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MEDICAL MALPRACTICE AND FAMILY MEDICINE:

PHYSICIAN SUPPLY, CHOICES, AND PATIENT OUTCOMES

By

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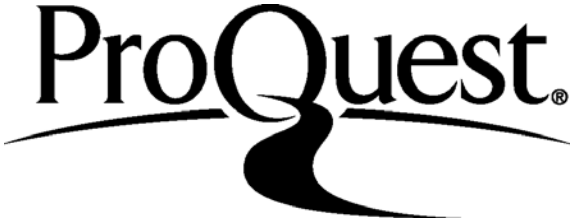

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I would like to dedicate this dissertation to the students at Ricks Institute in Liberia who taught me to be an economist that sees issues from multiple perspectives and to be a person that never stops learning or loving.

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ABSTRACT

The shortage of family medicine physicians is a predominant issue in the medical field. This dissertation investigates whether medical malpractice liability reforms impact defensive medicine among general practice physicians. The first chapter, "Medical Malpractice Liability Reform and Family Medicine Physician Supply," explores the decision made by physicians to decline to provide care that has medical value in order to reduce the risk of malpractice liability. This analysis focuses on behaviors that affect family medicine physician supply by assessing the differential impact of states that adopt medical malpractice reforms compared to those without reforms from 1992-2007. The reforms of bordering states are used in instrumental variables estimation to account for the endogeneity of reforms. Statistical tests suggest that these instruments are likely valid. Results indicate that noneconomic damage caps, punitive damage caps, and collateral source reform increase the supply of family medicine physicians per state while joint and several liability decreases physician supply. However, these results are mostly insignificant. Furthermore, this analysis distinguishes between temporary and permanent reforms and finds that only permanent punitive damage caps have a statistically significant impact on family medicine physician supply.

The second chapter, "Movement and Reorganization: Negative Defensive Medicine among Family Medicine Physicians," measures the effect of liability reforms on practice choices of family medicine physicians including location, mode, and service offerings. Existing literature considers the effect of tort reforms on physician supply. However, physicians may employ defensive behaviors that may not be observed when testing for response at the aggregate level. For example, physicians may move from solo practice to group practice to share the liability burden. Physicians can also move their practice to a more defendant friendly legal environment by moving to a different state. They may also decline to provide certain types of care that are associated with a higher risk of medical malpractice suits, like obstetrical care. Using a random sample of family medicine physicians in the United States, this paper models these three choices using multinomial logit and fixed effects logit. Results indicate that family medicine physicians alter their choice of location and practice mode with the implementation of reforms in substantial ways.

Lastly, the third chapter, "Short-term and Long-term Effects of Liability Reform on Preventable Disease," models the short-term and long-term effects of liability reform on two patient outcomes,

obesity and type II diabetes. Reforms impact these patient outcomes indirectly through changes to both positive and defensive medicine strategies. Reforms that reduce malpractice pressure on physicians will also reduce the practice of negative defensive medicine. This increases access to care and will likely lead to an increase in prevalence rates, initially. However, over time as physicians counsel these previously untreated patients, changes will be made to diet, exercise, and lifestyle. If these changes are effective, patients may lose weight which will decrease the prevalence of obesity and the prevalence of type II diabetes. Models use differences-in-differences and state and time fixed effects to assess this hypothesis. Results indicate that reforms impact defensive medicine, especially avoidance behaviors, with no changes to patient health in the long-run.

CHAPTER 1

INTRODUCTION

This dissertation investigates whether medical malpractice liability reforms eliminate defensive medicine practices among family medicine physicians. Several studies [Kessler and McClellan (1996), Nesbitt (2002), Dubay, Kaestner and Waidmann (1999), Dubay, Kaestner and Waidmann (2001), Matsa (2007), and Currie and MacLeod (2008)] have shown that physicians often practice defensive medicine to avoid the burdens of the medical malpractice system. Though some of these studies have investigated differences by specialty, none have considered the possibility that malpractice reforms may impact family medicine physicians differently than other specialties.

There are three important questions in this area. First, how does the medical malpractice liability system impact family medicine physician supply at the state level? Since medical malpractice liability reforms are instituted at the state level, this is the first level of analysis to consider. It is reasonable to expect that given other factors, physician supply will be the largest in states with less burdensome medical malpractice liability. It is also reasonable to expect that states that institute tort reforms that ease the burden of medical malpractice experience an increase in family medicine physician supply. My research will assess the differential impact of reforming states compared to states without reforms, using the differences-in-differences approach, combined with state and time fixed effects. This will enable comparisons with previous literature, which looks at overall physician supply [Kessler, Sage and Becker (2005) and Matsa (2007)].

Second, how do changes in liability reforms impact family medicine physician practice choices like location, mode, and service offerings? This portion of the analysis will focus on specific avoidance behaviors that alter a physician's availability to patients. This includes physician decision-making like moving from non-group to group practice, or from a malpractice climate in one state to another. It is also important to explore whether family physicians choose to decline to provide obstetrical care due to medical malpractice risks. This may disproportionately affect patients in rural communities. Physician level data from the Physician Masterfile (maintained by the Ameri-

can Medical Association and sold by Medical Marketing Service, Inc) will provide a unique insight into physician choices by tracking them from 1992-2007.

Third, this research will consider how defensive medicine influences patient health outcomes. This study will assess two health outcomes that may be influenced by the availability of family medicine physicians such as obesity rates, and type II diabetes rates. The methods employed to study this will be similar to those used in several studies that look at the impact of malpractice pressure or tort reform on patient health outcomes [Kessler and McClellan (1996); Dubay, Kaestner and Waidmann (1999, 2001); Currie and MacLeod (2008)].

1.1 Tort Law, Tort Reform, and Medical Malpractice Liability

Tort is a civil wrong which causes a person to suffer a loss or harm and often results in legal liability for the party who commits the civil wrong, called a tort feasor. Tort reform is a term used to describe legal changes that are intended to reduce tort litigation or damages. Most tort reforms cover changes to the law over all torts, product liability, defamation, negligence, fraud, and legal and medical malpractice, just to name a few. Generally, tort reform is less appropriate when the variation in claims is due to the variation in the frequency and type of medical treatments. However, "if variation in claims is due primarily to variation in incentives created by the legal system, then tort reforms which reduce these incentives will reduce claim costs (Danzon 1984, 116).

Medical malpractice liability is a specific type of tort. In the United States, the medical malpractice liability system provides compensation to patients and their families who have suffered an injury or death due to professional negligence of a health care provider where treatment falls below the standard of care. Ideally, this system would achieve the following goals. First, it would provide restitution to patients harmed by professional negligence. Second, it would discourage negligence among health care providers. However, critics of the system, including many health care providers, describe the system as exceedingly expensive, adversarial, unpredictable, and inefficient. Kessler and McClellan (1996) summarize this sentiment; "on the one hand, these penalties for malpractice may deter doctors and other providers from putting patients at excessive risk of adverse health outcomes. On the other hand, these penalties may also drive physicians to be too careful - to administer precautionary treatments with minimal expected medical benefit out of fear of legal liability - and thus to practice 'defensive medicine' " (354).

There are both monetary and non-monetary costs of medical malpractice litigation. Most physicians hold malpractice insurance policies to cover the direct monetary costs of litigation (settlement, legal fees, etc.). Malpractice insurance premiums depend on coverage limits, specialty, practice location, and whether or not the physician performs high-risk procedures such as surgery. Premiums are not experienced rated or physician rated. However, the non-monetary costs of malpractice litigation are often more detrimental than the monetary costs. It is often difficult to repair the damage done to a physician's reputation after a medical malpractice suit is filed, regardless of whether or not the physician is found liable. Also, many physicians spend a large amount of time away from patients or the office to dispute a suit. Zuckerman, Bovbjerg and Sloan (1990) find that at the median, a physician loses three to five days to defend a malpractice suit, which translates to a loss of $3,357 to $5,595 (473). However, some physicians in their study lost more than twenty days from practice because of suit (Zuckerman, Bovbjerg and Sloan 1990, 474). This opportunity cost is often not included in estimates of litigation costs. Not only does the physician lose business for those days away, it may also cause patients to look for another physician or deter new patients.

1.2 Defensive Medicine

1.2.1 What is defensive medicine?

Defensive medicine is any action taken by health professionals to reduce their legal liability. There are two types of defensive medicine. Positive defensive medicine occurs when a physician over provides care through excessive testing or unnecessary procedures. That is, a provider orders tests or procedures whose expected marginal costs exceed expected marginal benefits. Negative defensive medicine occurs when a physician declines to provide services whose expected marginal benefits outweigh expected marginal costs. This includes refusing to perform certain procedures or treat certain diseases or patients (Kessler, Sage and Becker 2005, 2623). It also includes physicians who discontinue certain services because of legal liability or those who alter the make up of their practice to reduce liability. This research focuses on negative defensive behaviors of family medicine physicians, though it is possible that family medicine physicians also practice positive defensive medicine to reduce the risk of lawsuit. Negative defensive medicine, or avoidance behaviors, has "received significantly less academic attention than defensive medicine manifested in excessive testing or unnecessary procedures" (Kessler, Sage and Becker 2005, 2623).

1.2.2 How is defensive medicine measured?

Some studies consider certain aspects of medical practice in an attempt to measure defensive medicine. Kessler and McClellan (1996) and Dubay, Kaestner, and Waidmann (1999) and (2001) use various health outcomes to measure the effects of defensive medicine. Other studies have used changes in physician supply to measure the extent of defensive medicine by geographic region [Kessler, Sage and Becker 2005, Matsa 2007]. I will measure defensive medicine in family medicine in three ways; assessing changes in family medicine physician supply, examining physician decision-making in three areas, practice location, practice mode, and service offerings, and determining the impact on health outcomes. This should provide a comprehensive analysis of several different forms of defensive medicine among family medicine physicians.

1.2.3 Why are family medicine physicians different?

Family Medicine, formerly family practice or general practice, is a broad specialty in the practice of medicine. Physicians in this specialty see patients in all stages of life. They have a broad base of medical knowledge to treat patients with common ailments and to screen patients and refer them to specialists in complicated cases. Residency training in this specialty includes rotations in internal medicine, pediatrics, obstetrics and gynecology, psychiatry, and geriatrics.[1] Family physicians may pursue fellowships in several fields, including emergency medicine, geriatric medicine, sports medicine, obstetrics, sleep medicine, women's health, among others.[2] Family medicine physicians comprise a large portion of the physician workforce available in rural areas (Rosenblatt and Hart, 2000). They are often a patients first or only point of contact with a medical professional. Existing literature [Matsa (2007), Klick and Stratmann (2007)] suggests that in many cases, family medicine physicians and other "low-risk" specialties are less sensitive the malpractice environment due to closer patient relationships and other factors. This study attempts to assess whether this assumption in the literature is valid. On the other hand, concerns in the medical community indicate that family medicine physicians may have less ability to cope with the risking costs of malpractice pressure. With a relatively low income, and falling reimbursement rates, additional malpractice pressure could be a contributing factor to the overall shortage of primary care physicians.

[1] https://www.theabfm.org/about/abfmbrochure.aspx
[2] https://nf.aafp.org/Directories/Fellowship/Search

CHAPTER 2

MEDICAL MALPRACTICE LIABILITY REFORM AND FAMILY MEDICINE PHYSICIAN SUPPLY

2.1 Introduction

Physicians are keenly aware of the state of medical malpractice, for good reason. Jena et al. (2011) estimate that among all physicians, approximately 8 percent will face a malpractice claim in a given year. By the age of 65, a large portion of all physicians, more than 75 percent, can expect to face at least one malpractice claim during their career (Jena et al. 2011). Whether or not a claim ends in payment, medical malpractice law suits are costly to physicians. Seabury et al. (2013) estimate that "the average physician spends 50.7 months - or almost 11 percent - of an assumed forty-year career with an unresolved, open malpractice claim" (111). This expectation of being sued and the large time and reputation costs can lead to avoidance behaviors or defensive medicine among physicians. Therefore, while the tort system provides an avenue for those harmed to seek restitution, it may also induce unintended side effects.

Much of the existing research on defensive medicine focuses on positive defensive medicine. This type of defensive medicine occurs when a physician over provides care through excessive testing or unnecessary procedures. That is, a provider orders tests or procedures whose expected marginal costs exceed expected marginal benefits for the patient, but reduce the provider's liability. However, negative defensive medicine is another possible side effect of a burdensome medical malpractice system. Negative defensive medicine occurs when a physician declines to provide services whose expected marginal benefits for the patient outweigh expected marginal costs. For example, physicians who discontinue certain services because of legal liability or those who refuse to see certain patients to reduce liability may be practicing this type of defensive medicine. These "avoidance behaviors" have received less academic attention than excessive testing or procedures (Kessler, Sage and Becker 2005).

Though some studies have investigated differences by specialty, most generally assume that the medical malpractice system is more burdensome on "high-risk" specialties like surgery, obstetrics,

and gynecology, than "low-risk" specialties like family medicine, pediatrics, psychiatry, and dermatology. For this reason, this study focuses on the impact of liability reform on a "low-risk" specialty, family medicine, a significant portion of the physician workforce available in rural areas (Rosenblatt and Hart 2000). These physicians are often a patient's first or only point of contact with a medical professional. Certain factors may make family medicine physicians particularly vulnerable to the stress of coping with the rising costs of liability pressure. On average, family medicine physician income is $189,000 compared to other specialties like general surgery, $343,000, or radiology, $358,000 (Smith 2012). This comparatively low income makes general practitioners particularly vulnerable to financial difficulties like rising malpractice premiums, or falling reimbursement rates. However, "high-risk" specialties are also more likely to face a suit in a given year. Approximately 16 percent of physicians in high-risk specialties face a claim each year, compared to 5.2 percent of family medicine physicians (Jena et al. 2011). Malpractice premiums are also higher for this group of physicians. Therefore, this analysis focuses only on family medicine physicians to assess whether negative defensive medicine is a problem in low-risk specialties. It does not assess the relative effect among various specialties.

A key question in this investigation asks how the medical malpractice liability system impacts family medicine physician supply at the state level. A decline in physician supply may signal that negative defensive medicine is a problem in a state. Since medical malpractice liability reforms are instituted at the state level, this is the first level of analysis to consider. Existing theory (Zuckerman, Bovbjerg and Sloan 1990) and empirical work [Kessler, Sage and Becker (2005); Hellinger and Encinosa (2005); Klick and Stratmann (2007)] suggests that given other factors, physician supply will be the largest in states with less burdensome medical malpractice liability, largely due to lower malpractice premiums and lower expected risks of litigation. Thus, it may be reasonable to expect that states that institute tort reforms that ease the burden of medical malpractice will experience an increase in family medicine physician supply.

2.2 Literature Review

There have been four studies that examine the impact of tort reform on physician supply. Results from studies have generally found that certain tort reforms increase the supply of physicians. Kessler, Sage and Becker (2005) find that states that adopt "direct" reforms have a greater increase

in the overall supply of physicians than states that do not adopt direct reforms. Direct reforms are reforms that affect the distribution of awards, by either capping the upper tail or shifting down the mean. Specifically, "three years after adoption, direct reforms increased physician supply by 3.3 percent" (Kessler, Sage and Becker 2005, 2618), controlling for unobserved effects across states, population, health markets, and political characteristics. Matsa (2007) reports that caps on noneconomic damages, awards that compensate for pain and suffering, do not affect the supply of physicians for the average resident of states that adopt reforms, but they do increase the supply of "frontier rural specialist physicians" by over 10 percent. Hellinger and Encinosa (2005) detail that counties in states adopting caps on noneconomic damages have 2.2 percent more physicians per capita on average, and 3.2 percent more physicians per capita in rural counties. Klick and Stratmann (2007) report that caps on noneconomic damages are the only reform with a statistically significant impact on the per capita number of physicians. Furthermore, they find that "this effect is concentrated among only those specialties that face the highest litigation exposure" (Klick and Stratmann 2007, S121).

There are three main problems in the existing literature. First, Kessler, Sage and Becker (2005), Hellinger and Encinosa (2005), and Matsa (2007) suffer from potential endogeneity problems. If states implement reforms in response to the supply of physicians, each of these studies which estimate the effect of tort reforms will have biased results. The effect of reforms will be understated if decreases in physician supply encourage states to adopt reforms and overstated if both effects are caused by an omitted factor. Klick and Stratmann (2007) suggest that only physicians practicing in "high-risk specialties" are affected by a medical malpractice crisis, and therefore, "physicians in low risk specialties represent a contemporaneous within-state comparison group" (S122). By using this contemporaneous within-state comparison group, Klick and Stratmann (2007) claim to overcome the overwhelmingly present endogeneity problem in previous work. However, if in fact physicians in low-risk specialties are affected by a medical malpractice crisis, the problem of endogeneity may still exist. This highlights a second issue in the existing literature.

Some authors suggest that physicians in "low-risk" specialties are less affected by issues of medical malpractice or in the least, affected differently than those in "high-risk" specialties. For example, Matsa (2007) argues that reforms limiting malpractice damage awards will not affect general practice physicians because the direct financial effect of caps may differ among specialties,

closer patient relationships may reduce family medicine physician sensitivity to the malpractice environment, and the increasing delivery of primary care by internists and pediatricians drives down the number of family medicine physicians nationwide. Although it may be true that the effect of tort reforms varies based on a physician's specialty, this does not necessarily imply that some specialties are protected from the problems of an overbearing medical malpractice system. Furthermore, physicians in all specialties report that they spend less time with patients and more time navigating cumbersome paperwork and dealing with administrative tasks (Rabin 2014). This likely limits the extent to which close patient relationships deter malpractice law suits. Finally, there is no evidence that the number of family medicine physicians alters how these physicians are impacted by medical malpractice. Though a smaller number of physicians may suggest there are less claims against these physicians, it does not imply that they do not experience benefits when malpractice pressure is decreased. In addition, relatively low-risk specialties also tend to be relatively low-income specialties. So, in a relative sense, the threat of litigation to a low-risk doctor's wealth may be as great or greater than the high-risk doctor's wealth.

Lastly, each of the aforementioned studies which examine the effect of tort reform on physician supply fail to consider the important institutional detail that not all reforms are permanent. Grace and Leverty (2013) suggest that the impact of reforms "depends on expectations about its future constitutionality" (1253) and that "given the significant risk of reform nullification" (1254) all parties impacted by reforms may be reluctant to make behavior or policy modifications until a reform's constitutionality is more certain. Their analysis focuses on insurance companies from 1985 to 2005. They find that permanent reforms significantly lower losses for insurance companies and increase profitability. In addition, they find that permanent reforms decrease malpractice premiums, and this effect is much larger than the effect observed using the binary indicator variables in previous work. Furthermore, temporary reforms never significantly impact these measures for insurance markets. Physicians, like insurance companies, are not likely to respond to tort reforms unless there is a reasonable expectation that they will last. Previous studies [Kessler, Sage and Becker (2005); Hellinger and Encinosa (2005); Matsa (2007); Klick and Stratmann (2007)] only account for this reluctance by using lagged values of reform variables. This paper improves on the existing literature in three ways. First, this analysis tests the general presumption that low-risk specialties are unaffected by tort reform. Second, it controls for the endogeneity that is inadequately

addressed in previous literature. Lastly, it tests to see if the failure to consider the difference between temporary and permanent reforms may explain the lack of a measurable response among low-risk physicians to changes in tort law.

2.3 Econometric Models

Since malpractice premiums are fixed costs and are not physician-rated, physicians can do little to offset these costs. Therefore, their net income will be higher in states with lower malpractice premiums. Thus, states with more burdensome medical malpractice climates are less attractive to physicians. States with higher claim frequency or a history of large settlement awards have higher malpractice premium rates. Therefore, if tort laws and legal precedent tend to favor the plaintiff, claim frequency will be large, premiums high, and these states will likely deter physicians. Tort reforms can have several effects. "Changes in legal rules that reduce costs or increase the payoff to suit tend to increase the stock of potential claims. Changes which expand the scope of liability have a similarly effect, to the extent that changes are applied retroactively" (Danzon 1984, 122).

Existing theory and empirical evidence suggests that states enacting reforms should see an increase in physicians (Matsa, 2007). The theoretical result relies on Bertrand-Nash competition models of physician services which model the number of physicians in a given market as dependent on the level of fixed costs and economic profit, the price mark-up, and the market demand for health care. Zuckerman, Bovbjerg and Sloan (1990) find that reforms reduce malpractice premiums, thereby reducing fixed costs, or the insured costs of litigation. According to Matsa (2007), this means reforms will increase the number of physicians in a given market. The empirical strategy here will attempt to test this hypothesis. The differences-in-differences estimator identifies the effect of state malpractice reforms by comparing the change in family medicine physician supply in states that enacted reforms between 1992 and 2007 to the change in family medicine physician supply in states that did not. The treatment group includes states that adopted tort reforms during the panel. This approach controls for fixed differences between states, and national trends affecting all states. Several different specifications for the adoption of tort reforms will be used to verify the results.

The first set of regressions models the number of family medicine physicians in state s at time t as a function of state and time fixed effects, time-varying state characteristics and state level tort

reforms. Because of the non-normality of this distribution, the natural logarithm of the dependent variable is used in our regressions. Figures 1 and 2 in the Appendix demonstrate the need for this transformation graphically. Once the natural logarithm of the number of family medicine physicians is used, traditional OLS assumptions are valid. These regressions take the following form:

$$\ln(N_{st}) = \alpha_s + \theta_t + \beta L_{st} + \delta \ln(P_{st}) + \eta R_{st} + \lambda M_{st} + \mu X_{st} + u_{st} \tag{2.1}$$

where N_{st} is the number of family medicine physicians in state s in year t, and α_s and θ_t are state and time fixed effects. P_{st} is the population in state s in year t. R_{st} is the number of civilian family medicine residency programs in state s in year t, and M_{st} is a measure of managed care usage, health maintenance organization (HMO) enrollment per capita in state s in year t. X_{st} is a set of other covariates in state s in year t which include income per capita, and the civilian unemployment rate. L_{st} denotes the specification of state liability law reforms. It is the explanatory variable of interest for this study. For each reform, there is a binary variable where a one indicates that a reform was active, and a zero indicates otherwise. To correct for heteroskedasticity and serial dependence in unobserved factors, all standard errors are clustered at the state level. This approach has one major disadvantage. The effects of reforms adopted in 1992 that are either upheld or never challenged by the courts cannot be distinguished from other time-constant differences between states.

Although the models include several variables to control for other factors that impact the supply of family medicine physicians, there are some potentially important factors that are unobserved (e.g., the price of physicians services, preferences over medical care, the structure of health care provision). If these omitted variables are correlated with the implementation of tort reforms, ordinary least squares estimates will be biased. To attempt to address this estimation issue, we estimate the models using state-fixed effects. This controls for the problem of correlation of omitted variables from entering into our estimates of tort reform to the extent that these omitted factors are relatively time-invariant. Another type of endogeneity, simultaneity, may also be a problem in these models if the number of family medicine physicians and the implementation of tort reforms are determined jointly. Therefore, the second set of regressions treats tort reform variables as endogenous, using the tort laws of bordering states as instruments.

Two-stage least squares can be used to treat L_{st} as a set of endogenous regressors. In the first stage, each endogenous reform is modeled as

$$L_{st} = \gamma W_{st} + \omega Z_{st} + e_{st} \tag{2.2}$$

where W_{st} includes all the exogenous variables from the original model and Z_{st} includes instruments mentioned above. The second stage uses fitted values of L_{st} from the first stage regressions in the original model to estimate the impact of legal reforms on the supply of family medicine physicians. Second stage regressions include fixed effects in some cases, but not all. For estimation without state fixed effects to be valid, three requirements must be met. First, the instruments must be contemporaneously exogenous, that is $E(Z'_{st}u_{st}) = 0$, $t = 1992, \ldots, 2007$. This is the weakest exogeneity assumption, and it requires that instruments are not correlated with the idiosyncratic error in the same time period. Second, there must be sufficient correlation on average among the instruments and the endogenous regressors, that is $Rank \sum_{t=1992}^{2007} E(Z'_{st}L_{st}) = k$. Third, instruments cannot be correlated with state fixed effects, α_s. When state fixed effects are included, the instruments must be strictly exogenous, that is $E(Z'_{st}u_{sr}) = 0$, $\forall t$ and $\forall r$. Strict exogeneity is the strongest exogeneity assumption. It requires that the instruments are not correlated with the idiosyncratic error in any time period. If the strict exogeneity assumption holds, contemporaneous exogeneity will also hold. The next section will discuss which exogeneity assumptions are appropriate for this analysis.

2.3.1 Dealing with endogeneity: border state policies

An alternative approach to the more traditional instrumental variables analysis draws from the policy diffusion literature [Berry and Berry (1990) and Canon and Baum (1981)]. This literature shows that states tend to adopt policies when bordering states do. Thus, bordering states tort reform implementation may be a feasible instrument for the adoption of tort reforms. Atkins and Bradford (2014) use this type of policy diffusion approach to deal with policy endogeneity to assess the impact of sex education policies on risky sexual behavior among youths. This study borrows from their approach using a version of their method and arguments to underpin the use of border state policies to instrument for tort reform endogeneity. Just like traditional instrumental variables, these instruments must be correlated with the adoption of tort reform in a given state, but uncorrelated with the number of physicians in that state for this approach to be valid. Border state policy instruments may fail the second requirement if there are regional characteristics that influence both border state passage of tort reforms and the number of physicians in each state (Atkins and Bradford 2014). For example, Vermont has three border states, New York, Massachusetts, and New Hampshire. Although the number of family medicine physicians in Vermont should not be

Table 2.1: Bordering States

State	Bordering States	State	Bordering States
Alabama	MS, FL, GA, TN	Montana	ID, WY, SD, ND
Alaska	Not Applicable	Nebraska	SD, WY, CO, KS, MO, IA
Arizona	CA, NV, UT, NM	Nevada	ID, OR, CA, AZ, UT
Arkansas	OK, TX, LA, MS, TN, MO	New Hampshire	VT, MA, ME
California	OR, NV, AZ	New Jersey	NY, PA, DE
Colorado	UT, AZ, NM, OK, KS, NE, WY	New Mexico	CO, UT, AZ, TX, OK
Connecticut	NY, RI, MA	New York	VT, MA, CT, NJ, PA
Delaware	MD, PA, NJ	North Carolina	VA, TN, GA, SC
Florida	GA, AL	North Dakota	MT, SD, MN
Georgia	AL, FL, TN, SC, NC	Ohio	MI, IN, KY, WV, PA
Hawaii	Not Applicable	Oklahoma	KS, CO, NM, TX, AR, MO
Idaho	WA, OR, NV, UT, WY, MT	Oregon	WA, ID, NV, CA
Illinois	WI, IA, MO, KY, IN	Pennsylvania	NY, OH, WV, MD, DE, NJ
Indiana	MI, IL, KY, OH	Rhode Island	CT, MA
Iowa	MN, SD, NE, MO, IL, WI	South Carolina	NC, GA
Kansas	NE, CO, OK, MO	South Dakota	ND, MT, WY, NE, IA, MN
Kentucky	OH, IN, IL, MO, TN, VA, WV	Tennessee	KY, MO, AR, MS, AL, GA, NC, VA
Louisiana	AR, TX, MS	Texas	OK, NM, LA, AR
Maine	NH, VT, MA	Utah	WY, ID, NV, AZ, NM, CO
Maryland	DE, PA, WV, VA	Vermont	NH, NY, MA
Massachusetts	NH, VT, NY, CT, RI	Virginia	MD, WV, KY, TN, NC
Michigan	WI, IN, OH	Washington	ID, OR
Minnesota	ND, SD, IA, WI	West Virginia	MD, PA, OH, KY, VA
Mississippi	TN, AR, LA, AL	Wisconsin	MI, MN, IA, IL
Missouri	IA, NE, KS, OK, AR, TN, KY, IL	Wyoming	MT, ID, UT, CO, NE, SD

affected by tort laws in New York, Massachusetts, and New Hampshire, there may be regional characteristics (access to health care and availability of medical education) that are common to all states and thus correlated with the number of family medicine physicians in Vermont. In this case, the border state policy instrument would include regional effects and may not be exogenous to the number of family medicine physicians in Vermont. To eliminate the regional influences from border state policies, Atkins and Bradford (2014) difference out the regional effect by subtracting the average of the border state policies from the state policy of interest. Similarly, we calculate the instrument, J_{st}, as

$$J_{st} = L_{st} - \overline{L}_{bt} \tag{2.3}$$

where L_{st} is the policy of state s in year t, and $\overline{L}_{bt}$ is the average of the border states policies. We calculate a border-state policy instrument for each reform in the dataset.[1] Table 2.1 describes the bordering states for each state considered here.

These instruments should be valid for two reasons. First, there is a theoretical basis from policy diffusion literature, as well as supporting empirical work in the field [Berry and Berry 1990 and Gray (1973)]. Political research generally indicates that states are more likely to adopt laws or policies that their neighboring states also adopt, and this should be sufficient to be strongly correlated with endogenous tort reform variables. Second, while legislatures may enact tort reform in response to the supply of family medicine physicians in their own state, they are highly unlikely to do so in response to the supply of physicians in neighboring states. As long as policy makers in bordering states do not consider the supply of physicians of their neighboring states at any time, past, present, or future, both strict exogeneity and contemporaneous exogeneity will hold.

2.3.2 Temporary and permanent reforms

There are two features of liability reform and liability insurance that appear to play nontrivial roles in the impact of tort reform on physicians supply. First, several studies indicate that tort reforms may not impact malpractice premiums immediately because of insurers' reluctance to reduce premiums until several years of claims data are available. Secondly, as Dubay, Kaestner and Waidmann 2001 explain, this delay in premium changes is compounded by the uncertainty

[1] Atkins and Bradford (2014) use the modal change in policy in the instrument set because of the varying number of bordering states. The models in this paper include all of the border-state policy instruments and account for the varying number of bordering states.

surrounding the permanence of reforms. "If an insurer forecasts that the State's judiciary will declare a tort reform unconstitutional, then it may be reluctant to change its pricing, underwriting, or loss-reserving practices, since any cases occurring from torts committed today may be adjudicated when the reform is no longer binding" (1254). Several authors lag reforms by two or more years to deal with this issue.

Recent work by Grace and Leverty (2013) suggests that simply lagging reforms is not sufficient to address the reluctance on the part of insurance providers to change premiums as tort reforms occur. They contend that reforms should be segmented into temporary and permanent reforms. Temporary reforms are those that are eventually declared unconstitutional or repealed, and permanent reforms are those that are unchallenged or upheld by the courts. They find that the effects of temporary reforms are never statistically significant and conclude that studies that combine temporary and permanent reforms in the literature incorrectly estimate the effect of tort reform. To account for these issues, specifications are estimated that assess the difference between temporary and permanent reforms. A new set of variables is created for these tests. $Perm_{st}$ is equal to one if a reform is unchallenged or upheld by the courts during the length of the panel and zero otherwise. In addition, $Temp_{st}$ is equal to one for reforms that are enacted and then declared unconstitutional by courts or reversed by legislative action and zero otherwise. Therefore, there are two new variables for each reform in the dataset.

2.3.3 Other specification checks

Several specification checks are conducted to verify the validity of estimates. First, other instruments in addition to border state policy instruments are tested. Political covariates, proposed as instruments for tort reform by Klick and Stratmann (2005), and loss ratios were also considered as instruments for this analysis.[2] Next, each specification is run with lagged reform variables to compare results to the existing literature. In addition, several different specifications of tort reforms will be tested. There are several different bundles of reforms considered in this analysis, including

[2]Loss ratios, losses incurred to premiums earned, measure the profitability of insurance companies. The political covariates considered here include the political party with the majority of each body of the state legislature, and the political party of the governor. Each variable is a binary equal to one if the political majority is Republican in state s in year t. Though we cannot reject the null of exogeneity of these instruments, they are weak, and thus, estimates are biased. Furthermore, with weak instruments that are almost valid, using this set of instruments could be misleading in the assessment of the impact of tort reform on family medicine physician supply. Therefore, these results are not reported. Results are available upon request from the author.

bundles used in previous literature. Lastly, the number of family medicine physicians relative to the total number of physicians will be used as the dependent variable to test whether the provision of care simply shifts from one type of physician to another.[3]

2.4 Data

Data for this analysis came from four primary sources. First, the number of family medicine physicians was obtained from reports of the Physician Masterfile, maintained by the American Medical Association (AMA) and purchased through a database licensing agreement.[4] This is the primary dependent variable of interest. This number includes both M.D.and D.O. physicians whose primary specialization is family medicine. Next, data describing state medical malpractice laws were collected from the Database of State Tort Law Reforms, DSTLR 4^{th} (Avraham 2011). The DSTLR $\left(4^{th}\right)$ is the most comprehensive and complete dataset on tort reforms. It provides information about tort reforms in all states and the District of Columbia from 1980 to 2010. Each tort reform variable is coded as a binary variable. [5] Therefore, there is no distinction between different levels of caps, or variations on other reforms.[6] This analysis considers four primary reforms, caps on noneconomic and punitive damages, joint and several liability, and collateral source reform.[7] Awards for noneconomic damages compensate the plaintiff for pain and suffering. Reforms cap these awards at a statutorily specified amount. Similarly, awards for punitive damages attempt to punish the defendant's misconduct and may be imposed in addition to economic and noneconomic damages. This reform caps punitive damages at a statutorily specified amount. Under the joint and several liability rule, a plaintiff can collect damages from any defendant regardless of their share

[3]It may be the case that while the number of family medicine physicians is declining, the number of internal medicine physicians or pediatricians is increasing. If so, patients may simply shift their primary care needs to one of these specialties.

[4]Medical Marketing Service (MMS Inc) is an authorized AMA Database Licensee (DBL) and supplied requested data extracted from the AMA-PPD database for research and statistical analysis.

[5]Tort reform indicator variables are equal to one at the start of the calendar year in which the reform is active, unless the effective date of the reform was on or after July 1. In this case, it is coded as a one beginning with the following calendar year.

[6]Born and Neale (2013) find an improvement in insurer profitability with noneconomic damage cap levels set at $250,000, but caps in excess of this amount have little to no effect. All but two observations in this dataset have caps on noneconomic damages in excess of $250,000. All observations in this dataset have caps on punitive damages in excess of $250,000. Therefore, it seems unlikely that the cap level will be a binding constraint in most cases.

[7]Caps on total damages, caps on contingency fees, mandatory periodic payments, and patient compensation fund reform are also considered. However, there is little variance between observations for these reforms. Therefore, in some fixed effects regressions, the effects of only one state are being identified.

Table 2.2: Summary Statistics, State Level

Variable	Mean	Std. Dev.	Minimum	Maximum
Number of FM Physicians	1,595.69	1,615.35	129	10,798
Total Number of Physicians	14,007.05	16,761.07	658	97,743
Population (in millions)	5.485	6.091	0.466	36.250
FM Residency Programs	9.22	8.81	1	42
Number of FM Residents	189.27	193.66	7	1004
HMO Enrollment (in millions)	1.276	2.312	0	17.944
Income per capita	28,368.97	7,365.83	14,651	64,579
Civilian Unemployment Rate	5.031	1.377	2.3	11.2
Republican Governor	0.539	0.499	0	1
Republican Majority: Senate	0.514	0.500	0	1
Republican Majority: HOR	0.469	0.499	0	1
Loss Ratio	0.763	0.343	0	3.478
Caps: Noneconomic Damages	0.354	0.479	0	1
Caps: Punitive Damages	0.440	0.497	0	1
Caps: Total Damages	0.123	0.328	0	1
Collateral Source Reform	0.618	0.486	0	1
Caps on Contingency Fees	0.357	0.479	0	1
Mandatory Periodic Payments	0.575	0.495	0	1
Joint and Several Liability	0.716	0.451	0	1
Patient Compensation Fund	0.240	0.427	0	1

of liability. This is commonly known as the "deep pockets" rule. Reforms to this rule primarily limit what a plaintiff may recover from each defendant according to the proportional share of their liability.[8] These three reforms, noneconomic and punitive damage caps and joint and several liability reform are considered direct reforms. These "direct" reforms affect the distribution of awards by truncating the upper tail or by shifting the mean of awards down. The common law collateral source rule prevents the admission of evidence that the plaintiff has received compensation from a source other than damages sought against the defendant. For example, a plaintiff's personal insurance coverage for the harm cannot be admitted into evidence under this rule. Reforms to this rule allow other sources of compensation to offset damages awarded. Collateral source rule reform is considered an "indirect" reform, affecting the type of evidence permitted which may indirectly affect malpractice pressure. What effect should these four reforms have on physician supply? Since noneconomic damage caps, punitive damage caps, and reforms to the collateral source rule should lessen the burden on physicians, the implementation of one of these reforms in a state should correspond to an increase in family medicine physician supply. On the other hand, joint and several liability increases the burden on physicians since they tend to be held responsible for patient care and outcomes. So, the implementation of reforms to this rule in a state should correspond to a decrease in physician supply.

In addition, the number of residents and residency programs (family medicine and total) were obtained from information in the National Graduate Medical Education Census, also maintained by the AMA.[9] Data for other explanatory variables was obtained from various United States government resources including the Census Bureau (Statistical Abstract of the United States), the Bureau of Economic Analysis, and the Bureau of Labor Statistics. This data includes population, income per capita, civilian unemployment rate, and HMO enrollment per capita. Summary statistics for all these variables are presented in Table 2.2. The panel data used in these models spans from 1992 to 2007. The study ends with the year 2007 to avoid effects of the financial market crisis and

[8] In many cases, the attending physician is held liable, at least partially, for any mistakes made by subordinate practitioners. Physicians may bear liability for granting nurse practitioners, physicians assistants, and other providers too much authority, or inadequately supervising them. Joint and several liability reform essentially prohibits plaintiffs from suing any party that is not directly involved in patient care, and limits the amount of damages based on each defendants share of the liability. Without this reform in place, physicians may be able to use hospital or group resources to fight a malpractice case, since the hospital or group may also be at risk of facing suit. With this reform, attending physicians tend to bear the burden of a malpractice case alone.

[9] Data from the Graduate Medical Education Database, Copyright 2013, American Medical Association, Chicago, Illinois.

Table 2.3: First-stage Regression Summary Statistics: Border State Instruments

Variable	Robust F_{50}	$Prob > F$
Caps on Noneconomic Damages	115.675	0.0000
Caps on Punitive Damages	124.232	0.0000
Collateral Source Reform	45.727	0.0000
Joint and Several Liability	40.813	0.0000

the recession in 2008. This sixteen year period includes different tort reforms enacted by different states in different years. Most states enacted at least one reform during the panel.[10,11]

2.5 Empirical Results

2.5.1 Dealing with endogeneity: preliminary tests

All border-state policy instruments that have sufficient variation are included in the instrument set.[12] Border-state policy instruments perform well under traditional instrument tests. There are several tests which assess the strength and validity of instruments. Post-estimation commands which conduct the Durbin-Wu-Hausman test indicate that the null of exogeneity is rejected at the one percent level ($F_{50} = 5.396$ and p-value $= 0.0011$). This implies that tort reforms are likely endogenously determined.[13] F-statistics reported in Table 2.3 indicate that border states' policies are strong instruments for the implementation of reforms. The Kleibergen and Paap LM test reports a test statistic of 25.906 with a p-value of 0.000. This confirms that instruments are strong even when considering all endogenous variables and instruments together.[14] Lastly, Hansen's J-test of over-identifying restrictions which assesses the validity of instruments was conducted. It assumes that at least one instrument is valid and then tests for the validity of all other instruments. This test

[10]Only one state enacted caps on total damages (South Dakota), caps on contingency fees (Nevada), and patient compensation fund reform (West Virginia) during the panel. Other states either had the reform in place before 1992, or did not enact the reform until after 2007. For these variables, this means that in some fixed effects regressions, the effects of one state are being identified.

[11]Tables including the active years of reforms in each state and the number of changes to tort laws during the panel period are available upon request.

[12]This includes border-state policy variables for caps on noneconomic and punitive damages, collateral source reform, joint and several liability, and mandatory periodic payments in addition to the other exogenous variables in the model.

[13]Exogeneity tests assume that the instruments are valid.

[14]Critical values for more than 3 endogenous variables are not recorded in the ivreg2 routine, so Stock and Yogo simulated critical values are unavailable for this case with four endogenous variables. However, Cragg-Donald Wald F-test statistics are very large, 187.024. Odds are that this would exceed any critical value anyway.

cannot reject the null hypothesis that these instruments are valid (Hansen's J chi-squared = 0.0384 and p-value = 0.845).[15] Results from first-stage regressions are presented in the Table 2.4. Table 2.5 presents results for second-stage regressions. Results from traditional Ordinary Least Squares (OLS) estimation including state fixed effects are presented in the first column. Instrumental variables (IV) specifications which includes Two-stage Least Squares (2SLS), Generalized Method of Moments (GMM), and Fixed Effects (FE), are given in the other three columns. Results from 2SLS and GMM are practically identical, and with regard to the tort reform variables, the implications of all specifications are statistically identical.

None of the reforms considered here, caps on noneconomic and punitive damages, collateral source reform, and joint and several liability, have a statistically significant impact on the number of family medicine physicians. This insignificant result for the effect of noneconomic damages caps on family medicine physician supply differs from the existing literature which suggests that these types of caps consistently increase physician supply and insurer profitability. There are several explanations for this result, but one seems more likely than others. States that enact noneconomic damage caps lower the perceived risks of specializing in high-risk areas, like surgery or obstetrics. It is possible that this popular reform induces graduating medical students in these states to choose a higher-paying, higher-risk specialty, thereby decreasing the number of medical students that choose to focus in family medicine. This may also support claims in previous work [Klick and Stratmann (2007); Matsa (2007)] that suggest that tort reforms are most effective at encouraging physicians to work in high-risk specialties. While this result is unexpected, there are two important aspects of these preliminary tests to consider. Foremost, using border state policies appears to be a favorable strategy to deal with policy endogeneity. Both this study and Atkins and Bradford (2014) have success with the strategy, so it may be an important approach to consider for researchers in policy and law. In addition, control variables including the number of family medicine residency programs, managed care enrollment, population, income per capita, and the unemployment rate have expected effects.

Each additional family medicine residency program increases the number of family medicine physicians in a state by around 3 percent, holding other variables constant. This result is statistically significant in all IV specifications at the one percent level, except when fixed effects are

[15]This test asks whether any of the instruments are invalid, but assumes that at least enough are valid to exactly identify the equation. If instruments are invalid, this test may be biased and inconsistent.

Table 2.4: First-stage Regression Results, Border State Policies as Instruments

Variable	NEDC	PDC	CSR	JSL
Border State: NEDC	0.8243	0.0333	−0.0522	−0.0130
	(.0383)***	(.0406)	(.0599)	(.0540)
Border State: PDC	0.0380	0.8462	0.0132	0.0103
	(.0423)	(.0370)***	(.0772)	(.0578)
Border State: CSR	−0.0203	−0.0222	0.8265	−0.0436
	(.0448)	(.0414)	(.0591)***	(.0604)
Border State: JSL	−0.0312	−0.0397	0.0093	0.7965
	(.0464)	(.0416)	(.0745)	(.0657)***
Border State: PE	−0.0752	−0.0598	−0.0818	−0.0941
	(.0374)**	(.0382)	(.0709)	(.0509)*
FM Residency Programs	0.0054	−0.0029	0.0080	0.0045
	(.0036)	(.0040)	(.0067)	(.0052)
HMO Enrollment per capita	0.3327	−0.3572	0.6788	0.0204
	(.2140)	(.1885)*	(.3868)*	(.2766)
ln(*Population*)	−0.0836	0.1293	−0.0692	−0.0042
	(.0353)**	(.0352)***	(.0681)	(.0471)
Income per capita	−0.00001	−0.00002	0.000001	−0.00002
	(.000005)***	(.000005)***	(.00001)	(.000006)***
Civilian Unemployment rate	−0.0287	−0.0090	−0.0602	−0.0665
	(.0174)*	(.0142)	(.0389)	(.0217)***
State-fixed effects	No	No	No	No
Year-fixed effects	Yes	Yes	Yes	Yes
F-statistic	50.9***	91.8***	18.0***	15.4***
Number of Observations	759	759	759	759

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. Each column represents a separate first-stage regression. The dependent variable (endogenous regressor) is given at the top of each column. The following abbreviations are used to simplify presentation: NEDC (noneconomic damage caps), PDC (punitive damage caps),CSR (collateral source reform), (JSL) joint and several liability, and PER (punitive evidence reform).
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Table 2.5: Regression Results, Border State Policies as Instruments

Variable	OLS, FE	IV, 2SLS	IV, GMM	IV, FE
Caps on Noneconomic Damages	-0.0206	-0.0654	-0.0655	-0.0145
	(.0131)	(.0652)	(.0652)	(.0166)
Caps on Punitive Damages	0.0186	0.0410	0.0410	0.0281
	(.0172)	(.0606)	(.0606)	(.0201)
Collateral Source Reform	-0.0150	0.0350	0.0349	0.0074
	(.0185)	(.0633)	(.0633)	(.0201)
Joint and Several Liability	-0.0031	0.0024	0.0023	-0.0087
	(.0151)	(.0730)	(.0730)	(.0159)
FM Residency Programs	0.0034	0.0228	0.0228	0.0033
	(.0031)	(.0034)***	(.0034)***	(.0030)
HMO Enrollment per capita	0.1573	-0.2732	-0.2732	0.1604
	(.0716)**	(0.2766)	(.2766)	(.0711)**
ln(Population)	0.6914	0.7422	0.7422	0.7095
	(.1277)***	(.0332)***	(.0332)***	(.1243)***
Income per capita	-0.000004	-0.00002	-0.00002	-0.000004
	(.000004)	(.000007)***	(.000007)**	(.000004)
Civilian Unemployment rate	-0.0149	-0.0530	-0.0530	-0.0139
	(.0048)***	(.0236)**	(.0236)*	(.0048)***
State-fixed effects	Yes	No	No	Yes
Year-fixed effects	Yes	Yes	Yes	Yes
F-statistic	143.09***			
Wald Statistic		5869.12***	5868.85***	4978.34***
Number of Observations	759	759	759	759

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. First-stage instruments in all instrumental variables specifications include border state policy instruments for caps on noneconomic and punitive damages, collateral source reform, joint and several liability, and mandatory periodic payments in addition to the other exogenous variables in the model. The dependent variable in all specifications is the natural logarithm of the number of family medicine physicians.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

included. This is consistent with medical literature which indicates that many physicians continue to practice medicine in the location where they complete their residency training (West et al. 1996). Other time-varying state characteristic variables have expected effects. A one percent increase in the population of a state corresponds to an increase in the number of family medicine physicians of 99.7 percent. This result is statistically significant at the one percent level in all specifications. Thus, more populated states have more physicians after controlling for other important factors. A one point increase in the unemployment rate corresponds to a 5.3 percent decrease in the supply of family medicine physicians, but this effect is smaller once state-fixed effects are included, 1.4 percent. In both cases, this negative relationship between the unemployment rate and the supply of family medicine physicians is statistically significant at the five percent level. Lastly, a one dollar increase in income per capita decreases the supply of family medicine physicians by 0.002 percent. This counterintuitive result is statistically significant at the five percent level in some specifications, but loses significance once state fixed-effects are taken into account.

2.5.2 Temporary and permanent reforms

Results for regressions that account for the difference between temporary and permanent reforms are presented in Table 2.6. Similar to Grace and Leverty (2013), temporary reforms never have a statistically significant effect on the supply of family medicine physicians. States that enact permanent caps on noneconomic damages have 2.5 percent fewer family medicine physicians than states that enact temporary caps or states that do not enact caps once state fixed-effects are included. This result is statistically significant at the five percent level. Perhaps when this reform is enacted, students completing medical school are relatively more likely to choose a specialty other than family medicine. In contrast, however, states that enact permanent caps on punitive damages have 3.9 percent more family medicine physicians than states that enact temporary caps or states that do not enact caps. This result is also statistically significant at the one percent level after accounting for state fixed-effects. Therefore, failing to differentiate between temporary and permanent reforms can mask the substantial impact of permanent reforms on family medicine physician supply. Intuitively, these results appear to be inconsistent. The possible explanation for the negative impact of noneconomic damage caps proposed above suggests that the effect may reflect the choice of specialty, however, so looking at the change in family medicine physicians relative to total physicians may be revealing.

Table 2.6: Regression Results, Temporary and Permanent Reforms with Border State Policies as Instruments

Variable	IV, 2SLS	IV, GMM	IV, FE
Temporary: Caps on Noneconomic Damages	-0.1530	-0.1397	-0.0105
	(.1340)	(.1237)	(.0157)
Temporary: Caps on Punitive Damages	0.0192	0.0149	0.0334
	(.0788)	(.0775)	(.0238)
Temporary: Collateral Source Reform	0.0161	0.0139	0.0022
	(.0932)	(.0934)	(.0239)
Temporary: Joint and Several Liability	-0.0888	-0.0897	.5327
	(.1451)	(.1449)	(.3810)
Permanent: Caps on Noneconomic Damages	-0.0816	-0.0778	0.2391
	(.0667)	(.0653)	(.2410)
Permanent: Caps on Punitive Damages	0.0685	0.0637	0.1350
	(.0704)	(.0673)	(.0339)***
Permanent: Collateral Source Reform	0.0465	0.0429	0.3105
	(.0611)	(.0598)	(.2323)
Permanent: Joint and Several Liability	-0.0240	-0.0235	-0.0015
	(.0877)	(.08720)	(.0286)
FM Residency Programs	0.0245	0.0245	0.0034
	(.0061)***	(.0061)***	(.0031)
HMO Enrollment per capita	-0.2957	-0.2833	0.1528
	(.2859)	(.2822)	(.0728)**
ln(Population)	0.7408	0.7405	0.6862
	(.0382)***	(.0381)***	(.1210)***
Income per capita	-0.00002	-0.00002	-0.000004
	(.000007)***	(.000007)***	(.000004)
Civilian Unemployment rate	-0.0511	-0.0524	-0.0145
	(.0238)**	(.0234)**	(.0050)***
State-fixed effects	No	No	Yes
Year-fixed effects	Yes	Yes	Yes
Wald Statistic	9055.62***	9847.94***	40739.9***
Number of Observations	759	759	759

Note. Reported standard errors are given in parenthesis and are clustered at the state level for all specifications. First-stage instruments in all instrumental variables specifications include border state policy instruments for caps on noneconomic and punitive damages, collateral source reform, joint and several liability, and mandatory periodic payments in addition to the other exogenous variables in the model. All regressions include state-level control variables (population, income per capita, and the civilian unemployment rate), and health market control variables (HMO enrollment per capita and family medicine residency programs). The dependent variable in all specifications is the natural logarithm of the number of family medicine physicians.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

2.5.3 Other specification checks

There are several other factors considered in this analysis. First, though the temporary and permanent reform specification brings attention to this important institutional detail, specifications with lagged reform variables are also tested. However, the results in all of the specifications reported above are robust to using lagged values of the reform variables.[16] In addition, since reform variables are coded as being active in the next calendar year if the reform is enacted after July 1, specification checks including leads of reform variables were also conducted. Results in previous sections are also robust to using leads of reform variables. All of these results are insignificant, confirming that leading or lagging reforms does not fully account for the differences between temporary and permanent reforms.

Secondly, since different bundles of reforms are often enacted together, some bundles may be jointly significant but individually insignificant. In order to determine if this is the case, several different bundled sets of reforms that have been considered in previous literature are considered here. The results from the bundle set used by Grace and Leverty (2013), Currie and MacLeod (2008), and this analysis which includes caps on noneconomic and punitive damages, collateral source reform, and joint and several liability, are presented in the first column and second columns of Table 2.7. The first column gives results from instrumental variables regressions using 2SLS. The second column provides results from instrumental variables regressions using state fixed effects. In addition, the direct[17] and indirect[18] reform bundles introduced by Kessler, Sage and Becker (2005) are examined with results presented in the next four columns. Again, each bundle is estimated using 2SLS and FE instrumental variables specifications. Control variables included in previous regressions are also included in these regressions, but not presented in Table 2.7. Unfortunately, there are too few instruments to assess the bundle of seven reforms used by Klick and Stratmann (2007). The results in Table 2.7 are consistent with previous results. No reforms have a statistically significant impact on the supply of family medicine physicians. Table 2.7 also presents tests of the joint significance of the bundle of reforms to determine whether estimates are significantly different from zero. None of the bundles included here are jointly statistically significant. The

[16] This specification check uses one-year, two-year, and three-year lags.
[17] Caps on noneconomic, punitive, and total damages, and joint and several liability
[18] Collateral source reform, caps on contingency fees, mandatory periodic payments, and patient compensation fund reform

Table 2.7: Regression Results, Bundled Reforms with Border State Policies as Instruments

Variable	(4) Original IV, 2SLS	(5) Original IV, FE	(6) Direct IV, 2SLS	(7) Direct IV, FE	(8) Indirect IV, 2SLS	(9) Indirect IV, FE
Caps on Noneconomic Damages	-0.0654	-0.0145	-0.0730	-0.0353		
	(.0652)	(.0166)	(.0605)	(.0356)		
Caps on Punitive Damages	0.0410	0.0281	0.0544	0.0279		
	(.0606)	(.0201)	(.0788)	(.0203)		
Collateral Source Reform	0.0350	0.0074	0.0389	0.0105		
	(.0633)	(.0201)	(.0716)	(.0194)		
Caps on Total Damages			-0.0643	-0.3380		
			(.3354)	(.6223)		
Joint and Several Liability	0.0024	-0.0087			-0.1003	0.0115
	(.0730)	(.0159)			(.1782)	(.0815)
Caps on Contingency Fees					0.2785	0.1826
					(.5155)	(.2758)
Mandatory Periodic Payments					-0.1815	-0.0869
					(.3383)	(.1302)
Patient Compensation Fund					0.1231	-0.0032
					(.4971)	(.2912)
State-fixed effects	No	Yes	No	Yes	No	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Joint Significance of Reforms	2.29	2.82	3.17	2.51	0.35	2.5
Wald Statistic	5869.12***	4978.34***	5593.82***	6052.79***	3180.95***	3675.51***
Number of Observations	759	759	759	759	759	759

Note. Reported standard errors are given in parenthesis. Standard errors for 2SLS regressions are clustered at the state level. First-stage instruments in all instrumental variables specifications include border state policy instruments for caps on noneconomic and punitive damages, collateral source reform, joint and several liability, and mandatory periodic payments in addition to the other exogenous variables in the model. All regressions include state-level control variables (population, income per capita, and the civilian unemployment rate), and health market control variables (HMO enrollment per capita and family medicine residency programs). The dependent variable in all specifications is the natural logarithm of the number of family medicine physicians.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

lack of joint significance and the absence of significance for individual coefficients are consistent with the hypothesis that there is no systematic response of family medicine physician supply to the implementation of tort reform.

Finally, in contrast to the general assumptions made in the literature, some types of tort reform do appear to affect family medicine physicians, especially once the difference between temporary and permanent reforms is considered. Perhaps the general assumption should be modified to suggest that the impact on family medicine is relatively small or not important once the supply of other physicians are taken into account. Matsa (2007) argues that the delivery of primary care by internists and pediatricians may drive down the number of family medicine physicians. In this case, while the number of family medicine physicians is decreasing, the number of other physicians is increasing. To test for this possibility, a separate set of specifications were run which include the ratio of family medicine physicians to total physicians as the dependent variable. Unfortunately, the results for this specification do not provide any statistically significant or conclusive explanation. Perhaps the ratio has been incorrectly specified as family medicine physicians to total physicians and should actually be characterized as general practitioners to internists and pediatricians.[19] It may be possible that using total physicians masks the relationship. However, data for this is not readily available.

A few results from these ratio regressions warrant discussion. First, it is intuitive that the signs on the coefficient for family medicine residency programs and total residency programs are positive and negative, respectively. States with an additional family medicine residency program tend to have a larger ratio of family medicine physicians to total physicians. On the other hand, states with more residency programs in other specialties tend to have a smaller ratio of family medicine to total physicians. Furthermore, population, HMO enrollment per capita, income per capita, and the civilian unemployment rate all correspond with a smaller ratio of family medicine to total physicians. This result is statically significant and robust to various specifications. This implies that more populated states, and those with more managed care, higher income levels, and higher civilian unemployment rates tend to see less family medicine physicians relative to other physicians. Thus, economic and educational factors have an expected influence the supply of family medicine physicians relative to total supply.

[19]Table 2.8 in the Appendix presents the results for these specifications.

2.6 Conclusions

The most interesting result in this study reveals that once we distinguish between permanent and temporary reforms, temporary reforms never have a statistically significant impact of family medicine physician supply, but some permanent reforms do. In fact, states that enact permanent caps on noneconomic damages have 2.5 percent fewer family medicine physicians than states that enact temporary caps or states that do not enact caps once state fixed-effects are included. Similarly, states that enact permanent caps on punitive damages have 3.9 percent more family medicine physicians than states that enact temporary caps or states that do not enact caps. Both of these effects are statistically significant at the five percent level and robust to the inclusion of state and year fixed effects. This suggests, similar to the work introduced by Grace and Leverty (2013), that collapsing temporary and permanent reforms into one binary variable may muddle the impact of reforms that last. This also suggests that family medicine physicians may be more inclined to respond to these types reductions in medical malpractice pressure if they are permanent.

In addition, the passage of reforms may not simply increase the supply of physicians as previous work suggests. Certain types of reforms, like caps on noneconomic damages, may encourage physicians to choose specialties which tend to be higher risk, leading to a decrease in general practitioners but an increase in high-risk specialists. On the other hand, punitive damage caps, reforms to the collateral source rule, and joint and several liability have the traditionally expected effects. Punitive damage caps and collateral source rule reform lighten a physician's liability, thereby increasing supply, while joint and several liability increases physician liability, thereby decreasing supply. Moreover, there are other important policies that this analysis has not considered like loan forgiveness programs or policies that change the length of and/or curriculum in medical school to encourage medical students to pursue family medicine. These important institutional policies may have substantial effects on physician supply and are not measured in this study.

Furthermore, it is important to note that a small or insignificant effect of reforms on physician supply does not rule out the practice of avoidance behaviors by physicians. There are several avoidance behaviors discussed in medical literature that would be undetectable by aggregate analysis. In fact, recent work by Li and Dor (2015) suggests that family medicine physicians practice defensive medicine in ways that do not impact overall supply, like the over-ordering of diagnostic imaging. If this is the case, the extent of defensive medicine among general practitioners is understated by our

analysis. This suggests that more research on individual physician practice and treatment choices may be necessary to diagnose defensive medicine among general practitioners.

Finally, this analysis does not attempt to model the change in welfare that may or may not occur when there is a change in the supply of family medicine physicians. To assess whether an increase in the supply of family medicine physicians is welfare enhancing, further work is necessary. There are several important welfare factors to consider. First, if it is assumed that an increase in the supply of physicians increases competition and thereby lowers the price of physician services to consumers, reforms that increase physician supply may be welfare enhancing. Second, if the implementation of tort reform also decreases positive defensive medicine, it may also reduce the costs of excessive testing and other costs from positive defensive medicine. Again, these cost savings could be welfare enhancing. However, because of the nature of data used here, this study can not assess these effects. Third, tort reform may reduce welfare if it decreases incentives for physicians to take care with patients and leads to more injuries or poor outcomes. Any policy decision making should consider these factors.

Table 2.8: Regression Results, FM Physician Supply Compared to Total Supply

Variable	IV, 2SLS	IV, FE	IV, 2SLS with Fixed Effects Temporary	Permanent
Caps on Noneconomic Damages	-0.0017	-0.0008	-0.0624	
	(.0095)	(0.0018)	(.1396)	
Caps on Punitive Damages	-0.0073	0.0047	0.0014	
	(.0098)	(.0035)	(.0199)	
Collateral Source Reform	0.0121	0.0051	0.0069	
	(.0134)	(.0019)***	(.0124)	
Joint and Several Liability	0.0073	-0.0020	0.0027	
	(.0121)	(.0024)	(.0258)	
Caps on Noneconomic Damages				-0.0028
				(.0135)
Caps on Punitive Damages				0.0028
				(.0145)
Collateral Source Reform				-0.0025
				(.0150)
Joint and Several Liability				0.0196
				(.0224)
Total Residency Programs	-0.0002	-0.0001	−0.0004	
	(.00005)***	(.0005)	(.0001)***	
FM Residency Programs	0.0049	0.0007	0.0081	
	(.0009)***	(.0004)*	(.0017)***	
HMO Enrollment per capita	-0.1831	0.0065	-0.2035	
	(.0563)***	(.0069)	(.0598)***	
ln(*Population*)	-0.0276	-0.0225	-0.0254	
	(.0073)***	(.0132)*	(.0133)*	
Income per capita	-0.000003	-0.000001	-0.000003	
	(.000001)**	(.0000002)***	(.000001)**	
Civilian Unemployment rate	-0.0082	-0.0015	-0.0027	
	(.0045)*	(.0005)***	(.0028)	
State-fixed effects	No	Yes	Yes	
Year-fixed effects	Yes	Yes	Yes	
Wald Statistic	233.25***	36250.37***	5868.85***	
Number of Observations	711	711	711	

Note. Each of these regressions uses the ratio of the number of family medicine to total physicians as the dependent variable. Reported standard errors are given in parenthesis and are clustered at the state level. First-stage instruments in all instrumental variables specifications include border state policy instruments for caps on noneconomic and punitive damages, collateral source reform, joint and several liability, and mandatory periodic payments in addition to the other exogenous variables in the model.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

CHAPTER 3

MOVEMENT AND REORGANIZATION: NEGATIVE DEFENSIVE MEDICINE AMONG FAMILY MEDICINE PHYSICIANS

3.1 Introduction

Physicians daily make patient care and practice decisions where they must weigh marginal costs and benefits based on the amount of medical malpractice pressure they face. Although malpractice pressure varies across the United States, Jena et al. (2011) and Seabury et al. (2013) find that most physicians face at least one malpractice claim during their career and spend a substantial portion, 11 percent of an assumed fourty-year career, dealing with an unresolved malpractice case. Unfortunately, physicians have little control over the amount of malpractice pressure they face. Malpractice insurance premiums depend on coverage limits, specialty, practice location, and whether or not the physician performs high-risk procedures such as surgery. In addition, premiums are not experience-rated or physician-rated. A clean record of patient care will not lower malpractice premiums. Though most physicians hold malpractice insurance policies to cover the direct monetary costs of litigation (settlement, legal fees, etc.), non-monetary costs of malpractice litigation can be even more detrimental. Even if a claim does not result in damages awarded, physicians often experience reputation damage and spend a large amount of time away from patients or the office to dispute a suit. The combined expectation of being sued and the large time and reputation costs can lead to defensive medicine among physicians.

Though much of the existing research focuses on positive defensive medicine, characterized by excessive testing or unnecessary procedures, physicians may also choose avoidance behaviors, or negative defensive medicine. Negative defensive medicine may include behaviors like the discontinuation of certain service offerings, changes to the organization of the medical practice, or movement of the practice to a different malpractice climate. In an effort to determine whether such negative defensive practices are significant or not, this analysis considers a sample of 28,227 family medicine physicians from 1992-2007. These physicians are randomly selected proportional to the total num-

ber of physicians in each state and the District of Columbia at the beginning of the panel. Table 3.1 shows the percent distribution of the sample considered here by practice mode. Over the panel, the percentage of physicians practicing in solo, partnership and hospital practices has declined, while the percentage of physicians practicing in a group setting has almost doubled. This analysis will test to see if liability reforms had any impact on this transition. Physicians can also move their practice to a more defendant-friendly legal environment. Table 3.2 and Table 3.3 list the states with the most and least family medicine physicians per capita, respectively. During the length of the panel, the states with the most physicians per capita had 1.1 more reforms than the states with the least physicians per capita, on average. Physicians may also decline to provide certain types of care that are associated with a higher risk of medical malpractice suits, like obstetrical care. This may disproportionately affect patients in rural communities who have limited access to care.

Tort reform, legal changes intended to reduce tort litigation or damages, is a proposed solution to reduce defensive medicine. A body of the literature investigates the effect of tort reforms on negative defensive medicine that may in turn alter physician supply. [Kessler, Sage and Becker 2005; Hellinger and Encinosa (2005); Matsa (2007); Klick and Stratmann (2007); Ellyson (2015*a*)]. These studies, conducted at an aggregate level, consider the impact of tort reform on physician supply and generally find that the impact on physician supply is small and positive or applies only in certain circumstances. However, a lack of a relationship between reforms and aggregate physician supply does not necessarily imply an absence of defensive medicine. Individual physicians may utilize behaviors or decisions that reduce their legal liability, but may not affect the quantity of physicians supplied. These behavioral changes may alter other aspects of care like the waiting time to get an appointment, the price of services rendered, or the quality of care. In addition, there may be changes to service offerings like refusing to perform certain procedures or refusing to care for high-risk patients.

This study attempts to address these types of behaviors that may be categorized as negative defensive medicine among family medicine physicians. There are several reasons to focus on these physicians. First, Ellyson (2015*a*) finds that it may be incorrect to assume that the medical malpractice system only affects "high-risk" specialties like surgery, obstetrics, and gynecology. There may also be a response to malpractice pressure among "low-risk" specialties like family medicine, pediatrics, psychiatry, and dermatology. Second, family medicine practitioners are a

Table 3.1: Percent Distribution of Sample in each Mode of Practice, 1992-2007

Year	Solo	Partner	Group	Government	Hospital	Other
1992	23.99	7.62	23.19	7.95	12.43	24.82
1993	25.16	8.12	27.06	6.68	9.11	23.87
1994	24.99	8.08	28.23	5.47	6.79	26.45
1995	24.87	8.08	29.11	4.85	4.43	28.67
1996	24.42	7.94	31.57	4.60	4.49	26.98
1997	24.35	7.86	32.35	4.84	4.40	26.21
1998	24.04	7.75	33.37	4.78	4.30	25.77
1999	23.85	7.67	32.72	4.55	4.31	25.91
2000	23.28	7.74	34.95	4.91	4.31	24.82
2001	22.97	7.63	35.39	4.89	4.30	24.82
2002	23.01	7.45	35.81	4.94	4.32	24.46
2003	22.35	7.03	38.82	4.92	4.31	22.57
2004	21.68	6.22	42.67	4.88	4.19	20.35
2005	21.65	6.23	42.69	4.95	4.21	20.27
2006	21.67	5.76	47.29	5.65	4.05	15.58
2007	21.73	5.56	48.77	6.08	3.71	14.14

Table 3.2: States with the Most Family Medicine Physicians per capita, 1992-2007

	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}
1992	Minnesota	North Dakota	Washington	Wyoming	Iowa
1993	North Dakota	Minnesota	Wyoming	Washington	Iowa
1994	North Dakota	Minnesota	Nebraska	Wyoming	South Dakota
1995	North Dakota	Minnesota	Maine	Iowa	South Dakota
1996	North Dakota	Minnesota	Maine	Nebraska	South Dakota
1997	North Dakota	Minnesota	Maine	Iowa	South Dakota
1998	North Dakota	Maine	Minnesota	South Dakota	Iowa
1999	North Dakota	Maine	Minnesota	Iowa	Nebraska
2000	North Dakota	Maine	Alaska	Minnesota	Iowa
2001	Maine	North Dakota	Alaska	Minnesota	Iowa
2002	Maine	North Dakota	Alaska	Minnesota	Nebraska
2003	Maine	Alaska	North Dakota	Minnesota	Nebraska
2004	Maine	Alaska	North Dakota	Minnesota	Nebraska
2005	Alaska	Maine	North Dakota	Minnesota	Iowa
2006	Alaska	Maine	North Dakota	Minnesota	Iowa
2007	Maine	Alaska	North Dakota	Minnesota	Iowa

Table 3.3: States with the Least Family Medicine Physicians per capita, 1992-2007

	50^{th}	49^{th}	48^{th}	47^{th}	46^{th}
1992	Massachusetts	Connecticut	Hawaii	New Jersey	New York
1993	Massachusetts	Connecticut	New York	New Jersey	Hawaii
1994	Massachusetts	Connecticut	New York	New Jersey	Louisiana
1995	Massachusetts	Connecticut	New York	Louisiana	Hawaii
1996	Connecticut	Massachusetts	New York	Hawaii	Louisiana
1997	Connecticut	Massachusetts	New York	Louisiana	New Jersey
1998	Connecticut	Massachusetts	New York	Nevada	New Jersey
1999	Connecticut	Massachusetts	New York	Nevada	New Jersey
2000	Connecticut	Massachusetts	New York	New Jersey	Nevada
2001	Connecticut	Massachusetts	New York	New Jersey	Maryland
2002	Connecticut	Massachusetts	New York	Maryland	New Jersey
2003	Connecticut	Massachusetts	New York	New Jersey	Maryland
2004	Connecticut	Massachusetts	New York	New Jersey	Maryland
2005	Connecticut	Massachusetts	New York	New Jersey	Maryland
2006	Connecticut	Massachusetts	New York	New Jersey	Maryland
2007	Connecticut	Massachusetts	New York	New Jersey	Maryland

significant portion of the physician workforce, especially in rural areas (Rosenblatt and Hart 2000), and often a patient's first or only point of contact with a medical professional in many cases. Third, recent work by Li and Dor (2015) finds that general practitioners may be more likely than other physicians to practice positive defensive medicine. If family medicine physicians are prone to using positive defensive medicine to combat malpractice pressure, they may also use avoidance behaviors to reduce their legal liability. Lastly, certain factors may make family medicine physicians particularly vulnerable to the stress of coping with the rising costs of liability pressure. Lower than average income compared to other specialties may make general practitioners more susceptible to other financial difficulties like rising malpractice premiums or falling reimbursement rates. This paper focuses on negative defensive medicine behaviors only among family medicine physicians.

Family medicine physicians may make several choices in response to malpractice liability that could be of interest. As described by Li and Dor (2015), "If physicians are sensitive to malpractice liability pressures, then tort reforms that limit malpractice liability should have an effect on physician behavior, either directly through relieving their concern about being sued or indirectly through lowering medical malpractice insurance premiums." First, physicians may respond to liability reforms by moving to a state that enacts these reforms. Second, they may change their

practice organization to reduce legal liability, either by joining with other physicians to form a group practice or by joining a hospital practice. This response would likely follow the implementation of joint and several liability. Lastly, they may choose to discontinue the practice of obstetrical care, services which often carry high malpractice risk. Each of these avoidance behaviors can have detrimental effects on health markets and patient outcomes. To test for these types of defensive medicine, this analysis uses discrete choice models and a unique individual level dataset of family medicine physicians.

3.2 Literature Review

There are several studies in the economic and medical literature which assess physician specialty choice and practice location [McKay (1990); Hurley (1991); Seifer, Vranizan and Grumbach (1995); Bolduc, Fortin and Fournier (1996); Kiker and Zeh (1998); Thornton (2000); Thornton and Esposto (2003); Chou and Sasso (2009)]. Some of these studies assess the extent to which medical students respond to economic incentives in their choice of specialty [McKay (1990); Kiker and Zeh (1998); Thornton (2000); Thornton and Esposto (2003)] while others consider both specialty and location choices [Hurley (1991); Seifer, Vranizan and Grumbach (1995); Bolduc, Fortin and Fournier (1996); Chou and Sasso (2009)]. These studies are crucial to policy decisions since the effectiveness of any policy designed to affect the location and specialty distribution of physicians depends on the responsiveness of physicians to income differentials and other economic incentives. However, there are several weaknesses in the existing literature.

First, only three of these studies consider the relationship between medical malpractice pressure or tort reform and physician decision-making [Kiker and Zeh (1998); Thornton (2000); Chou and Sasso (2009)]. Kiker and Zeh (1998) use a question from the Medical Student Graduation Questionnaire to measure the influence of expected malpractice premiums on specialty choice. Students responded whether or not this factor was "influential" or "not influential" in their choice of specialty. Therefore, their measure of malpractice pressure is a binary variable. They find that concerns about malpractice insurance premiums reduce the probability of a student choosing a surgical specialty, but increase the probability of a student choosing a primary care specialty, all else being equal. Thornton (2000) also studies the choice of specialty by new physicians, but measures malpractice pressure using the number of malpractice claims per 100 physicians for each specialty.

He does not find a significant effect and suggests that malpractice pressure may not be an important consideration in the specialty choice. Chou and Sasso (2009) use malpractice premiums for three specialties – internal medicine, general surgery, and obstetrics-gynecology – to measure how malpractice pressure influences the initial location of new physicians from the state of New York. They also include state level caps on noneconomic damages, but only five states altered laws in this area during their sample period. Both measures only have a statistically significant impact on surgeons. Therefore, the existing literature provides unclear and contradictory information about the extent to which malpractice pressure impacts physician specialty and location choices.

Second, while many of these studies use discrete choice to examine a physician's choice of specialty, only a few take advantage of the method to also study the impact of economic factors on a physician's choice of location [Hurley (1991); Bolduc, Fortin and Fournier (1996); Chou and Sasso (2009)]. Hurley (1991) studies the impact of income-based policies on the geographic distribution of physicians by estimating the effects of income incentives on three decisions – specialty, community size, and mode of practice (solo, group, etc) – using the nested logit model. Hurley finds that many physicians prefer the noneconomic attributes of primary care specialties, "but the relatively large income differentials pull the physicians into the highly paid specialties" (64). In terms of the location choice, simulations from this study indicate that increasing the expected income by ten percent of practicing in small communities[1] shifts the distribution of specialty shares towards primary care (64). Similarly, Bolduc, Fortin and Fournier (1996) model the initial practice choice for general practitioners in Canada and find that the average price elasticity of the supply of general practitioners in a region is 0.70 and the average income elasticity is 1.11. Bolduc, Fortin and Fournier (1996) also tests for physician response to several policy simulations. However, both Hurley (1991) and Bolduc, Fortin and Fournier (1996) do not attempt to assess malpractice pressure as an economic incentive in location choice. In an attempt to address this shortcoming, Chou and Sasso (2009) model the initial practice location choice of new physicians using conditional logit models with two measures of malpractice pressure. The authors use the average annual premium charged by each company by state and reforms that cap noneconomic damages by state. They find that only surgeons respond to changes in malpractice pressure. There are several weaknesses in this analysis. First, the effects of changes to laws pertaining to noneconomic damage caps are identified

[1]Population less than 10,000

by changes in only five states. Furthermore, these measures do not take into account recent work [Grace and Leverty (2013) and Ellyson (2015*a*)] that indicates the permanence of reforms has a considerable impact on whether or not physicians and insurance companies respond to reforms.

Lastly, all of these studies use data for recent medical school students or physicians still in residency training. Though this is an appropriate sample for a study of specialty choice, since the majority of physicians do not change their specialty over the course of their career, this sample cannot measure the responses of practicing physicians to the implementation of tort reform. Estimates of elasticities for malpractice premiums are based only on a student or resident's expectations. Even if these physicians in training have appropriate expectations about malpractice climates, most are bound by the medical education training system, National Residency Matching Program (NRMP), in terms of their decision-making over location and specialty. On the other hand, practicing physicians can readily alter their behavior and decisions in response to tort reform. Furthermore, the existing supply of physicians often have a considerable impact on the choices of medical students. These students rely heavily on rotations conducted during their third and fourth years of medical school with current physicians to influence their choices and opinions at the beginning of their career.

This study improves on the previous literature in the following ways. First, it uses data on both practicing physicians, and residents, physicians still in training. Second, it considers other decisions that may be made in conjunction with practice location. This includes type of practice (solo, group, etc.), and whether or not to provide obstetric services. Lastly, it includes varying measures of malpractice pressure, taking into account the difference between temporary and permanent reforms.

3.3 Data

Table 3.4 and Table 3.5 provide a description of each variable in this analysis, followed by Table 3.6 which gives the summary statistics. Data for this analysis came from four primary sources. First, individual level data for family medicine physicians was obtained from records of the Physician Masterfile, maintained by the American Medical Association (AMA) and purchased through a database licensing agreement.[2] There are 28,227 physicians in the panel which spans

[2]Medical Marketing Service (MMS Inc) is an authorized AMA Database Licensee (DBL) and supplied requested data extracted from the AMA-PPD database for research and statistical analysis.

Table 3.4: Physician Level Variable Descriptions

Variable Name	Description
Location Change	=1 if state of practice changes between year $t-1$ and t =0 otherwise
Practice Mode	Type of Organization (0) Solo (1) Partnership (2) Group (3) Government (4) Hospital (5) Other
OB	=1 if classified as having a secondary specialty in obstetrics =0 otherwise
DO	=1 if medical degree is doctor of osteopathic medicine =0 otherwise
Female	=1 if female =0 if male
Age	Physician's age (in years)
Direct Patient Care	=1 if primary activity in practice is direct patient care =0 otherwise
Teach	=1 if primary activity in practice is teaching =0 otherwise
Research	=1 if primary activity in practice is research =0 otherwise
Hours	Percent of hours physician practices at hospital
US trained	=1 if attended medical school in the United States =0 otherwise
Same Location as Residency	=1 if practicing in same state where physician completed residency =0 otherwise
Years of Experience	Number of years between current year and year of medical school graduation

Table 3.5: State Level Variable Descriptions

Variable Name	Description
ln(Population)	Natural logarithm of total population
Percent Urban Population	Percentage of total population living in urban areas
Civilian Unemployment rate	Number of unemployed as a percentage of the labor force
Income per capita	Gross Domestic Product in a state divided by total population of that state
HMO Enrollment per capita	Total HMO Enrollment in a state divided by total population of that state
FM Residency Programs	Number of family medicine residency programs per state
Residency Programs	Total number of residency programs per state
Caps on Noneconomic Damages	Awards for noneconomic damages compensate the plaintiff for pain and suffering. Reforms cap these awards at a statutorily specified amount.
Caps on Punitive Damages	Awards for punitive damages attempt to punish the defendant's misconduct and may be imposed in addition to economic and noneconomic damages. Similar to noneconomic damage caps, this reform caps punitive damages at a statutorily specified amount.
Collateral Source Reform	The common law collateral source rule prevents the admission of evidence that the plaintiff has received compensation from a source other than damages sought against the defendant. For example, a plaintiff's personal insurance coverage for the harm cannot be admitted into evidence under this rule. Reforms to this rule allow other sources of compensation to offset damages awarded.
Joint and Several Liability Reform	Under the joint and several liability rule, a plaintiff can collect damages from any defendant regardless of their share of liability. This is commonly known as the "deep pockets" rule. Reforms to this rule primarily limit what a plaintiff may recover from each defendant according to the proportional share of their liability.

from 1992-2007.[3] This is a random sample of family medicine physicians from each state from 1992 to 2007, proportional to the total number of family medicine physicians in that state in 1992. Each physician in the dataset has a unique, de-identified research identification number. These data include the variables used to measure the choices of these physicians, the practice location, practice mode, and obstetric service offerings. This research data also includes information about each physician's primary and secondary specialty, medical education and training, other personal characteristics such as gender, age, nationality, and the extent of teaching, research, and direct patient care in their daily activities. This information is used to control for individual characteristics affecting physician choices. In some specifications, these variables are used to estimate the effect of certain physician attributes.

In addition to individual-specific data, this analysis also includes information pertaining to the alternatives – locations, practice mode types, and obstetric service offerings – to control for alternative-specific factors that may influence physician decision-making. Data describing location-specific attributes were collected from various United States government resources including the Census Bureau (Statistical Abstract of the United States), the Bureau of Economic Analysis, and the Bureau of Labor Statistics. These data include population, income per capita, civilian unemployment rate, percent of total population living in urban areas,[4] and HMO enrollment per capita.[5] In addition, the number of residents and residency programs (family medicine and total) were obtained from information in the National Graduate Medical Education Census, also maintained by the AMA.[6] Models that assess the choice of practice mode also control for differences between practice types including the extent of direct patient care, teaching, and research. These mode-specific attributes may encourage a physician to choose one practice mode over another and

[3]Panel is unbalanced, but 75 percent of the physicians are observed 15 years or more, and only 8.65 percent were observed less than 3 years.

[4]This variable is not available every year, so a straight line interpolation was used to generate yearly levels as described in Census methodology

[5]Ideally, this analysis would also control for the activity of National Health Service Corps (NHSC) programs. These programs were designated specifically to reduce shortages of health care professionals, mostly in rural areas. The programs mainly focus on providing loan repayment and scholarship to providers who are willing to provide primary care in underserved areas. Unfortunately, data on the amount of loan repayment and scholarships by state is only available after 2010.

[6]Data from the Graduate Medical Education Database, Copyright 2013, American Medical Association, Chicago, Illinois.

vice-versa. Unfortunately, there is no information available for the attributes of different obstetric service offerings.[7]

To assess the impact of changes in liability reforms, data describing state medical malpractice laws were collected from the Database of State Tort Law Reforms, DSTLR 4^{th}(Avraham 2011). This analysis considers four primary reforms, caps on noneconomic and punitive damages, joint and several liability, and collateral source reform.[8] Each tort reform variable is coded as a binary variable.[9] Therefore, there is no distinction between different levels of caps, or variations on other reforms.[10]

It is reasonable to expect that noneconomic damage caps, punitive damage caps, and reforms to the collateral source rule should lessen the burden on physicians, thereby increasing the probability that a physician will choose to practice in a location with these reforms in effect. These reforms should also increase the probability of offering obstetric services. On the other hand, joint and several liability increases the burden on physicians since the reform limits the extent to which other practitioners like nurses, physician assistants, or other physicians in the practice, as well as hospitals or other administrative agencies can be held liable for patient harm.[11] This places most of the responsibility for liability on the attending physician and other defendants may only be held accountable for their proportional share of the liability. So, reforms to this rule should decrease the probability that a physician will choose to practice in a location with these reforms. The implementation of this reform should also decrease the probability that a physician offers obstetric

[7]The measure of obstetric service offerings is a binary variable equal to one if the physician is board certified to practice obstetrics. Information on the cost of obtaining this training, the timing of certification, and other attributes is not available.

[8]Caps on total damages, caps on contingency fees, mandatory periodic payments, and patient compensation fund reform are also considered as a specification check. Models including these reforms are not reported because convergence cannot be achieved using these specifications.

[9]Tort reform indicator variables are equal to one at the start of the calendar year in which the reform is active, unless the effective date of the reform was on or after July 1. In this case, it is coded as a one beginning with the following calendar year.

[10]Born and Neale (2013) find an improvement in insurer profitability with noneconomic damage cap levels set at $250,000, but caps in excess of this amount have little to no effect. All but two observations in this dataset have caps on noneconomic damages in excess of $250,000. Therefore, it seems unlikely that the cap level will be a binding constraint in most cases.

[11]In many cases, the attending physician is held liable, at least partially, for any mistakes made by subordinate practitioners. They may bear liability for granting nurse practitioners, physicians assistants, and other providers too much authority, or inadequately supervising them. Joint and several liability reform essentially prohibits plaintiffs from suing any party that is not directly involved in patient care, and limits the amount of damages based on each defendants share of the liability. Without this reform in place, physicians may be able to use hospital or group resources to fight a malpractice case, since the hospital or group may also be at risk of facing suit. Without this reform, attending physicians tend to bear the burden of a malpractice case alone.

Table 3.6: Summary Statistics, Physician Level

Variable	Mean	Std. Dev.	Minimum	Maximum
Location Change	0.025	0.156	0	1
Practice Mode	2.330	1.819	0	5
Obstetric Services	0.013	0.113	0	1
DO	0.078	0.268	0	1
Female	0.212	0.409	0	1
Age	48.898	10.902	24	107
Direct Patient Care	0.940	0.238	0	1
Teach	0.021	0.143	0	1
Research	0.002	0.048	0	1
Hours	16.592	26.350	0	100
US trained	0.849	0.358	0	1
Same Location as Residency	0.569	0.495	0	1
Years of Experience	21.095	11.030	0	91
$ln(Population)$	15.850	0.923	13.052	17.406
Percent Urban Population	77.182	12.663	38.18	100
Civilian Unemployment Rate	5.328	1.407	2.3	11.2
Income Per Capita	28,369.54	6,670.461	14,651	64,579
HMO Enrollment per capita	0.220	0.119	0	0.773
Family Medicine Residency Programs	17.108	11.805	1	42
Residency Programs	299.314	265.173	1	1116
Caps on Noneconomic Damages	0.356	.479	0	1
Caps on Punitive Damages	0.521	.500	0	1
Collateral Source Reform	0.652	0.476	0	1
Joint and Several Liability Reform	0.725	0.447	0	1

care. The effect on the choice of practice mode may be different. To influence a physician's choice of practice mode, a reform must affect how the malpractice liability burden is shared among practitioners. Theoretically, noneconomic damage caps, punitive damage caps, and collateral source reform affect the financial pressure of malpractice claims, but not how that pressure is shared among practitioners. So, these reforms could either increase or decrease the probability that a physician chooses a certain type of practice relative to others. Reforms to the joint and several liability rule reduce the liability of practitioners who were not directly involved in patient care. So, if medical malpractice liability induces physicians to choose practices where they may be shielded from liability by the "deep pockets" rule, the implementation of this reform would eliminate this advantage to practicing in a hospital, for example. Therefore, the probability of choosing hospital practice should decrease relative to other options in association with the implementation of reforms to the joint and several liability rule.

3.4 Econometric Models

Discrete choice models have been used in economics to explain choices between discrete alternatives, as opposed to the continuous choices made in most traditional economic models. These models answer the questions of which one instead of how much as in standard continuous cases. Thus, they are an appropriate approach to the following questions: does tort reform affect in which state a physician chooses to practice, does tort reform impact the type of organization of a physician's practice, and does tort reform alter the decision of whether or not a family medicine physician will offer obstetric services? These models estimate the probability that an individual chooses a particular alternative. Probabilities are a function of observed variables that relate to the individual or the alternative. The use of discrete choice models in economics is derived from utility theory. This makes discrete choice models a sensible tool to examine physician decision-making on the choices of practice location, mode, and obstetric service offerings. All of these choices can be modeled in a similar way using the logistic distribution. The specific model will depend on the set of alternative-specific and case-specific regressors used.

3.4.1 Fixed effects logit - location choice

The first set of models assess the effect of tort reforms on the choice of practice location. Since reforms change at the state level, the choice of location is considered at the state level. A full

listing of the location alternatives and the distribution of the sample over each state is provided in Table 3.7. A physician's utility of choosing any state depends on both characteristics of the physician and the state. The utility for physician i of choosing state s in year t is given by

$$U_{ist} = V_{ist} + \epsilon_{ist} \tag{3.1}$$

where V_{ist} is the deterministic portion of the utility function and ϵ_{ist} is the stochastic error. The deterministic portion is comprised of a vector of attributes of choosing the alternative, state-specific regressors, and a vector of characteristics of physician i. The deterministic portion of utility can be modeled as

$$V_{ist} = \alpha_s + \theta_t + c_i + L_{st}\beta + \delta \ln(P_{st}) + X_{st}\eta + Z_{it}\gamma_s \tag{3.2}$$

where α_s, c_i, and θ_t are state, physician, and time fixed effects, respectively. L_{st} denotes the specification of state liability law reforms. It is the explanatory variable of interest for this study. For each reform, there is a binary variable where a one indicates that a reform was active, and a zero indicates otherwise. Other state-specific regressors are also included in the model. P_{st} is the population in state s in year t. X_{st} includes the number of civilian family medicine residency programs in state s in year t, a measure of managed care usage, health maintenance organization (HMO) enrollment per capita in state s in year t, the percentage of the population living in urban areas in state s in year t, income per capita in state s in year t, and the civilian unemployment rate in state s in year t. The model also includes a binary variable equal to one if the state chosen is the same state in which the physician completed residency. Z_{it} includes the following physician-specific regressors: a binary variable equal to one if the physician is female, the physician's age given in years, and years of experience, measured as the difference between the current year and the year the physician graduated from medical school. The probability that the observed outcome is physician i choosing state s in year t is given by

$$Pr(y_{it} = s) = Pr(U_{ist} \geq U_{irt}), \forall r \tag{3.3}$$

Though information is available for both the chosen alternative and the other alternatives in the set, the dependent variable is altered into a simple binary movement choice equal to one if the physician moved from one state in year $t-1$ to another state in year t and zero otherwise. Because

Table 3.7: Distribution of Sample over Location Alternatives

	Count	Percent		Count	Percent
AL	5,528	1.43	MT	1,945	0.50
AK	1,585	0.41	NE	3,866	1.00
AZ	6,786	1.75	NV	1,898	0.49
AR	4,908	1.27	NH	1,953	0.50
CA	41,118	10.62	NJ	7,057	1.82
CO	8,212	2.12	NM	3,208	0.83
CT	2,866	0.74	NY	17,043	4.40
DE	1,028	0.27	NC	11,955	3.09
DC	772	0.20	ND	1,507	0.39
FL	18,977	4.90	OH	15,990	4.13
GA	8,675	2.24	OK	4,707	1.22
HI	1,193	0.31	OR	5,679	1.47
ID	2,535	0.65	PA	20,733	5.36
IL	16,274	4.20	RI	1,044	0.27
IN	10,918	2.82	SC	6,535	1.69
IA	6,676	1.72	SD	1,860	0.48
KS	5,544	1.43	TN	7,442	1.92
KY	5,990	1.55	TX	25,876	6.68
LA	4,556	1.18	UT	2,832	0.73
ME	3,056	0.79	VT	1,385	0.36
MD	5,799	1.50	VA	10,914	2.82
MA	4,984	1.29	WA	13,305	3.44
MI	12,500	3.23	WV	3,055	0.79
MN	13,292	3.43	WI	10,780	2.78
MS	3,636	0.94	WY	930	0.24
MO	6,184	1.60	Total	387,091	100.00

there are 51 alternatives in the location choice set, programming packages struggle to converge without this transformation.[12] This set of regressions will use the fixed effects logit model. There are three main advantages to using this estimation method. First, using the fixed effects logit model controls for time-invariant unobservables that may affect a physician's choice of location, like climate and other factors. Second, it is possible to control for observable state-level characteristics, which is not possible if the multinomial logistic regression is used. Lastly, though this model uses data only for those whose location changes over the sixteen year panel,[13] the results are generalizable as long as the assumptions of the model are valid. There are two distinct disadvantages to using this estimation method. First, there is a loss of the ability to estimate effects for each alternative. However, since the goal is to assess the impact of a reform on physician location choice, and not the impact of reforms across states, the results from a mixed logit specification are not that useful anyway. Second, this estimation method is only valid if there is no serial correlation. Serial dependence is present if errors in different time periods are correlated. This may be a problem if there is a time-varying factor that affects physician location during the panel. Therefore, to use this estimation method we must assume that there is serial independence conditional on the observed covariates and the unobserved effect.

3.4.2 Multinomial logit models - practice mode and obstetric service choices

There are two other physician choices considered in this analysis. Practice mode describes the organization of the practice. Physicians can choose to practice in a solo environment, partnership, group setting, hospital, or government facility.[14] Previous research by Studdert et al. (2005) finds that solo practitioners are more likely to respond to tort reforms. In addition, family medicine physicians can also choose to provide obstetric services to patients after receiving proper training.[15,16] Family medicine physicians can be an important supplement to the existing supply of

[12] Author using STATA 11.2

[13] Information for physicians who do not move states in the course of the study is discarded. This is 17,396 physicians in the sample, or 61.63 percent of the physicians in the sample.

[14] There are several ways to classify these practice mode groups. Specification checks will include changes to these categories.

[15] Though few physicians change their primary specialty over the course of their career, it is common for a physician to take on additional training in the form of a fellowship, or other continuing education, to expand their medical knowledge and service offerings to patients, or to change the trajectory of their career. Family medicine physicians, in particular, often take fellowships in emergency medicine, geriatric medicine, sports medicine, and obstetrics.

[16] In this framework, physicians simply choose whether or not to offer obstetric care, all or nothing

obstetric care. A full listing of the alternative sets and distributions for practice mode and obstetric services is presented in Table 3.8. The models for these choices are also rooted in the physician's utility. Physician i's utility from choosing alternative j in year t can be described as

$$U_{ijt} = V_{ijt} + \epsilon_{ijt} \tag{3.4}$$

$$V_{ijt} = \theta_{jt} + W_{ijt}\beta + L_{it}\delta_j + \eta_j \ln(P_{it}) + X_{it}\lambda_j + Z_{it}\gamma_j \tag{3.5}$$

As in the location choice model, θ_{jt} are time fixed effects. W_{ijt} denotes alternative-specific factors. For practice mode models, this includes measures that control for direct patient care, teaching, and research in each practice type, depending on the primary activity of the physician. There are no alternative-specific variables available for the choice of obstetric services. State-specific controls are also included in this model. Since these state-specific variables do not vary over the alternatives – practice mode or obstetric service offerings – but rather vary based on the practice location of the physician they are considered case-specific regressors. This is why they depict the subscript i instead of s. L_{it} denotes the specification of state liability law reforms where physician i practices in year t. P_{it} is the population of the state where physician i practices in year t. X_{it} includes the percentage of the population living in urban areas in the state where physician i practices in year t, as well as income per capita, and the civilian unemployment rate. State-specific variables are included in these other choice models to address concerns about whether states that implement reforms are somehow different from states that do not because of other characteristics, a type of omitted variable bias. Finally, Z_{it} includes the following physician-specific regressors; a binary variable equal to one if the physician is female, a binary variable equal to one if the physician obtained a doctor of osteopathic medicine degree, the physician's age given in years, and years of experience.

When only case-specific regressors are included and no alternative-specific regressors are included, as is done in this analysis, the model reduces to the multinomial logistic regression.[17] These data limitations are common in categorical studies with several mutually exclusive options because it is rare that explanatory variables are available for all alternatives. Typically, only characteristics of the chosen alternative are documented. Therefore, the multinomial logit model is commonly

[17]Unfortunately, the inclusion of direct patient care, teach, and research prevent these models from converging. Therefore, effects of these variables cannot be estimated

Table 3.8: Distribution of Sample over Practice Mode and Obstetric Alternatives

Practice Mode	Count	Percent	Obstetrics Services	Count	Percent
Solo	90,841	23.47	Do Not Offer	382,073	98.70
Partnership	28,474	7.36	Offer	5,018	1.30
Group	134,458	34.74	Total	387,091	100.00
Government	20,666	5.34			
Private Hospital	20,788	5.37			
Total	387,091	100.00			

used and also simpler than other multinomial models. Parameters are also easy to interpret since the multinomial logit model is an expansion of the binary logit model. The primary difference between the interpretation is the simplification of the base category. In multinomial logit estimation, the parameters for one alternative must be set to zero. This alternative is called the base alternative, and all estimate interpretations are relative to the base alternative. The probability that the observed outcome is physician i choosing alternative j is given by

$$Pr(y_{it} = j|y_{it} = 1) = \frac{exp(\theta_{jt} + W_{ijt}\beta + L_{it}\delta_j + \eta_j \ln(P_{it}) + X_{it}\lambda_j + Z_{it}\gamma_j)}{1 + \sum_j exp(\theta_{jt} + W_{ijt}\beta + L_{it}\delta_j + \eta_j \ln(P_{it}) + X_{it}\lambda_j + Z_{it}\gamma_j)} \tag{3.6}$$

where $y_{it} = 1$ is the base alternative. The base alternative for practice mode choice is solo practice and the base alternative for offering obstetric services is not offering obstetric care. One disadvantage of the multinomial logit model is the independence of irrelevant alternatives (IIA) assumption.[18] When a standard logit model assumes no correlation, it includes a particular pattern of substitution for alternatives that may not be accurate. If we expect a deviation from this assumption, another type of model is needed. Therefore, this study will also estimate mixed logit models, which allow for a time-constant, unobserved effect and relax this assumption.[19]

3.4.3 Temporary and permanent reforms

Finally, there is one institutional characteristic of the reform environment that must be considered. Not all reforms are permanent. If there is significant risk of reform nullification, decision-makers may delay behavior modifications until a reform's future is more certain (Grace and Leverty 2013). Recent work by Grace and Leverty (2013) defines reforms in a different way to adequately

[18]The conditional (FE) logit doesn't require this assumption

[19]Multinomial probit models also relax this assumption using the additive random utility model (ARUM). Computation is often difficult. Both methods use a maximum simulated likelihood estimator.

address reluctance on the part of insurance providers to change premiums as tort reforms occur. They contend that reforms should be segmented into temporary, those that are eventually declared unconstitutional or repealed, and permanent reforms, those that are unchallenged or upheld by the courts. They find that studies that combine temporary and permanent reforms in the literature incorrectly estimate the effect of tort reform. Ellyson (2015*a*) also tests for this effect in physician supply, but the effects are smaller and not as significant. Therefore, this study also tests for this effect in physician-level decision making since it may be lost in the aggregate analysis.

3.5 Analysis

3.5.1 Location choice

Table 3.9 presents results for regressions modeling the choice of practice location. The most flexible specification of reforms is presented. It includes reforms active in the current year, t, reforms active in the previous year, $t-1$, and an interaction between the two, $t-1$ and t, where the reform is active in both years. Each of these reforms is coded in relation to the location of the physician. For example, if a physician lived in Florida in year $t-1$ where caps on noneconomic damages were not active in year $t-1$, and then moved to Georgia in year t where caps on noneconomic damages were active, the reform variable, Caps on Noneconomic Damages$_t$ would be equal to one, the reform variable, Caps on Noneconomic Damages$_{t-1}$ would be equal to zero, and the interaction, Caps on Noneconomic Damages$_{t-1,t}$ would be equal to zero. The variable Caps on Noneconomic Damages$_t$ would measure the effect of the active reform in Georgia while controlling for the lack of an active cap on noneconomic damages in the physician's previous location of Florida.

The first column presents a model which includes only reforms and year dummies. Results presented in the second column include reforms, year dummies, and alternative-specific regressors. The third column displays results which include all the aforementioned regressors, and adds individual-specific attributes including female, age, same location as residency, and years of experience. The impact of all reforms is statistically significant at the one percent level in all specifications. For each of the reforms enacted in t, the probability that a physician moves between year $t-1$ and year t increases with the implementation of a reform. So, physicians may decide to move to a state where reforms will be active in the next year. The probability also increases that a physician moves between year $t-1$ and year t if a reform is implemented in year $t-1$. This effect is expected

Table 3.9: Fixed Effects Logit Estimated Coefficients for Location Choice

Variable	(1)	(2)	(3)
Caps on Noneconomic Damages$_t$	2.081	2.161	2.081
	(0.073)***	(0.075)***	(0.080)***
Caps on Punitive Damages$_t$	2.929	3.001	3.023
	(0.072)***	(0.076)***	(0.082)***
Collateral Source Reform$_t$	3.420	3.376	3.447
	(0.104)***	(0.108)***	(0.117)***
Joint and Several Liability$_t$	2.836	2.773	2.723
	(0.103)***	(0.105)***	(0.111)***
Caps on Noneconomic Damages$_{t-1}$	2.886	2.886	2.882
	(0.093)***	(0.094)***	(0.100)***
Caps on Punitive Damages$_{t-1}$	4.092	4.109	4.165
	(0.106)***	(0.107)***	(0.117)***
Collateral Source Reform$_{t-1}$	3.564	3.583	3.648
	(0.105)***	(0.107)***	(0.115)***
Joint and Several Liability$_{t-1}$	2.791	2.886	2.942
	(0.120)***	(0.122)***	(0.130)***
Caps on Noneconomic Damages$_{t-1,t}$	-5.578	-5.577	-5.525
	(0.123)***	(0.125)***	(0.133)***
Caps on Punitive Damages$_{t-1,t}$	-7.868	-7.926	-8.021
	(0.130)***	(0.132)***	(0.144)***
Collateral Source Reform$_{t-1,t}$	-6.866	-6.961	-6.977
	(0.142)***	(0.146)***	(0.156)***
Joint and Several Liability$_{t-1,t}$	-5.243	-5.453	-5.477
	(0.150)***	(0.154)***	(0.165)***

Results continued on next page

Table 3.9 Continued

Variable	(1)	(2)	(3)
FM Residency Programs$_t$		-0.066	-0.046
		(0.005)***	(0.006)***
HMO Enrollment per capita$_t$		1.617	1.345
		(0.378)***	(0.402)***
Percent Urban Population$_t$		0.012	0.006
		(0.004)***	(0.004)
ln(Population)$_t$		0.330	0.276
		(0.064)***	(0.068)***
Income per capita$_t$		0.000	0.000
		(0.000)	(0.000)
Civilian Unemployment rate$_t$		0.116	0.128
		(0.029)***	(0.031)***
Female$_t$			1.920
			(1.316)
Age$_t$ (in years)			-0.007
			(0.154)
Same Location as Residency$_t$			-1.323
			(0.068)***
Years of Experience$_t$			-0.092
			(0.554)
Years of Experience$_t{}^2$			0.004
			(0.0003)***
Physician- and Year-fixed effects	Yes	Yes	Yes
Likelihood Ratio χ^2	$28,170.73^{***}$	$28,206.67^{***}$	$26,363.41^{***}$
Degrees of Freedom	26	32	37
Number of Observations	78,261	77,767	72,138

Note. The dependent variable is a binary variable equal to one if the physician moved from one state in year t-1 to another in year t. Reported standard errors are not clustered at the physician-level since serial independence conditional on observed covariates and unobserved effect are assumed. The following case-specific regressors are not included because convergence cannot be achieved; percent of hours practicing in hospital, and binary variable equal to one if training obtained in the US and zero otherwise.

* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

for joint and several liability reform since it increases the malpractice pressure on physicians, but it is a strange result for the other reforms.[20] Furthermore, the impact of reforms enacted on the probability of moving is quite large. For example, the probability of moving is 701.2 percent[21] higher for states that enact new caps on noneconomic damages in year t than states that do not. The effect is similarly large for other reforms.[22] The size of these effects seems very large, but could be plausible if the initial probability of moving when there are no reforms is very small.

To obtain the total effect of reforms in place in both years $t-1$ and t, the coefficients for reforms active in $t-1$, reforms active in t, and reforms active in both $t-1$ and t must be added together ($\delta_{Reform_t}+\delta_{Reform_{t-1}}+\delta_{Reform_{t-1,t}}$ = Total Effect of Reform). These calculations yield the following values: Caps on Noneconomic Damages (-0.562), Caps on Punitive Damages (-0.833), Collateral Source Reform (0.118), Joint and Several Liability (0.189). Wald tests[23] indicate that the total effect of caps on noneconomic damages and caps on punitive damages are statistically different than zero at the one percent level. The total effect of joint and several liability is statistically different than zero at the ten percent level, but the total effect of collateral source reform is not significantly different than zero. So, caps on noneconomic damages and caps on punitive damages that are active in both years $t-1$ and t correspond to a decrease in the probability of a physician changing states between year $t-1$ and t. Specifically, the probability of moving is 43.0 percent lower for states that have active noneconomic damage caps in year $t-1$ and t and 56.5 percent lower for states that have active punitive damage caps in year $t-1$ and t than states that do not. In other words, physicians in states with stable damage cap reforms are less likely to change location. Joint and several liability reform that is active in both years $t-1$ and t has a positive but smaller and less significant effect on the probability of moving. Therefore, physicians are more likely to move out of states with stable and active joint and several liability reform.

Other location-specific controls and individual-specific controls have expected signs. Family medicine physicians are less likely to move if there are more family medicine residency programs

[20] Lagged state characteristic variables were included in regressions as a specification check, to see if perhaps the lack of controls of state characteristics for year $t-1$ lead to this strange result. However, this check does not dramatically alter the size, sign, or significance of these effects.

[21] This interpretation of the coefficient uses the natural logarithm of the odds ratio. So, these percent changes are found using the following formula: $exp(\delta)-1$

[22] The probability that a family medicine physician changes state between $t-1$ and t is 1955.3 percent higher for states that enact new caps on punitive damages, 3040.6 percent higher for states that enact new collateral source reform, and 1422.6 percent higher for states that enact new joint and several liability reform than those that do not enact these reforms.

[23] The null for these tests for each reform is H_0: $\delta_{Reform_t}+\delta_{Reform_{t-1}}+\delta_{Reform_{t-1,t}}=0$

in the state in which they currently practice. An additional family medicine residency program decreases the probability of moving by 4.5 percent. This indicates that physicians prefer locations which supply more teaching opportunities. This result is statistically significant at the one percent level. Family medicine physicians are also less likely to change states if they live in the state where they completed their graduate medical training. The probability of moving is 73.4 percent lower if a physician is currently practicing in a state where they completed residency than if they are not. Physicians are more likely to move out of states with high HMO enrollment per capita, high population, and high civilian unemployment rates. These three results are also statistically significant at the one percent level.

Table 3.10 presents results that control for the difference between temporary and permanent reforms in the model of the choice of practice location. A new set of variables is created for these tests. Permanent: Reform_t is equal to one if a reform enacted in year t is unchallenged or upheld by the courts during the rest of the panel and zero otherwise. In addition, Temporary: Reform_t is equal to one for reforms that are enacted in year t and then declared unconstitutional by courts or reversed by legislative action and zero otherwise. Therefore, there are two new variables for each reform in the dataset. Permanent: Reform_{t-1} and Temporary: Reform_{t-1} are the lagged values of Permanent: Reform_t and Temporary: Reform_t. Coefficients for reform regressors are presented in Table 3.10 while control regressors and summary results are given in Table 3.5.1. Similar to the results presented in Table 3.9 and Table 3.5.1, these specifications present a flexible definition of reforms, including reforms active in the current year, t, and reforms active in the previous year, $t-1$.[24] As in the first specification, the first column presents a model which includes only reforms and year dummies. The second column includes reforms, year dummies, and alternative-specific regressors. The third column provides results for all the aforementioned regressors, and adds case-specific variables.

Similar to Grace and Leverty (2013), this analysis reveals that permanent reforms have a more significant impact on physician location choice than temporary reforms. Permanent caps on damages, noneconomic and punitive, and permanent collateral source reform enacted in both year $t-1$ and t statistically significantly impact the probability of moving at the one percent level. Permanent caps on noneconomic damages and collateral source reform enacted in $t-1$ increase the

[24]Interactions between these two variables are not included because convergence cannot be achieved

Table 3.10: Fixed Effects Logit Estimated Coefficients for Location Choice: Temporary and Permanent Reforms

Variable	(1)	(2)	(3)
Temporary: Caps on Noneconomic Damages$_t$	0.149	0.066	-0.004
	(0.083)*	(0.084)	(0.089)
Temporary: Caps on Punitive Damages$_t$	0.153	0.187	0.200
	(0.089)*	(0.092)**	(0.099)**
Temporary: Collateral Source Reform$_t$	-0.136	-0.240	-0.323
	(0.117)	(0.118)**	(0.127)**
Temporary: Joint and Several Liability$_t$	-1.127	-0.685	-0.332
	(0.088)***	(0.100)***	(0.108)***
Permanent: Caps on Noneconomic Damages$_t$	-0.160	-0.119	-0.149
	(0.047)***	(0.048)**	(0.051)***
Permanent: Caps on Punitive Damages$_t$	0.285	0.273	0.219
	(0.045)***	(0.047)***	(0.050)***
Permanent: Collateral Source Reform$_t$	-0.197	-0.236	-0.202
	(0.051)***	(0.053)***	(0.056)***
Permanent: Joint and Several Liability$_t$	-0.046	0.024	0.081
	(0.062)	(0.064)	(0.068)
Temporary: Caps on Noneconomic Damages$_{t-1}$	-0.155	-0.135	-0.081
	(0.081)*	(0.081)*	(0.083)
Temporary: Caps on Punitive Damages$_{t-1}$	-0.158	-0.0791	-0.105
	(0.085)*	(0.086)	(0.088)
Temporary: Collateral Source Reform$_{t-1}$	0.410	0.391	0.404
	(0.114)***	(0.114)***	(0.118)***
Temporary: Joint and Several Liability$_{t-1}$	1.083	1.080	0.889
	(0.084)***	(0.085)***	(0.087)***
Permanent: Caps on Noneconomic Damages$_{t-1}$	0.156	0.176	0.174
	(0.048)***	(0.048)***	(0.049)***
Permanent: Caps on Punitive Damages$_{t-1}$	-0.366	-0.361	-0.354
	(0.044)***	(0.044)***	(0.046)***
Permanent: Collateral Source Reform$_{t-1}$	0.144	0.136	0.197
	(0.049)***	(0.048)***	(0.051)***
Permanent: Joint and Several Liability$_{t-1}$	-0.077	-0.030	-0.053
	(0.060)	(0.060)	(0.062)

Results continued on next page

Table 3.10 Continued

Variable	(1)	(2)	(3)
Family Medicine Residency Programs$_t$		-0.028	-0.015
		(0.004)***	(0.004)***
HMO Enrollment per capita$_t$		0.547	0.254
		(0.224)**	(0.238)
Percent Urban Population$_t$		0.005	-0.002
		(0.003)***	(0.003)
ln(Population)$_t$		0.100	0.108
		(0.042)**	(0.046)**
Income per capita$_t$		-0.00002	0.000001
		(0.00001)**	(0.00001)**
Civilian Unemployment rate$_t$		-0.044	-0.018
		(0.017)**	(0.019)
Female$_t$			1.014
			(0.693)
Age$_t$ (in years)			-0.097
			(0.066)
Same Location as Residency$_t$			-1.564
			(0.044)***
Years of Experience$_t$			-0.244
			(0.303)
Years of Experience$_t{}^2$			0.004
			(0.0002)***
Year-fixed effects	Yes	Yes	Yes
Likelihood Ratio χ^2	1383.51***	1501.25***	3243.27***
Number of Observations	78,261	77,767	72,138

Note. The dependent variable is a binary variable equal to one if the physician moved from one state in year t-1 to another in year t. The following case-specific regressors are not included because convergence cannot be achieved; percent of hours practicing in hospital, and binary variable equal to one if training obtained in the US and zero otherwise. Interactions for reform variables are also not included because convergence cannot be achieved.
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

probability of a physician changing state between year $t-1$ and t. Physicians are 19.0 percent more likely to move when permanent caps on noneconomic damages are enacted in $t-1$ and 21.8 percent more likely to move when permanent collateral source reform is enacted in $t-1$. However, the probability of moving is 13.8 percent and 18.3 percent lower when these reforms are enacted in t, respectively. On the other hand, caps on punitive damages have the opposite effect. Physicians are 29.8 percent less likely to move between $t-1$ and t if permanent caps on punitive damages are enacted in $t-1$ and 24.5 percent more likely to move between $t-1$ and t if permanent caps on punitive damages are enacted in t. So, permanent caps on punitive damages have expected effects, while permanent noneconomic damage caps and permanent collateral source reform have opposite effects.

Interestingly, only temporary joint and several liability reform has a statistically significant effect on the probability of moving, while permanent joint and several liability reforms only have insignificant effects. While other reforms have more lasting effects if they are permanent, temporary joint and several liability quickly impacts the movement choice of family medicine physicians, even if the reform is eventually reversed or overturned. Temporary joint and several liability reform enacted in $t-1$ increases the probability of moving between year $t-1$ and year t. Physicians move out of states that enact this reform quickly. Temporary joint and several liability reform enacted in t decreases the probability that a family medicine physician changes state between year $t-1$ and t. This may indicate that physicians respond quickly to news about changes in liability reform, and are less likely than insurers to delay response until a reform is upheld by the courts.

3.5.2 Practice mode

Table 3.11 presents results for regressions modeling the choice of practice mode. All reforms have a statistically significant effect on the choice of practicing in a group or government setting relative to solo practice. None of the reforms have a statistically significant effect on choosing a partnership over solo practice. Physicians in states that implement joint and several liability reform are more likely to choose group and government practice compared to solo practice at the one percent significance level. Practicing in a state where this reform is enacted increases the relative odds of choosing government practice relative to solo practice that is 1.25 times[25] what the

[25]The interpretation of coefficients from multinomial logit uses the odds ratio or relative-risk ratio of choosing alternative j rather than the base alternative using the coefficient on alternative j, β_j. $exp(\beta_j)$ gives the proportionate

relative odds were before the reform. This result is plausible since military physicians in particular are shielded from malpractice claims through laws and judicial rulings that prohibits claim against military physicians.[26] Family medicine physicians are less likely to choose hospital practice than solo practice with the enactment of joint and several liability reform at the one percent level of significance. Practicing in a state where joint and several liability is enacted decreases the relative odds of choosing hospital practice relative to solo practice that is .808 times what the relative odds were before the reform. This is also an expected result since this reform would increase the burden on physicians practicing in hospitals. They can no longer expect to face suit with their employer, thereby decreasing legal aid and institutional support. These results match theoretical expectations since joint and several liability reform impacts how the liability burden is shared among practitioners.

Physicians in states that pass caps on punitive damages are less likely to choose group and government practice relative to solo practice. Practicing in a state where caps on punitive damages are enacted decreases the relative odds of choosing group practice relative to solo practice that is .930 times what the relative odds were before the reform, while it decreases the relative odds of choosing government practice by .868 times. These effects are statistically significant at the five percent and one percent levels, respectively. This reform does not have a statistically significant impact on choosing a partnership or hospital practice relative to solo practice. These results suggest that a decline in the expected payout of malpractice claims encourages physicians to pursue solo practice through altering financial incentives. Caps on noneconomic damages have the opposite effect, increasing the relative odds of choosing group and government practice relative to solo practice. Therefore, there must be some differences in how these two types of caps affect financial incentives for group and government practices. Perhaps noneconomic damages are covered by the employer in group and government employment contracts, but punitive damage caps are not.

change in the relative risk of choosing alternative j rather than the base alternative when the regressor changes by one unit, If the regressor is a binary variable, $exp(\beta_j)$ gives the proportionate change in the relative risk of choosing alternative j rather than the base alternative when the binary variable is equal to one.

[26]The Federal Tort Claims Act (FTCA) signed into law in 1946 sets the basis for this legal doctrine. Court cases including Feres v. United States (1950), and United States v. Johnson (1987), and a hearing before the United States Senate Committee on the Judiciary of the Second Session of the 177^{th} Congress entitled "The Feres Doctrine: An Examination of this Military Exception to the Federal Tort Claims Act" challenged this doctrine to shield the government from lawsuit, but it has been consistently upheld. Additional cases in recent years are ongoing (German et al v. United States and Price et al v. United States, both filed in 2010), but it is unlikely that the doctrine will be nullified.

Table 3.11: Multinomial Logit Estimated Coefficients for Practice Mode Choice

Variable	Partnership	Group	Government	Hospital
Constant	3.168	5.242	4.653	4.447
	(0.520)***	(0.343)***	(0.504)***	(0.559)***
Caps on Noneconomic Damages	0.030	0.188	0.116	-0.023
	(0.048)	(0.031)***	(0.048)**	(0.054)
Caps on Punitive Damages	0.049	-0.073	-0.142	0.013
	(0.043)	(0.029)**	(0.046)***	(0.048)
Collateral Source Reform	0.001	0.259	-0.118	0.214
	(0.050)	(0.035)***	(0.055)**	(0.055)***
Joint and Several Liability Reform	-0.036	0.180	0.226	-0.213
	(0.051)	(0.036)***	(0.056)***	(0.055)***
Female	0.355	0.304	0.615	0.606
	(0.066)***	(0.045)***	(0.066)***	(0.063)***
DO	-0.054	-0.356	-0.233	0.100
	(0.092)	(0.063)***	(0.098)**	(0.089)
Age	-0.060	-0.068	-0.027	-0.037
	(0.008)***	(0.005)***	(0.007)***	(0.007)***
Experience	0.002	-0.0006	-0.164	-0.210
	(0.010)	(0.007)	(0.010)***	(0.009)***
Experience^2	0.0004	0.00008	0.003	0.003
	(0.0001)***	(0.0001)	(0.0001)***	(0.0001)***
Percent Urban Population	-0.012	-0.016	0.004	-0.010
	(0.003)***	(0.002)***	(0.003)	(0.004)***
ln(Population)	-0.063	-0.067	-0.256	-0.116
	(0.036)*	(0.024)***	(0.036)***	(0.038)***
Income per capita	0.00003	0.00003	0.00002	0.0001
	(0.00001)***	(0.00001)***	(0.00001)**	(0.00001)***
Civilian Unemployment rate	-0.045	-0.117	0.050	0.013
	(0.020)**	(0.014)***	(0.021)**	(0.020)

Note. The base alternative in this specification is solo practice. Regressions include year dummies. Reported standard errors are given in parenthesis and are clustered at the individual level. The following case-specific regressors are not included because convergence cannot be achieved; percent of hours practicing in hospital, binary variable equal to one if training obtained in the US and zero otherwise, binary variable equal to one if state of practice is same as state of residency and zero otherwise. Measures of work environment, direct patient care, teach, and research, are also not included because convergence cannot be achieved.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

Similar to the effects of joint and several liability, physicians in states that enact collateral source reform are more likely to choose group practice relative to solo practice, but less likely to choose government and hospital practice relative to solo practice. All of these results are statistically significant at the one percent level. Overall, these reforms seem to have the strongest effect on choosing group or government practice relative to solo practice.

Other case-specific regressors have expected results. Being female increases the probability of choosing partnership, group, government and hospital practice relative to solo practice. This result is statistically significant at the one percent level for all alternatives.[27] Obtaining a doctor of osteopathic medicine degree decreases the probability of practicing in group, or government practice relative to solo practice. This is statistically significant at the five percent level. An additional year in physician age decreases the probability of choosing the four alternatives relative to solo practice, and this result is statistically significant at the one percent level. So, family medicine physicians are more likely to choose solo practice as they grow older. Experience level also has the expected result, with a non-linear effect on choosing an alternative other than solo practice. Physicians are less likely to choose any of these alternatives relative to solo practice as they become more experienced.

Table 3.12 presents results for regressions modeling the choice of practice mode using the temporary and permanent reform specification. These results differ slightly from those using the traditional reform specification. Unexpectedly, temporary reforms have a more significant effect on practice mode choice than permanent reforms. Both temporary caps on noneconomic damages and joint and several liability have a significant effect on the choice of all alternatives. Temporary caps on noneconomic damages decrease the probability of practicing in partnership, group, and hospital practices compared to solo practice but increase the probability of choosing government practice relative to solo practice. Results for group, government, and hospital alternatives are statistically significant at the one percent level, while results for partnership alternative is statistically significant at the five percent level. Temporary joint and several liability has the opposite effect. This reform increases the probability of choosing partnership, group, and hospital practice relative to solo practice, but decreases the probability of choosing group practice compared to solo practice.

[27]Physician surveys find that 53.5 percent of female physicians are employed by a hospital or group compared to 40 percent of male physicians, and only 37.2 percent of female physicians are a practice owner, partner, or associate compared to 52.5 percent of male physicians (Physicians Foundation 2012).

Table 3.12: Multinomial Logit Coefficient Estimates for Practice Mode Choice: Temporary and Permanent Reforms

Variable	Partnership	Group	Government	Hospital
Constant	3.494	5.437	4.528	4.956
	(0.536)***	(0.353)***	(0.516)***	(0.578)***
Temp: Caps on Noneconomic Damages	-0.205	-0.255	0.380	-0.250
	(0.090)**	(0.058)***	(0.089)***	(0.093)***
Temp: Caps on Punitive Damages	-0.315	-0.338	-0.453	-0.080
	(0.090)***	(0.062)***	(0.107)***	(0.088)
Temp: Collateral Source Reform	-0.148	-0.103	-0.272	-0.071
	(0.121)	(0.080)	(0.126)**	(0.141)
Temp: Joint and Several Liability Reform	0.297	0.286	-0.473	0.395
	(0.090)***	(0.061)***	(0.105)***	(0.089)***
Perm: Caps on Noneconomic Damages	0.026	0.179	0.108	-0.001
	(0.051)	(0.033)***	(0.053)**	(0.059)
Perm: Caps on Punitive Damages	0.098	-0.020	-0.213	0.046
	(0.050)**	(0.033)	(0.052)***	(0.056)
Perm: Collateral Source Reform	0.035	0.320	-0.097	0.239
	(0.053)	(0.037)***	(0.058)*	(0.060)***
Perm: Joint and Several Liability Reform	-0.053	0.188	0.050	-0.103
	(0.069)	(0.048)***	(0.72)	(0.076)
Female	0.351	0.303	0.617	0.603
	(0.066)***	(0.045)***	(0.066)***	(0.063)***
DO	-0.075	-0.371	-0.173	0.048
	(0.092)	(0.063)***	(0.099)*	(0.090)
Age	-0.059	-0.067	-0.029	-0.035
	(0.008)***	(0.005)***	(0.007)***	(0.007)***
Experience	0.001	-0.001	-0.163	-0.207
	(0.010)	(0.007)	(0.001)***	(0.009)***
Experience^2	0.0004	0.0001	0.003	0.003
	(0.0001)***	(0.0001)	(0.0001)***	(0.0001)***
Percent Urban Population	-0.015	-0.019	0.004	-0.013
	(0.003)***	(0.002)***	(0.003)	(0.004)***
ln(Population)	-0.069	-0.063	-0.214	-0.148
	(0.037)*	(0.025)**	(0.037)***	(0.039)***
Income per capita	0.00002	0.00003	0.00002	0.0001
	(0.00001)**	(0.00001)***	(0.00001)*	(0.00001)***
Civilian Unemployment rate	-0.048	-0.120	0.040	0.010
	(0.021)**	(0.014)***	(0.021)*	(0.021)

Note. The base alternative in this specification is solo practice. Regressions include year dummies. Reported standard errors are given in parenthesis and are clustered at the individual level. The following case-specific regressors are not included because convergence cannot be achieved; percent of hours practicing in hospital, binary variable equal to one if training obtained in the US and zero otherwise, binary variable equal to one if state of practice is same as state of residency and zero otherwise. Measures of work environment, direct patient care, teach, and research, are also not included because convergence cannot be achieved.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

These results are all significant at the one percent level. In addition, temporary caps on punitive damages decrease the probability that a physician chooses any alternative relative to solo practice, but only the results for partnership, group, and government practice are statistically significant at the one percent level.

Permanent effects are not as strong. Permanent caps on both noneconomic and punitive damages have selectively significant effects on the choice of practice mode. Permanent caps on noneconomic damages increase the probability of choosing both group and government practice relative to solo practice. This is statistically significant at the one percent and five percent levels, respectively. Both permanent joint and several liability and collateral source reforms increase the probability of a physician choosing group practice relative to solo practice. Both of these results are statistically significant at the one percent level. As in the original specification, being female increases the probability of choosing all alternatives relative to solo practice, and older physicians are less likely to choose these alternatives relative to solo practice. Both of these results are statistically significant at the one percent level.

3.5.3 Obstetric service offerings

Finally, this analysis considers a family medicine physician's choice of whether or not to provide certain obstetric services like prenatal care, and labor and delivery. This decision to provide obstetric care is especially important since family medicine physicians are either the closest obstetric provider for rural patients or provide all obstetric care to rural patients (American College of Obstetricians and Gynecologists 2014). Specifically, "in 2010, 49% of the 3,143 U.S. counties lacked an obstetrician-gynecologist" (American College of Obstetricians and Gynecologists 2014, 2). Despite this need for ready access to prenatal care and obstetric care, "practice data show that obstetric services provided by family physicians is decreasing, with only 19.2% providing routine deliveries" (2). If these providers decide to discontinue the provision of obstetric services due to malpractice pressure, patients may have to travel far distances for basic prenatal care and labor and delivery. No previous studies have considered the impact of liability reforms on the choice of offering obstetrics.

Table 3.13 presents results for regressions modeling the choice of obstetric service offerings. These reforms do have the expected directional impact. Caps on noneconomic and punitive damages and collateral source reform decrease malpractice pressure, so family medicine physicians should

Table 3.13: Multinomial Logit Estimated Coefficients for Obstetric Care Choice

Variable	(1)	(2)	(3)
Intercept	-4.737	-5.602	-3.019
	(0.107)***	(0.399)***	(1.110)***
Caps on Noneconomic Damages	0.065	0.061	0.061
	(0.098)	(0.098)	(0.099)
Caps on Punitive Damages	0.136	0.140	0.197
	(0.095)	(0.095)	(0.100)**
Collateral Source Reform	0.238	0.230	0.325
	(0.113)**	(0.113)**	(0.117)***
Joint and Several Liability	-0.110	-0.117	-0.157
	(0.118)	(0.118)	(0.119)
Female		0.188	0.208
		(0.129)	(0.130)
Age (in years)		0.019	0.018
		(0.012)	(0.012)
DO		-0.113	-0.096
		(0.216)	(0.217)
Experience		0.008	0.009
		(0.018)	(0.018)
Experience^2		0.000	-0.0003
		(0.000)	(0.0003)
HMO Enrollment per capita			0.607
			(0.649)
Percent Urban Population			-0.004
			(0.007)
ln(Population)			-0.145
			(0.074)**
Income per capita			-0.00004
			(0.00002)*
Civilian Unemployment rate			0.076
			(0.040)*
Year-fixed effects	Yes	Yes	Yes
Wald Statistic	61.70***	94.22***	114.17***
Number of Observations	774,182	774,182	774,182

Note. The base alternative in this specification is no obstetric service offerings. Reported standard errors are given in parenthesis and are clustered at the individual level in all specifications. The following case-specific regressors are not included because convergence cannot be achieved; percent of hours practicing in hospital, binary variable equal to one if training obtained in the US and zero otherwise, binary variable equal to one if state of practice is same as state of residency and zero otherwise, direct patient care, teach, and research.

* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Table 3.14: Multinomial Logit Estimated Coefficients for Obstetric Care Choice: Temporary and Permanent Reforms

Variable	(1)	(2)	(3)
Intercept	-4.779	-5.640	-2.949
	(0.137)***	(0.413)***	(1.121)***
Temporary: Caps on Noneconomic Damages	0.267	0.260	0.192
	(0.171)	(0.172)	(0.172)
Temporary: Caps on Punitive Damages	0.225	0.238	0.168
	(0.196)	(0.196)	(0.197)
Temporary: Collateral Source Reform	0.177	0.183	0.034
	(0.259)	(0.259)	(0.261)
Temporary: Joint and Several Liability	-0.136	-0.126	0.029
	(0.190)	(0.191)	(0.198)
Permanent: Caps on Noneconomic Damages	0.048	0.045	0.035
	(0.107)	(0.107)	(0.108)
Permanent: Caps on Punitive Damages	0.090	0.094	0.169
	(0.106)	(0.106)	(0.113)
Permanent: Collateral Source Reform	0.232	0.224	0.292
	(0.121)**	(0.121)**	(0.127)**
Permanent: Joint and Several Liability	-0.050	-0.053	-0.063
	(0.150)	(0.151)	(0.151)
Female		0.194	0.208
		(0.129)	(0.130)
Age (in years)		0.018	0.018
		(0.012)	(0.012)
DO		-0.103	-0.097
		(0.217)	(0.217)
Experience		0.008	0.008
		(0.018)	(0.018)
Experience^2		-0.0003	-0.0003
		(0.0003)	(0.0003)
HMO Enrollment per capita			0.754
			(0.646)
Percent Urban Population			-0.004
			(0.007)
ln(Population)			-0.150
			(0.076)**
Income per capita			-0.00003
			(0.00002)**
Civilian Unemployment rate			0.067
			(0.040)*
Year-fixed effects	Yes	Yes	Yes
Wald Statistic	63.42***	96.58***	115.37***
Number of Observations	774,182	774,182	774,182

Note. The base alternative in this specification is no obstetric service offerings. Reported standard errors are given in parenthesis and are clustered at the individual level in all specifications. The following case-specific regressors are not included because convergence cannot be achieved; percent of hours practicing in hospital, binary variable equal to one if training obtained in the US and zero otherwise, binary variable equal to one if state of practice is same as state of residency and zero otherwise, direct patient care, teach, and research.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

be more likely to offer obstetric care to their patients with the implementation of these reforms. Joint and several liability increases the liability burden on physicians, so the probability that a family medicine physician will offer obstetric care should decrease with the implementation of this reform. However, results indicate that reforms have no statistically significant effect or a minimal one on the choice of supplying obstetric care to patients. Table 3.14 presents results for regressions modeling the choice of obstetric service offerings that control for the temporary or permanence of reforms. Similarly, reforms present with the expected increase or decrease in the probability of offering obstetric care to patients, and these effects also have a minimal statistically significant effect.

There are three possible complications in this part of the analysis. First, the measure used for obstetric care is a very crude measure. The available data only provides information on whether or not the physician has board certification in providing obstetric care. It does not indicate whether the physician uses this ability to serve their patients. The analysis would likely provide much more information with a better measure, like the number of prenatal visits in a given year and/or the number of deliveries in a given year. In addition, even if a physician chooses to exercise this ability, the type of obstetric care provided may vary. Some physicians may simply offer prenatal care, but require that their patients report to a hospital or group practice for labor and delivery. Since prenatal care is relatively low risk, and most complications occur during labor and delivery, the variable used here will not measure these deviations in behavior. Lastly, there may not be enough variation in this variable to adequately measure the physician's choice. There are few family medicine physicians in this sample that have board certification in obstetric care. In addition, the percent distribution of the binary choice of offering obstetric care or not does not change much over the panel. Specifically, in 1992, 1.02 percent of the sample offered obstetric care in their practice. In 2007, 1.37 percent of the sample offered obstetric care in their practice. This may limit any ability to identify the effects of reforms on this choice.

3.6 Conclusions

This study provides strong evidence that physicians consider liability reforms in practice decision-making, especially the choices of location, and practice mode. All of the reforms considered here have a statistically significant impact on a physician's choice of practice location. Reforms also

statistically significantly alter a physician's choice of practice organization, especially those that change how the liability burden is shared in the practice. Furthermore, physicians alter their behavior regardless of whether or not reforms are permanent. So, even though temporary reforms are not effective in impacting malpractice premiums or insurance underwriting, they may be a potent tool in changing the distribution of physicians, especially in terms of practice mode. The most powerful conclusion from this analysis demonstrates that traditionally "low-risk" specialties almost certainly alter their behavior in response to changes in malpractice pressure.

There is certainly room to expand on this research. First, this analysis only considers liability reforms, but there are other levels of malpractice pressure that could also influence physician decision-making. Future studies should consider combining malpractice insurance coverage and premiums with liability reforms to measure the impact on physician choices. Second, this research agenda focused primarily on obtaining alternative-specific regressors for the location choice models since reforms change at the state level. This makes the location choice models the most robust in terms of the controls included. However, simple multinomial logistic models of practice mode choice indicate that physicians have strong practice choice responses to the enactment of reforms. More information on the characteristics of practice mode alternatives would certainly improve the investigation. In addition, a more accurate measure of obstetric service offerings as well as attributes for these offerings would also improve the analysis of practice choices.

CHAPTER 4

SHORT-TERM AND LONG-TERM EFFECTS OF LIABILITY REFORMS ON PREVENTABLE DISEASE

4.1 Introduction

Recent research reveals that traditionally "low-risk" specialties are not immune to the burden of malpractice pressure [Li and Dor (2015); Ellyson (2015*a*); Ellyson (2015*b*)]. Physician surveys consistently report that defensive medicine, any action taken by health care professionals to reduce their legal liability, is a common practice and problematic for the United States healthcare system. [Bishop, Federman and Keyhani (2010); Jena et al. (2011); Sirovich, Woloshin and Schwartz (2011)]. Positive defensive medicine is characterized by excessive testing or unnecessary procedures, over-prescribing care to avoid a malpractice claim. This type of defensive medicine is often called "assurance behaviors." Negative defensive medicine often involves the avoidance of risky patients or discontinuance of risky services that may be medically beneficial. This type of defensive medicine is also referred to as "avoidance behaviors." Avoidance behaviors may disproportionately affect patients in rural communities or patients in a low socioeconomic class who have limited access to care. Existing studies consider "high-risk" patient outcomes like birth outcomes or heart illness outcomes. If physicians in "low-risk" specialties like family medicine also practice defensive medicine, it is important to consider outcomes for the patients of these physicians as well.

One approach to model the impact of tort reform on primary care patient outcomes is to study preventable diseases. By addressing these preventable diseases with low-cost, lifestyle changes, many patients can reverse or delay the onset of complications from conditions like obesity, and type II diabetes which often have a number of comorbidities (Waring et al. 2009). Both of these diseases are quite common in the American population. In 2011-2012, the prevalence of obesity in the United States was 16.9 percent in youth and 34.9 percent in adults (Ogden et al. 2014). Family medicine physicians, as the most common first point of contact with a healthcare professional, are uniquely placed to help patients deal with obesity using exercise, healthy eating, and counseling.

Type II diabetes accounts for about 90 to 95 percent of all diagnosed cases of diabetes. Several risk factors for type II diabetes can be identified in a patient by a family medicine physician including age, obesity, family history of diabetes, history of gestational diabetes, impaired glucose metabolism, physical inactivity, and race or ethnicity (Centers for Disease Control and Prevention 2014). Patients with type II diabetes can significantly improve the likelihood of good outcomes by healthy eating, regular exercise, losing weight, and oral and/or injectable medications to lower blood glucose levels.

Moreover, both of these illnesses place a tremendous financial burden on the health care system. Finkelstein et al. (2009) find that "obesity is associated with a 9.1 percent increase in annual medical spending" (w828) and "obese people had per capita medical spending that was $1,429 (42 percent) greater than spending for normal-weight people in 2006" (w826). The Centers for Disease Control (CDC) estimate that the United States spent $176 billion on direct medical costs for diabetes in 2012. If indirect costs like disability, work loss, and premature death are included, tis figure climbs to $245 billion. Furthermore, "after adjusting for population age and sex differences, average medical expenditures among people with diagnosed diabetes were 2.3 times higher than people without diabetes" (Centers for Disease Control and Prevention 2014, 8).

In the study of outcomes, tort reform can only indirectly affect outcomes through the behavior of physicians. Positive defensive medicine may be present in the study of preventable disease if physicians overuse testing. For example, checking hemoglobin A1C, fasting blood sugar, or oral glucose tolerance more frequently than medically necessary. Physicians may also order blood tests for lipids to identify other risk factors. Assurance behaviors may also increase the rates of obesity in type II diabetes if, for example, individuals with pre-diabetes are included in these rates. Negative defensive medicine limits patient access to care. For patients at risk for obesity and type II diabetes, more interaction with a health care professional would involve counseling the patient to alter lifestyle choices and reduce the risk of developing both conditions. Therefore, if patients have difficulty obtaining access to primary care physicians, it is reasonable to suspect that there may be some impact on the prevalence of obesity and type II diabetes.

4.2 Literature Review

Several existing studies consider the impact of liability reforms or malpractice pressure on patient outcomes. Kessler and McClellan (1996) measure the impact of defensive medicine using total hospital expenditures on a patient in the year after a major heart health incident (acute myocardial infarction, AMI, or ischemic heart disease, IHD) among elderly Medicare beneficiaries. They also measure the extent of damage from defensive medicine using two measures of patient outcomes, mortality within one year of the cardiac illness, and a subsequent cardiac illness (AMI or heart failure) requiring hospitalization in the year following the initial illness. They find that the adoption of liability reforms during the panel led to a decline in hospital expenditures by about 5 percent for AMI and 9 percent for IHD for five years following adoption, but there is little to no effect on mortality or additional hospitalization (Kessler and McClellan 1996).

Three other studies focus on the treatment choices and patient outcomes in obstetrics. Dubay, Kaestner and Waidmann (1999) models the probability of delivery by cesarean section and the probability of a low Apgar[1] score as a function of several factors, including malpractice environment. They measure malpractice pressure using malpractice premiums and find that physicians perform more cesarean sections with an increase in malpractice claims risk, even more so for mothers of lower socioeconomic status. However, they do not find that an increase in malpractice claims risk has any beneficial effect on infant health, measured by Apgar scores. Similarly, Dubay, Kaestner and Waidmann (2001) investigates the effect of malpractice premiums on prenatal care utilization and infant health, using both Apgar scores and low-birth weight. In this analysis, they find that malpractice pressure is associated with both a decline in the number of prenatal care visits, and a later start to prenatal care. Again, there is no evidence that an increase in malpractice pressure affects infant health. Currie and MacLeod (2008) study the same issues in childbirth, but use liability reforms as the measure of malpractice pressure instead of premiums. Using linear probability models, they consider cesarean sections, preventable complications, and low-Apgar scores. They find that joint and several liability reform reduces complications of labor, but caps on noneconomic damages increase them (Currie and MacLeod 2008).

[1]APGAR - activity, pulse, grimace, appearance, respiration. Each attribute has a maximum score of 2 points, for a total maximum score of 10. Most children receive a 9 or higher. Some studies use below a 7 to indicate a low Apgar score, others use below 8.

Lastly, Montanera (2015) attempts to reconcile some of the conflicting results from aforementioned studies. The author contends that the relationships between tort reform and provider behavior are "fundamentally non-monotonic" and models physician incentives where both positive and negative defensive medicine are possible. He finds that rising malpractice pressure leads to a removal of the marginal patient from a physician's caseload, negative defensive medicine. In addition, the resources that would have been used on this marginal patient are distributed among the rest of the caseload. This increases treatment intensity, positive defensive medicine. Furthermore, the effect of malpractice pressure depends on the population studied. Therefore, physician response to malpractice pressure can involve both positive and defensive medicine and depends on the population. If the model proposed in Montanera (2015) accurately illustrates the behavior of family medicine physicians, there should be an increase in access to care following tort reforms and an increase in the prevalence rate of diabetes as physicians screen more often for type II diabetes. We test for this response here.

4.3 Econometric Models

To assess the influence of liability reforms on patient outcomes, this study uses a differences-in-differences approach and controls for state and time fixed-effects. The prevalence of both adult obesity and type II diabetes can be modeled as

$$Diabetes_{st} = \alpha_s + \theta_t + \beta L_{st} + \delta \ln(P_{st}) + \eta X_{st} + \gamma N_{st} + \kappa Obesity_{st} + u_{st} \tag{4.1}$$

and

$$Obesity_{st} = \alpha_s + \theta_t + \beta L_{st} + \delta \ln(P_{st}) + \eta X_{st} + \gamma N_{st} + u_{st} \tag{4.2}$$

where $Diabetes_{st}$ and $Obesity_{st}$ are the prevalence of diabetes and obesity in state s in year t, respectively, α_s and θ_t are state and time fixed effects, P_{st} is the population in state s in year t, and X_{st} is a set of covariates for state s in year t. This set includes income per capita, the civilian unemployment rate, the percentage of the population living in urban areas, and HMO enrollment per capita. This model also controls for access to care by including the number of family medicine physicians in each state, and the number of family medicine residency programs, denoted by N_{st} in the model. Note that the obesity rate is included in the model of the occurrence of diabetes, since obesity is one risk factor for a diagnosis of type II diabetes. Finally, L_{st} denotes the specification

of liability reforms. For each reform, there is a binary equal to one that indicates a reform was active, and zero otherwise.

Although these models control for several factors that may influence the prevalence of obesity and type II diabetes, there may be some elements that are unobserved (e.g., attitudes about health, the relative price of healthy foods to less healthy alternatives, other genetic attributes that alter the risk of these diseases). When these unobservables, or omitted variables, are correlated with the variable of interest, in this case liability reforms, estimates from ordinary least squares regressions are biased. Therefore, this study will assess the impact of reforms on patient outcomes using state fixed-effects. As long as these omitted factors are time-invariant, using fixed effects prevents correlation of omitted variables from entering into the estimates of tort reform. Another complication of this analysis is that, similar to the models in Chapter 2 of physician supply, it is possible that the implementation of reforms and these patient outcomes are determined jointly. Therefore, it is important to test for this type of policy endogeneity. The border state policy instruments used in Chapter 2 are also used here to treat these reforms as endogenous. Please refer to Chapter 2 for details of this approach and assumptions required to maintain validity of the estimates.

Reforms will only impact patient outcomes indirectly through changes to both positive and defensive medicine strategies. Fortunately, this assessment controls for access to care, so it is possible to isolate the impact of reforms. As stated previously, we expect caps on noneconomic and punitive damages, and collateral source reform to reduce the malpractice pressure on physicians, thereby reducing the practice of negative defensive medicine and increasing access to care. With an increase in access to care, prevalence rates of diabetes will increase initially. However, over time as physicians counsel these previously untreated patients, changes will be made to diet, exercise, and lifestyle. If these changes are effective, patients may lose weight which will decrease the prevalence of obesity and the prevalence of type II diabetes. On the other hand, joint and several liability increases malpractice pressure on physicians, increasing the practice of negative defensive medicine. We expect this reform to decrease access to care, and immediately reduce the prevalence of type II diabetes as less patients, especially "high-risk" patients, are removed or excluded from physician case-loads. In the long run, this limit to care will likely increase the prevalence of obesity and type II diabetes. Similarly, caps on noneconomic and punitive damages, and collateral source reform will

Table 4.1: Expected Effects of Reforms on Defensive Medicine and Prevalence Rates

Reform	Positive Defensive Medicine	Negative Defensive Medicine	Short-run Effect on Prevalence Rate	Long-run Effect on Prevalence Rate
Caps on Noneconomic Damages	↓	↓	↑	↓
Caps on Punitive Damages	↓	↓	↑	↓
Collateral Source Reform	↓	↓	↑	↓
Joint and Several Liability Reform	↑	↑	↓	↑

also reduce the practice of positive defensive medicine, while joint and several liability will increase the practice of defensive medicine. The extent to which the effects of a reduction in positive defensive medicine outweigh the reduction in negative defensive medicine depends entirely on the extent of usage of each strategy. Therefore, these effects are ultimately an empirical question. Table 4.1 presents the short-term and long-term expected effects of reforms on both defensive medicine and prevalence rates. To account for the timing of these effects, several different lag operators are tested.

4.4 Data

Data for this analysis came from several sources. First, patient outcome measures, the prevalence of diabetes and the prevalence of obesity, were obtained from the CDC's Behavioral Risk Factor Surveillance System (BRFSS).[2] BRFSS is an ongoing, monthly telephone survey of adults, 18 years or older, in each state. Major changes occurred to BRFSS in 2011, so this analysis will only consider data from the BRFSS before 2010. The prevalence of diabetes is measured as a "yes" response to the question, "Have you ever been told by a doctor that you have diabetes?" Women who indicate that they were diagnosed with gestational diabetes are not included in prevalence calculations. This prevalence rate is provided as the number of those with diabetes per 100 per state. Data for the prevalence of diabetes is available from 1994 to 2007. BRFSS includes two questions which are used to calculate body mass index (BMI) and the prevalence of obesity. Respondents are asked "About how much do you weigh without shoes?" which gives the respondents

[2]BRFSS is a telephone survey so households without a phone may be excluded. This includes groups like those living in the South, racial or ethnic minorities, and those in lower socioeconomic groups. Both diabetes and obesity are more common among some races and ethnicities as well as those with a lower socioeconomic status, so BRFSS likely underestimates prevalence rates for these populations.

Table 4.2: Summary Statistics, State Level with Patient Outcomes

Variable	Mean	Std. Dev.	Minimum	Maximum
Prevalence of Diabetes	6.045	1.588	2.4	11.5
Prevalence of Obesity	21.213	4.075	10.3	32.6
Caps: Noneconomic Damages	0.370	0.482	0	1
Caps: Punitive Damages	0.457	0.498	0	1
Collateral Source Reform	0.618	0.486	0	1
Joint and Several Liability	0.723	0.448	0	1
Total Number of Active Reforms	3.822	1.908	0	8
Population (in millions)	5.545	6.155	0.480	36.30
Income per capita	29,511.11	7,083.142	16,512	64,579
Civilian Unemployment Rate	4.810	1.199	2.3	8.8
Percent Urban Population	72.482	14.992	38.18	100
HMO Enrollment (in millions)	1.346	2.399	0	17.9
Family Medicine Physicians	1,660.95	1661.26	129	10,798
Family Medicine Residency Programs	9.273	8.867	1	42
Family Medicine Residents	190.924	195.581	7	1004

approximate weight and "About how tall are you without shoes?" which gives the approximate height. BMI is the respondents weight in kilograms divided by the respondents height in meters squared $BMI = weight(kg)/height(m)^2$. Respondents are placed into categories according to BMI as follows; Neither overweight nor obese (BMI less than 24.9), Overweight (BMI greater than 25.0 and less than 29.9), Obese (BMI greater than 30.0). The prevalence rate of obesity is given as the percentage of respondents with a calculated BMI greater than 30.0 per state. This is available from 1996 to 2007.

Second, data describing state medical malpractice laws were collected from the Database of State Tort Law Reforms, DSTLR 4^{th}(Avraham 2011). See previous chapters for a discussion of this source. This analysis considers the same four primary reforms, caps on noneconomic and punitive damages, joint and several liability, and collateral source reform, as in previous chapters. Lastly, data controlling for other factors were collected from various resources including the Census Bureau (Statistical Abstract of the United States), the Bureau of Economic Analysis, and the Bureau of Labor Statistics. These data includes population, income per capita, civilian unemployment rate, the percentage of the population living in urban areas, and HMO enrollment per capita. As a control for access to care, the number of family medicine physicians was obtained from reports of

the Physician Masterfile, maintained by the American Medical Association (AMA) and purchased through a database licensing agreement.[3] In addition, the number of residents and residency programs (family medicine and total) were obtained from information in the National Graduate Medical Education Census, also maintained by the AMA.[4] The sample considered here spans from 1994 to 2007 and includes different tort reforms enacted by different states in different years. Most states enacted at least one reform during the panel.[5] Summary statistics for all these variables are presented in Table 4.2.

4.5 Empirical Results

4.5.1 Preliminary tests for policy endogeneity

Two-stage least squares (2SLS) estimation for patient outcomes is implemented in this analysis in the same manner as the implementation for physician supply. Similar to the physician supply study, border state policy instruments perform well under traditional instrument tests. For both obesity and type II diabetes, Durbin-Wu-Hausman tests reject the null hypothesis of exogeneity of reforms at the five percent ($F_{50} = 3.080$ and p-value $= 0.0241$) and one percent level ($F_{50} = 6.415$ and p-value $= 0.0003$), respectively. Therefore, 2SLS is preferred to OLS. Furthermore, border state policies are strong instruments for the implementation of reforms in patient outcome models as well. F-statistics for these first-stage regressions for both outcome variables are presented in Table 4.3. For obesity, instruments are strong even when considering all endogenous variables and instruments together, indicated by the Kleibergen and Paap LM test statistic of 24.359 with a p-value of 0.000.[6] Hansen's J-test cannot reject the null hypothesis that these instruments are valid (Hansen's J chi-squared $= 0.004$ and p-value $= 0.947$). Instrument tests for type II diabetes provide

[3]Medical Marketing Service (MMS Inc) is an authorized AMA Database Licensee (DBL) and supplied requested data extracted from the AMA-PPD database for research and statistical analysis.

[4]Data from the Graduate Medical Education Database, Copyright 2013, American Medical Association, Chicago, Illinois.

[5]Only one state enacted caps on total damages (South Dakota), caps on contingency fees (Nevada), and patient compensation fund reform (West Virginia) during the panel. Other states either had the reform in place before 1994, or did not enact the reform until after 2007. For these variables, this means that in some fixed effects regressions, the effects of one state are being identified.

[6]Critical values for more than 3 endogenous variables are not recorded in the ivreg2 routine, so Stock and Yogo simulated critical values are unavailable for this case with four endogenous variables. However, Cragg-Donald Wald F-test statistics are very large, 141.350. Odds are that this would exceed any critical value anyway.

Table 4.3: First-stage Regression Summary Statistics: Border State Instruments

Obesity

Variable	Robust F_{50}	$Prob > F$
Caps on Noneconomic Damages	119.753	0.0000
Caps on Punitive Damages	105.88	0.0000
Collateral Source Reform	52.452	0.0000
Joint and Several Liability Reform	41.944	0.0000

Type II Diabetes

Variable	Robust F_{50}	$Prob > F$
Caps on Noneconomic Damages	119.081	0.0000
Caps on Punitive Damages	104.115	0.0000
Collateral Source Reform	50.464	0.0000
Joint and Several Liability Reform	49.522	0.0000

similar results.[7] Table 4.4 presents results from first-stage regressions when the dependent variable of interest is the prevalence of obesity. Table 4.5 presents results from first-stage regressions when the dependent variable of interest is the prevalence of type II diabetes. Tables 4.6 and 4.7 present results for second-stage regressions of the prevalence of obesity and type II diabetes, respectively. Results from traditional Ordinary Least Squares (OLS) estimation including state fixed effects are presented in the first column. Instrumental variables (IV) specifications which includes Two-stage Least Squares (2SLS), Generalized Method of Moments (GMM), and Fixed Effects (FE), are given in the other three columns.

In fixed effects specifications, collateral source reform has a statistically significant effect on the prevalence of obesity, but generally reforms have insignificant effects on the obesity rate. The impact of liability reforms on the prevalence of type II diabetes is stronger. Caps on noneconomic damages have a statistically significant effect on the rate of type II diabetes at the one percent level in fixed effects specifications, but not in 2SLS or GMM regressions. States that enact this cap have a higher rate of type II diabetes between 50.8 and 52.7 percent than states that do not enact this cap holding other factors in the model constant. Caps on punitive damages have a significant effect on the prevalence of type II diabetes, regardless of the specification. However, the size of the

[7]The Kleibergen and Paap LM test reports a test statistic of 23.348 with a p-value of 0.000. Cragg-Donald Wald F-test statistics are very large, 152.837, and Hansen's J-test cannot reject the null that instruments are valid (Hansen's J chi-squared = 0.367 and p-value = 0.545)

Table 4.4: First-stage Regression Results Models of Obesity, Border State Policies as Instruments

Variable	NEDC	PDC	CSR	JSL
Border State: NEDC	0.820	0.022	-0.050	-0.016
	(0.035)***	(0.042)	(0.060)	(0.053)
Border State: PDC	0.029	0.844	0.016	0.005
	(0.041)	(0.039)***	(0.077)	(0.060)
Border State: CSR	-0.034	-0.018	0.806	-0.060
	(0.044)	(0.041)	(0.060)***	(0.060)
Border State: JSL	-0.024	-0.047	0.006	0.795
	(0.043)	(0.042)	(0.074)	(0.059)***
Border State: PE	-0.077	-0.060	-0.079	-0.094
	(0.043)*	(0.039)	(0.081)	(0.051)*
ln(*Population*)	-0.128	0.117	-0.106	-0.039
	(0.036)***	(0.036)***	(0.070)	(0.049)
Income per capita	-0.00002	-0.00001	-0.000	-0.00003
	(0.000005)***	(0.000006)**	(0.000)	(0.000007)***
Civilian Unemployment rate	-0.053	-0.002	-0.075	-0.072
	(0.019)***	(0.016)	(0.042)*	(0.024)***
Percentage Urban	0.005	0.0005	0.005	0.004
	(0.0017)***	(0.002)	(0.003)	(0.003)
Family Medicine Physicians	0.000	0.0001	-0.000	0.000
	(0.000)	(0.00003)*	(0.000)	(0.000)
FM Residency Programs	-0.001	-0.011	-0.001	-0.003
	(0.006)	(0.006)*	(0.011)	(0.009)
HMO Enrollment per capita	-0.015	-0.508	0.381	-0.219
	(0.193)	(0.163)***	(0.374)	(0.240)
State-fixed effects	No	No	No	No
Year-fixed effects	Yes	Yes	Yes	Yes
F-statistic	67.86***	57.37***	23.46***	18.32***
Number of Observations	611	611	611	611

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. Each column represents a separate first-stage regression. The dependent variable (endogenous regressor) is given at the top of each column. The following abbreviations are used to simplify presentation: NEDC (noneconomic damage caps), PDC (punitive damage caps),CSR (collateral source reform), (JSL) joint and several liability, and PER (punitive evidence reform).

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

Table 4.5: First-stage Regression Results Models of Type II Diabetes, Border State Policies as Instruments

Variable	NEDC	PDC	CSR	JSL
Border State: NEDC	0.821	0.020	-0.044	-0.010
	(0.036)***	(0.041)	(0.059)	(0.047)
Border State: PDC	0.028	0.845	0.013	0.001
	(0.041)	(0.038)***	(0.078)	(0.056)
Border State: CSR	-0.034	-0.018	0.807	-0.059
	(0.044)	(0.041)	(0.058)***	(0.056)
Border State: JSL	-0.026	-0.046	0.002	0.791
	(0.043)	(0.040)	(0.074)	(0.056)***
Border State: PE	-0.077	-0.060	-0.078	-0.092
	(0.043)*	(0.038)	(0.079)	(0.045)**
Prevalence of Obesity	-0.012	0.015	-0.034	-0.035
	(0.010)	(0.009)*	(0.015)**	(0.009)***
ln(*Population*)	-0.115	0.997	-0.067	0.001
	(0.035)***	(0.039)**	(0.072)	(0.047)
Income per capita	-0.00002	-0.00001	-0.00001	-0.00003
	(0.000005)***	(0.000005)**	(0.00001)	(0.00001)***
Civilian Unemployment rate	-0.046	-0.011	-0.054	-0.050
	(0.019)**	(0.016)	(0.037)	(0.024)**
Percentage Urban	0.004	0.001	0.002	-0.035
	(0.002)**	(0.001)	(0.003)	(0.003)
Family Medicine Physicians	0.00004	0.0001	0.0001	0.00004
	(0.00003)	(0.00003)**	(0.0001)	(0.00004)
FM Residency Programs	-0.0001	-0.012	0.001	-0.001
	(0.006)	(0.006)**	(0.011)	(0.009)
HMO Enrollment per capita	-0.095	-0.407	0.152	-0.458
	(0.208)	(0.171)**	(0.391)	(0.231)**
State-fixed effects	No	No	No	No
Year-fixed effects	Yes	Yes	Yes	Yes
F-statistic	54.28***	58.23***	23.21***	23.94***
Number of Observations	611	611	611	611

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. Each column represents a separate first-stage regression. The dependent variable (endogenous regressor) is given at the top of each column. The following abbreviations are used to simplify presentation: NEDC (noneconomic damage caps), PDC (punitive damage caps),CSR (collateral source reform), (JSL) joint and several liability, and PER (punitive evidence reform).
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

effect varies. In 2SLS and GMM regressions, the type II diabetes rate is about 5 percent lower in states that enact these caps than states that do not. Yet in fixed effects specifications, the effect can be as large as 53 percent and as low as 35.6 percent.

In some cases, the effect of reforms on these two prevalence rates is unreasonably large. It is rare that indirect changes to practice conditions would have such a large effect on patient outcomes. This may indicate a problem in the analysis. For example, there may be a third omitted factor that is picked up by the implementation of reforms. Some possible omitted variables could be the structure of reimbursement, the structure of health care provision, or even the price of physician services. It could also include population characteristics like race, age, and so on, especially if physicians are choosing to decline to provide care to certain types of patients. Though the estimation includes state fixed-effects to control for correlation of omitted variables and tort reforms, this will only be effective to the extent that omitted factors are relatively time-invariant. Thus, it is expected that the exclusion of at least one of these omitted factors overstates the impact of reforms on rates of type II diabetes.

Furthermore, this estimation technique lumps many differing health care and insurance market characteristics into a simple aggregate analysis that does not allow for changing dynamics in either of these markets. Reimbursement rate structures, coverage of certain prescriptions, and other factors that impact the patient health outcomes considered here must be accounted for to fully understand the dynamic and indirect nature between liability reforms and obesity and type II diabetes. For example, suppose that liability reforms make covering patients with a certain type of insurance less costly to the insurance company. In response to this reform, insurance companies may increase the reimbursement rate to physicians or begin covering certain aspects of care that were not previously covered. These changes to coverage or reimbursement may by themselves increase access to care, but it is also possible that they will induce physicians to see more of these patients. Without accounting for these various aspects of the process, it is difficult to make strong conclusions about the effects of reforms on these patient outcomes.

Some state-specific controls also have large effects. A one percent increase in the prevalence of obesity increases the rate of type II diabetes by between 12.7 and 28.7 percent holding other variables in the model constant. This result is statistically significant at the one percent level in all specifications. Population, income per capita, the civilian unemployment rate and the percentage

Table 4.6: Regression Results Prevalence of Obesity, Border State Policies as Instruments

Variable	OLS, FE	IV, 2SLS	IV, GMM	IV, FE
Intercept	-24.064	14.157	14.115	-28.956
	(41.460)	(6.278)**	(6.251)**	(29.215)
Caps on Noneconomic Damages	0.528	0.210	0.211	0.236
	(0.361)	(0.452)	(0.452)	(0.498)
Caps on Punitive Damages	-0.004	-0.133	-0.135	-0.322
	(0.324)	(0.372)	(0.371)	(0.388)
Collateral Source Reform	0.873	0.027	0.031	0.945
	(0.333)**	(0.521)	(0.518)	(0.440)**
Joint and Several Liability Reform	-0.278	-0.158	-0.147	-0.175
	(0.334)	(0.485)	(0.455)	(0.430)
ln(Population)	2.139	1.182	1.188	2.595
	(2.907)	(0.438)***	(0.432)***	(2.925)
Income per capita	-0.0001	-0.000	-0.000	-0.0001
	(0.00005)**	(0.000)	(0.000)	(0.00005)**
Civilian Unemployment Rate	0.085	0.624	0.621	0.075
	(0.081)	(0.188)***	(0.183)***	(0.082)
Percent Urban Population	0.288	-0.063	-0.063	0.270
	(0.156)*	(0.21)***	(0.021)***	(0.154)*
Family Medicine Physicians	-0.0002	-0.0004	-0.0004	-0.0002
	(0.0002)	(0.0002)***	(0.0002)*	(0.0002)
Family Medicine Residency Programs	0.044	0.062	-0.062	0.052
	(0.102)	(0.044)	(0.043)	(0.103)
HMO enrollment per capita	0.847	-6.871	-6.888	0.900
	(1.432)	(1.641)***	(1.621)***	(1.391)
State-fixed effects	Yes	No	No	Yes
Year-fixed effects	Yes	Yes	Yes	Yes
F-statistic	205.58***			
Wald Statistic		3147.02***	3200.94***	7239.05***
Number of Observations	611	611	611	611

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. First-stage instruments in all instrumental variables specifications include border state policy instruments for caps on noneconomic and punitive damages, collateral source reform, joint and several liability reform, and mandatory periodic payments in addition to the other exogenous variables in the model. The dependent variable in all specifications is the prevalence of obesity.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

Table 4.7: Regression Results Prevalence of Type II Diabetes, Border State Policies as Instruments

Variable	OLS, Fixed Effects	IV, 2SLS	IV, GMM	IV, Fixed Effects
Intercept	-14.676	-4.505	-4.664	-10.967
	(23.978)	(1.747)***	(1.721)***	(15.374)
Caps on Noneconomic Damages	0.508	0.058	0.074	0.527
	(0.171)***	(0.179)	(0.180)	(0.211)**
Caps on Punitive Damages	-0.356	-0.330	-0.322	-0.530
	(0.173)***	(0.162)**	(0.165)**	(0.201)***
Collateral Source Reform	0.385	-0.052	-0.048	0.303
	(0.149)***	(0.233)	(0.244)	(0.168)*
Joint and Several Liability Reform	-0.229	-0.202	-0.177	-0.387
	(0.209)	(0.178)	(0.170)	(0.273)
ln(Population)	1.080	0.307	0.277	1.050
	(1.739)	(0.307)*	(0.146)**	(1.719)
Income per capita	-0.0001	-0.000	-0.000	-0.00008
	(0.00003)**	(0.000)	(0.000)	(0.00003)***
Civilian Unemployment Rate	-0.115	0.111	0.133	-0.124
	(0.065)*	(0.088)	(0.085)	(0.065)**
Percent Urban Population	0.050	0.012	0.013	0.053
	(0.061)	(0.006)**	(0.006)**	(0.060)
Prevalence of Obesity	0.127	0.278	0.287	0.129
	(0.030)***	(0.036)***	(0.031)***	(0.029)***
Family Medicine Physicians	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Family Medicine Residency Programs	-0.024	-0.004	-0.005	-0.025
	(0.051)	(0.019)	(0.020)	(0.048)
HMO enrollment per capita	0.007	0.733	0.712	0.007
	(0.666)	(0.778)	(0.807)	(0.649)
State-fixed effects	Yes	No	No	Yes
Year-fixed effects	Yes	Yes	Yes	Yes
F-statistic	95.44***			
Wald Statistic		1360.23***	1379.45***	1827.79***
Number of Observations	611	611	611	611

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. First-stage instruments in all instrumental variables specifications include border state policy instruments for caps on noneconomic and punitive damages, collateral source reform, joint and several liability reform, and mandatory periodic payments in addition to the other exogenous variables in the model. The dependent variable in all specifications is the prevalence of type II diabetes.
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

of the population living in an urban area have significant effects on both obesity and type II diabetes, depending on the specification. Access to care, measured by the number of family medicine physicians, and managed care, measured by HMO enrollment per capita, have a statistically significant effect on the prevalence of obesity, but only in 2SLS and GMM specifications. According to the results, an additional HMO enrollee decreases the obesity rate by 688 percent, holding other variables constant. Though this effect is quite large, many states have worked with managed care organizations to improve body-mass index screening and education on healthy weight, physical activity, and lifestyle choices. This supports the hypothesis that factors in both insurance markets, health care markets impact patient outcomes. Ignoring the dynamic nature of these intertwined markets makes it difficult to form conclusions about the marginal effects of liability reforms on the prevalence of obesity and type II diabetes.

4.5.2 Testing short-term and long-term effects

The results so far do not account for different long-term and short-term effects that may exist as changes in defensive medicine occur in response to tort reform. This section presents results that test for changes in patient outcomes as the length of time from the implementation of a reform increases. Immediate responses, or short-term effects, may be immediate and seen in the concurrent year as the patient outcome, t. Short-run changes may also happen more slowly and not yet be visible till year one year after the reform is implemented, $t-1$. Instability in short-run effects may take some time to level off before long-run effects become dominant. Though there is no formal definition of how many years exactly after implementation can be considered the "long-run," initial analysis considers the long-run to be three or four years.[8]

Table 4.8 and Table 4.9 present short-term and long-term effects of reforms on the prevalence of obesity and type II diabetes. The dependent variable is given in the head of each column. These results are largely mixed and for the most part, do not match the expected results presented in Table 4.1. For both patient outcomes, only a few reforms have significant effects on patient outcomes once policy endogeneity is adequately addressed. Collateral source reform and joint and several liability reform have significant effects on the rate of obesity, but not consistently. Joint and several liability has an immediate positive effect on the prevalence of obesity, but this effect is reversed the year following the implementation of the reform. There are lingering positive effects

[8]Five year lags are also tested but results do not differ from those presented in this section.

Table 4.8: Short- and Long-term Effects on the Prevalence of Obesity and Type II Diabetes

Variable	Obesity$_t$ IV, 2SLS	Obesity$_t$ IV, Fixed Effects	Diabetes$_t$ IV, 2SLS	Diabetes$_t$ IV, Fixed Effects
Caps on Noneconomic Damages$_t$	-0.548	-0.450	0.806	0.416
	(0.576)	(0.309)	(0.317)**	(0.270)
Caps on Punitive Damages$_t$	-0.707	-0.191	-0.432	-0.558
	(0.674)	(0.460)	(0.343)	(0.282)**
Collateral Source Reform$_t$	0.237	-0.272	0.612	0.526
	(0.608)	(0.445)	(0.393)	(0.341)
Joint and Several Liability Reform$_t$	1.130	1.119	-1.064	-0.633
	(0.573)***	(0.366)***	(0.359)**	(0.359)*
Caps on Noneconomic Damages$_{t-1}$	0.719	0.739	-0.263	-0.129
	(0.468)	(0.461)	(0.252)	(0.224)
Caps on Punitive Damages$_{t-1}$	-0.259	0.016	-0.080	-0.043
	(0.757)	(0.592)	(0.280)	(0.277)
Collateral Source Reform$_{t-1}$	0.266	0.347	0.060	0.210
	(0.369)	(0.421)	(0.659)	(0.484)
Joint and Several Liability Reform$_{t-1}$	-1.281	-1.516	0.375	0.191
	(0.446)***	(0.397)***	(0.265)	(0.250)
Caps on Noneconomic Damages$_{t-2}$	-0.034	0.303	-0.217	0.040
	(0.591)	(0.479)	(0.304)	(0.230)
Caps on Punitive Damages$_{t-2}$	0.064	-0.051	0.463	0.260
	(0.373)	(0.312)	(0.338)	(0.305)
Collateral Source Reform$_{t-2}$	1.083	1.202	-0.740	-0.574
	(0.408)***	(0.325)***	(0.345)**	(0.276)**
Joint and Several Liability Reform$_{t-2}$	-0.608	-0.763	0.188	0.133
	(0.572)	(0.502)	(0.349)	(0.298)

Results continued on next page.

Table 4.9: Short- and Long-term Effects on the Prevalence of Obesity and Type II Diabetes

Variable	Obesity$_t$ IV, 2SLS	Obesity$_t$ IV, Fixed Effects	Diabetes$_t$ IV, 2SLS	Diabetes$_t$ IV, Fixed Effects
Caps on Noneconomic Damages$_{t-3}$	0.448	0.566	0.069	0.113
	(0.510)	(0.433)	(0.371)	(0.347)
Caps on Punitive Damages$_{t-3}$	0.243	-0.022	-0.289	-0.316
	(0.419)	(0.352)	(0.324)	(0.303)
Collateral Source Reform$_{t-3}$	-1.345	-1.332	0.255	0.201
	(0.375)***	(0.426)***	(0.301)	(0.217)
Joint and Several Liability Reform$_{t-3}$	1.299	1.377	-0.888	-0.623
	(0.831)	(0.697)**	(0.409)**	(0.357)*
Caps on Noneconomic Damages$_{t-4}$	-0.246	-0.441	-0.507	0.131
	(0.693)	(0.446)	(0.330)	(0.276)
Caps on Punitive Damages$_{t-4}$	0.555	-0.065	-0.075	-0.191
	(0.446)	(0.346)	(0.225)	(0.166)
Collateral Source Reform$_{t-4}$	-0.451	-0.182	-0.302	-0.002
	(0.661)	(0.473)	(0.344)	(0.222)
Joint and Several Liability Reform$_{t-4}$	-0.603	-0.628	1.190	0.389
	(0.699)	(0.549)	(0.445)***	(0.347)
State-fixed effects	Yes	Yes	Yes	Yes
F-statistic	106.85***		74.89***	
Wald-statistic		51262.21***		3375.91***
Number of Observations	509	509	509	509

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. State level controls are also included in these models, but results are not presented. This includes population, income per capita, civilian unemployment rate, percent urban population, number of family medicine physicians and residency programs, and HMO enrollment per capita. The prevalence of obesity is also included in models where type II diabetes is the dependent variable. All regressions include year fixed effects.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

three years after the implementation of joint and several liability reform. This lingering positive effect is quite large. Three years after joint and several liability reform is enacted, the prevalence of obesity is 137 percent higher in states with this reform than those without the reform. The same is true for collateral source reform, but at a different pace and without a lingering effect. There is an initial increase in the prevalence of obesity two years following the implementation of collateral source reform, but this effect is reversed three years following enactment.

Caps on noneconomic damages and punitive damages have immediate effects on the prevalence of type II diabetes, depending on whether or not state fixed-effects are included. Though we expect both of these reforms to increase the prevalence rate of type II diabetes in the short-run, only noneconomic damage caps follow this expected result, while caps on punitive damages have the opposite effect. These prompt effects may be overstated if there is omitted variables bias as discussed previously. In addition, these reforms do not have long-term effects on the prevalence of type II diabetes. On the other hand, the results for joint and several liability do follow the expected pattern given in Table 4.1. Initially, states that implement joint and several liability have between 63.3 and 106.4 percent lower rates of type II diabetes than states that do not implement this reform, holding other variables constant. This negative effect prevails three years following the implementation of joint and several liability. However, four years following the enactment, the prevalence of type II diabetes is between 38.9 and 119 percent higher in states that implemented the reform four years ago compared to those that did not. These results also indicate that accounting for state fixed-effects is important. When states fixed-effects are not included, some reform effects are unreasonably large.

In summary, reforms do not significantly alter the prevalence of obesity, except for a positive effect from joint and several liability reform that lingers about three years after implementation. This reform has a stronger effect on the prevalence of type II diabetes. Ultimately, there is no consistent pattern that emerges for short-term and long-term effects of reforms on these patient outcomes. This may indicate that reforms are not strong enough to increase access to care sufficiently to alter patient health. It is also possible that more access to care cannot improve the prevalence of obesity or type II diabetes. This would be the case if patients refuse to implement treatment as suggested by physicians, or if the treatment prescribed does not adequately improve

patient health. Lastly, it likely indicates a more precise model of intertwined insurance and health care markets is needed before

4.5.3 Temporary and permanent reforms

Another way to account for these short-term and long-term effects is to redefine reforms based on whether they are permanent or not. Grace and Leverty (2013) contend that reforms should be segmented into temporary and permanent reforms. Temporary reforms are those that are eventually declared unconstitutional or repealed, and permanent reforms are those that are unchallenged or upheld by the courts.[9] Two new variables are created for these tests. $Perm_{st}$ is equal to one if a reform is unchallenged or upheld by the courts during the length of the panel and zero otherwise. In addition, $Temp_{st}$ is equal to one for reforms that are enacted and then declared unconstitutional by courts or reversed by legislative action and zero otherwise. These variables are created the same way as those in Chapter 2. Results for this specification are given in Table 4.10. Temporary reforms never have a statistically significant effect on the prevalence of obesity or type II diabetes. Only permanent caps on punitive damages have a significant effect on these patient outcomes. States that enact permanent caps on punitive damages have a 64.6 percent lower incidence of obesity and between a 29.1 and 48.8 percent lower incidence of type II diabetes. Though Grace and Leverty (2013) find more significant effects once they account for the permanence of reforms in insurance underwriting, there are less significant effects in this analysis. This suggests that the permanence of reforms does not matter as much in physician decision-making as it does for insurer decision-making.

4.6 Conclusions

This study finds some significant short-term and long-term effects of reforms on the rate of preventable diseases, specifically obesity and type II diabetes. However, no dominant pattern emerges in the analysis. In initial regressions, reforms have a stronger effect on the prevalence of type II diabetes than obesity. Once short- and long-term effects are considered, results do not differ sub-

[9] Grace and Leverty (2013) test the hypothesis that tort reforms may not impact malpractice premiums immediately because of insurers' reluctance to reduce premiums until several years of claims data are available. A delay in premium changes may be compounded by uncertainty about the stability of reforms. They find that the effects of temporary reforms are never statistically significant and conclude that studies that combine temporary and permanent reforms in the literature incorrectly estimate the effect of tort reform.

Table 4.10: Prevalence of Obesity and Type II Diabetes, Temporary and Permanent Reforms

Variable	Obesity IV, 2SLS	Obesity IV, FE	Diabetes IV, 2SLS	Diabetes IV, FE
Temporary: Caps on Noneconomic Damages	0.600	0.109	0.116	-0.062
	(0.643)	(1.005)	(0.425)	(0.302)
Temporary: Caps on Punitive Damages	-1.086	0.006	-0.278	-0.051
	(1.403)	(1.182)	(0.261)	(0.334)
Temporary: Collateral Source Reform	0.146	0.264	0.010	0.129
	(0.775)	(0.608)	(0.363)	(0.315)
Temporary: Joint and Several Liability Reform	-2.957	1.908	-0.349	-0.444
	(1.875)	(1.200)	(0.500)	(0.379)
Permanent: Caps on Noneconomic Damages	0.162	-0.090	0.030	-0.204
	(0.469)	(0.494)	(0.178)	(0.211)
Permanent: Caps on Punitive Damages	-0.202	-0.646	-0.291	-0.488
	(0.409)	(0.392)*	(0.155)**	(0.201)**
Permanent: Collateral Source Reform	0.002	0.747	-0.120	-0.155
	(0.604)	(0.463)	(0.245)	(0.155)
Permanent: Joint and Several Liability Reform	-0.543	0.809	-0.239	-0.069
	(0.574)	(0.566)	(0.205)	(0.149)
ln(Population)	0.906	1.467	0.267	0.847
	(0.446)**	(0.540)***	(0.157)*	(0.182)***
Income per capita	-0.000	-0.0002	-0.000	-0.0001
	(0.000)	(0.00005)***	(0.000)	(0.00003)**
Civilian Unemployment Rate	0.588	0.114	0.111	-0.074
	(0.193)***	(0.088)	(0.086)	(0.059)
Percent Urban Population	-0.058	-0.129	0.013	-0.014
	(0.023)***	(0.033)***	(0.006)**	(0.007)**
Prevalence of Obesity			0.269	0.194
			(0.038)***	(0.030)***
Family Medicine Physicians	-0.001	-0.000	-0.000	-0.000
	(0.0004)*	(0.000)	(0.000)	(0.000)
Family Medicine Residency Programs	0.194	-0.017	0.007	-0.013
	(0.107)*	(0.107)	(0.034)	(0.040)
HMO enrollment per capita	-7.055	-0.076	0.766	-0.907
	(1.836)***	(1.353)	(0.767)	(0.717)
State-fixed effects	No	Yes	No	Yes
Year-fixed effects	Yes	Yes	Yes	Yes
F-statistic	118.07***		54.86***	
Wald statistic		15967.16***		1518.59***
Number of Observations	611	611	611	611

Note. Reported standard errors are given in parenthesis and are clustered at the state level in all specifications. State level controls are also included in these models, but results are not presented. This includes population, income per capita, civilian unemployment rate, percent urban population, number of family medicine physicians and residency programs, and HMO enrollment per capita. The prevalence of obesity is also included in models where type II diabetes is the dependent variable.

* Statistically significant at the 10 percent level

** Statistically significant at the 5 percent level

*** Statistically significant at the 1 percent level

stantially. Tests that account for the permanence of reforms demonstrate that only permanent caps on punitive damages have a significant effect on either prevalence rate. Unfortunately, this analysis likely suffers from two substantial problems. First, it is likely that some factors are not adequately addressed, and omitted variables bias may be present. Second, the difference-in-differences estimation technique does not completely capture complex issues in liability markets, health care markets, and insurance markets. Both of these weaknesses may provide unreliable results. More extensive analysis into these markets must be considered before valid policy recommendations can be made.

APPENDIX A

INSTITUTIONAL REVIEW BOARD APPROVAL AND RENEWAL

Florida State
UNIVERSITY

Office of the Vice President for Research
Human Subjects Committee
Tallahassee, Florida 32306-2742
(850) 644-8673 · FAX (850) 644-4392

APPROVAL MEMORANDUM

Date: 07/11/2015

To: Alice Crisp [redacted]

Address: 288 Bellamy Building Tallahassee, FL 32306

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Use of Human Subjects in Research
Medical Malpractice and Family Medicine: Physician Supply, Choices, and Patient Outcomes

The application that you submitted to this office in regard to the use of human subjects in the proposal referenced above have been reviewed by the Secretary, the Chair, and two members of the Human Subjects Committee. Your project is determined to be Expedited per 45 CFR § 46.110(7) and has been approved by an expedited review process.

The Human Subjects Committee has not evaluated your proposal for scientific merit, except to weigh the risk to the human participants and the aspects of the proposal related to potential risk and benefit. This approval does not replace any departmental or other approvals, which may be required.

If you submitted a proposed consent form with your application, the approved stamped consent form is attached to this approval notice. Only the stamped version of the consent form may be used in recruiting research subjects.

If the project has not been completed by 09/09/2015 you must request a renewal of approval for continuation of the project. As a courtesy, a renewal notice will be sent to you prior to your expiration date; however, it is your responsibility as the Principal Investigator to timely request renewal of your approval from the Committee.

You are advised that any change in protocol for this project must be reviewed and approved by the Committee prior to implementation of the proposed change in the protocol. A protocol change/amendment form is required to be submitted for approval by the Committee. In addition, federal regulations require that the Principal Investigator promptly report, in writing any unanticipated problems or adverse events involving risks to research subjects or others.

By copy of this memorandum, the chairman of your department and/or your major professor is reminded that he/she is responsible for being informed concerning research projects involving human subjects in the department, and should review protocols as often as needed to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

This institution has an Assurance on file with the Office for Human Research Protection. The Assurance Number is IRB00000446.

Cc: Bruce Benson <bbenson@fsu.edu>, Advisor
HSC No. 2014.13628

Florida State
UNIVERSITY

Office of the Vice President For Research
Human Subjects Committee
P. O. Box 3062742
Tallahassee, Florida 32306-2742
(850) 644-8673 · FAX (850) 644-4392

RE-APPROVAL MEMORANDUM

Date: 07/21/2015

To: Alice Crisp [redacted]

Address: 288 Bellamy Building Tallahassee, FL 32306

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Re-approval of Use of Human subjects in Research:
Medical Malpractice and Family Medicine: Physician Supply, Choices, and Patient Outcomes

Your request to continue the research project listed above involving human subjects has been approved by the Human Subjects Committee. If your project has not been completed by 07/19/2016 , you are must request renewed approval by the Committee.

If you submitted a proposed consent form with your renewal request, the approved stamped consent form is attached to this re-approval notice. Only the stamped version of the consent form may be used in recruiting of research subjects. You are reminded that any change in protocol for this project must be reviewed and approved by the Committee prior to implementation of the proposed change in the protocol. A protocol change/amendment form is required to be submitted for approval by the Committee. In addition, federal regulations require that the Principal Investigator promptly report in writing, any unanticipated problems or adverse events involving risks to research subjects or others.

By copy of this memorandum, the Chairman of your department and/or your major professor are reminded of their responsibility for being informed concerning research projects involving human subjects in their department. They are advised to review the protocols as often as necessary to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

Cc:
HSC No. 2015.15991

APPENDIX B

DATA LICENSING AGREEMENTS

One Time Use Agreement For the AMA Physician Professional Data (PPD) Statistical Research or Count File.

It is expressly agreed that the terms and conditions set forth in this Agreement are applicable to all American Medical Association (AMA) information and data in any form and any and all information or data derived therefrom (all of which is hereinafter called "AMA-PPD").

Medical Marketing Service (MMS Inc.), an authorized AMA Database Licensee (DBL), is supplying the requested statistical data extracted from the AMA-PPD database for the purposes of research and statistical analysis which are stated within MMS Inc. order Acknowledgement, by the undersigned hereinafter called "One-Time User". No mailing uses are allowed under this agreement.

AMA-PPD provided by MMS Inc hereunder may only be used for the purposes set forth herein and for no other purpose. Any AMA-PPD, and any other information or data derived therefrom, will not pass into the possession of any party for any use whatsoever and will not be copied or incorporated into any database of the One-Time User or any other person. Only aggregate summary data shall be released (without any physician identifiers) in report form as a result of analysis of AMA-PPD data. One-Time User may publish aggregated results of research. In the event One-Time User is intending to publish the research data derived from AMA-PPD, One-Time User must place within each publication an acknowledgement that AMA is the source for the raw physician data; statistics, tables or tabulations were prepared by One-Time User (name of One-Time User) using AMA Masterfile data.

One-Time User acknowledges and agrees that the AMA-PPD represents confidential data and material and that the AMA reserves all rights in such property including, but not limited to, common law and statutory rights of literary property and copyright. Such ownership rights are not assigned or released as a result of this Agreement. By way of illustration and not by way of limitation, One-Time User shall not:

(1) Publish the AMA-PPD as its own;
(2) Publish or copy any directory of physicians based upon information derived from the AMA-PPD;
(3) Incorporate the AMA-PPD or any portion thereof or the data contained therein or derived therefrom in any separate file, data bank or listing compiled by One-Time User or otherwise appropriate the AMA-PPD data for its own use; and
(4) Make the AMA-PPD available to any other person, the public, any organization, entity (whether or not affiliated with the One-Time User) or government agency for any purpose whatsoever
(5) use AMA-PPD or any data contained therein or derived therefrom as a vehicle to create a new database or enhance any other database.

It is mutually agreed between the parties hereto that the AMA-PPD: (a) is being provided under a non-exclusive limited license; and (b) that upon a breach of any of the foregoing covenants, such license to use and possess the AMA-PPD shall be automatically and immediately terminated and any AMA-PPD provided under the terms of this Agreement or derived therefrom shall be returned to MMS Inc. immediately, but, in no event, later than seventy-two (72) hours after such termination.

One-Time User shall defend, indemnify and hold harmless MMS Inc., the AMA, and their respective officers, directors, agents, assigns and successors in interest (collectively the "DBL Indemnities") from and against any claims, demands, suits, causes of action, legal or administrative proceedings ("Claims") and pay all damages, costs and expenses, including, without limitation, reasonable attorneys' fees, paid, incurred or suffered by any of DBL Indemnities arising out of or resulting from (i) One-Time User's breach of this Agreement or any act, error or omission by One-Time User or any third party in the performance or non-performance of any of its obligations under this Agreement, or (ii) One-Time User's or any third party's use or disclosure of all or any part of the AMA-PPD, or other activities pursuant to this Agreement.

This product includes AMA-PPD which is commercial technical data and/or computer data bases and/or commercial computer software and/or commercial computer software documentation, as applicable which were developed exclusively at private expense by the American Medical Association, 515 North State Street, Chicago, Illinois, 60610. U.S. Government rights to use, modify, reproduce, release, perform, display, or disclose these technical data and/or computer data bases and/or computer software and/or computer software documentation are subject to the limited rights restrictions of DFARS 252.227-7015(b)(2) (June 1995) and/or subject to the restrictions of DFARS 227.7202-1(a) (June 1995) and DFARS 227.7202-3(a) (June 1995), as applicable for U.S. Department of Defense procurements and the limited rights restrictions of FAR 52.227-14 (June 1987) and/or subject to the restricted rights provisions of FAR 52.227-14 (June 1987) and FAR 52.227-19 (June 1987), as applicable, and any applicable agency FAR

Supplements, for non-Department of Defense Federal procurements.

One-Time User agrees that the receipt of an order acknowledged from MMS Inc. constitutes a qualified acceptance only and may, at AMA's discretion, be subject to AMA's approval.

One-Time User agrees that AMA-PPD will not be used in connection with any unlawful purpose.

It is expressly agreed that there are no promises or understandings other than those contained in this Agreement with respect to the AMA-PPD or any other information or materials provided or used under, or as a result of, this Agreement, and that except as otherwise expressly provided for herein no modification of its terms shall be valid unless in writing signed by both parties.

This Agreement is governed by the laws of the State of Illinois without regard to choice of law principles. The AMA is a third party beneficiary to this Agreement. One-Time User releases MMS Inc. and the AMA and their respective agents and employees, from any and all liability whatsoever for inaccurate or incomplete information contained in AMA-PPD. In addition to the foregoing, MMS Inc. and AMA specifically disclaim any responsibility, and MMS Inc. and the AMA shall incur no liability whatsoever, for any errors or omissions contained in any third party data provided to the AMA, incorporated into the AMA-PD, and provided to One-Time User hereunder. Any damages to One-Time User from MMS Inc. arising from the provision of data from the AMA pursuant to this Agreement shall be limited to the amount paid by One-Time User to obtain the data hereunder.

One-Time User shall submit in writing to MMS Inc. any request involving the AMA or AMA-PPD for any current or potential claim, lawsuit, administrative hearing, or preceding pending, or to the best of One-Time User's knowledge, threatened against, by or affecting One-Time User in any court, or by or before any federal, state, municipal or other governmental department, commission, board, bureau, agency or instrumentality.

The term of this Agreement shall begin upon its execution and shall be effective for a one year period. Upon expiration One-Time user will return or destroy AMA-PPD, as well as any information copied or derived therefrom to MMS Inc within three (3) business days. Should the AMA-PPD be destroyed as provided hereunder One-Time User shall immediately submit a letter to MMS Inc. executed by a duly authorized person indicating that the data has been properly destroyed and that no unauthorized uses of the data or materials have been made.

This Agreement shall be binding upon the undersigned, its principals, employees, and servants.

One Time User

By: [redacted]

Printed Name: Alice Crisp

Title: PhD Candidate / Research Assistant

Date: 3/12/2014

Job Number: QUO-130585-43R339
Florida State University

AMA
AMERICAN MEDICAL ASSOCIATION

ama-assn.org
t (312) 464-5000

February 27, 2014

Alice Crisp
Department of Economics
Florida State University
288 Bellamy Building
Tallahassee, FL 32306

Dear Ms. Crisp,

Thank you for contacting the American Medical Association (AMA) Medical Education Department of Data Acquisition Services. We would be pleased to provide you and the Florida State University (FSU) with an Excel data file consisting of information on the total number of ACGME-accredited training programs and residents per state, and the number of family medicine programs and family medicine residents per state, for the academic years 1993-1994 through 2007-2008, 15 years total.

These data are to aide your dissertation examining the effects of medical malpractice liability reforms on the family physician workforce. You and FSU are authorized to use these data for internal purposes only. These data should be destroyed within 3 years of receipt, or March 2017. The following source citation must be used:

Data from the Graduate Medical Education Database, Copyright 2013, American Medical Association, Chicago, Illinois.

You and FSU agree that AMA data are unique and confidential and shall not use such data and material for any purpose whatsoever except those specifically authorized herein. You and FSU shall not copy, sell or otherwise disclose AMA files to any third party except independent public accountants, staff or consultants employed by FSU (who are subject to the terms of an AMA agreement). You and FSU shall not incorporate any AMA data in any separate file, data bank, or listing for use other than in connection with this Agreement. You and FSU shall not use the data to create, update, or enhance its own database consisting of data that is the same or similar to the AMA data.

It is expressly understood and agreed that the AMA's right in the data are not assigned or released as the result of this Agreement, but at all times are reserved and retained by the AMA. We will send you the requested data in an email attachment no more than 10 business days after receipt of a signed letter of agreement. The fee for this request is $1000, for which you will receive an invoice separately.

Please process for signature and return this letter of agreement to me in an email.

AMA PLAZA | 330 N. WABASH AVE. | SUITE 39300 | CHICAGO, IL 60611-5885

Sincerely yours,

[redacted]

Sarah Brotherton, PhD
Director, Data Acquisition Services

Accepted and Agreed on behalf of FSU:

By: [redacted]

Title: PhD candidate / Research Assistant

Date: 2/27/2014

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BIOGRAPHICAL SKETCH

Alice Marie Ellyson was born in Huntington, West Virginia. Alice graduated from Sandy Creek High School in 2007, and attended Mercer University where she earned a Bachelor's degree in 2011, summa cum laude. She completed her Master's degree in 2013 at Florida State University, and entered candidacy in July of 2014.