (internists, general surgeons and ob-gyns)	200,852	179,349	436,796	10,249	97,565
Number of malpractice payments per physician[1, 3]	0.22	0.12	12.74	0.01	0.67
Payments per physician (Internists) [1, 3]	0.14	0.10	1.67	0.01	0.15
Payments per physician (General Surgeons) [1, 3]	0.13	0.10	3.53	0.01	0.31
Payments per physician (Obstetrician-Gynecologists) [1, 3]	0.56	0.34	12.74	0.03	1.35
Real median of malpractice pmts (all three specialties)[1, 3]	160,448	138,418	2,467,076	12,153	141,545
Real median of payments (Internists) [1, 3]	165,946	142,477	1,758,610	12,153	150,977
Real median of payments (General Surgeons) [1, 3]	144,738	128,702	683,021	22,547	81,209
Real median of payments (Obstetrician-Gynecologists) [1, 3]	172,072	149,805	2,467,076	23,391	184,373
Real premium (all three specialties)[2, 3]	24,538	18,431	166,482	772	22,199
Real premium (Internists) [2, 3]	9,756	7,882	52,086	772	7,220
Real premium (General Surgeons) [2, 3]	31,056	26,186	133,081	3,362	18,086
Real premium (Obstetrician-Gynecologists) [2, 3]	47,862	41,245	166,482	4,005	25,294
Years of experience	13.5	12	64	0	9.8
Number of weeks worked in previous year	47.6	48	52	26	3.2
Hours of medically-related work in previous week	54.8	55	84	20	13.2
% patient care practice revenue coming from Medicare	35.4	35	100	0	22.7
% patient care practice revenue coming from Medicaid	13.2	10	100	0	14.9
% patient care practice revenue coming from managed care	41.6	40	100	0	26.2
Number of managed care contracts	12.4	10	98	0	14.6
Salaried physician	0.52	1	1	0	0.5
Salary adjustable during contract period	0.19	0	1	0	0.4
Doctor of osteopathy	0.05	0	1	0	0.2
Graduate of foreign medical school	0.18	0	1	0	0.4
Female	0.17	0	1	0	0.4
Boarded	0.84	1	1	0	0.4

Table 3-1. Continued

Number and proportion of physicians by practice type	#	%
Group practice (>2 physicians)	3,687	34.5
Solo/two physician practice	3,403	31.8
Medical School	1,158	10.8
Hospital Based	1,098	10.3
Other	872	8.2
HMO	475	4.4
Number and proportion of physicians by specialty	#	%
Internists	7,291	68.2
General Surgeons	2,549	23.8
Obstetrician-Gynecologists	853	8.0

[1] Variable at the MSA level. [2] Variable at the state, within-state region, or MSA level, depending on data source. [3] Summary measures calculated using one observation from each MSA-year-physician specialty cell.

Table 3-2. No interactions

	State-year fixed-effects	State-year-specialty fixed-effects
Payments per physician	0.0147	0.0118
	(0.0460)	(0.3630)
Median payment size ($100,000s)	0.0206	0.0204
	(0.0280)	(0.0260)
Premium ($1,000s)	-0.0014	-0.0014
	(0.0050)	(0.0230)
Index	-0.0651	-0.0419
	(0.5490)	(0.7540)
Doctor of osteopathic medicine	0.0341	0.0347
	(0.1190)	(0.1110)
Foreign graduate	-0.0215	-0.0234
	(0.1420)	(0.0890)
Female	-0.2323	-0.2327
	(0.0000)	(0.0000)
Experience	0.0268	0.0269
	(0.0000)	(0.0000)
Experience^2	-0.0007	-0.0007
	(0.0000)	(0.0000)
Boarded	0.1233	0.1198
	(0.0000)	(0.0000)
ln(weeks worked in previous year)	-0.0797	-0.0755
	(0.2750)	(0.2670)
ln(hours worked in previous week)	0.3510	0.3469
	(0.0000)	(0.0000)
% Practice income from Medicare	-0.0004	-0.0005
	(0.0370)	(0.0400)
% Practice income from Medicaid	-0.0012	-0.0013
	(0.0010)	(0.0000)
% Practice income from managed care	-0.0006	-0.0006
	(0.0010)	(0.0020)
Number of managed care contracts	0.0019	0.0018
	(0.0000)	(0.0000)
Salaried	-0.0566	-0.0522
	(0.0000)	(0.0000)
Adjustable salary	0.0024	0.0008
	(0.8220)	(0.9420)
General surgeon	0.2939	
	(0.0000)	
Obstetrician-gynecologist	0.2798	
	(0.0000)	
Adj. R^2	0.2695	0.1985
F	199.87	74.96
N	10,660	10,660

Dependent variable is the natural log of real income, net of all expenses (including malpractice insurance premium). Two-tailed p-values in parentheses. Estimated coefficients for practice type and year fixed-effects are omitted for brevity.

Table 3-3. With interactions

	State-year fixed-effects
Payments per physician (internists)*	0.1286 (0.0000)
Payments per physician (general surgeons)*	0.0383 (0.0190)
Payments per physician (ob-gyns)*	0.0137 (0.1240)
Median payment size (internists)* ($100,000s)	0.0176 (0.0400)
Median payment size (general surgeons)* ($100,000s)	-0.0073 (0.7400)
Median payment size (ob-gyns)* ($100,000s)	0.0238 (0.1850)
Premium (internists)* ($1,000s)	-0.0004 (0.3910)
Premium (general surgeons)* ($1,000s)	-0.0012 (0.0730)
Premium (ob-gyns)* ($1,000s)	-0.0006 (0.4580)
Index	-0.3781 (0.0010)
Doctor of osteopathic medicine	0.0326 (0.1360)
Foreign graduate	-0.0278 (0.0610)
Female	-0.2332 (0.0000)
Experience	0.0269 (0.0000)
Experience^2	-0.0007 (0.0000)
Boarded	0.1226 (0.0000)
ln(weeks worked in previous year)	-0.0796 (0.2780)
ln(hours worked in previous week)	0.3529 (0.0000)
% Practice income from Medicare	-0.0005 (0.0270)
% Practice income from Medicaid	-0.0013 (0.0000)
% Practice income from managed care	-0.0006 (0.0020)

Table 3-3. Continued

	State-year fixed-effects
Number of managed care contracts	0.0019 (0.0000)
Salaried	-0.0557 (0.0000)
Adjustable salary	0.0031 (0.7740)
General surgeon	0.3445 (0.0000)
Obstetrician-gynecologist	0.2621 (0.0000)
Adj. R^2	0.2712
F	282.03
N	10,660

Dependent variable is the natural log of real income, net of all expenses (including malpractice insurance premium). *For interacted variables, the omitted specialty is internist. The reported estimates for surgeons and ob-gyns are equal to the sum of the internist coefficient and the interacted coefficient. Two-tailed p-values in parentheses. Estimated coefficients for practice type and year fixed-effects are omitted for brevity.

Table 3-4. Further investigation, no interactions

	(A) State and year fixed-effects	(B) State-year fixed-effects, excluding institutional physicians	(C) State-year-specialty fixed-effects, excluding institutional physicians	(D) State-year fixed-effects, excluding salary variables	(E) State-year-specialty fixed-effects, excluding salary variables
Payments per physician	0.0147	0.0162	0.0055	0.0147	0.0124
	(0.1460)	(0.2520)	(0.7760)	(0.0490)	(0.3370)
Median payment size ($100,000s)	0.0145	0.0195	0.0216	0.0196	0.0194
	(0.0940)	(0.1450)	(0.1080)	(0.0360)	(0.0340)
Premium ($1,000s)	-0.0010	-0.0012	-0.0009	-0.0014	-0.0015
	(0.0390)	(0.0270)	(0.1150)	(0.0040)	(0.0110)
Cap	0.0092				
	(0.5330)				
Adj. R^2	0.2700	0.2251	0.1697	0.2678	0.1969
N	10,660	7,071	7,071	10,693	10,693

Dependent variable is the natural log of real income, net of all expenses (including malpractice insurance premium). Two-tailed p-values in parentheses.

Table 3-5. Robustness, with interactions

	(A) State and year fixed-effects	(B) State-year fixed-effects, excluding institutional physicians	(C) State-year fixed-effects, excluding salary variables
Payments per physician (internists)*	0.1346	0.1197	0.1310
	(0.0000)	(0.0000)	(0.0000)
Payments per physician (general surgeons)*	0.0465	0.0636	0.0377
	(0.0060)	(0.0000)	(0.0180)
Payments per physician (ob-gyns)*	0.0164	0.0015	0.0137
	(0.0280)	(0.9220)	(0.1250)
Median payment size (internists)* ($100,000s)	0.0127	0.0141	0.0169
	(0.0510)	(0.2660)	(0.0500)
Median payment size (general surgeons)* ($100,000s)	0.0000	0.0105	-0.0096
	(0.9980)	(0.6530)	(0.6620)
Median payment size (ob-gyns)* ($100,000s)	0.0058	0.0366	0.0207
	(0.7850)	(0.0510)	(0.2560)
Premium (internists)* ($1,000s)	-0.0008	-0.0010	-0.0004
	(0.0190)	(0.0560)	(0.3580)
Premium (general surgeons)* ($1,000s)	-0.0014	-0.0017	-0.0012
	(0.0550)	(0.0120)	(0.0740)
Premium (ob-gyns)* ($1,000s)	-0.0007	-0.0001	-0.0006
	(0.4480)	(0.9270)	(0.4620)
Cap (internists)*	-0.0111		
	(0.5660)		
Cap (general surgeons)*	0.0551		
	(0.0070)		
Cap (ob-gyns)*	0.0658		
	(0.0440)		
Adj. R^2	0.2725	0.2261	0.2695
N	10,660	7,071	10,693

Dependent variable is the natural log of real income, net of all expenses (including malpractice insurance premium). Two-tailed p-values in parentheses. *For interacted variables, the omitted specialty is internist. The reported estimates for surgeons and ob-gyns are equal to the sum of the internist coefficient and the interacted coefficient.

Table 3-6. Dropping variables of interest, no interactions

	(A) State-year fixed-effects, excluding frequency	(B) State-year-specialty fixed-effects, excluding frequency	(C) State-year fixed-effects, excluding median	(D) State-year-specialty fixed-effects, excluding median	(E) State-year fixed-effects, excluding premium	(F) State-year-specialty fixed-effects, excluding premium
Payments per physician			0.0163	0.0136	0.0060	0.0026
			(0.0260)	(0.2970)	(0.3960)	(0.8260)
Median payment size ($100,000s)	0.0217	0.0211			0.0207	0.0203
	(0.0200)	(0.0210)			(0.0270)	(0.0270)
Premium ($1,000s)	-0.0010	-0.0008	-0.0014	-0.0014		
	(0.0260)	(0.2930)	(0.0050)	(0.0340)		
Adj. R^2	0.2694	0.1984	0.2692	0.1981	0.2691	0.1984
N	10,660	10,660	10,660	10,660	10,660	10,660

	(G) State-year fixed-effects, excluding frequency and median	(H) State-year-specialty fixed-effects, excluding frequency and median	(I) State-year fixed-effects, excluding frequency and premium	(J) State-year-specialty fixed-effects, excluding frequency and premium	(K) State-year fixed-effects, excluding median and premium	(L) State-year-specialty fixed-effects, excluding median and premium
Payments per physician					0.0075	0.0045
					(0.2740)	(0.7070)
Median payment size ($100,000s)			0.0212	0.0206		
			(0.0220)	(0.0240)		
Premium ($1,000s)	-0.0009	-0.0007				
	(0.0350)	(0.3880)				
Adj. R^2	0.2689	0.1980	0.2692	0.1984	0.2688	0.1980
N	10,660	10,660	10,660	10,660	10,660	10,660

Dependent variable is the natural log of real income, net of all expenses (including malpractice insurance premium). Two-tailed p-values in parentheses.

Table 3-7. Dropping variables of interest, with interactions

	(A) State-year fixed-effects, excluding frequency	(B) State-year fixed-effects, excluding median	(C) State-year fixed-effects, excluding premium	(D) State-yr fixed-effects, excl. frequency and median	(E) State-yr fixed-effects, excl. frequency and premium	(F) State-yr fixed-effects, excl. median and premium
Payments per physician (internists)*		0.1341	0.1305			0.1360
		(0.0000)	(0.0000)			(0.0000)
Payments per physician (general surgeons)*		0.0434	0.0241			0.0264
		(0.0090)	(0.0620)			(0.0400)
Payments per physician (ob-gyns)*		0.0148	0.0101			0.0111
		(0.0950)	(0.1180)			(0.0840)
Median pmt size (internists)* ($100,000s)	0.0238		0.0179		0.0239	
	(0.0110)		(0.0380)		(0.0110)	
Median payment size (gen surgeons)* ($100,000s)	0.0024		-0.0107		-0.0022	
	(0.9130)		(0.6200)		(0.9200)	
Median payment size (ob-gyns)* ($100,000s)	0.0329		0.0227		0.0312	
	(0.0760)		(0.2050)		(0.0910)	
Premium (internists)* ($1,000S)	-0.0002	-0.0003		-0.0001		
	(0.9000)	(0.4750)		(0.9560)		
Premium (general surgeons)* ($1,000s)	-0.0013	-0.0013		-0.0014		
	(0.0350)	(0.0390)		(0.0280)		
Premium (ob-gyns)* ($1,000s)	-0.0006	-0.0006		-0.0005		
	(0.2950)	(0.4520)		(0.3270)		
Adj. R^2	0.2695	0.2708	0.2711	0.2690	0.2693	0.2707
N	10,660	10,660	10,660	10,660	10,660	10,660

Dependent variable is the natural log of real income, net of all expenses (including malpractice insurance premium). Two-tailed p-values in parentheses. *For interacted variables, the omitted specialty is internist. The reported estimates for surgeons and ob-gyns are equal to the sum of the internist coefficient and the interacted coefficient.

Table 3-8. Physician categories and allegation natures

Total physicians	Family/general practice	Medical specialties	Surgical specialties	Other specialties	Hospital-based
All allegation natures	Diagnosis	Diagnosis	Diagnosis	All allegation natures	Diagnosis
	Treatment	Treatment	Anesthesia		Surgery
	Medication	Medication	Surgery		Medication
	IV and blood products	IV and blood products	Medication		IV and blood products
	Monitoring	Monitoring	IV and blood products		Obstetrics
	Behavioral health	Behavioral health	Treatment		Treatment
			Equipment/ product		Monitoring

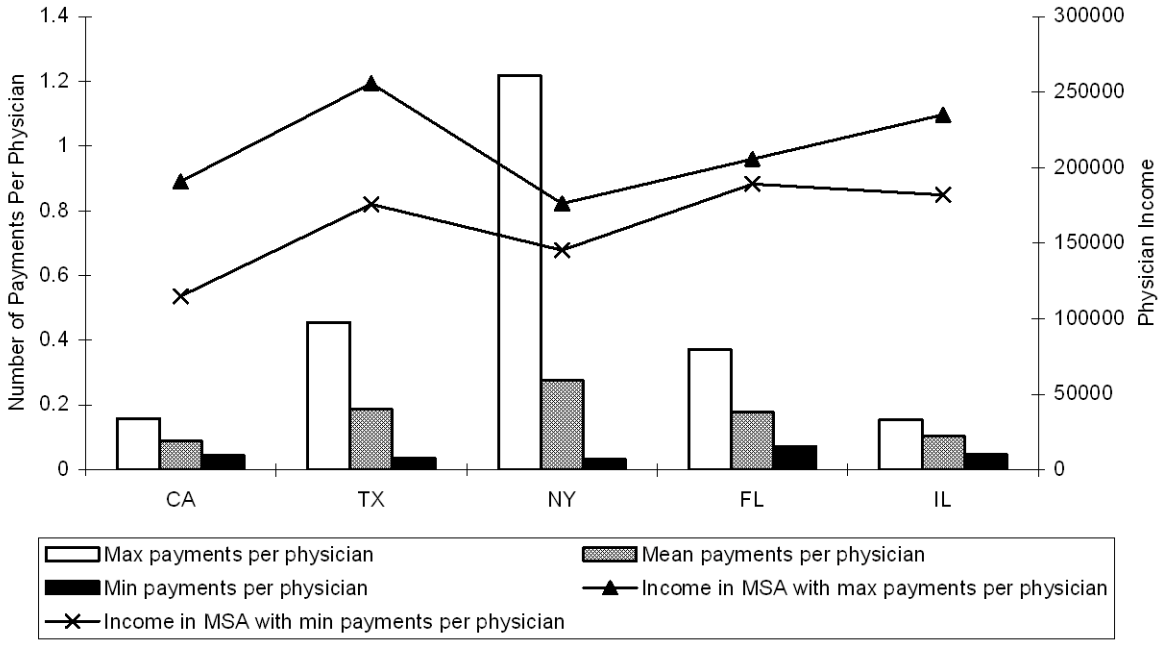


Figure 3-1. Minimum, maximum and mean number of malpractice payments per physician and physicians' incomes. Max (min) payments per physician are four-year averages of the number of payments per physician in the MSA with the max (min) number of payments per physician in each year. Mean payments per physician are four-year averages of the mean number of payments per physician in each state. Income is the four-year average of mean physician income in the MSA with the max (min) number of payments per physician.

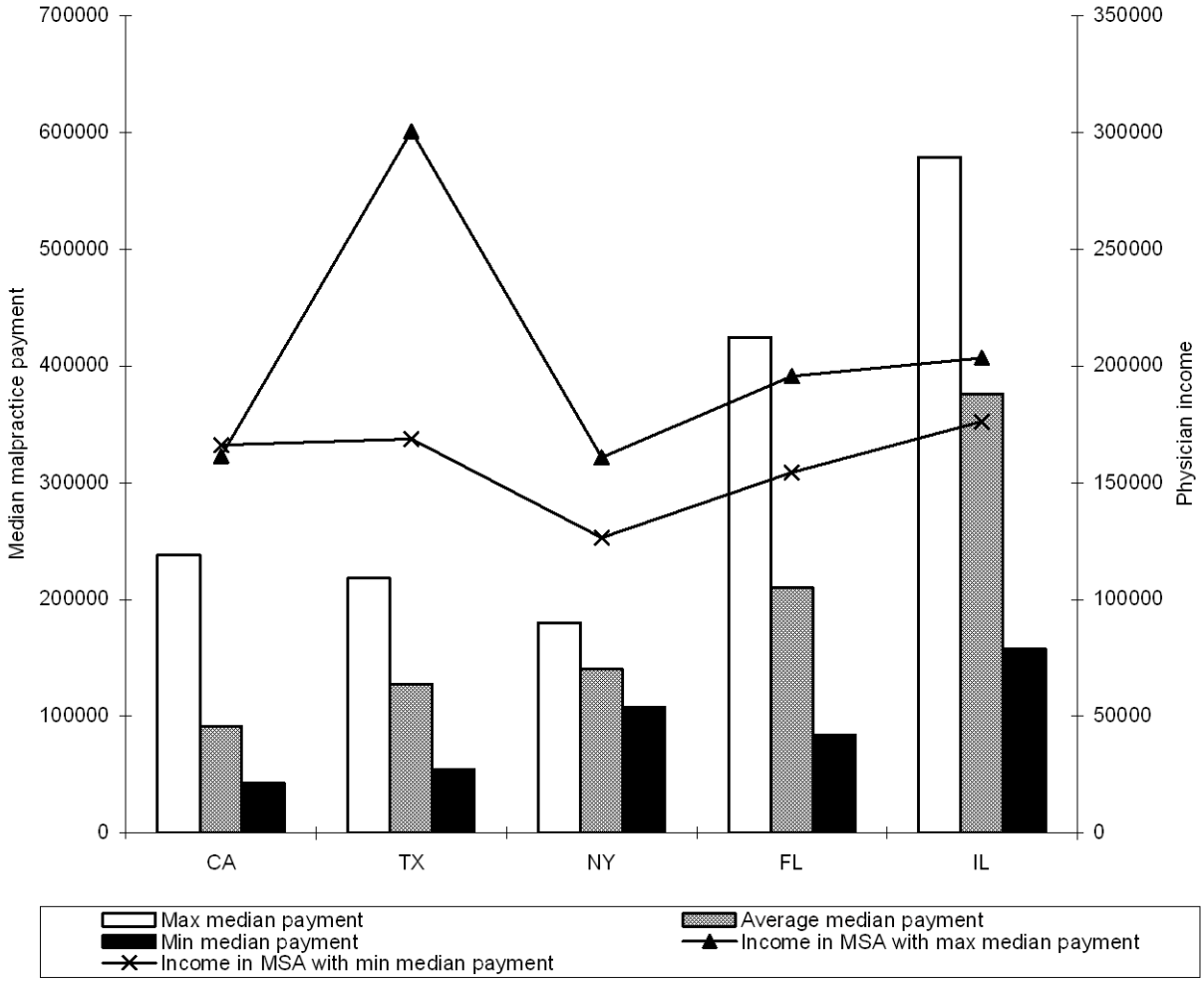


Figure 3-2. Minimum, maximum and average median malpractice payment size and physicians' incomes. Max (min) payment size is the four-year averages of the median payment size in the MSA with the max (min) payment size in each year. Average median payment is the four-year average of the average median payment size in each state. Income is the four-year average of mean physician income in the MSA with the max (min) payment size.

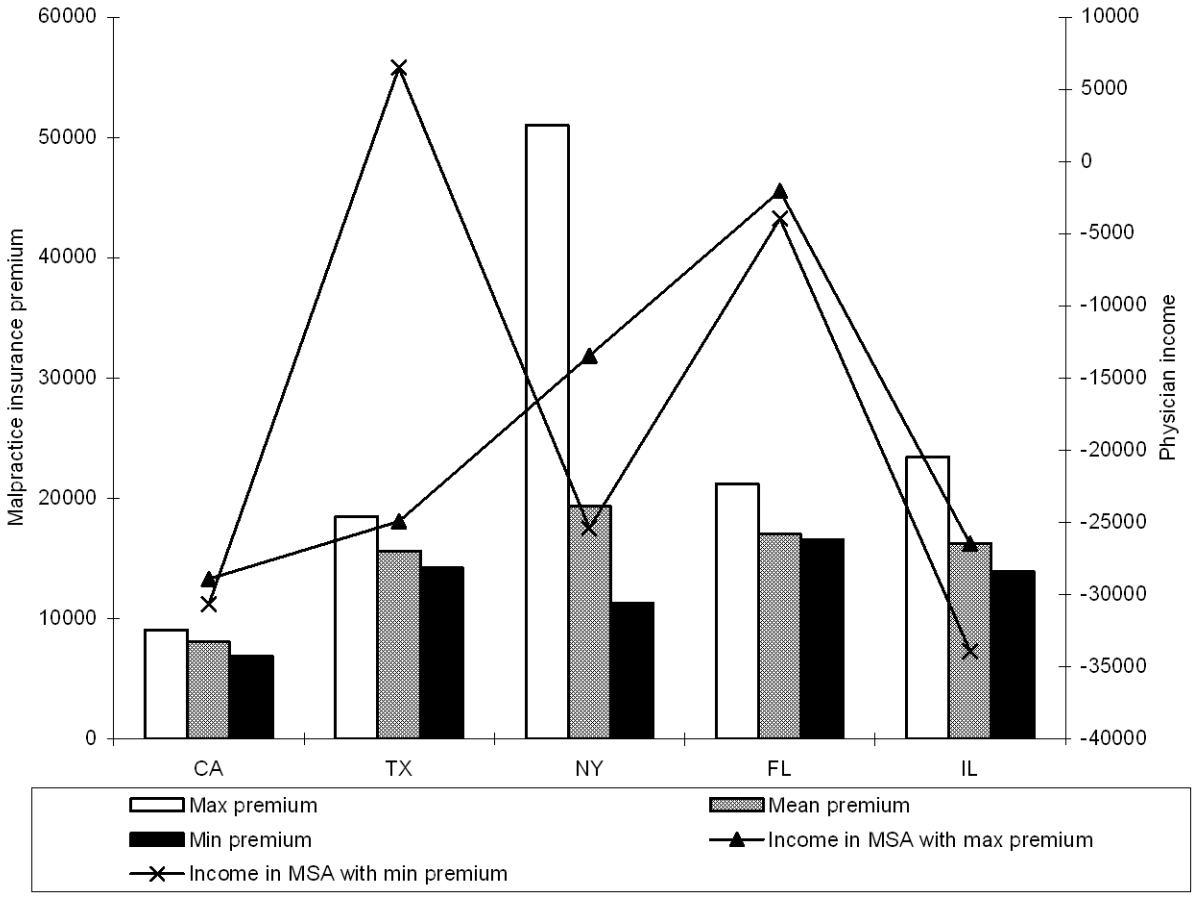
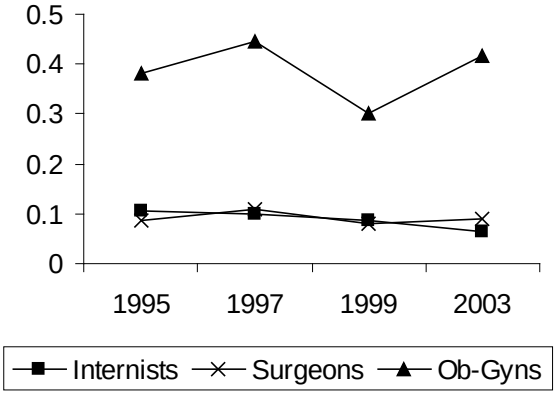


Figure 3-3. Minimum, maximum and mean malpractice insurance premiums and physicians incomes. Max (min) premium is the four-year average of the mean premium in the MSA with the max (min) premium in each year. Mean premium is the four-year average of the mean premium in each state. Income is the four-year average of mean physician income in the MSA with the max (min) payment size.



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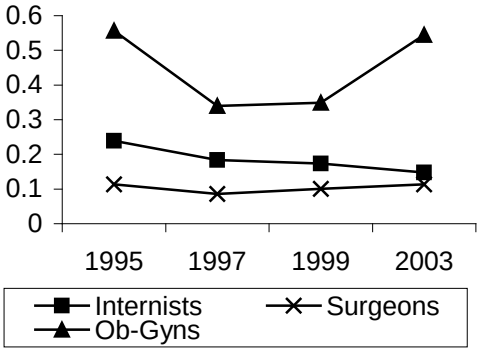

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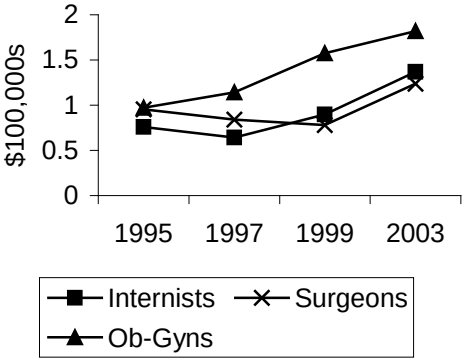
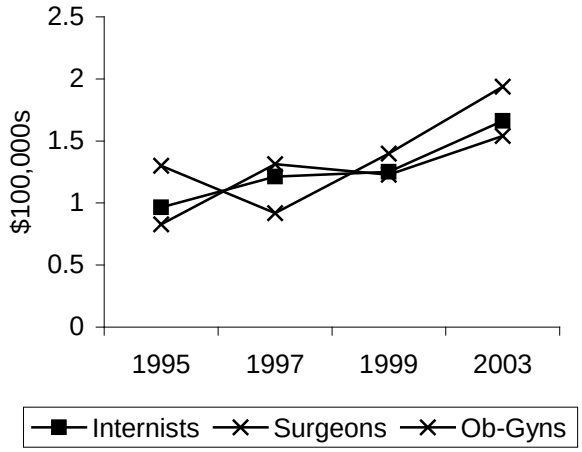


C

Figure 3-4. Differences in malpractice experiences by specialty. A) Frequency of malpractice payments by specialty over time in California. B) Frequency of malpractice payments by specialty over time in Texas. C) Median malpractice payments by specialty over time in California. D) Median malpractice payments by specialty over time in Texas. E) Malpractice premiums by specialty over time in California. F) Malpractice premiums by specialty over time in Texas.



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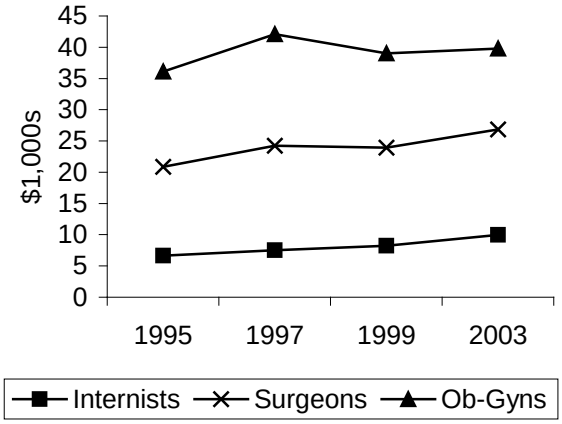

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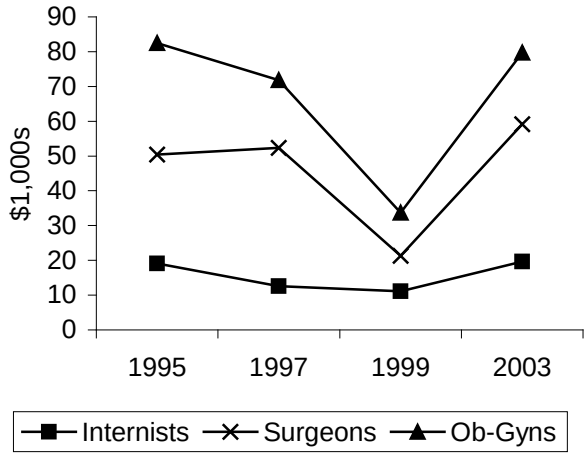


F

Figure 3-4. Continued

CHAPTER 4
THE EFFECT OF MEDICAL MALPRACTICE LIABILITY ON ACCESS TO CARE

Introduction

According to several researchers and media outlets, the United States is in the midst of a medical malpractice liability crisis, characterized by increasing malpractice settlement amounts, higher frequency of settlement payments per physician, higher medical malpractice insurance premiums, and diminished availability of professional liability insurance for health care providers.[1] A number of researchers have examined the effects of malpractice liability on variables affecting health delivery. The evidence on the effect of liability on physicians' incomes is mixed[2], but there is evidence suggesting that liability reduces physician supply per capita[3] and causes defensive medicine practices[4].

This paper examines another important way in which medical malpractice liability can affect the delivery of health care: Does liability reduce access to care? I employ individual level access to care data from four rounds of the Community Tracking Study along with metropolitan statistical area liability measures (a panel at the metro level) to estimate the effect of malpractice liability on access to care. I find evidence that increased liability diminishes several measures of access to care for vulnerable (i.e., uninsured) populations.

The effect of malpractice liability on access to care may work through a number of channels. Diminished access could result from a lack of available care, which is caused by the reduced number of physicians per capita that accompanies increased liability. Also, patient access may suffer if risk averse physicians attempt to avoid lawsuits by refusing to treat high-risk

[1] See Appleby (2000), Boulton (2004), Clarke (2004), Krishnan (2006), Mello (2006), Mille (2002), Skidmore (2002), Solomont (2007), Thorpe (2004)
[2] See Danzon, Pauly and Kington (1990) and Weinberg (2009).
[3] See Baicker and Chandra (2005), Danzon, Pauly and Kington (1990), Encinosa and Hellinger (2005), Kessler, Sage and Becker (2005), Klick and Stratmann (2007), Matsa (2007), and Weinberg (2008).
[4] See Dubay, Kaestner and Waidmann (1999), Kessler and McClellan (1996), O'Neill and Hennesy (2005)

patients, who are more likely to suffer poor outcomes, or if physicians limit the scope of their practices by refusing to perform high-risk *procedures* (e.g., family physicians doing less obstetrics). Alternatively, in a climate where physicians charge higher fees to defray costs associated with bearing high liability risk (e.g., malpractice liability insurance, disutility of lawsuits, effect of suits on reputation), uninsured patients may have even more difficulty affording care, resulting in reduced utilization among the uninsured. Another possibility is that physicians will attempt to focus on or attract high-return patients (those whose insurers reimburse most generously) in an effort to deal with higher liability costs.

While the extant literature that examines the effects of malpractice liability on health care delivery focuses on the questions discussed above (i.e., workforce, physician income, defensive medicine), there is scant evidence regarding the question of how liability affects individuals' access to care. The available evidence may suggest that increases in liability are associated with diminished access. Mello et al. (2005) survey over 800 specialist physicians in Pennsylvania in 2003 to examine whether physicians' reactions to increased malpractice liability (e.g., relocation, early retirement, elimination of high risk aspects of practice, focusing on patients whose insurance has high reimbursement rates, avoiding high-risk patients) have affected physicians' *perceptions* of their patients' access to care over the past three years. Access is measured by physicians' reports of their patients' driving times; waiting times for appointments, surgical procedures or emergency department services; and the number of patients who have switched doctors. Physician specialties included general surgeons, neurosurgeons, orthopedic surgeons, obstetrician-gynecologists, emergency physicians, and radiologists. The evidence suggests that in Pennsylvania access to physicians practicing in high-risk specialties is diminished because of reduced physician supply and physicians' decisions to eliminate riskier aspects of their practices.

When considering the evidence provided by Mello et al., it is important to remember that the data are gathered entirely from a random sample of physician respondents. No adjustments are made for physician characteristics that might affect perceptions of access. Also, although the physicians cite both liability and managed care penetration as "important contributing factors" to changes in access, it is also possible that myriad other unobservable factors (e.g., local market conditions, legislative changes) could also be causing the diminished access.

Dranove and Gron (2005) analyze Florida data from 1997, 2000, and 2003 on activity levels of neurosurgeons and obstetricians, as well as the incidence of high-risk surgeries and patient travel times. They find that craniotomy[5] patients experienced an increase in travel times, though the incidence of craniotomies increased over the sample period. High-risk pregnant women did not experience increased travel times. The proportion of physicians who reduced the number of high-risk procedures they performed was greater among those physicians who initially performed high volumes of craniotomies and high-risk deliveries, compared with physicians who initially performed only a few of these procedures. It is important to note that Dranove and Gron are comparing travel times, incidence and activity rates over time periods during which the malpractice liability environment changed. The changes in the access measures may not be entirely caused by changes in liability risk. As with Mello et al., there could be other unobserved factors that impact access.

The present paper expands and improves upon the current literature examining the link between malpractice liability and access to care. I use a variety of access measures, including travel time, having a usual source of care, emergency department utilization, unmet or postponed health care needs, and others. I use regression techniques to estimate the direct effects of

[5] A craniotomy involves the removal of part of the skull in order to access the brain.

liability on access while controlling for time and metropolitan area unobservables. Additionally, I have individual-level data on access, demographics, and insurance status, enabling me to control for many relevant covariates. Also, the previous literature presents evidence from only two states while I utilize a nationally representative sample.

Empirical Strategy

I employ data from several sources to test the hypothesis that increased medical malpractice liability results in diminished access to care. I use individual-level data on access to care, demographics, and attitudes, together with metropolitan statistical area (MSA) level data on malpractice liability to form a four-year repeated cross-section (1997, 1999, 2001 and 2003). All of the access measures I use as dependent variables (discussed in detail below) are either dichotomous (in which case I estimate logit models) or take on non-negative integer values (in which case I estimate Poisson regressions). The logit models assume:

$$\Pr(y_L = 1 \mid X) = \frac{e^{X\beta}}{1 + e^{X\beta}}. \tag{4-1}$$

In Equation 4-1, y_L is a dichotomous access measure and X consists of independent variables discussed below. The Poisson models assume that the probability density function, $f(\cdot)$, of the count access measure, y_C, is given by:

$$f(y_C = y \mid X) = \frac{e^{-\mu}\mu^{y}}{y!}, \; y=0, 1, 2, \ldots \tag{4-2}$$

In Equation 4-2, $\mu = e^{X\beta}$. The structure of my data (individual level access data coupled with metro level variables of interest) allow me to parameterize the models in the following way:

$$X\beta_{imst} = \sum_{j=1,2,3} \gamma_j MEDIAN_{mst} + \sum_{j=1,2,3} \delta_j FREQUENCY_{mst} + \lambda Z_{imst} + \varphi_{ms} + \eta_t. \tag{4-3}$$

In Equation 4-3, *i*, *m*, *s* and *t* index the individual, metropolitan statistical area, state and year, respectively. *MEDIAN* is the real median malpractice settlement size (in $100,000s) in metropolitan area *m* in year *t*, and *FREQUENCY* is the number of settlements per physician paid by (or on behalf of) physicians to plaintiffs in MSA *m* in year *t*. The variables of interest (*MEDIAN* and *FREQUENCY*) are interacted with indicators signifying insurance status so that subscript *j* indexes insurance status and takes on the values *PRIVATE*, *MEDICAID*, and *UNINSURED*[6]. Thus, the model estimates insurance-status-specific parameters for the effect of malpractice liability on access. *Z* consists of exogenous individual- and metro-level control variables that affect access to care; these variables are discussed in more detail below. φ_{ms} is the MSA fixed-effect, capturing any metropolitan level time-invariant unobservables that might be correlated with both access and liability, and would otherwise bias my estimates of γ and δ. For example, the MSA fixed-effects protect my identification strategy from unobserved heterogeneity in physician quality or consumer preferences for health care across metro areas. η_t is a year fixed-effect, capturing unobserved variables that affect all individuals' access similarly, regardless of their MSA of residence.

I use several measures of individuals' access to care, all of which are present in (or constructed from) the four rounds of Community Tracking Study Household Survey data. The estimated coefficients for the independent variables of interest (size and frequency of malpractice settlement payments) are expected to be positive for the first 6 dependent variables and negative for the last 5. I follow this ordering throughout the paper.

- EMERGENCY DEPARTMENT UTILIZATION. This variable is equal to the number of times in the last twelve months that the respondent went to the emergency department (ED), excluding visits resulting in hospital admission. This variable is a measure of the extent to which the individual uses emergency department visits in lieu of doctor's visits. Higher values of

[6] My sample excludes individuals aged 65 and older, so Medicare beneficiaries do not appear in the sample.

this variable suggest diminished access to regular care and/or easier access to emergency care. The estimated parameters for the variables of interest (*MEDIAN* and *FREQUENCY*) are expected to be positive. This variable is top coded at seven visits (accounting for 0.25% of the sample) for confidentiality reasons; I estimate the model using censored Poisson regression.

- YEARS SINCE MAMMOGRAM. This variable is equal to the number of years since the respondent's last mammogram. The question was asked of women aged 40 or older. The parameter estimates for the liability measures are expected to be positive since higher values of this dependent variable are suggestive of diminished access. This variable is top coded at three or more years (accounting for 8.4% of the sample) for confidentiality reasons; I estimate the model using censored Poisson regression.

- PUTOFF. This variable is equal to unity if, in the last twelve months, the respondent postponed necessary health care. The variables of interest are expected to increase the likelihood of putting off care.

- PUTOFF ACCESS. This variable is equal to unity for those individuals who postponed needed health care in the last twelve months for any of the following reasons:

    - Could not get an appointment soon enough
    - Travel time to doctor was too long
    - Waiting time at the doctor's office was too long
    - Did not know where to go for care
    - Transportation problems

- UNMET NEEDS. This variable is equal to unity if the respondent did not receive necessary medical care in the last twelve months. The parameter estimates for the liability variables are expected to be positive.

- TRAVEL TIME. This variable is equal to the approximate travel time in minutes to the respondent's last appointment. Respondents to this question must have had a physician visit in the last twelve months for routine care or sickness. Because of the "lumpy" nature of the responses (e.g., 13% of respondents report 5 minutes, 22% report 10 minutes, 20% report 15 minutes), I group reported travel times and estimate the model using ordered logit. The first category includes travel times of 2 minutes or less; the second category includes travel times of 3 to 12 minutes; the third includes travel times of 13 to 32 minutes; the fourth, 33 to 62 minutes; and the fifth, 63 to 480 minutes[7]. I expect higher liability measures to have a positive impact on travel time.

- PREVENTIVE CARE. This variable is equal to unity if the individual saw a physician for routine or preventive care in the last twelve months. More preventive care is associated with better access. Thus, I expect the parameter estimates for the liability measures to be

[7] Creating 5-minute intervals and estimating a Poisson regression yields similar results.

negative. I estimate this model using logit. This variable is not available in Round 1 of the Community Tracking Study data.

- MAMMOGRAM EVER. This variable is equal to unity if the respondent ever had a mammogram. This question was asked of women aged 40 or older (an annual mammogram is recommended for this group) since mammograms are considered routine preventive care for this population. The liability measures are expected to have a negative impact on the likelihood of ever having had a mammogram.

- FLUSHOT. This variable is equal to unity if the respondent had a flu shot in the last twelve months. I restrict the sample to those who are at least 50 years old since the Centers for Disease Control and Prevention suggest that this group be vaccinated. The liability variables are expected to have a negative effect on the likelihood of having a flu shot. This variable is not available in Round 4 of the Community Tracking Study data.

- USUAL SOURCE. This variable is equal to unity if the respondent has one or more places where s/he goes for health care or advice about health issues. The liability measures are expected to negatively impact the likelihood of having a usual source of care.

- NUMBER VISITS. This variable is equal to the number of times the respondent has seen a doctor in the last twelve months, excluding interactions that took place during hospital stays and ED visits. The liability measures are expected to have a negative impact on the number of doctor visits. This variable is top coded at 30 or more visits (accounting for 1% of the sample) for confidentiality reasons; I estimate the model using a censored Poisson regression.

As described above, Z consists of one metro- and several individual-level control variables. In addition to controlling for differences in access to care across individuals, several of the following variables also address utilization. Whereas an individual's utilization refers to the amount of services consumed, access is a measure of the availability of services. Several of the variables listed below (e.g., smoking status, age, gender) control for both of these closely related measures. This is necessary since some of the dependent variables (e.g., number of visits, ED utilization) measure both access and utilization; failing to control for demographic differences that impact utilization would bias my estimates of the effect of liability on the dependent variable.

- POPULATION DENSITY. Population density is measured at the MSA level and proxies for the availability of specialist physicians. It also captures transportation costs. Since my sample is restricted to metropolitan areas only, as population density increases, I would expect

there to be greater time costs associated with obtaining health care because of congestion. The sign of the parameter estimate for this variable is therefore ambiguous: Availability of specialists would tend to improve access while congestion would tend to reduce access.

- RISK AVERSE, RISK LOVING. The variable RISK AVERSE (RISK LOVING) is equal to unity if the respondent "strongly disagrees" ("strongly agrees") with the statement "I'm more likely to take risks than the average person." Respondents who "somewhat agree," "neither agree nor disagree," or "somewhat disagree" make up the excluded category of individuals who rate themselves as neither risk averse nor risk loving. Risk averse (risk loving) individuals are more (less) likely to seek out medical care and thus have a preference for higher (lower) utilization. I expect risk averse (risk loving) individuals to have better (worse) access to care measures.

- SMOKE DAILY, SMOKE SOMEDAYS, SMOKED QUIT. These smoking status variables are equal to unity if the respondent smokes daily, smokes some days, or used to smoke but has quit, respectively. The excluded smoking status category includes individuals who have no smoking experience. The impact of smoking status on access measures is theoretically ambiguous. Compared to individuals who have never smoked, I expect smokers to consume more health care because they are at higher risk for health problems. Here, smokers are substituting one health input for another: Someone who smokes must consume more health care in order to maintain a given level of health. This substitution could manifest as increased utilization of medical services and, thus, better access measures among smokers. Alternatively, the tendency to smoke may capture an individual's attitude toward inputs to health in general. If smokers tend not to invest time, effort, and other inputs to improve health, I would expect a smoker to consume less health care. In this scenario, smokers would appear to have diminished access.

- AGE. This variable is the age of the individual. Since older people tend to require more health care and thus consume more, I expect age to have a positive impact on access to care measures.

- FEMALE. This variable is equal to one if the individual is female. Since women appear to have a greater preference for health care and tend to consume more health care than men, (particularly true during childbearing years), I expect this variable to have a positive impact on access.

- HISPANIC, BLACK, OTHER. These variables equal unity if the individual is Hispanic, African-American, or a member of some other minority group. Minorities may experience more barriers to access (e.g., discrimination by Caucasian providers, distrust of Caucasian doctors, lack of information about the health care system), so I expect these variables to negatively impact access to care.

- EDUCATION. This variable is equal to the number of years of education the individual completed. Empirically, educational attainment is positively correlated with consumption of health care. This might be because non-monetary costs (e.g., search costs) associated with obtaining health care are lower for the highly educated, or because the return to health

care in terms of health status is greater for educated individuals. I expect educational status to improve access to care.

- INCOME. This variable is equal to total family income divided by the federal poverty level for the relevant family size. Using this ratio rather than simply the level of family income is useful because the federal poverty line is adjusted for family size. Since health care is a normal good, I expect higher income individuals to have better access to care.

- STUDENT. This variable is equal to one if the individual is a full-time student. Holding age and other relevant factors constant, full-time students may have better access to care since colleges and universities often provide subsidized health services to their students.[8]

- GOOD HEALTH, POOR HEALTH. The variable GOOD HEALTH (POOR HEALTH) is equal to unity if the individual rates her general health status as "excellent" or "very good" ("fair" or "poor"). The excluded general health status category consists of individuals rating their health status as "good." The effect on access of having "excellent" or "very good" self-rated health status is ambiguous. The tendency to perceive oneself as healthy may be positively correlated with access measures; thus, we would observe better access for people with better self-rated health. However, healthier individuals may not need as much care and thus would have worse access measures.[9]

- The following five variables provide detail about the generosity and type of insurance that the privately insured have (the variables are equal to zero for the uninsured and Medicaid patients).[10]

    - PRIMARY CARE. This variable is equal to unity if the individual must sign up with a particular primary care provider (PCP). While this requirement restricts the patient's choice, it also may result in increased likelihood of having a regular source of care. Thus, the effect of PRIMARY_CARE on access is ambiguous.

    - HMO. This variable is equal to unity if the individual is insured through a health maintenance organization. HMOs are designed to deliver comprehensive care at minimal out-of-pocket cost; however, they also restrict patients' options. The effect on access of being insured through an HMO is thus ambiguous.

    - REFERRAL PAY. This variable is equal to one if the insurer will pay part of the costs associated with seeing a physician who is not part of the plan's network, even if the patient does not have a referral. This question was asked of individuals who are insured with an HMO or whose insurance plan has a list of physicians associated with the plan. Plans for which REFERRAL_PAY is equal to one are likely to be more generous; I expect this variable to have a positive impact on access.

[8] Eliminating full-time students (accounting for 4% of the sample) does not change the results.
[9] Eliminating the self-rated health measures from the model does not change the results.
[10] Eliminating these five private health insurance variables does not change the results.

- REFERRAL. This variable is equal to one if the insurer requires the individual to obtain a referral from the PCP in order to see a specialist. The requirement of a referral is expected to reduce access by restricting choice and increasing the time cost of obtaining specialty care.

- DIRECTORY. This variable is equal to unity if there is a book, directory or list of providers associated with the health insurance plan. A directory reduces the insured's options and is thus expected to diminish access.

- MEDICAID, UNINSURED. These variables equal unity if the individual has health insurance coverage through Medicaid or is uninsured, respectively. The excluded insurance category consists of individuals who are privately insured through their employer. Compared to the privately insured, I expect Medicaid patients to experience diminished access since Medicaid reimbursements are less generous. Also, the uninsured will experience diminished access because of inability to pay for care.

My estimation strategy uses metropolitan area fixed-effects as well at time dummies. Thus, identification of the effect of liability on access to care comes from variation over time and within metro areas. Figure 4-1 shows plots of the access measures (vertical axis) against the liability variables (horizontal axis) with lines of best fit superimposed. The values are adjusted by regressing the access and liability variables on MSA and year dummies. The residuals generated by these regressions capture any variation other than that caused by cross-MSA and cross-year differences. Each marker represents the mean of the residuals in an MSA-year cell, and, for comparison purposes, the values are standardized to have mean zero and standard deviation one. Of course, there are many important covariates that affect access that I do not control for in these diagrams; I do, however, hold these factors constant in the regression analysis. Considering Figure 4-1, it appears that in general, there is no relationship between the access and liability measures since the lines of best fit have near-zero slopes. Occasionally, there is a non-zero relationship in which liability affects access in the anticipated way (e.g., the effect of median settlement payment size on having a usual source of care). When the slope is non-zero, however, it is more likely that the relationship will be the *opposite* of what is expected (e.g., the effect of payment size on ED use; the effect of the frequency of settlement payments on

the likelihood of ever having a mammogram). Examining subpopulations can help to illuminate the seeming lack of evidence for a consistent relationship between access and liability.

I hypothesize that the availability of health care for vulnerable populations (i.e., Medicaid patients and the uninsured) is more likely to suffer (relative to the privately insured) as a result of increases in malpractice liability. Figure 4-2 plots the same relationships as those exhibited in Figure 4-1, but for the subsample of uninsured individuals. Compared to the plots generated by the full sample (Figure 4-1), those from the uninsured sample are more likely to exhibit a nonzero relationship between liability and access. Furthermore, among the non-zero relationships, malpractice liability generally has the anticipated effect on access (e.g., the effect of settlement payment frequency on ED utilization; the effects of payment size and frequency on preventive care). This pattern will be more apparent in the results of the regression analysis (discussed in Section IV), but the diagrams provide illustrative evidence of a negative liability-access relationship for the uninsured.

Data

I employ several data sets to test the hypothesis that increased medical malpractice liability diminishes access to care. My main data source is composed of four rounds of the restricted version of the Community Tracking Study (CTS) Household Survey. The data were collected by the Center for Studying Health System Change in an effort to enable researchers to study changes in the health care system and how those changes affect delivery of care. The four rounds of data were collected over the periods July 1996-October 1997, July 1998-October 1999, September 2000-September 2001, and February 2003-February 2004. The restricted version differs from the public use version in that the restricted version includes geographical identifiers for the individual. Obtaining the restricted use files requires an application process in which the

researcher provides a data protection plan and explains the specific purpose for which the data will be used.

I keep only the observations in the "Site Sample," which is composed of 60 sites intended to represent health care markets. Although the sites are generally defined as MSAs, in some instances, the site definition is finer than the geographical level at which I have physician workforce data. I had to consolidate some sites for this reason; thus, there are 47 unique geographical units in my final data set. From the four rounds of data, this produces approximately 200,000 observations. I drop individuals who are younger than 18 or older than 64. This eliminates heterogeneity associated with the fact that these excluded groups have special access to care that is not applicable to the rest of the population (e.g., minor children may be eligible for programs such as SCHIP while the elderly are covered by Medicare). I keep only individuals who are insured privately through their employer, insured through Medicaid, or are uninsured. This accounts for 88% of the adult non-elderly sample. Other insurance types that are excluded from the analysis include "other" public insurance, directly purchased private insurance, and military coverage. The remaining sample is composed of approximately 112,000 individual observations.

The variables capturing malpractice liability come from the National Practitioner Data Bank (NPDB), which is maintained by the Department of Health and Human Services. The NPDB contains data on all disclosable reports regarding malpractice payments and adverse actions (e.g., loss of clinical privileges, professional association membership revocation) against licensed physicians, dentists, and other health care professionals. One criticism of the NPDB is that malpractice settlements that include the dismissal by a hospital or other corporation of at least one health care provider need not be reported. Nevertheless, the NPDB is recognized as

one of the most comprehensive databases of medical malpractice actions and enables researchers to construct measures of liability at the state level. Because of confidentiality concerns, data at geographical units finer than the state require a special request. For the purpose of this study, I obtained MSA-level data on the median size and total number of malpractice settlement payments in each MSA from the Division of Practitioner Data Banks at the Health Resources and Services Administration.

In order to scale the number of malpractice settlement payments by the number of physicians in a metropolitan area, I collected physician workforce data from the American Medical Association's (AMA) publication, Physician Characteristics and Distribution in the US. According to the AMA website, this source

> is the most accurate and complete source for statistical data about the physician supply in the United States...All data are derived from the American Medical Association Physician Masterfile, which obtains data from primary sources only. Primary sources include medical schools, hospitals, medical societies, the National Board of Medical Examiners, state licensing agencies and many others. The stringent verification process is unique and one of the most thorough in the industry.

The AMA tracks physician movement both through physicians' reporting their new addresses as well as through the postal service's address correction system.

Other data include the 2000 land area (in square miles) and population (for each data year) of the MSAs (both from the Bureau of the Census), which were used to calculate population density. Also, median payment size was deflated using the Consumer Price Index, available from the Bureau of Labor Statistics. After all data sources were merged and the CTS sites were appropriately allocated to their respective Metropolitan Statistical Areas, 47 MSAs in 34 states were represented in the sample.

Table 4-1 presents summary statistics for the access to care measures used in the analysis.[11] On average, respondents in my sample had 0.27 emergency department visits in the last year. Among females aged 40 or older, the average time since the last mammogram is 1.6 years. Twenty-five percent of the sample put off necessary care and 2.5% of the sample put off care for access reasons. Only 8% of the sample had unmet health care needs and average travel time to see a provider is 17.5 minutes. Among individuals who had a doctor visit in the last year, 74% had a routine care visit. Eighty-six percent of women between the ages of 40 and 64 had a mammogram in their lifetime. Eighty-six percent of respondents had a usual source of care and, on average, individuals had 3.5 visits with a health care provider in the last year.

The Table 4-1 also displays 95% confidence intervals for the means of the access measures for each insurance type. Keeping in mind that there are many factors other than insurance status that affect the utilization measures, there appear to be clear differences in the access measures by insurance status. The fact that individuals with different insurance statuses have varying access experiences highlights the usefulness of estimating insurance-status-specific parameters. The uninsured have the poorest access measures for all categories except ED utilization, putting off care for access reasons, and travel time. Medicaid patients use the ED much more frequently than do the privately insured, and the uninsured use an intermediate amount. This pattern is explained by two opposing forces: Those with less generous or no insurance will tend to use the ED more frequently because they lack access to more conventional providers. On the other hand, for the uninsured, the cost of using the ED is higher than it is for Medicaid patients. On net, the second effect appears to dominate. It appears that the uninsured are least likely to put off care for access reasons (could not get an appointment promptly, travel time, wait time, etc.). It is

[11] Sample sizes vary because of missing observations, inapplicability of questions, and the fact that not all questions were included in each CTS round. See variable descriptions above and notes to Table 4-1 for details.

important to recall, however, that the means are not adjusted for income, so that insurance status may be acting as a proxy for income (lower income individuals will likely put off care for reasons other than access, such as dollar cost). Travel time to a health care provider is longest for Medicaid patients and shortest for the privately insured.

Table 4-2 presents summary statistics for the malpractice liability variables as well as demographic and health insurance variables. Malpractice settlement payments average $140,000 and there are approximately 2 payments for every 100 physicians. Forty-four percent of the sample classifies itself as either risk averse or risk loving. Also, 45% are smokers or former smokers, average age is 40, slightly over half are female, and approximately 27% are minorities. Average education is slightly less than 14 years and 3.6% are full-time students. Almost two-thirds perceive their health as good and 11% rate their health as poor. Over 80% of the sample is privately insured through work, around 4% are Medicaid patients, and the remainder are uninsured.

Results

Table 4-3 presents the main results of this paper; results for other covariates are discussed below and are displayed in Table 4-4. Each row represents a regression and each column (aside from the last) reports estimated coefficients for a variable of interest. The null hypothesis that all coefficients (other than the MSA and year fixed-effects) are equal to zero is rejected for all models. Asterisks denote statistical significance, and shaded cells contain parameter estimates for which the coefficient is statistically significant and has the expected sign. An obvious pattern is that the median payment size variable is more likely to be statistically significant with the expected sign than is the number of payments per physician. In fact, of 8 statistically significant estimates for the payment frequency variables, 6 have unexpected signs, suggesting that more liability improves access. Dropping the frequency of payments variables has no effect on the

payment size estimates, suggesting that collinearity among the variables is not the cause of this unintuitive pattern. Perhaps the divergence in the results for the payment size and frequency variables can be explained by the fact that it is settlement size, rather than settlement frequency, that gains notoriety, and thus may be more likely to affect physician behavior.

Focusing on the payment size results, it appears that liability adversely affects several access to care measures for the uninsured and, to a lesser degree, for those insured through Medicaid. Among the uninsured, an increase of one within-MSA, over time[12] standard deviation in payment size results in a 4.3% increase in the number of emergency department visits that do not result in hospital admission. It appears that increases in median settlement payment size increase the number of years since the last mammogram by 0.4% and 2.6% for the privately insured and uninsured, respectively. Also, higher frequency of suits increases the number of years since the last mammogram by 1.2% for Medicaid patients. Among the uninsured, a one-standard-deviation increase in median payment size tends to increase the odds ratio of putting off care by 5%. For Medicaid patients, a one-standard-deviation increase in payment size is accompanied by a 19.7% increase in the odds ratio of putting off care for access reasons. Finally, travel time for both Medicaid patients and the uninsured increases in response to larger median malpractice settlement payments. There is also evidence that, for the uninsured, the relative odds of having preventive care and ever having had a mammogram fall by 6.6% and 8.1%, respectively, in response to one-standard-deviation increases in payment size. Among the uninsured, a one-standard-deviation increase in the number of settlement payments per physician decreases the odds ratio of having a flu shot by 0.6%.

[12] In discussing marginal effects, I use the within-MSA, over time standard deviations of payment size and frequency. This is the relevant measure of variation since identification in these models comes from changes in the variables over time and within MSAs. These values are 0.64 for median payment size and 0.0059 for number of payments per physician. Payment size is measured in $100,000s and frequency is measured in payments per physician.

Table 4-4 displays results for the other covariates used in the regression analysis. As examples, I have displayed results for two dependent variables (ED utilization and preventive care) and I discuss the results for the other regressions as well (available upon request). Many coefficients generally have the expected signs: risk loving/averse, age, income, referral, referral pay, and insurance status. Several variables that had ambiguous predictions had mixed evidence or a lack of statistical significance. These include population density and primary care, both of which were generally not statistically significant; and smoking history and health status, both of which had inconsistent effects on access. The hypothesis for HMO was ambiguous, but the evidence suggests that HMO coverage is more often associated with greater access, particularly for prevention measures (routine care, mammograms, flu shots, usual source of care). Also, having a directory of providers (ambiguous prediction) appears to improve some measures of access (mammograms, flu shots, usual source, number of doctor visits).

Interestingly, the evidence for the effect of gender on access is mixed. Some measures of access are better for females (preventive care, flu shot, usual source, number of doctor visits), while others are worse (ED utilization, putting off care, putting off care for access reasons, unmet needs). The results for the race/ethnicity variables are mixed as well. The impact on access of being a minority is as anticipated in several instances: Minorities are less likely to have ever had a mammogram, more likely to put off care for access reasons, less likely to have a usual source of care, have fewer visits with health care providers, and have longer travel times to their appointments. For a number of access measures, however, minorities appear to have better access to care (preventive care, years since last mammogram, putting off care, unmet needs). The result that minorities have more preventive care may be explained by the fact that only individuals who had at least one doctor visit in the last year were asked whether one of those

visits was for routine care, and minorities tend to have fewer doctor visits. Thus, among individuals who go to the doctor, minorities are more likely than Caucasians to have seen a doctor for preventive care. A similar intuition is present for mammography: Minorities are less likely to have ever had a mammogram, but among those who *have* had a mammogram, the time since the most recent scan is shorter. These instances highlight the importance of coefficient interpretation for the relevant population (e.g., the effect of an independent variable on the likelihood of having preventive care, *conditional upon* having had any care).

In order to further investigate the results presented above, I estimate another set of models in which I include the physician workforce per capita (interacted with the insurance status indicators) as a control variable. As explained in the introduction, one possible way in which liability may diminish access to care is through a decline in the physician workforce per capita. If this were the primary channel through which liability is causing diminished access, I would expect the liability variables to lose significance when workforce per capita is included in the model, and I would expect workforce per capita to improve access to care. Table 4-5 displays the results for the variables of interest. Like Table 4-3, asterisks denote statistical significance and the highlighted cells contain statistically significant coefficients with the anticipated signs. The pattern of significance of the coefficients of interest is quite similar for the models with and without workforce per capita, and the workforce variables are generally not statistically significant.[13] The lack of effect that workforce per capita has on the impact of the other variables on access to care (with the exception of travel time) suggests that attenuated physician supply may not be the cause of liability-driven changes in access.

[13] Eliminating the size and frequency of settlement payments does not change the results for the workforce variables, except for the years since last mammogram dependent variable, for which the estimated coefficient for workforce is statistically significantly positive.

Since the main results of this paper suggest that the uninsured experience diminished access to care for some measures of access, it is important to ensure that the results are not driven by metropolitan area composition. Specifically, if higher rates of uninsurance result in more malpractice liability, and if the uninsured experience diminished access (for reasons other than greater liability), the foregoing estimates could be biased. In order to examine this possibility, I use MSA-year level data to regress the proportion of the population that is uninsured on the liability measures (median size and frequency per physician of settlement payments)[14]. The liability variables are never statistically significant, so the results are not spurious for the reason described above.

Conclusions

This paper extends the existing literature on the effects of medical malpractice liability on the delivery of health care. I use individual-level access to care data and metro-level liability measures to investigate the hypothesis that increased medical malpractice liability results in diminished access to care. I find that this is the case among uninsured individuals for some access to care measures (ED utilization, preventive care, mammography, putting off needed care, and travel time) but not for others (flu shot, unmet medical care needs, having a usual source of care, and number of visits with a health care provider). Furthermore, I find that access measures tend to be more sensitive to the *size* of medical malpractice claims payments, than to the *frequency* per physician of such payments. Possible explanations for diminished access resulting from liability include reductions in physician workforce and changes in physician behavior such as rejecting high-risk patients, refusing to perform high-risk procedures, and focusing on high-profit patients whose insurers are relatively generous payers. The evidence from this paper

[14] I try several specifications of this model, including MSA effects, year effects, mean income, mean education level, and mean age. The coefficients on the liability variables are never statistically significant.

suggests that changes in physician supply are not the primary cause of the effect of liability on access.

In light of the evidence presented herein, policymakers whose goals include expanding access to care among the uninsured (by, for example, reducing unnecessary ED utilization and increasing preventive care) might consider expanding Medicaid to include the working uninsured, who account for the majority of the uninsured in the United States. In my analysis, Medicaid patients' access measures are much less sensitive to changes in malpractice liability. Another policy implication is that reducing the size of malpractice settlement payments will likely improve access among the uninsured. Caps on noneconomic damage awards may help to accomplish this goal.

Table 4-1. Descriptive statistics, access to care

Access to care measure	N	Mean	Std. Dev.	Min	Max
ED utilization	111,919	0.268	0.753	0	7
Years since mammogram	19,858	1.57	0.929	1	4
Put off	106,543	0.249	0.433	0	1
Put off for access reasons	106,262	0.025	0.157	0	1
Unmet needs	106,497	0.078	0.268	0	1
Travel time	80,747	17.582	12.367	2	65
Preventive care	62,575	0.743	0.437	0	1
Mammogram ever	23,644	0.856	0.351	0	1
Flu shot	20,733	0.328	0.469	0	1
Usual source of care	111,626	0.855	0.352	0	1
Number of doctor visits	111,919	3.544	4.886	0	30

	95% Confidence Intervals for Means of Access Measures, by Insurance Type		
Access to care measure	Privately Insured	Medicaid	Uninsured
ED utilization	(0.22, 0.23)	(0.81, 0.9)	(0.33, 0.35)
Years since mammogram	(1.5, 1.53)	(1.53, 1.71)	(2.07, 2.19)
Put off	(0.22, 0.23)	(0.3, 0.32)	(0.36, 0.38)
Put off for access reasons	(0.02, 0.02)	(0.05, 0.07)	(0.02, 0.03)
Unmet needs	(0.06, 0.06)	(0.12, 0.14)	(0.17, 0.18)
Travel time	(17.1, 17.28)	(20.19, 21.17)	(19.34, 19.97)
Preventive care	(0.74, 0.75)	(0.78, 0.81)	(0.64, 0.67)
Mammogram ever	(0.88, 0.89)	(0.71, 0.77)	(0.61, 0.65)
Flu shot	(0.34, 0.35)	(0.32, 0.41)	(0.15, 0.18)
Usual source of care	(0.9, 0.9)	(0.87, 0.89)	(0.62, 0.64)
Number of doctor visits	(3.7, 3.76)	(5.89, 6.33)	(1.87, 2)

Sample sizes vary because not all questions were included in all CTS rounds (Flu shot, Preventive care) and not all questions apply to the entire sample (Years since mammogram, Put off for access reasons, Travel time, Preventive care, Mammogram ever, Flu shot).

Table 4-2. Summary statistics, covariates

Variable	N	Mean	Std. Dev	Min	Max
Median malpractice settlement payments size ($100,000s, 2000 dollars)*	179	1.404	0.943	0	6.662
Number of malpractice settlement payments per physician*	179	0.020	0.013	0	0.063
Population density (persons per square mile)*	178	527.171	400.966	34.205	2092.672
Risk loving	106370	0.159	0.366	0	1
Risk averse	106370	0.282	0.450	0	1
Daily smoker	111919	0.182	0.385	0	1
Occasional Smoker	111919	0.059	0.235	0	1
Former smoker	111919	0.210	0.407	0	1
Age	111919	40.407	11.915	19	64
Female	111919	0.537	0.499	0	1
Hispanic	111919	0.112	0.315	0	1
African-American	111919	0.115	0.319	0	1
Other race	111919	0.044	0.206	0	1
Years of schooling	111919	13.711	2.554	6	19
Family income/poverty line	111919	4.345	2.887	0	18.376
Full-time student	111902	0.036	0.186	0	1
Good health	111919	0.641	0.480	0	1
Poor health	111919	0.111	0.315	0	1
Primary care	111919	0.466	0.499	0	1
Health maintenance organization	111919	0.429	0.495	0	1
Pay without referral	111919	0.701	0.458	0	1
Referral required	111919	0.526	0.499	0	1
Directory of care providers	111919	0.704	0.456	0	1

	N	%
Privately insured through employer	90330	80.7
Medicaid	4239	3.8
Uninsured	17350	15.5

* Summary statistics are based upon one observation per MSA-year.

Table 4-3. Parameter estimates for variables of interest

	Median payment size			Payments per physician			
Dependent variable	Private	Medicaid	Uninsured	Private	Medicaid	Uninsured	N
ED utilization[1]	-0.04**	-0.017	0.067*	0.451	-1.934	-3.331	106,349
Yrs. since mammogram[1]	0.006**	0.014	0.041***	0.133	2.023*	-1.448*	19,342
Put off[2]	-0.008	0.037	0.075*	-2.584*	-4.108	-7.288***	106,243
Put off Access[2]	-.049	.241***	.104	3.689	-6.318	-7.027	105,955
Unmet needs[2]	-0.071**	-0.019	0.058	-1.308	-4.145	-2.17	106,199
Travel time[3]	0.001	0.062*	0.046*	0.538	4.082	1.624	80,555
Preventive care[2]	0.027	0.075	-0.109***	-3.406	6.773	7.723**	59,212[4]
Mammogram ever[2]	0.004	-0.025	-0.135***	4.685	-2.555	6.099	22,980
Flu shot[2]	-.032	.024	.040	.116	-10.62	-9.78*	19,831[5]
Usual source[2]	-0.029	0.056	-0.016	2.21	8.086*	-2.859	106,108
Number visits[1]	-0.011	0.027	0.067**	0.935	4.335***	-3.204	106,349

*p < .10, **p<.05, ***p<.01. [1] Estimates obtained using censored Poisson regression. [2]Estimates obtained using logit. [3] Estimates obtained using ordered logit. [4] Variable not available in CTS Round 1. [5] Variable not available in CTS Round 4. All regressions have MSA and year fixed-effects and standard errors are clustered by MSA. The null hypothesis that all coefficients (other than the MSA and year fixed-effects) are equal to zero is rejected for all models.

Table 4-4. Other covariates

Independent Variable	ED Utilization	Preventive Care
Population density	<0.001	<0.001
Risk loving	0.233***	-0.127***
Risk averse	0.017	0.007
Smoke daily	0.323***	-0.306***
Smoke some days	0.288***	-0.189***
Smoked quit	0.112***	0.058
Age	-0.021***	0.02***
Female	0.214***	0.787***
Hispanic	-0.194	0.605***
Black	0.205***	0.913***
Other race	-0.026	0.292***
Education	-0.01	0.008
Family income/poverty line	-0.036***	0.03***
Student	-0.241***	-0.056
Good health	-0.352***	0.088
Poor health	0.448***	-0.306***
Primary care	-0.023	0.033
HMO	0.085***	0.114***
Referral pay	-0.023	0.024
Referral	-0.057	-0.028
Directory	-0.007	0.005
Medicaid	0.617***	0.034
Uninsured	-0.099	-0.294***

*** p<.01

Table 4-5. Parameter estimates for variables of interest, including physician workforce per per capita

Dependent variable	Median payment size			Payments per physician			Workforce per capita		
	Private	Medicaid	Uninsured	Private	Medicaid	Uninsured	Private	Medicaid	Uninsured
ED Utilization[1]	-0.042***	-0.02	0.047*	0.082	-2.06	-2.2	122.9	142.3	290.7**
Yrs. Since Mammogram[1]	0.002	0.002	0.039***	0.118	2.276**	-1.5*	49.0	95.8	33.448
Put Off[2]	<0.001	0.083*	0.081**	-2.4	-5.627	-7.0***	-236.9**	-413.9***	-220.4**
Put Off Access[2]	-.024	.254***	.110	3.6	-5.790	-5.9	-546.2*	-449.439	-349.4
Unmet Needs[2]	-0.059**	-0.022	0.068*	-1.298	-3.549	-2.1	-259.3	-191.3	-248.1
Travel Time[3]	-0.008	0.041	0.03	0.317	4.448	1.9	275.7***	329.0**	338.7**
Preventive Care[2]	0.029	0.057	-0.094***	-3.303	7.618	7.3*	-71.8	-0.83	-167.107
Mammogram Ever[2]	-0.004	-0.037	-0.146***	4.621	-2.551	6.1	150.7	165.8	176.105
Flu Shot[2]	-0.019	0.017	-0.038	-1.064	12.485*	-1.5	396.4	779.1**	357.4
Usual Source[2]	-0.025	0.048	-0.041	1.734	8.32*	-1.9	59.1	144.2	281.2
Number Visits[1]	-0.01	0.021	0.067**	0.916	4.596***	-3.1	-21.2	10.4	-8.2

*p < .10, **p<.05, ***p<.01. [1] Estimates obtained using censored Poisson regression. [2] Estimates obtained using logit. [3] Estimates obtained using ordered logit. [4] Variable not available in CTS Round 1. [5] Variable not available in CTS Round 4. All regressions have MSA and year fixed-effects and standard errors are clustered by MSA.

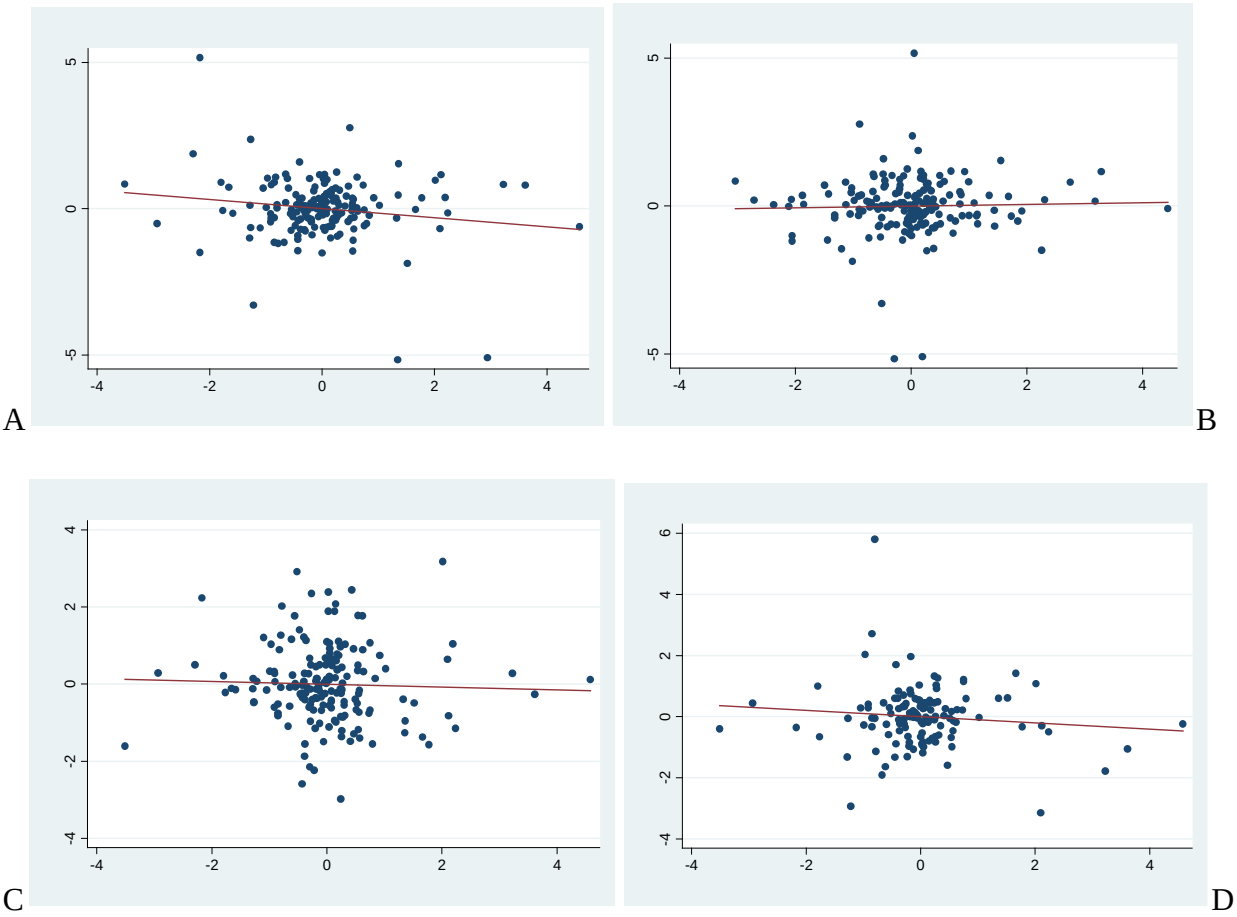
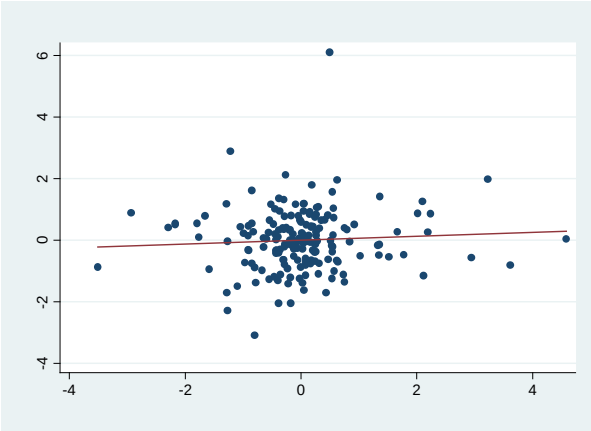


Figure 4-1. Effect of liability variables on access variables, full sample. A) ED Utilization and Median Payment Size. B) ED Utilization and Payment Frequency. C) Yrs. Since Mammogram and Median Payment Size. D) Yrs. Since Mammogram and Payment Frequency. E) Put Off and Median Payment Size. F) Put Off and Payment Frequency. G) Put Off Access and Median Payment Size. H) Put Off Access and Payment Frequency. I) Unmet Needs and Median Payment Size. J) Unmet Needs and Payment Frequency. K) Travel Time and Median Payment Size. L) Travel Time and Payment Frequency. M) Preventive Care and Median Payment Size. N) Preventive Care and Payment Frequency. O) Mammogram Ever and Median Payment Size. P) Mammogram Ever and Payment Frequency. Q) Flu Shot and Median Payment Size. R) Flu Shot and Payment Frequency. S) Usual Source and Median Payment Size. T) Usual Source and Payment Frequency. U) Number of Visits and Median Payment Size. V) Number of Visits and Payment Frequency.



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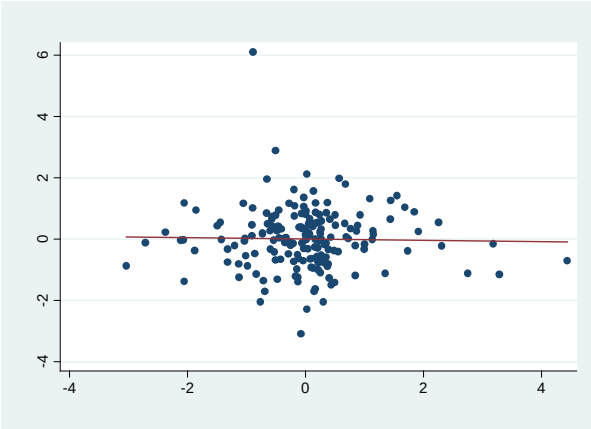

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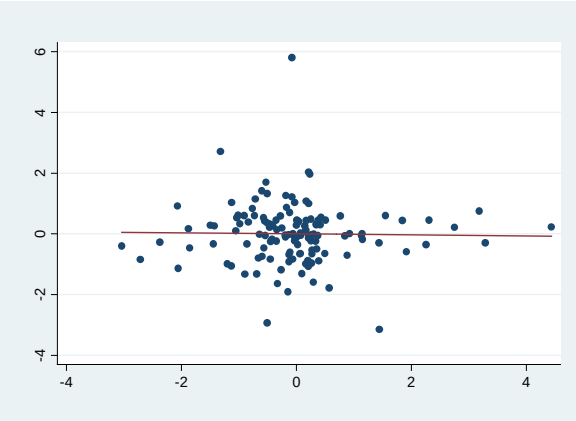

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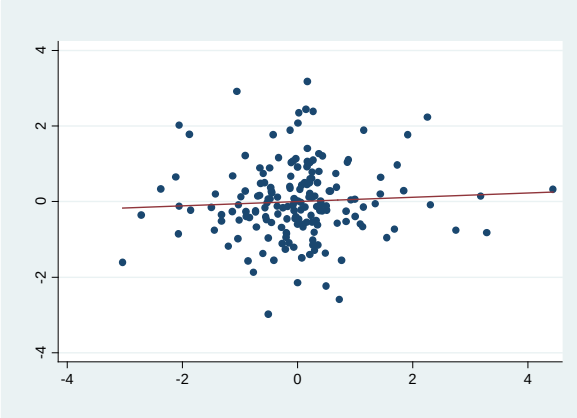

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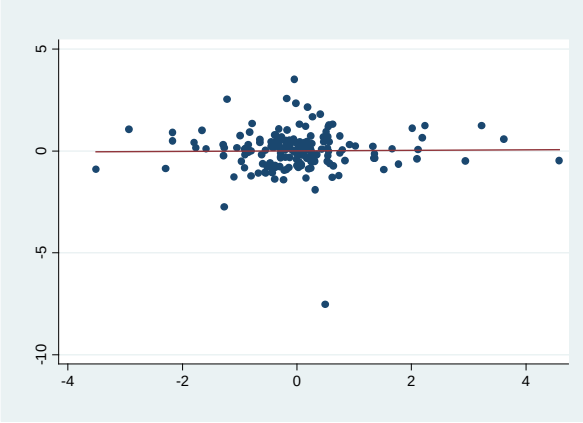

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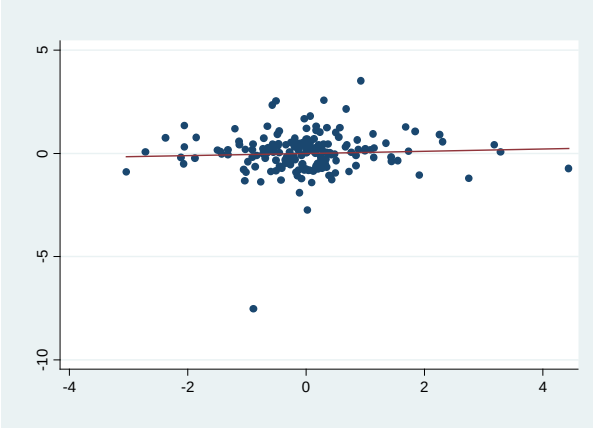

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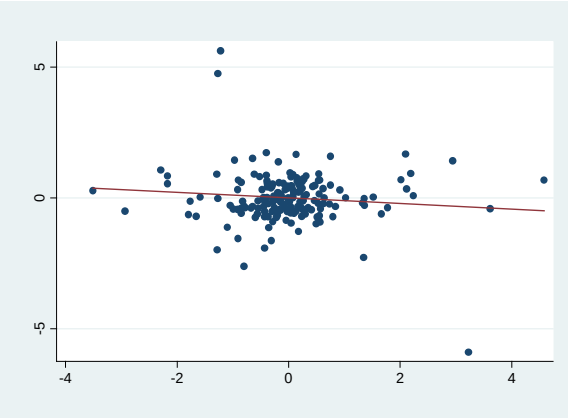

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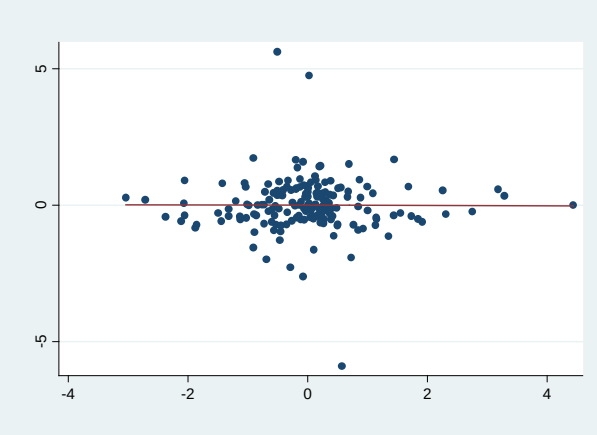

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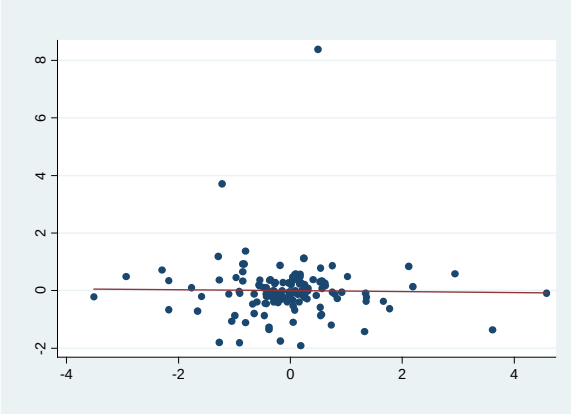

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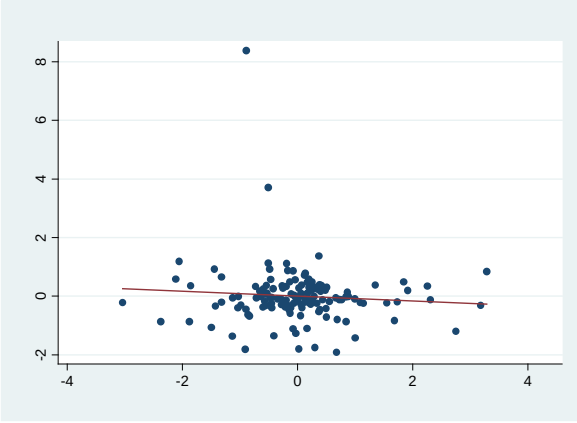

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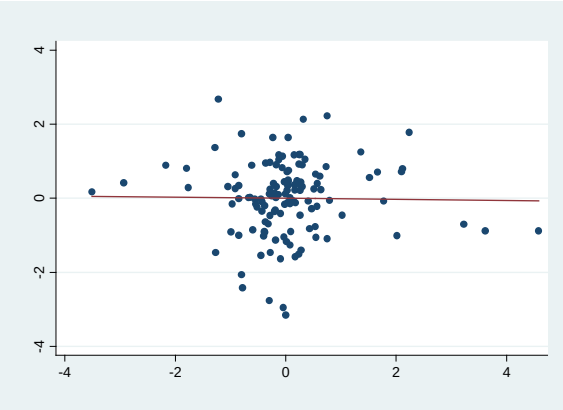

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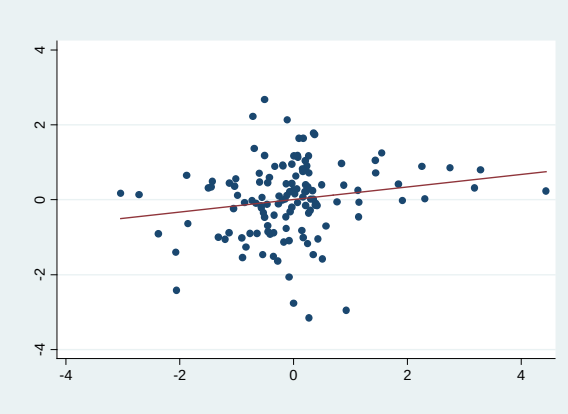

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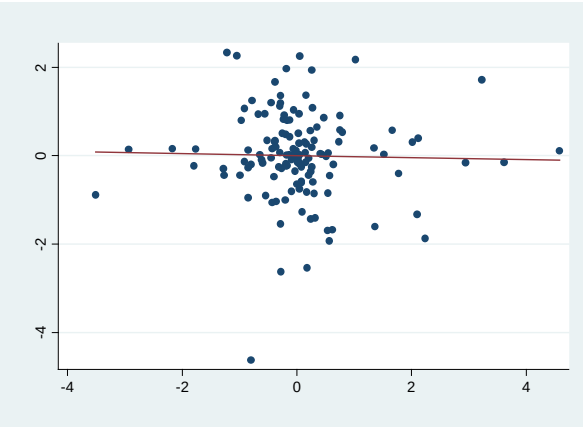

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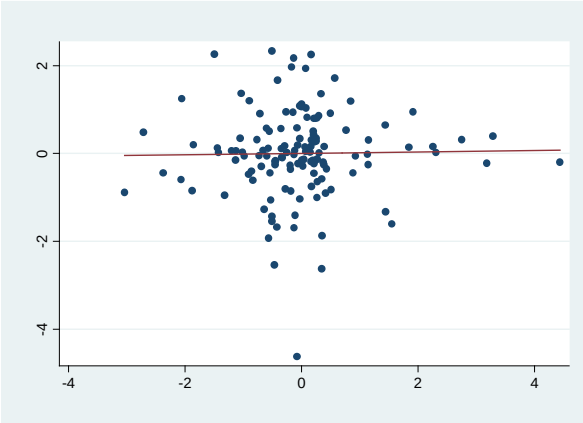

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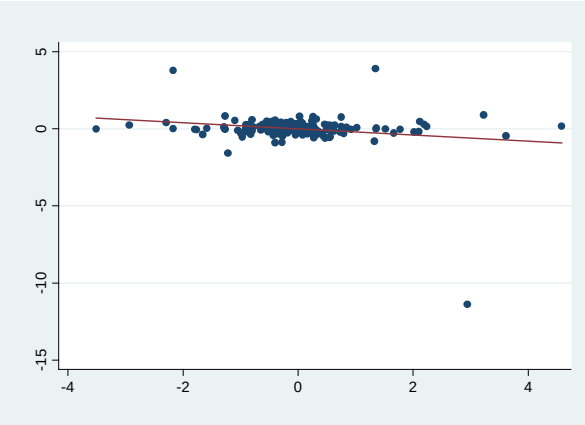

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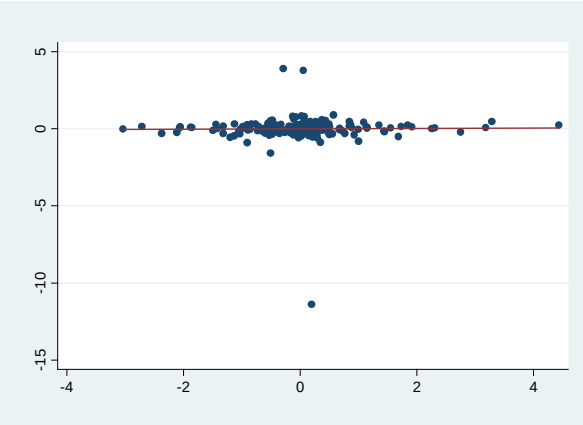

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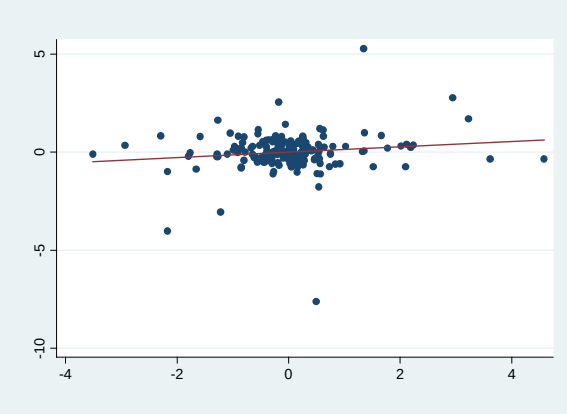

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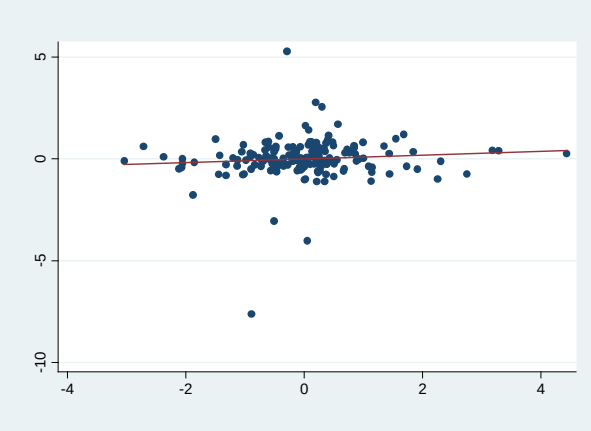

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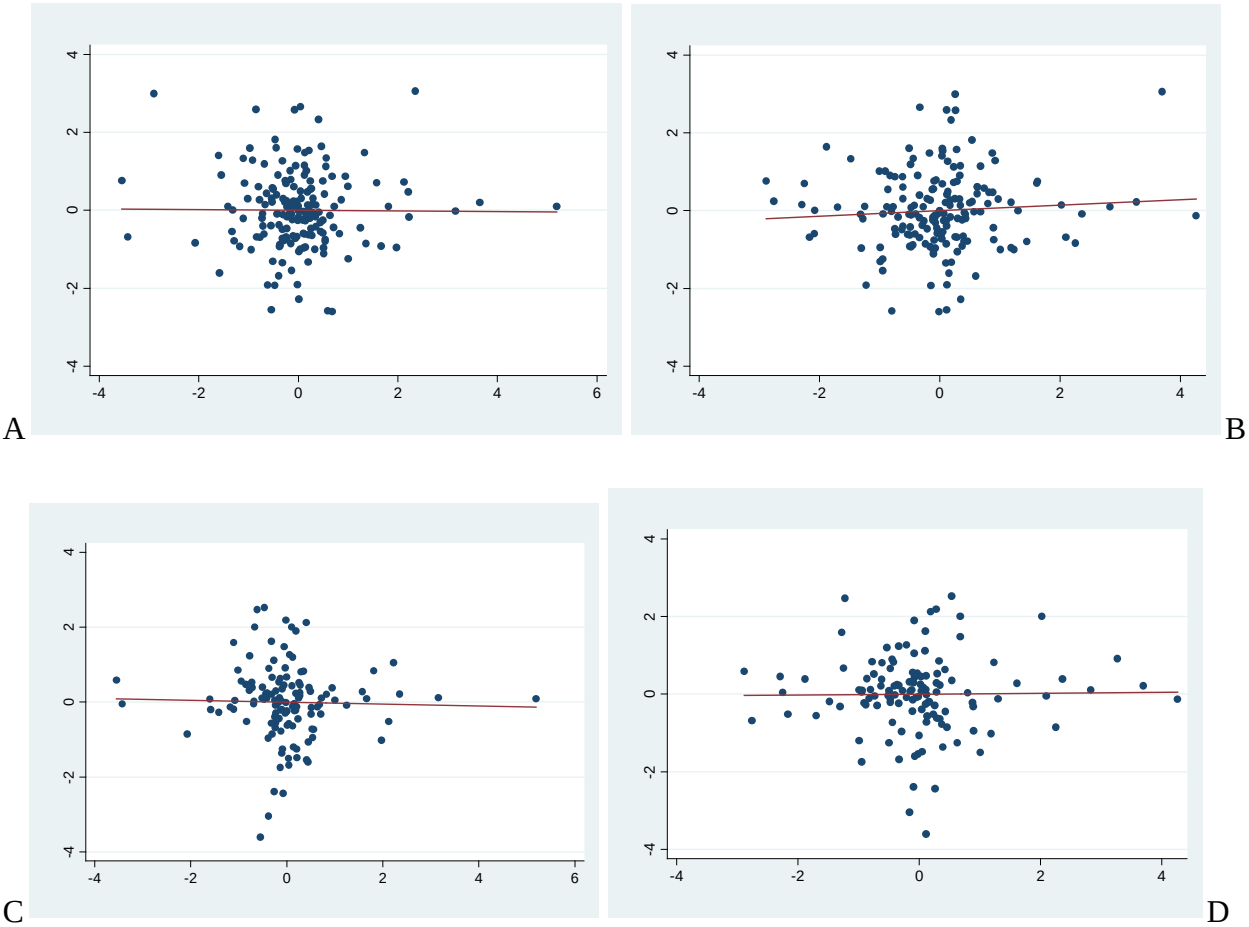
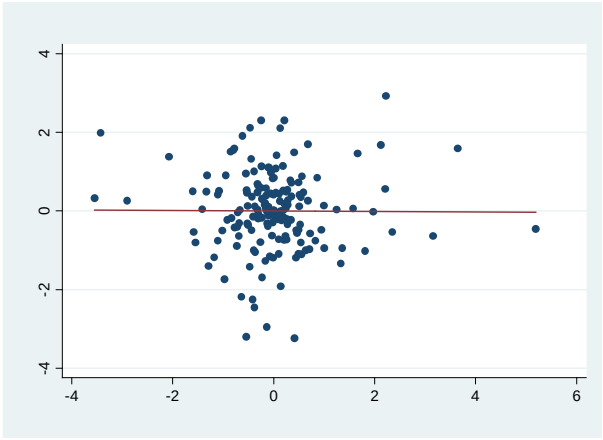


Figure 4-2. Effect of liability variables on access variables, uninsured subsample. A) ED Utilization and Median Payment Size. B) ED Utilization and Payment Frequency. C) Yrs. Since Mammogram and Median Payment Size. D) Yrs. Since Mammogram and Payment Frequency. E) Put Off and Median Payment Size. F) Put Off and Payment Frequency. G) Put Off Access and Median Payment Size. H) Put Off Access and Payment Frequency. I) Unmet Needs and Median Payment Size. J) Unmet Needs and Payment Frequency. K) Travel Time and Median Payment Size. L) Travel Time and Payment Frequency. M) Preventive Care and Median Payment Size. N) Preventive Care and Payment Frequency. O) Mammogram Ever and Median Payment Size. P) Mammogram Ever and Payment Frequency. Q) Flu Shot and Median Payment Size. R) Flu Shot and Payment Frequency. S) Usual Source and Median Payment Size. T) Usual Source and Payment Frequency. U) Number of Visits and Median Payment Size. V) Number of Visits and Payment Frequency.



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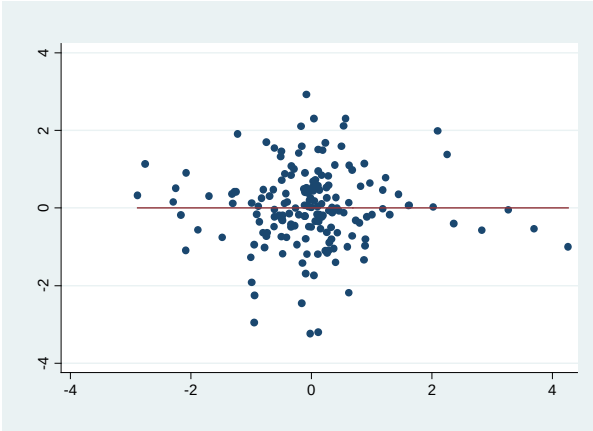

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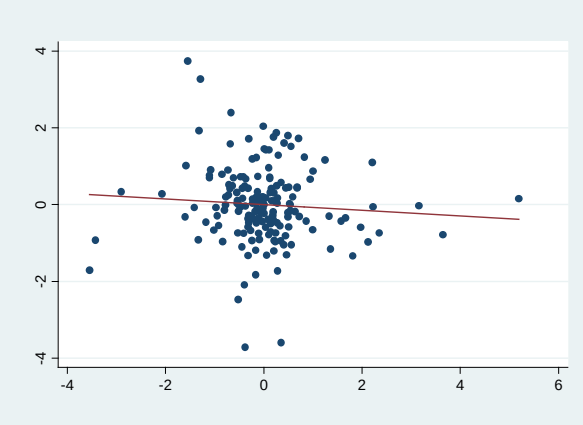

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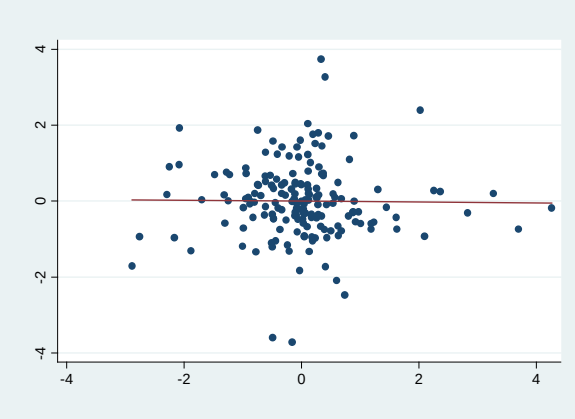

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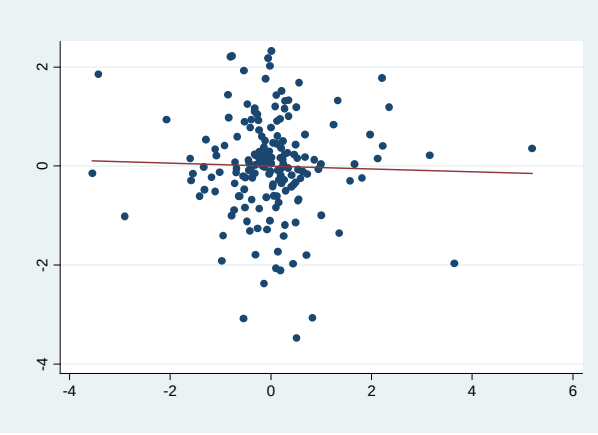

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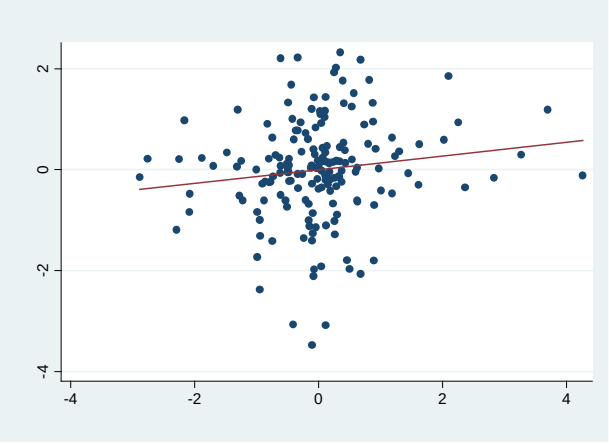

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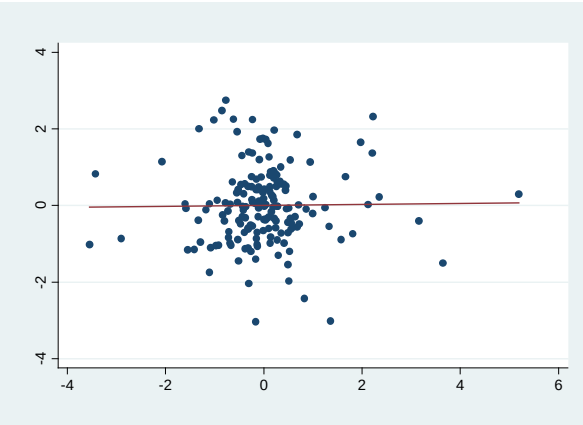

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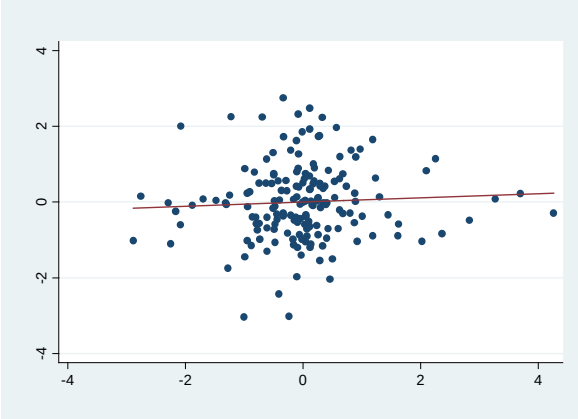

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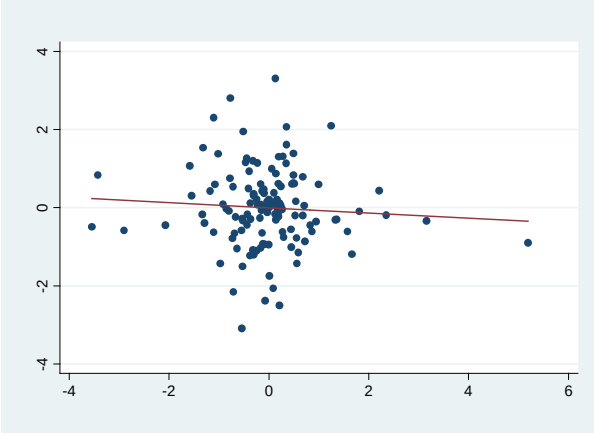

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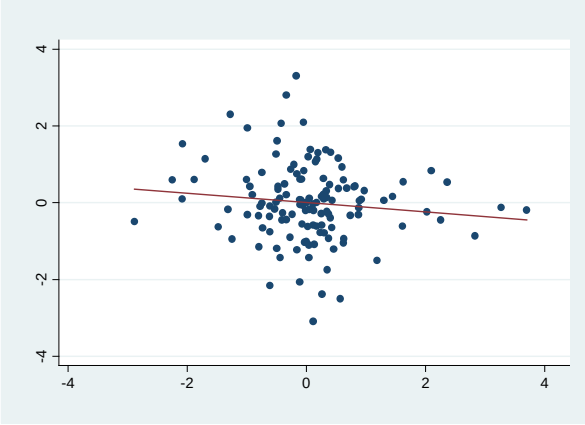

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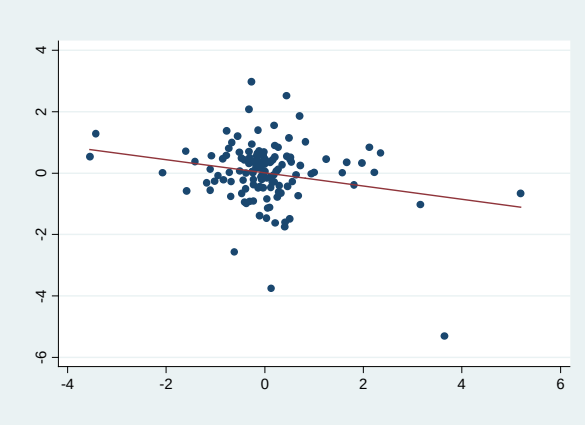

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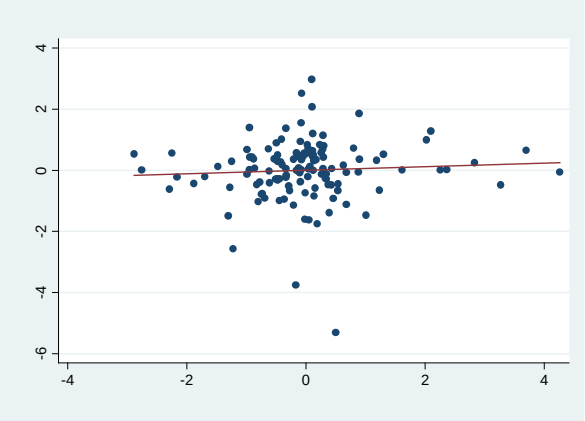

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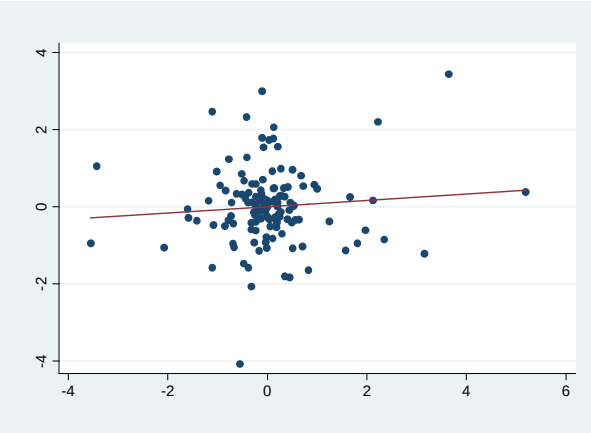

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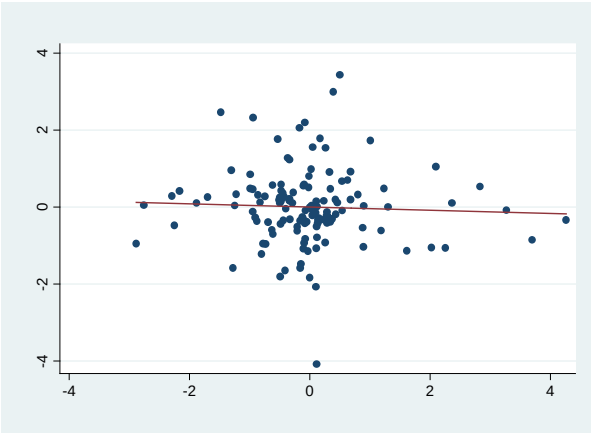

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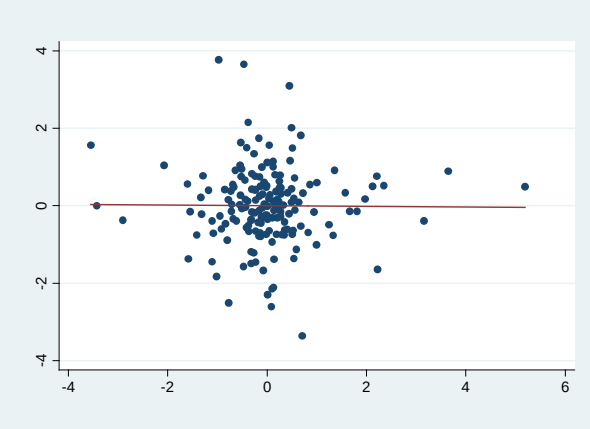

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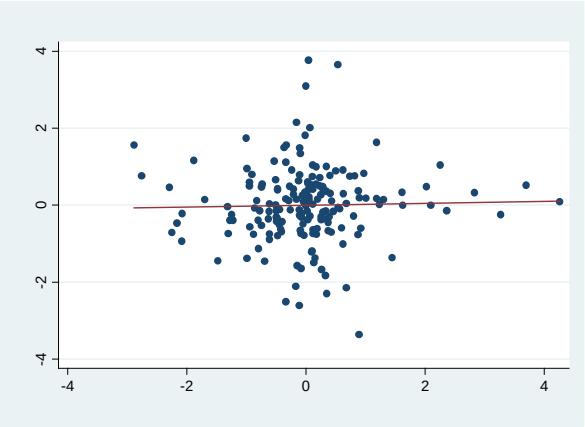

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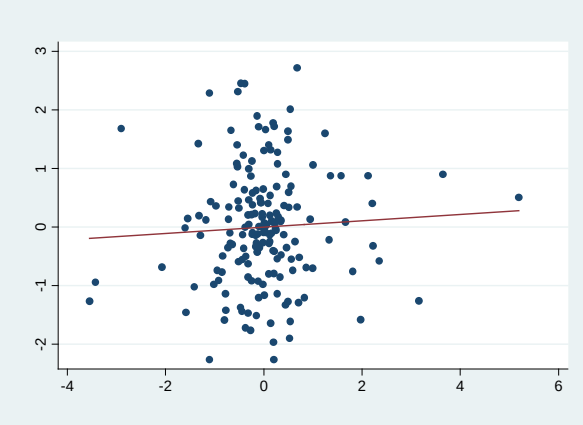

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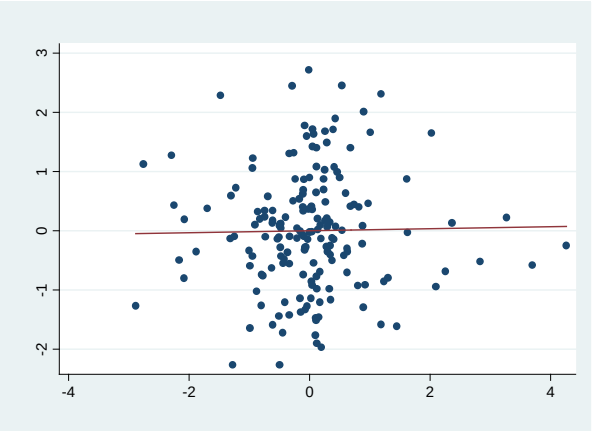

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CHAPTER 5
CONCLUSION

The purpose of this work is to examine the effects of medical malpractice liability on the delivery of health care in the United States. I examine three facets of this interaction: liability's effects on physician workforce per capita, physician compensation, and access to care. I find that increases in malpractice liability reduce state-level physician workforce per capita for some physician specialties. Also, physicians in high-liability metropolitan areas are compensated through higher incomes for bearing liability risk; at the same time, however, physicians' incomes net of malpractice premiums tend to fall as a result of higher premiums coupled with stagnant reimbursement rates. Finally, for some access to care measures, larger malpractice settlement payments result in diminished access among the uninsured. There is some evidence that diminished access results from physicians' responses (i.e., turning away high-risk patients or patients with low-reimbursement insurance) to liability risk.

The evidence presented herein has several policy implications. Measures, such as tort reform, designed to limit the liability risk that physicians face may have several effects. These include maintaining the primary care physician workforce per capita, containing health care costs by removing the compensating differential for physicians practicing in high-liability areas, and enhancing access to care. Also, the evidence presented regarding access to care suggests that Medicaid patients have better access than the uninsured. Policymakers whose goals include bettering access among the uninsured might consider expansions of Medicaid programs to the working uninsured, who account for the majority of uninsured in the United States.

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