ABSTRACT

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Health care market has often been regulated by government legislation. A California law passed in 1999 regulating minimum nurse to patient ratios in hospital units is one of them. This legislation was prompted by results from previous research showing higher adverse patient outcomes when hospital nurse/patient ratios are low. In the second chapter of my dissertation, I use a census of hospital discharges in California during 1996-2000 to estimate the impact of hospital staff levels on adverse events by examining whether outcomes are correlated with the number of admissions in the hospital over the next two days. I find quantitatively small and statistically insignificant effects of Friday and Saturday admission shocks on mortality rates of patients admitted on Thursdays. These results suggest that the portion of the California law designed to guarantee adequate staffing when the patient census increases unexpectedly should have little impact on patient outcomes.

Another regulation which has been proposed by the government is federal tort reform. One frequent justification for tort reform proposals is the potential impact of liability on defensive medicine. There is however, scant and conflicting evidence on whether malpractice risk alters physician practices. In the third chapter of my dissertation, I examine whether malpractice risk alters the procedure choices of obstetricians, who face one of the highest

rates of malpractice lawsuits and pay much larger malpractice premiums than most other specialties. By focusing on obstetricians, I can observe the impact of malpractice risk on the use of procedures such as cesarean sections, vaginal births after cesareans, prenatal care visits, the use of diagnostic tests such as ultrasound and amniocentesis, and the use of various equipment and techniques during the delivery such as fetal monitoring, forceps and vacuum extraction. Because the measured malpractice risk may signal something unobserved about physician quality or practice style, I use malpractice claims against non-OB/GYNs as an instrument for OB/GYN claims. I find that cesarean section rates and most other measures of physician behavior are not sensitive to medical malpractice risk.

LEGISLATING HEALTHCARE QUALITY.

By

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Dissertation submitted to the Faculty of the Graduate School of the
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Dedication

To my wife, Taehee, who could not have supported me any better. She gave up her professional career to give me a chance to fulfill my dream of studying Economics.

To my parents who loved me and who wanted to give me the best education possible. They were always with me providing me emotional support even though we are 7,000 miles apart. I also appreciate my parents-in-law's encouragement throughout the study.

I am grateful to my daughter, Youngkyung, and son, Youngjun, who are growing up as lovely children even though I could not spend much time with them during my graduate studies.

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Chapter 1: Overview

In 2002, Health care spending in US reached $1.5 trillion which was 14.9 percent of GDP (The World Health Report, 2005) and it is expected to rise more in the future. Government expenditure comprises 45 percent of total medical expenditure.

The medical care industry and efficacy with which it satisfies the needs of society differ from a competitive model (Arrow, 1963). Private and public medical insurance shields people from true cost of health care and might induce moral hazard. Healthcare market is far from perfectly competitive. As a result, governments intervene in the market frequently to ensure the proper function of the market. For example, to control the quality of the medical services provided the government regulates many things including doctors' license.

There are growing concerns of a causal relationship between nurse to patient ratio and patient outcomes. This becomes a bigger concern when the whole nation is experiencing a nurse shortage. Currently, there are 126,000 vacant nursing positions in U.S. hospitals.[1] There are 20 percent vacancy rates for nursing positions in California and 16 percent in Florida.[2] Nationwide, the number of employed registered nurses per 100,000 people has declined by 2 percent between 1996 and 2000, a time period when total heath care costs increased 26 percent.[3] California adopted the "Minimum Staffing Ratio" law based on growing evidence of a causal relationship between nurse staffing and patient outcomes. It will cost the California governments at least an extra $40 million per year to implement

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[1] TrendWatch, American Hospital Association, June 2001.
[2] Government Accounting Office, "*Nursing Workforce: Emerging Nurse Shortages Due to Multiple Factors*" (GAO-01-944), July 2001.
[3] Centers for Medicare & Medicaid Services, Office of the Actuary: National Health Statistics Group.

the new law. Due to the expected costs and controversy surrounding the legislation, it took five years to determine the specifics of the law before the regulations went into effect. The legislated minimum nurse/patient ratios range from one-to-one in operating rooms to one-to-eight in a well-baby nursery. The law also requires that the minimum staffing levels must be maintained at all times, without exception, except for extreme events such as earthquakes.

Although the bulk of the evidence suggests that lower nursing staff ratios in hospitals lead to higher rates of adverse events, the studies that prompted the California law may not have measured the causal impact of staff size on outcomes. Many previous studies compare outcomes and staff level across hospitals. The estimates in these studies are likely to suffer from an omitted variable bias. Hospitals differ along many dimensions and a hospital with higher staff levels may have other hard-to-measure characteristics, such as better surgeons, newer technology and more diagnostic equipment, that may help explain why these hospitals have higher quality of care. Similarly, studies that use within-hospital variation such as those studies that compare patients admitted to hospitals on weekend versus during the workweek may suffer from a selection bias. Many patients admitted during the week are scheduled admissions with less severe conditions. In contrast, patients admitted during the weekend tend to be more acute cases.

In the second chapter of the dissertation, I will use unexpected shocks to admissions as an exogenous source of variation in effective staff levels. When a patient is admitted to a hospital on a particular day, the effective staff level (e.g., the staff per patient ratio) the patient experiences will be determined by the number of admissions and discharges during his/her stay. If a patient in a particular hospital admitted on a Thursday faces an abnormally high number of admissions during his/her hospitalization starting the next day, then the effective staffing ratio will be lower for those Thursday admissions. The number of admissions over the next few days is highly variable and provides little if any information about patients already in the hospital. Therefore, I compare the outcomes of patients admitted to the same hospital but on different Thursdays and see whether outcomes are correlated with higher unexpected admission on the following Friday and Saturday. I focus on the outcomes of Thursday admissions for two reasons. First, hospital staffs are much smaller on the weekend so hospitals are potentially more vulnerable to admission shocks during this period. Second, according to our index of daily admissions, admissions during Friday and Saturday demonstrate more fluctuation than on weekdays, providing necessary variation to identify the model.

Although there are large unexpected shocks to admissions in a census of hospital discharges in California over the 1996-2000 period, I find only limited evidence that these swings in caseload adversely impact patients. In some samples, we find that large shocks to Friday/Saturday admissions reduce length of stay and increase readmission probabilities for patients admitted on Thursdays. However, these coefficients are very small. Most other outcomes such as in-hospital mortality and 7- and 14-days patient mortality are not impacted by the unexpected shocks.

Hardly a week goes by without a front page news article on rising health care costs (Newhouse, 1992). Even though health care cost increases faster than inflation, no one has come up with a credible answer for why it is rising this fast. One possible scenario is the over-use of medical care due to some factors like increased malpractice suits. In the

U.S., malpractice insurance premiums increased rapidly in the 1970's and 1980's and again in the early part of this decade. These recent hikes have led the American Medical Association to declare a 'malpractice premium crisis' in 20 states, including Florida, Pennsylvania and New York.[4] Nationwide, malpractice insurance premiums increased 15 percent between 2000 and 2002.[5] During the same time period, specialists in some areas experienced premium increases of more than 100 percent.[6]

In response to these premium hikes, many researchers and policy makers have attempted to pass federal tort reform bill to lower malpractice risk that doctors face.

Reform sponsors also argue that greater malpractice risk adversely affects the delivery of health care in two ways. First, an increase in risk may discourage doctors from treating people with certain conditions or from conducting risky (but potentially beneficial) procedures. Second, healthcare utilization may increase as doctors alter their practice patterns. To avoid a lawsuit or increase the chance of winning a malpractice suit, a doctor may perform procedures that have little or no medical benefit to the patients, but that protect him from possible future litigation. Opponents of reform bills believe that limiting compensation for the injured can be unfair to the people who have already suffered significantly from medical malpractice.

At the center of this controversy is the question whether doctors are changing their practice patterns and procedure choices based on malpractice risk that they face. To date, there is little hard evidence on this point.

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[4] http://www.ama-assn.org/ama/noindex/category/11871.html

[5] Based on the author's calculation using Medical Liability Monitor annual premium survey. No weights were used.

[6] Malpractice premium increased 115 percent for OB/GYNs in Oregon and 110 percent for general surgeons in Mississippi.

I focus on obstetric care since obstetric care is the practice area that is thought to be particularly affected by high malpractice risk. It is often suggested that malpractice concerns encourage OB/GYNs to perform more cesarean sections (c-sections) than medically needed. For example, the birth of a neurologically-impaired infant, the most prevalent reason for a lawsuit in the OB/GYN specialty, can occur before or during deliveries (ACOG survey, 2003). However, in deliveries involving a birth injury, doctors are more likely to be suspected as negligent when the baby is delivered vaginally due to the limited control of progress compared to cesarean section (Sachs, 1989). Therefore, some have suggested that plaintiffs can more easily argue doctors' negligence, such as the lack of a timely response for more invasive procedures, when a baby is delivered vaginally. Others have argued that the rapid increase of the U.S. c-section rate in the 1980's relative to England (which showed a similar c-section rate to the U.S. in the 1970's) is attributable to the difference in legal environments. There is also more than a 10 percentage point difference in c-section rates between some states, with some of this potentially explained by differences in legal environments.[7]

In my empirical analysis, I measure the malpractice risk that doctors face using the National Practitioner Data Bank. This data set is a national universe of resolved malpractice claims either by settlement or jury verdicts for the 15 year period from 1990 to 2005. This long time period allows me to exploit the considerable variation both between states and within a state over time in malpractice risk at the extensive (number of cases) and intensive (awards per case) margins. I calculate malpractice risk in two ways: as either the number of OB/GYN claims per 1,000 births in each state over the last

[7] In 2003, the c-section rate in Florida was 30.8 percent compared to 19.2 percent in Utah based on National Vital Statistics Report, Vol. 54, No. 2, 2005.

three years, or the amount of OB/GYN claims paid per birth in $1,000s in each state over the last three years.[8] I will combine these malpractice measures with a census of birth in US which has delivery method information as well as various measures of prenatal care.

One challenge for reliable identification is that malpractice risk as defined above may be correlated with other factors related to the treatment decision, such as unobserved patient characteristics, physician quality or practice style. For example, if doctors respond to malpractice risk by performing more c-sections, this may decrease the malpractice risk since c-section lowers the probability of a lawsuit compared to vaginal delivery (reverse causality). To address this issue, I use an instrumental variables (IV) identification strategy that will capture only the malpractice risk generated by a state's legal environment. In particular, I use the malpractice risk in non-OB/GYN specialties as an instrument for the OB/GYN risk measure.

My findings demonstrate that cesarean section rates are not responsive to medical malpractice risk. Additionally, access to health care, measured by the number of prenatal visits during pregnancy, is also insignificantly related to malpractice risk. I also find that malpractice risk has no statistical or qualitatively important impact on the use of other procedures such as ultrasound, fetal monitoring, forceps, or vacuum.

[8] I use three years for the following two reasons. Average litigation process takes three to four years and malpractice risk is a noisy measure, since the incidence rate is low.

Chapter 2: Patients Outcomes When Hospitals Experience a Surge in Admissions

2.1 Introduction

In 1999, California's Governor Gray Davis signed into law AB 394 which requires hospitals to maintain specific ratios of nurse to patients. In January of 2004, after years of hearings to determine the specifics of the law, the regulations went into effect. The legislated minimum nurse/patient ratios range from one to one in operating rooms to one to eight in a well-baby nursery. The law also requires that the minimum staffing levels must be maintained at all times, without exception. The law does not even allow deviations from legislated limits during short time breaks for necessary needs.

The California law was motivated by a growing number of studies documenting higher adverse events in hospitals with lower nurse/patient ratios. For example, a 1999 report by the Institute for Health and Socio-economic Policy examined four years worth of hospital discharge data from California and found that over the time period analyzed, inpatient outcomes were declining and so were hospital staff levels. Studies by Aiken *et al*. (2002) and Provonost et al. (1999) found higher adverse event rates in hospitals with lower staffing ratios. Finally, Bell and Redelmeier (2001) found higher mortality rates among patients admitted over the weekend, a period when hospital staffs are typically lower, compared to admissions during the week, when the hospital staff levels are greatest.

The call for higher nurse/patient ratios is coming at a time when some hospitals are having a difficult time filling positions. A number of medical groups have documented a nursing shortage in the United States. Currently, there are 126,000 vacant nursing positions in U.S. hospitals.[9] Vacancy rates for nursing positions are 20 percent in California and 16 percent in Florida.[10] Nationwide, the number of employed registered nurses per 100,000 people has declined by 2 percent between 1996 and 2000,[2] a time period when total heath care costs increased 26 percent.[11] Given the specifics of the California law and the current shortage of nurses, the costs of AB 394 are expected to be large. Dobkin (2003) cited work that estimates a cost of at least $400 million per year to implement the California nurse/staffing rule. Nevertheless, many health care groups are looking closely at the consequences of the California law and other states are preparing similar statutes.

Although the bulk of the evidence suggests that lower nursing staff ratios in hospitals lead to higher rates of adverse events, the studies that prompted the California law may not have measured the causal impact of staff size on outcomes. As we document below, the bulk of the studies compare outcomes and staff level across hospitals. The estimates in these studies are likely to suffer from an omitted variable bias. Hospitals differ along many dimensions and a hospital with higher staff levels may have other hard-to-measure characteristics, such as better surgeons, newer technology and more diagnostic equipment, that may help explain why these hospitals have higher quality of care. Similarly, studies that use within-hospital variation such as those studies that compare patients admitted to hospitals on weekend versus during the workweek may suffer from a selection bias. Many patients admitted during the week are scheduled

[9] TrendWatch, American Hospital Association, June 2001.
[10] Government Accounting Office, "*Nursing Workforce: Emerging Nurse Shortages Due to Multiple Factors*" (GAO-01-944), July 2001.
[11] Centers for Medicare & Medicaid Services, Office of the Actuary: National Health Statistics Group.

admissions with less severe conditions. In contrast, patients admitted during the weekend tend to be more acute cases.

In this chapter, we return to the original question that led to the California legislation and examine whether lower hospital staff levels cause adverse patient outcomes. Specifically, we will use unexpected shocks to admissions as an exogenous source of variation in effective staff levels. When a patient is admitted to a hospital on a particular day, the effective staff level (e.g., the staff per patient ratio) the patient experiences will be determined by the number of admissions and discharges during his/her stay. If a patient in a particular hospital admitted on a Thursday faces an abnormally high number of admissions during his/her hospitalization starting the next day, then the effective staffing ratio will be lower for those Thursday admissions. The number of admissions over the next few days is highly variable and provides little if any information about patients already in the hospital. In this chapter, we compare the outcomes of patients admitted to the same hospital but on different Thursdays and see whether outcomes are correlated with higher unexpected admission on the following Friday and Saturday. We focus on the outcomes of Thursday admissions for two reasons. First, hospital staffs are much smaller on the weekend so hospitals are potentially more vulnerable to admission shocks during this period. Second, according to our index of daily admissions, admissions during Friday and Saturday demonstrate more fluctuation than on weekdays, providing necessary variation to identify the model.

The data for this project come from restricted-use hospital discharge data in the state of California over the 1996 to 2000 period. The data set contains a census of hospital discharges in the state over this period. The restricted-use version of the data contains the exact date of admission and discharge, and an encrypted Social Security Number allowing us to track patients over time and merge records with vital statistical data.

Our identification strategy has a number of distinct advantages over those used in previous papers. First, unlike previous cross-hospital studies, this chapter exploits the variation over time in admissions within a hospital, allowing us to control for permanent differences across hospitals in the quality of care. Second, unlike studies that rely on within-hospital variation by the day of the week, we are not comparing admissions across different days of the week when patient acuity may be fundamentally different. Third, our identification strategy mimics the exact situation the California minimum staffing law was designed to avoid, namely, large swings in patient case load. Fourth, we use much larger samples than have been used in the past.

Although there are large unexpected shocks to admissions, we find only limited evidence that these swings in caseload adversely impact patients. In some samples, we find that large shocks to Friday/Saturday admissions reduce length of stay and increase readmission probabilities for patients admitted on Thursdays. However, these coefficients are very small. We find quantitatively small and statistically insignificant impacts of Friday/Saturday admission shocks on in-hospital mortality and 7- and 14-day patient mortality. The shocks to admissions that we observe in the data are quite large. In five percent of the Thursdays in our sample, subsequent Friday/Saturday admissions are 40 percent over 'predicted' numbers, yet there is limited evidence that patient health suffered to any degree. These results suggest that we should expect little impact on patient outcomes from the portions of the California hospital staffing law designed to insure adequate staffing when patient census fluctuates.

2.2 Recent Literature on the Impact of Hospital Staff Levels

Previous research on the impact of nurse staffing ratios on adverse patient events can be classified into two types of studies: cross-hospital studies and within-hospital comparisons. In a typical cross-hospital study, researchers merged administrative data on hospital discharges with survey data on average hospital staff levels. The researcher then compared outcomes across hospitals with high and low staff levels, controlling for a variety of factors. For example, using discharge data from 168 hospitals, Aiken *et al.* (2002) found a higher probability of adverse outcomes, such as death within 30 days and failure to rescue, for hospitals with low registered nurse-to-patient ratios, after adjusting for a variety of patient and hospital characteristics. They also observed a positive relationship between low registered nurse-to-patient hospitals and a registered nurses' emotional exhaustion or job dissatisfaction.

Using discharges from 799 hospitals, Needleman *et al.* (2002) found better patient outcomes, such as shorter length of stay or a lower probability of a urinary tract infection, when more nursing care was provided by registered nurses instead of licensed practical nurses or nurses aides. The nurse-to-patient ratio had a statistically significant impact on determining patient outcomes.

Pronovost *et al.* (1999) examined outcomes for abdominal aortic surgery patients in 39 Intensive Care Units (ICU) in Maryland hospitals from 1994 to 1996. The authors found that daily rounds by an ICU physician were associated with reduced risk of in-hospital mortality after adjusting for patient characteristics, co-morbid disease,[12] severity of illness, hospital and surgeon volume, and hospital characteristics. Also, a lower nurse/staffing ratio and not having a full-time ICU medical director increased the ICU length of stay significantly.

Although all of these cross-hospital studies suggest hospitals with more staff per patient have better patient outcomes, one can question whether these results represent a causal relationship. Hospitals vary considerably in both the characteristics of their patients and in their facilities, and in many cases, these characteristics are correlated. For example, it would not be surprising to find hospitals with higher nurse/patient levels to also have more advanced hospital technology. Likewise, lower-staffed hospitals may serve a decidedly sicker clientele. Therefore, these cross-hospital estimates are likely subject to omitted variable bias.[13]

The problems inherent in cross-hospital studies have been recognized by others. In response, researchers have suggested using within-hospital models as a way to address the omitted variables bias. These studies use variation in hospital staffing levels at different points in time within the same hospital to identify the impact of staff levels. Having time-series variation in outcomes and staffing within a hospital allows the researchers to hold constant hospital-specific characteristics in their analysis. For example, one group of studies uses the differences in staff levels on weekends versus weekdays to identify the consequences of different staffing levels. In these studies, the

[12] Disease existed on admission as a preexisting condition.

[13] The bias does not necessarily go in one direction. Patient selection into hospitals may mean that the benefits of high hospital staff levels are understated. Capps *et al*. (2001) found that patients with more severe cases are more willing to endure higher costs and longer travel to get treatment from hospitals with better reputations. If hospitals with better reputations also have more staff per patient, then the selection of more severe patients into better hospitals will lower the observed performance of these hospitals and make it might look as if the higher staffing ratio does not matter for outcomes.

authors note that in most cases, acute care hospitals do not operate fully during the weekend. This will result in lower staffing ratios on the weekend compared to weekdays. Bell and Redelmeier (2001), using an administrative hospital discharge data set from Ontario, Canada over the 1988 to 1997 period, found that patients admitted during the weekend experienced a higher mortality rate than patients admitted on a weekday, after adjusting for age, sex and coexisting disorders. In this work, the authors restricted their attention to acute care admissions for three diseases (ruptured abdominal aortic aneurysm, acute epiglottitis, and pulmonary embolism) that originated in the emergency room. The three diseases were picked because they are more likely to show different outcomes as a result of differential care. Patients admitted through the emergency room are more likely to have less variation in their severity of illness between weekdays and weekend since less urgent cases will not use the emergency room. They found that weekend admissions were associated with significantly higher in-hospital mortality than weekday admissions for these three diseases. As a specification test, they picked three other diseases (myocardial infarction, intracerebral hemorrhage, and acute hip fracture) that are less impacted by staff-level variation. They did not find excessive mortality in any of the other three diseases for weekend admissions. In addition, they observed that 26 diseases among the 100 with the highest in-hospital mortality showed significantly higher odds ratio of in-hospital mortality and none of these top 100 diseases revealed a lower odds ratio. It is also possible that the results in this study can be explained by differences in the quality of the staff that works on weekdays and weekends.

Tarnow-Mordi *et al.* (2000) collected four years of information on admissions to the ICU from a hospital in Scotland. They found a statistically significant higher mortality rate when there is a high workload relative to the nursing staff in the ICU, even after adjusting for the severity of the illness using the patient's Acute Physiology and Chronic Health Evaluation (APACHE) II score.[14]

Although within-hospital studies should reduce some types of omitted variables bias, they are subject to selection bias from another source. Hospitals may admit a different type of patient on the weekend compared to the weekday. It is likely that given the lower staff levels on the weekend, hospitals may only admit patients with more severe conditions on the weekend. Patients might also try to avoid going to the hospital during the weekend unless their illness is severe enough to endure the inconvenience of a weekend emergency room visit. As a result, there is the possibility that high adverse outcomes among patients admitted during the weekend could be the result of differential severity and not because of differential staffing.

Dobkin (2003), using the same weekly staffing variation used by Bell and Redelmeier (2001), found no statistically significant relationship between the day of admission and in-hospital mortality using California hospital discharge data from 1995 to 1999. Dobkin used two methods to control for the different acuity of patients admitted during the weekday and weekend. First, Dobkin used only a subsample of patients who were admitted through the emergency room, which are all unplanned visits. Second, Dobkin added a variable to the regression that measures the difference between the actual number of admissions on the day of the week by disease and an evenly distributed number of admissions throughout the week. Dobkin argues that a similar number of patients should be admitted regardless of the day of the week for each illness if there is

[14] The APACHE II score is derived from a research of ICU admissions on 13 US hospitals between 1979 and 1982 to predict a patient's risk of death using routine information. It is the most widely used method of assessing the severity of illness in patients in intensive care units.

no selection by severity. Once this variable is added to the model, the higher mortality rate for patients admitted during the weekend disappears.   The question we address is conceptually similar to that posed by Dobkin, but our identification strategy is different. Dobkin compares patients admitted on weekdays (when staffs are high) and the weekend (when staffs are low). In contrast, we hold fixed the day of admission and compare outcomes when the next two day's admissions are high (and effective staff levels are low) and low (and effective staff levels are high).

2.3 Empirical Methodology

2.3.1 A Proxy for effective staff levels

Although there are many patient-level data sets that provide detailed information about hospital discharges, there are no large-scale data sets that identify hospital staff levels on a particular day. As a result, there is no published research correlating patient outcomes with actual daily staff levels. Researchers can only infer hospital staff levels from other variables such as Bell and Redelmeier's (2001) use of weekend versus weekday admissions. In contrast, most cross-hospital studies have some measure of hospital staff levels but the variable is averaged over many months of service.

In this chapter we use plausibly exogenous variation in 'effective' staff levels to identify the impact of staffing on outcomes. Daily hospital staff levels are determined some weeks in advance using the hospital's expectations about admissions to plan for staff needs. Given these fixed work schedules, the expected nurse/patient ratio is subject to variation based on unexpected surges in patient admissions.   As we demonstrate below, the number of admissions does vary considerably from day to day, and this variation will generate unexpected shocks to the effective hospital staffing level. Consider a patient admitted to a hospital on a Thursday. Over the next few days, the effective staff level for that patient will fall if an unexpectedly large number of patients are admitted to the hospital.

To measure the effective staff level faced by a patient, we will use admissions during the next two days as a proxy for effective nurse staffing levels. We decided not to use admissions on a patient's day of admission because this may create a selected sample – if admissions on a Thursday are running above average, hospitals may be less likely to admit less-severe cases, making the remaining admissions a selected sample. The next two days' admissions are by definition exogenous to the patient admitted today. The window that we observe is however open to debate. We choose two days for a couple of reasons. First, the majority of patients are only in the hospital for two to three days. If we considered a longer period of admission than two days, we could include unrelated staffing variation. On the other hand, we would be throwing out useful variation by restricting attention to only the next day's admissions.

As we noted earlier, we choose to focus on the outcomes of patients admitted to hospitals on Thursdays because hospitals tend to have much smaller staffs on the weekend and hospital admissions are more volatile on Friday and Saturday compared to other days. The difference in work schedules for hospital nurses based on the day of the week is reflected in Table 2-1. In that table, we report results from the Work Schedule Supplements to the Current Population Surveys (CPS). The CPS is a monthly survey of 60,000 households and their members. The survey results provide basic labor market data such as the monthly unemployment rates for the nation. Supplements to the basic monthly survey are sometimes administered and the two most recent Work Schedule Supplements were fielded in May of 1997 and May of 2001. In 1997 workers were asked whether they worked on particular days of the week, and in the 2001 supplement, they were asked what days of the week they usually work. We use the detailed industry and occupation codes to produce a subsample of registered nurses and nurse aides who work in hospitals. In the May 1997 survey, the fraction of nurses or nurse aides working during the weekend is one half the fraction working during the week. The fraction drops considerably in the May 2001 survey, most likely due to the slight but important change in the question between the two surveys. In any event, substantially fewer nurses and nurse aides work during the weekend than during the week. In the 1997 CPS data, there were 52 percent fewer registered nurses working on Saturday compared to Wednesday. This number increased to 81 percent in the 2001 CPS. In the final column of the table, we report the distribution of hospital admissions by the day of the week in 1997 and 2000. Note that the drop in admissions on the weekend is not as large as the drop in staff levels. In both 1997 and 2000, there was a drop of 42 and 41 percent respectively, in the number of admissions between Wednesday and Saturday.

As we discuss below, the primary data for this analysis are a census of hospital discharges in the state of California over the 1996-2000 period. To illustrate the volatility of admissions on the weekend versus weekdays, we constructed hospital-specific counts of daily admissions for each of the nearly 400 California hospitals in our sample. We then construct a daily eight-week moving average of admission counts, so for Tuesdays, we use the previous eight Tuesdays for the moving average.[15] Next, we

[15] We picked eight weeks as a window because our discussions with a number of hospital administrators indicated that hospital schedules were set one to two months in advance. We also estimated models with

divide the hospital's actual admission on a day by the day-specific eight-week moving average. The index is centered on 1 and its value represents the percentage difference between the actual and the previous eight weeks' average levels. Descriptive statistics for these series on a daily basis are printed in Table 2-2. For each day, there is substantial volatility in the admission index. On 10 percent of the days, hospital admissions are 31 to 42 percent above levels one would expect. Important for our analysis, however, is the volatility of admissions on Friday and Saturday. Admissions on these two days are the first and third most volatile days, based on the standard deviation of the index and the top two days when volatility is measured as the 90/10 ratio.

Hospitals have a number of options to deal with these surges in admissions, including using nurses from other units, extending overtime, calling in nurses from home, and using temporary nurses. Anecdotal evidence suggests these procedures are less than adequate to deal with large shocks to patient census. A Chief Financial Officer from a network of hospitals notes that "One of the most complex financial and operational challenges that hospitals and health systems face today is a widely fluctuating patient census…The question is, how should we flex when the census goes up by 40 percent in a short amount of time? We typically deal with that by calling all the available nurses in on short notice, but we really don't have a system that works smoothly for both the hospital and the nurses."[16] In a policy document discussing the California nurse staffing law, the Service Employees International Nurse Alliance notes that "Nurses on medical/surgical units report they experience large fluctuations in patient census throughout a shift, and a

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four-week moving averages and the results from these models were qualitatively similar to the ones presented here. In Table 7, we demonstrate that the results are not sensitive to the number of days after admission that we construct the admission index.

[16] http://www.hfma.org/resource/blackink/cfo_profile_michaelal.htm

lack of available nursing capacity to respond to this variation (p 8)."[17] In the winter of 1997/1998, California was hit with a particularly heavy flu season. Hospitals were filled to capacity during this period, and there were widespread reports of patients not receiving adequate care. A report about this particular event by the California state government noted that, "Given the lack of a similar seasonal respiratory disease burden over the preceding years and decreasing hospital staff resources, hospitals were unprepared to deal with the sudden demand for services."[18] The inability to deal with a shock to patient census can lead to a decline in patient health. A study by Leape *et al.* (1995) of 247 adverse drug events in one particular hospital noted that a major reason for the errors was "excessive workloads due to inability to match staffing assignments to the clinical load when there were fluctuations in patient census and severity of illness." In response to these concerns, Title 22 CCR 70217(q) of the California nurse/staffing law now requires hospitals to plan for routine fluctuations in patient census and requires that the nurse/staffing law hold at all points, not just on average. Unpredictable events such as an earthquake or a terrorist attack would not be considered 'routine fluctuations' but a spike in admissions during the flu season would be considered routine.

2.3.2 Data

The primary data set for this analysis is an administrative hospital discharge data set that contains a census of discharges in the state of California over the 1996-2000 periods. The data set is maintained by the State of California Office of Statewide Health Planning and Development (OSHPD). Public use versions of the data include

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[17] http://www.nurseallianceca.org/patients/OfficialSEIUNurseAllianceCommentstoDHSonRatios.pdf.

[18] http://www.emsa.ca.gov/dms2/report1.doc

information on hospital identification, month and day of admission, total length of stay, up to thirty diagnoses and up to twenty performed procedures, the lag between admission and the performance for each procedure, total charges, expected payer, patient characteristics (e.g., age, sex and race) and type of care (e.g., acute care, psychiatric care, chemical dependency recovery care etc.). The data also have information on the source of admission such as home or other acute care hospital and disposition, including whether the patient died during hospitalization or was transferred to another facility. The data do not contain the exact date of admission, which is needed for this project. This variable is, however, available in the restricted-use version of the data obtained for this project. The restricted-use data also contain a scrambled Social Security Number, which allows us to track patients over time. This same scrambled Social Security Number also allows us to link discharge records to Vital Statistics data. Over the five years in our sample, there are approximately 18 million hospital discharges in total. We initially restrict our attention to all discharges after excluding labor, children under 17-years-old, and mental health patients,[19] leaving us with 9.9 million records. We call this the adult medical and surgical admissions sample.

2.3.3 Outcomes

Although the hospital discharge files have very detailed data, there are a limited number of outcomes that one can use to measure adverse events. As we noted above, one way hospitals can deal with higher than expected admissions is to discharge patients early, so our first outcome is length of stay measured in days. If we assume that the

[19] We exclude sub-groups mentioned above since they are typically treated in separate wings of the hospitals.

patient's condition on discharge is homogeneous, decreased length of stay shows increased efficiency meaning that hospitals performed better care. Without the homogeneity assumption, a shorter length of stay may indicate patients were discharged before they were fully recovered.

The next outcome we will use is in-hospital mortality, which has been used in many other studies including Bell and Redelmeier, Tarnow-Mordi *et al.,* and Pronovost *et al*., and can be identified directly from the hospital discharge record. Death from less than optimal care may not always happen in a hospital, however. A patient may die soon after discharge from poor care in the hospital, a complication generated by the hospital stay, or being discharged too early. We can identify deaths over a fixed period after admission, either in the hospital or not, by linking the hospital discharge data with California vital statistics records. Given the immediacy of the shock to admissions that we measure, we identify whether the death occurred within 7 and 14 days from admission.

A third type of outcome is hospital readmission. If patients are discharged before they are fully recovered, they are at a greater risk of being rehospitalized. We can identify readmissions by matching inpatient records over time using the scrambled Social Security Number. Because we match by a patient identification number, we can identify readmissions even if the patient was admitted to a different hospital.[20] We construct indicators for whether a patient is readmitted within 7 and 30 days of the initial admission.

[20] We cannot however identify a readmission if it occurs out of the state of California or if the patient is readmitted to a federal hospital, which are not in our data. Likewise, we cannot identify an out-of-hospital death if it occurs outside the state of California.

In-hospital mortality is a rare event and regardless of the quality of care, most inpatients are not at risk of dying in the hospital. To focus attention on those most at risk of dying from substandard care, we restrict our attention to subsamples of patients who have the highest risk of death. First, from our sample of adult medical and surgical admissions, we consider a sample of people whose primary diagnosis is one of the 100 diseases with the highest number of in-hospital deaths in our five years worth of data.[21] We define disease categories by the first four digits of the ICD-9 CM codes. We also consider a sample of the 50 diseases with the highest number of in-hospital deaths. Third, we select from the 100 diseases with the highest mortality counts, the 50 diseases with the highest mortality rates (deaths divided by inpatient admissions).[22]

'Failure to rescue' is another subsample and it includes patients who present particular diseases that may be complications of admissions but who should, in the normal course of care, not die in the hospital from these complications . The sample includes patients with pneumonia, deep vein thrombosis/pulmonary embolism, sepsis, acute renal failure, shock/cardiac arrest, and gastrointestinal hemorrhage/acute ulcer. The underlying assumption is that hospitals that provide enough care by staff identify these complications quickly and treat them aggressively (Agency Healthcare Research and Quality, 2003). Following the standard sample selection criteria, we will not include in this sample patients aged 75 years and older, newborns and neonates, and patients

[21] Ten highest death counts diseases in this category are acute respiratory failure, pneumonia, organism unspecified, pneumonia due to inhalation of food or vomitus, congestive heart failure, unspecified, unspecified septicemia, intracerebral hemorrhage, cerebral artery occlusion, unspecified, subendocardial infarction, obstructive chronic bronchitis, acute renal failure, unspecified.

[22] Ten highest death rate diseases in this category are cardiac arrest, anoxic brain damage, shock without mention of trauma, abdominal aneurysm, ruptured, bronchus and lung, unspecified (malignant neoplasm), intracerebral hemorrhage, subarachnoid hemorrhage, ventricular fibrillation and flutter (cardiac dysrhythmias), pancreas, part unspecified (malignant neoplasm), acute and subacute necrosis of liver.

transferred from acute care or long- term care facilities. These patients are excluded because they can have high mortality rates for reasons having no relation to the quality of care, adding more noise than other groups to the model.

Table 2-3 contains information about outcomes and basic demographic characteristics for admissions in the California hospital discharge data for the various subsamples. Most variables are dichotomous so we simply report the fraction for these variables. For continuous variables, we report the sample mean and in parenthesis, the standard deviations of outcomes. In the first column, we report data for all 9.9 million admissions for the adult medical and surgical admissions sample.[23] During the five-year period of our analysis, patients were discharged from around 400 different hospitals. Twenty-eight hospitals opened sometime during our sample while sixty-five hospitals closed during the period.[24]

The other columns in the table contain information about the subsamples we will use in our analysis. These columns contain data for acute care admissions that began on a Thursday. As we move from the full sample to samples with progressively higher mortality rates, there is an increase in the mean length of stay, mortality, and the readmission probabilities. Comparing the outcomes for the 100 diseases with the highest mortality counts and the 50 diseases with the highest mortality rates, we see that mortality rates more than double and length of stay has increased by 42 percent. Looking

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[23] Patients only enter the data set once they are discharged from a hospital. As a result, some patients admitted to hospitals during the final months of 2000 would not have been discharged by the end of the year and therefore, they would not appear in our data set, making those patients who do appear a selected sample. To avoid this sample selection problem, we did not include the last three months of data for each hospital.

[24] We are observing this in the discharge data set using the hospital identification number. Therefore, we do not know the detailed reason of change. For example, merging of hospitals and closing down of a hospital will appear exactly the same to us.

at the demographic characteristics, moving to a higher mortality sample increases average age, except in the failure to rescue sample where we deleted those 75 years of age or older. Naturally, the fraction on Medicare increases and decreases accordingly with average age. In contrast, samples with higher mortality rates have a lower fraction of females and a lower fraction with private insurance.

2.3.4 Measuring Unexpected Hospital Admissions

The primary covariate of interest is a measure of the number of unexpected admissions to a hospital over the next two days. So for patients admitted on a Thursday, we count patients admitted on the following Friday and Saturday. These admissions will only be a shock to effective hospital staff levels if they represent a change from recent averages. We will use two methods to measure these unexpected shocks to admission. The first method is based on a moving average of admissions for a hospital. Specifically, we sum admissions over the next two days, then divide by the moving average of this value from the previous eight weeks.[25] Hospitals vary considerably in size, and this moving average index takes this into account. The eight-week average allows for expansions in hospital size and any secular or seasonal trends in admissions. To construct this index, we delete the first eight weeks of data for any hospital. We also delete small hospitals, which we define as those with fewer than 10 admissions per week. Even after this sample selection decision, we had a small number of cases with extreme values that could be due to a variety of factors, including coding errors in the hospital

[25] We also constructed an index where we created day-specific moving averages, then averaged the two days` indices. So for Thursday's admissions, we averaged the eight- week moving average for Friday and Saturday. Our results are robust regardless of moving average construction methods.

identification code. As a result, we dropped any moving average based admission index that is 150 percent larger than the 99th percentile of the distribution. We call this variable the *Friday/Saturday Moving Average Admission Index.*

There is tremendous seasonal variation in hospital admissions. Hospitals tend to have higher admissions during the winter season due to the high incidence of respiratory diseases such as pneumonia. This cyclic pattern is known to hospitals and they can adjust their staffing accordingly, so this type of movement in admissions may not be unexpected. We base a second index of hospital admissions on regression residuals from a hospital-specific regression that controls for this type of cyclic pattern in admissions. Specifically, using up to 60 months of data for each hospital, we run a regression of hospital admission counts for hospital h on day t on a time trend based on the months in the sample and trend squared (1 for the first month in the sample, 2 for the second, etc.), monthly dummy variables (January, February, etc.), day of the week dummy variables and a dummy variable for whether the day is a Federal holiday.[26]   For Thursday admissions, we sum the actual admissions on Friday and Saturday, then divide by the sum of the predicted admissions for these two days. We call this variable the *Friday/Saturday Regression-Based Admission Index*.

Table 2-4 shows the distribution for the *Friday/Saturday Moving Average Admission Index* and the corresponding values for the regression-based index. Both indices are unimodal and symmetric, with means and medians equal to 1. For the *Friday/Saturday Moving Average Admission Index*, the 25th and 75th percentiles are 13

[26] For most hospitals, these regressions appear to fit the data well, especially for larger hospitals. However, in some cases, in particular for smaller hospitals, the model fit is poor. We dropped hospitals where the R^2 was less than 0.5.   The poor fit was almost exclusively due to small daily admission counts, so the R^2 cutoff essentially increased the average size of hospitals in models that use the regression-based admission index.

percent below and above the median, respectively, and the 5th and 95th percentiles are 32 percent below and 37 percent above the median, respectively. For the *Friday/Saturday Regression-Based Admission Index*, the 25th and 75th percentiles are 10 percent above and below the median, respectively, and the 5th and 95th percentiles are 25 percent below and 27 percent above the median, respectively.

2.3.5 Econometric Specification

In contrast to the previous literature in this area that utilizes cross-hospital variation to identify the impact of hospital staffs, we use a within-group model that allows us to control for differences in fixed-hospital characteristics that may be correlated with outcomes. Outcomes (Y) vary across patients i, hospitals h and time t and the basic reduced-form model we estimate is of the form:

$$(1) \quad Y_{iht} = \beta X_{iht} + \lambda(\textit{Friday/Saturday Admission Index}_{ht}) + u_h + YEAR_{rst} + MONTH_{rst} + \nu_{iht}$$

The vector X contains individual-level control variables that measure patient demographic characteristics (such as age, race, sex, and insurance status), and acuity (as measured by the 4-digit version of the International Classification of Disease 9th version, Clinical Modification [27] (ICD-9CM) codes). For the failure to rescue sample, we controlled the severity of illness in more detail since this is a higher-risk sample. The variable u_h is a hospital-specific fixed effect which controls for permanent differences

[27] The ICD-9CM codes are comprised of 5 digits such as xxx.xx, and moving left to right in the code, the numbers have a hierarchical structure. The first digit is a broader classification and moving to the right in the code, the numbers provide more detailed information about the condition.

across hospitals in the outcomes of their patients. To control for seasonal and secular variation in outcomes, we also use monthly and yearly dummy variables. We also added a dummy variable to indicate whether the admission occurred on a Federal holiday. Because we expect hospitals of different sizes and in different parts of the state to have different seasonal and secular cycles, we allow the month and year effects to vary by hospital size (s) and region (r). We use four different hospital sizes (those with maximum occupancy less than 50, between 50 and 150, between 150 and 300 and 300 over) and 14 different service regions.[28] Finally, ν_{iht} is a random error.

The key covariate in the model is the *Friday/Saturday Moving Average Admission Index*. When Y is defined as an adverse event, we expect the coefficient λ to be positive, signifying that patient outcomes deteriorate when effective staff levels drop.

2.4 Results

Table 2-5 presents the results of equation (1) using the six key outcome measures and the 100 diseases with the highest mortality counts sample admissions on Thursdays. In these regressions, the measure of hospital staff levels is the *Friday/Saturday Moving Average Admission Index*. We report results for six outcomes: length of stay, died in the hospital, died within 7 and 14 days of admission, and readmitted within 7 and 30 days. We estimate all models as linear regressions, although the last five outcomes are dichotomous.[29] For each outcome, we estimate two models. The first uses the admission

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[28] These 14 regions are defined by the United States Department of Health and Human Services for health planning on a regional basis and include Los Angeles, Orange County, etc.

[29] Given the sample sizes and the large number of fixed-effects in our analysis, we were unable to estimate logit regression models for many of the dichotomous outcomes with standard software packages. However, we were able to estimate a complete set of logit regression results for the sample that contains the

index added linearly to the model. To capture possible non-linearities between the staff level and patient outcomes, the second model includes dummy variables for the quintile value of the *Friday/Saturday Moving Average Admission Index*. The cutoffs for these values are reported in the bottom of Table 2-4. In these models, index values between the 40th ad 60th percentile values are considered the reference group.

To save space, we do not report coefficients on the other covariates although the results for these variables are comparable to results from other studies.[30]

The first panel of the table reports results for the *Friday/Saturday Moving Average Admission Index* added as a linear variable. The numbers in parentheses in the table are standard errors. None of the results using this variable are statistically significant except for the result in the length of stay equation. Below each standard error, we report the magnitude of the coefficient in elasticity form, giving the percentage change in the outcome for a percentage change in the index. In all cases, the elasticities are very small, signifying that even large shocks to Friday/Saturday admission will have little impact on the outcomes of patients admitted on Thursdays. Focusing on one particular result, consider the coefficient on the index for the "died in hospital" equation. The value of the *Friday/Saturday Moving Average Admission Index* of 1.4 is 40 percent above the sample mean and occurs roughly 5 percent of the time. Even with this large

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50 diseases with the highest mortality rate sample. The estimated marginal effects on the *Friday/Saturday Moving Average Admission Index* variable are as follows: Died in hospital 0.0008, Died within 7 days of admission, -0.0014; Readmitted within 7 days, 0.0029; readmitted within 30 days, 0.0065. We note that these marginal effects are very close to the linear probability estimates for these samples that are reported in Table 6.

[30] For example, we found that males had lower in-hospital mortality rates compared to females, blacks had lower rates compared to whites, Hispanics had lower rates compared to non-Hispanics, and in-hospital mortality increased nearly monotonically with age. These results are similar to those found in Dobkin (2003). We also found higher mortality rates for patients admitted on Federal holidays. There was no statistically significant difference in in-hospital mortality rates among patients with different types of insurance.

shock, the coefficient in Table 2-5 suggests that the probability of death will increase by only 8 ten thousands of a percentage point or one tenth of one percent of the sample mean. The standard errors on many estimates in Table 2-5 are however very large, compared to the parameter estimates. As a result, we cannot rule out the possibility that a surge in admissions has a more substantial impact on outcomes. Again, focusing on the results from the 'died in hospital equation,' the top end of the 95 percent confidence interval for this parameter is 0.002748. If this value is the true impact of the admissions index on in-hospital mortality, then a surge in admissions over the next two days that is 40 percent over the sample mean would increase in-hospital mortality by 0.0011, which is now 1.2 percent of the sample mean. Subsequently, although we can conclude there is no statistically significant impact of a surge in admissions on inpatient mortality, we cannot reject a hypothesis that suggests the impact is of modest quantitative importance. Table 2-9 presents estimates from larger samples that reduce the sampling variance by a considerable amount and helps reduce the uncertainty about the estimated impact.

In contrast to the results for length of stay and mortality, the results suggest a larger impact of a surge in admissions on hospital readmission rates, but these results are again of modest magnitude. The elasticities for the 7- and 30-day hospital readmission rates are 0.042 and 0.028, respectively, but neither result is statistically significant at conventional levels. Looking at the 7-day readmission rates, a 40 percentage point increase in the admission index will increase readmission probabilities by .0007, or 1.65 percent of the sample mean.

In the bottom half of the table, we report results where we include the dummy variables for quintiles of the distribution for the moving average index. Looking at the coefficients individually, the only statistically significant coefficient is a negative coefficient for the dummy indicating less than the 20th percentile of the index in the 30-day readmission rate equation. In most cases, the coefficients are small in magnitude, and in general, there is no monotonic relationship in any of the regressions. For example, one might expect that if positive shocks to admission pose a risk to patients, that negative shocks imply more care from nurses. In most cases, the sign on the decile dummy variables below or above the median category are of the same sign and magnitude.

In Table 2-6, we report results for the three other samples (50 diseases with the highest mortality counts, 50 diseases with the highest mortality rate, and failure to rescue). At the top of the table, we repeat results for the 100 diseases with the highest mortality counts for comparison purposes. In these three samples, we reduce the sample to patients with a higher risk of in-hospital mortality. Although we lose sample size in the process, our hope is to examine data for patients who may be more at risk to lapses in care. In this table, we report results using the *Friday/Saturday Moving Average Admission Index*.[31]

In the middle two blocks of Table 2-6, we find a statistically significant negative coefficient on the *Friday/Saturday Moving Average Admission Index* in the length of stay equation indicating that when admissions surge on Friday and Saturday, patients admitted on Thursdays are more likely to be discharged early. The coefficient is however very small, generating an elasticity of -0.03 in the 50 diseases with the highest mortality rate

[31] One concern with the results in Tables 5 and 6 is that there is too much patient heterogeneity in severity to be accurately controlled for with a vector of ICD-9 dummy variables. We attempted to look at patients with more narrow disease definitions but the samples were too small to be meaningful. For example, we constructed samples of patients with pneumonia or acute myocardial infarctions admitted on Thursdays, and although patients with these diseases had higher than average death rates, these samples had only 74,456 and 39,171 observations, respectively.

models. Focusing on this sample for a moment, a 40 percentage point increase in the Friday/Saturday admissions index would reduce average length of stay by about a tenth of a day, which is about 1.2 percent of sample mean. To put this result into perspective, suppose a hospital that had 50 admissions on a Thursday and based on their conditions, each patient was expected to stay in the hospital for 8 days, which is close to the average in Table 2-6. If 1 of these 50 patients was discharged after only 2 days, the average length of stay for this particular Thursday would fall by 0.12. So although the coefficient is statistically significant, the result is quantitatively small.

In Table 2-6, we find statistically insignificant and quantitatively small impacts of Friday/Saturday admissions shocks on in-hospital mortality, plus 7 and 14 day mortality rates for both the 50 diseases with the highest mortality admissions and the 50 diseases with the highest mortality rate admissions. In these models, the coefficients are small relative to their standard errors and more importantly, even if we assume the true effect of the admission index is at the upper limit of the 95th percent confidence interval, the results suggest quantitatively small effects. The coefficients on the *Friday/Saturday Admission Index* in the readmission models for the first three samples in Table 2-6 are always positive, indicating admission shocks increase readmission rates. The results in the 50 diseases with the highest mortality counts regression model are statistically significant. The largest elasticity is 0.0691 for the 7-day readmission rate in the 50 diseases with the highest mortality model. The coefficient on the index in that model is 0.0033 indicating that a 40 percentage point movement in the index will increase the readmission probability by 0.0013 percentage points or about 2.7 percent of the sample mean. To put this result into perspective, for every 10,000 patients who are admitted on a

Thursday, we would expect 480 to be readmitted and this number would increase by 13 due to a 40 percent above average shock to Friday/Saturday admissions.

In the last block of results in Table 2-6, we present results for the "failure to rescue" sample. None of the estimates is statistically significant except readmission within 30 days. The results on length of stay show a negative sign which is consistent throughout different subsamples. Contrary to some previous samples, the coefficients on the admission index in all three mortality regressions and the 7-day readmission equation are all negative. The most striking result is for the admission index in the 30-day readmission, where the coefficient is a statistically significant 0.0229. A 40 percentage point increase in the index will increase the readmission probability by 0.0091 percentage point or 7.9 percent of the sample mean. This is a fairly large impact but the disparity between the results for the 7-day readmission rate is troubling.

In Table 2-7, we consider alternate ways to construct our analysis sample and our moving average admissions index. In the first block of the table, we reproduce the basic results from Table 2-5 where we use the *Friday/Saturday Moving Average Admissions Index* in the sample patients admitted on Thursday. In the next block of results, we retain the same sample of patients but define the moving average admission index only for Friday admissions. The results are qualitatively similar to those from the block of results. In the third block of results, we construct a moving average admissions index based on Friday, Saturday and Sunday admissions. The results from this model are uniformly larger than those from the first block of Table 2-7, with the coefficients on the index in the length of stay increasing by 29 percent, doubling in the readmissions equations, and increasing by a factor of 15 in the died within 14 days of admission equation. This last result is still not statistically significant, but the big standard errors on this coefficient suggest that the large change in the coefficient could reflect sampling variation. In later results where we expand the sample, we will re-examine this issue in detail. Given the increase in the coefficient on the admission index in the hospital readmission equations, these results are now statistically significant at the 95 percent confidence level. Finally, in the final block of results in this table, we estimate a model where we include in the sample patients admitted on a Friday and use a one-day admission index based on Saturday admissions only. These coefficients are uniformly smaller than the estimates from the first block of the table.

We have reproduced results similar to those in Table 2-6 using the *Friday/Saturday Regression-Based Admission Index*, and these results are reported in Table 2-8. Note that in general, the results from Table 2-8 are very similar to those in Table 2-6. However, because there is less variation in the regression-based admission index, the standard errors in Table 2-8 are larger than those for similar models from Table 2-6. The results in Table 2-8 for the 100 diseases with the highest mortality counts are very similar to those in Table 2-6. The only statistically significant coefficient on the regression-based admission index in this group of estimates is on length of stay. In the sample with the 50 diseases with the highest mortality counts, the coefficient on the regression-based index in the 7-day readmission equation is statistically significant, which is similar to the results for the moving-average index in Table 2-6. Like the results for the 50 diseases with the highest mortality rates in Table 2-6, the numbers in Table 2-8 indicate that one statistically significant impact in the regression-based models, which is the length of stay specification. In the failure to rescue sample, the coefficient on the moving-average index that was statistically significant in Table 2-6 is now much smaller in size and statistically insignificant in Table 2-8.

We have focused on Thursday admissions because lower staff levels on Saturday put the patients at risk to admissions fluctuations and because the admissions index has the largest variance on Friday and Saturday. This selection criterion does however come at a cost: we lose six-sevenths of the sample. In Table 2-5, although the parameter estimates for the admission index in the mortality outcome equations were quantitatively small, the standard errors were large enough that we could not reject the hypothesis that a surge in admissions produced modest impacts on outcomes. To boost the sample size, we generate estimates similar to those in Table 2-5 but instead of just using Thursday admissions, we use admissions from the 100 diseases with the highest mortality counts for all days of the week. This increases the sample by a factor 7 to almost 3.7 million observations. We use the same method of 8-week moving average of admissions over the next two days to construct the index. So for patients admitted on a Monday, we construct the index based on the Tuesday and Wednesday admissions. To control for variation within a week in the composition of patients who enter hospitals, we add dummies for the day of the week. The results from this regression are reported in the first block in Table 2-9 and the estimates are, in general, smaller in magnitude than the estimates in Table 2-5. We find a much smaller impact of admission shocks on length of stay. The coefficient on the admission index in the 7- and 14- day readmission equations are one half and one quarter the size of the estimates in Table 2-5, but the coefficient on the admission index in the 7-day readmission equation is now statistically significant at the 95 percent confidence level. The coefficients on the admission index in the three mortality regressions are now much smaller than the corresponding estimates in Table 2-5. Looking at the died in hospital result, the top end of the 95 percent confidence interval suggests that a 40 percent increase in admission above the mean would increase in-hospital mortality probabilities by .0027 percentage points which is only about .45 percent of the sample mean.

In Table 2-7 above, we reported results where we used our sample of patients admitted on Thursdays and we included as the key covariate an admission index that was based on the next three days' admissions (Friday, Saturday and Sunday). In that sample, we found a very large increase in the coefficient on the admission index in the died within 14 days equation, but the coefficient was still smaller than its standard error. We examine whether this same movement in the coefficient occurs when we re-estimated a similar model in our all-day sample of 3.7 million admissions. Following the model estimated for Table 2-7, we included as the key covariate an admission index based on the next three days. In this new model, the coefficient (standard error) on the three-day admission index in the died with 14 days of admission equation is 0.000099 (0.0007), which is a tiny mean estimate.

In the final block of results in Table 2-9, we consider an even larger sample of admissions in an attempt to reduce the stand errors even further. In these models, we begin with our sample of adult medical and surgical admissions, delete any disease that has less than 100 cases over the five years in our sample, and then delete any remaining disease that has no in-hospital mortality. This leaves us with a sample of 8.67 million admissions, but the mortality rates and 7-day readmission rates in this larger set of admissions are about half the rates found in the other sample in Table 2-9. In these regressions, we again use the two-day moving average admission index as the covariate of interest. In this model, we again find a modest and statistically significant impact of a surge in admission on the 7-day readmission rate. A surge in admissions of 40 percent over the sample mean would increase the readmission probability within 7 days by .028 percentage points which is 1.1 percent of the sample mean. The coefficient of interest in the 30-day readmission equation is statistically insignificant and even if the true estimate were at the top end of the 95 percent confidence interval, a 40 percent increase in admission over the mean rate would increase readmission probabilities less than 1 percent of the sample mean. The coefficients on the admission index in the three mortality regressions are all negative and statistically insignificant at conventional levels. The results suggest that at best, a surge in admission will increase mortality by a small amount. Consider the coefficient for the admission index in the died in hospital equation. Even if the true impact of the surge in admissions were at the top end of the 95 percent confidence interval, a 40 percent increase in admission over mean levels would increase in-hospital mortality rates by .0037 percentage points which is 0.1 percent of the sample mean. Expanding the sample to over 8.6 million observations, we find there is no discernable impact of a surge in admissions on in-hospital mortality, a trivial but statistically precise drop in length of stay, and a statistically significant but small increase in hospital 7-day hospital readmission rates.

2.5 Discussion and conclusion

Although hospitals are subject to large shocks to admissions, we find little evidence that these fluctuations have any quantitative importance on care. In some models, we find statistically significant impacts of the Friday/Saturday admission index on length of stay and readmission probabilities but the impacts are small. We find no impact of a surge in admission on any measure of mortality in any sample. In our largest samples, even if we consider the implied estimates at the top end of the 95 percent confidence interval, a surge in admissions is estimated to change mortality by a very small amount.

There are a number of limitations to this work. First, the data are for only one state in a five year period. Second, and more importantly, we have no independent data about how hospitals deal with a sudden influx of patients. Anecdotal evidence suggests that hospitals have a difficult time dealing with large fluctuations in patient census. This was of course part of the motivation of the California law. However, the lack of any quantitatively large impact of a surge in admissions could mean the hospitals have more degrees of freedom to deal with these situations than is typically revealed in the press on hospital workforce management.

There are two possible interpretations of the results presented above. The first suggests that contrary to the text we highlight in Section 3, hospitals were able to effectively deal with fluctuations in patient census prior to the passage of the California nurse staffing law. A second interpretation is that reductions in the effective nurse/staffing ratio such as those we isolate here have, at best, modest impacts on patient outcomes. We are unable to separate which hypothesis is correct. Either way, our results suggest that the California nurse-staffing law should have minor measurable benefits on the outcomes we've considered here.

Chapter 3: The Impact of Malpractice Risk on the Use of Obstetrics Procedures

3.1 Introduction

When a patient is injured due to the medical malpractice of a physician, the injured party can sue the physician for monetary compensation under negligent tort.[32] Compensation is typically paid for any loss of earnings capacity, pain and suffering, and reasonable medical expenses. Most physicians buy malpractice insurance to insure themselves against the payout of malpractice cases.

In the U.S., malpractice insurance premiums increased rapidly in the 1970's and 1980's and again in the early part of this decade. These recent hikes have led the American Medical Association to declare a 'malpractice premium crisis' in 20 states, including Florida, Pennsylvania and New York.[33] Nationwide, malpractice insurance premiums increased 15 percent between 2000 and 2002.[34] During the same time period, specialists in some areas experienced premium increases of more than 100 percent.[35]

Concerns about rising malpractice premiums have caught the attention of lawmakers at the federal and state level. Beginning in 2003, Congress considered federal tort law reform proposals four times, but each time the bills failed.[36] A fifth tort reform bill (H.R.5) has passed through the House, and is currently awaiting discussion in the

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[32] Torts are civil wrongs recognized by law as grounds for a lawsuit. Among three general categories (intentional torts, strict liability torts, negligent torts) medical malpractice falls into the negligent category.
[33] http://www.ama-assn.org/ama/noindex/category/11871.html
[34] Based on the author's calculation using the Medical Liability Monitor annual premium survey. No weights were used.
[35] Malpractice premiums increased 115 percent for OB/GYNs in Oregon and 110 percent for general surgeons in Mississippi.
[36] Tort law is state law. (http://www.law.cornell.edu/topics/torts.html).

Senate.[37] Most tort reform proposals include such restrictions as limits on non-economic jury damage awards, limits on attorney fees, and statutes of limitations in all medical malpractice lawsuits. Similar reforms have been passed in a number of states (Dewey, 2005).

One premise behind these reform bills is that the current tort system encourages an excessive amount of litigation, placing doctors at an increased risk of being sued for malpractice. Reform sponsors also argue that greater malpractice risk adversely affects the delivery of health care in two ways. First, an increase in risk may discourage doctors from treating people with certain conditions or from conducting risky (but potentially beneficial) procedures. The 2003 American College of Obstetricians and Gynecologists (ACOG) survey reported that 14 percent of respondents stopped practicing obstetrics as a result of the risk of litigation. This type of response to malpractice risk may be leading to some serious restrictions in access to necessary healthcare. Second, healthcare utilization may increase as doctors alter their practice patterns. To avoid a lawsuit or increase the chance of winning a malpractice suit, a doctor may perform procedures that have little or no medical benefit to the patients, but that protect him from possible future litigation. This type of behavior is typically referred to as defensive medicine (Kessler and McClellan, 1996).

Opponents of reform bills believe that limiting compensation for the injured can be unfair to the people who have already suffered significantly from medical malpractice. Reform critics are also often concerned that the deterrent effects of tort law would be

[37] H.R.5 passed the House in July 2005. The bill includes a $250,000 cap on non-economic damages.

weakened as a result.[38] It is thus ultimately an empirical question whether doctors respond to changes in malpractice risk by altering their practice patterns.

Given the potential importance of tort reform and the 'malpractice crisis', there is surprisingly little empirical evidence regarding the impact of malpractice risk on health care delivery. Kessler and McClellan (1996) found some evidence of defensive medicine in their analysis of heart attack patients covered by Medicare. But they focus primarily on health care expenditures and thus do not shed light on the mechanisms through which this effect operates. While informative, most other studies have important limitations including insufficient variation due to a short time span, possible omitted variable bias, or an inappropriate measure of malpractice risk.

First of all, we need to know whether doctors have an incentive to change their behavior as a result of malpractice risk. Most physicians are fully insured against the financial costs of malpractice such as damages and legal defense expenses.[39] In addition, medical malpractice insurance typically does not have a mechanism such as deductibles, or experience rating, either of which would give the physician a direct financial incentive to respond to increase in risk.[40] Litigation may, however, reduce current earnings due to the loss of practice time, or future earnings due to the loss of reputation. Additionally, there may be substantial legal expenses and emotional stress associated with a lawsuit.[41] Therefore, doctors may have a strong incentive to alter practice patterns even with malpractice insurance.

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[38] The two purposes of tort law are monetary compensation for the injured and the deterrence of similar negligence by physicians in the future.

[39] Of all OB/GYNs surveyed, 94.4 percent reported being covered by medical malpractice liability insurance (2003 ACOG survey). The most common insurance policies limit coverage to $1 million per case or $3 million per year. Only 1.6 percent of claims in my data paid more than $1 million.

[40] Most insurance premiums are experience rated. For example, someone with an auto accident history will pay a higher auto insurance premium compared to a similar person without an accident.

[41] Sometimes doctors hire their own attorneys.

One practice area that is thought to be particularly affected by high malpractice risk is OB/GYN.[42] Between 1994 and 2003, twenty four percent of OB/GYN doctors in the state of Massachusetts either made settlement payments themselves or had payments made from their insurance carriers. This is substantially higher than most other specialties. For example, 15 percent of general surgeons and only 4 percent of internal medicine specialists had such payments (The Board of Registration in Medicine, Massachusetts, 2004). The ACOG found that 76.3 percent of OB/GYNs experienced at least one lawsuit during their career in a 2003 survey.

In OB/GYN, it is often suggested that malpractice concerns encourage doctors to perform more cesarean sections (c-sections) than medically needed. For example, the birth of a neurologically-impaired infant, the most prevalent reason for a lawsuit in the OB/GYN specialty, can occur before or during deliveries (ACOG survey, 2003). However, in deliveries involving a birth injury, doctors are more likely to be suspected as negligent when the baby is delivered vaginally due to the limited control of progress compared to cesarean section (Sachs, 1989). Therefore, some have suggested that plaintiffs can more easily argue doctors' negligence, such as the lack of a timely response for more invasive procedures, when a baby is delivered vaginally. Others have argued that the rapid increase of the U.S. c-section rate in the 1980's relative to England (which showed a similar c-section rate to the U.S. in the 1970's) is attributable to the difference in legal environments. There is also more than a 10 percentage point difference in c-

[42] Based on Medical Liability Monitor, an OB/GYN practicing in New York City in 2003 paid 5.3 times more for malpractice insurance than an internist, and 1.6 times more than a general surgeon.

section rates between some states, with some of this potentially explained by differences in legal environments.[43]

In my empirical analysis, I measure the malpractice risk that doctors face using the National Practitioner Data Bank. This data set is a national universe of malpractice claims resolved either by settlement or jury verdicts for the 15 year period from 1990 to 2005. This long time period allows me to exploit the considerable variation both between states and within a state over time in malpractice risk at the extensive (number of cases) and intensive (awards per case) margins. I calculate malpractice risk in two ways: as either the number of OB/GYN claims per 1,000 births in each state over the last three years, or the amount of OB/GYN claims paid per birth in $1,000s in each state over the last three years.[44]

The malpractice risk data are then combined with the Natality detail data set which is a census of all live births in the U.S. Detailed information in the data is used to construct a series of treatment measures that have been suspected to be influenced by the malpractice risk OB/GYN doctors face. The large sample size allows me to examine subsamples that may be particularly susceptible to changes in practice style. For example, a doctor's response to higher malpractice risk might vary by patient characteristics such as history of a previous c-section, complications of labor (breech presentation, gestational diabetes, multiple births), or socioeconomic background of the mother. I therefore examine whether malpractice risk alters procedure choice overall and also for these particular at-risk subgroups.

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[43] In 2003, the c-section rate in Florida was 30.8 percent compared to 19.2 percent in Utah based on National Vital Statistics Report, Vol. 54, No. 2, 2005.
[44] I use three years for the following two reasons. The average litigation process takes three to four years and malpractice risk is a noisy measure because the incidence rate is low.

One challenge for reliable identification is that malpractice risk as defined above may be correlated with other factors related to the treatment decision, such as unobserved patient characteristics, physician quality or practice style. For example, if doctors respond to malpractice risk by performing more c-sections, this may decrease the malpractice risk because a c-section lowers the probability of a lawsuit compared to vaginal delivery (reverse causality). In other words, the probability of a lawsuit is not only a function of the legal environment but also a function of procedure choices and other factors as well. To address this issue, I use an instrumental variables (IV) identification strategy that will capture only the malpractice risk generated by a state's legal environment. In particular, I use the malpractice risk in non-OB/GYN specialties as an instrument for the OB/GYN risk measure.

My findings demonstrate that cesarean section rates are not responsive to medical malpractice risk. Additionally, utilization of health care, measured by the number of prenatal visits during pregnancy, is also insignificantly related to malpractice risk. I also find that malpractice risk has no statistical or qualitatively important impact on the use of other procedures such as ultrasound, fetal monitoring, forceps, or vacuum. The one exception is amniocentesis, a diagnostic procedure that is used substantially more as malpractice risk increases. Taken together, the findings suggest that malpractice risk does not have a significant effect on the behavior of obstetricians.

The chapter is arranged as follows. Section 2 reviews the previous literature on malpractice risk and its impact on physician behavior. Section 3 describes the data used, the empirical analysis and my identification strategy. Section 4 reports the empirical results and Section 5 discusses the implications of my findings.

3.2 Literature Review

Although there has been considerable discussion about the impact of defensive medicine on medical care costs, there is relatively little evidence that malpractice risk alters medical decisions. There are two types of studies that have attempted to measure the behavioral changes induced by malpractice risk. The first type uses surveys of providers while the second type examines the reduced-form relationship between healthcare expenditures and outcomes and the changes in the malpractice environment.

A number of different surveys have tried to assess how physicians responded to tort litigation. For example, the Office of Technology Assessment (OTA) conducted a survey of three specialties including OB/GYN. The survey described a hypothetical scenario and asked doctors which diagnostic procedures they would prescribe. Doctors were also asked to choose the major reason for the procedure choices with one possible response being malpractice risk. They found that, by their definition, 8 percent of diagnostic procedures preformed are medically unnecessary.

Kessler and McClellan (1998) combined survey data from the American Medical Association Socioeconomic Monitoring System (AMA SMS) with tort reform data and found that doctors who faced higher malpractice risk increased both record keeping and the number of diagnostic tests preformed. However, it is well known that surveys are potentially subject to response bias (Grant and McInnes, 2004). This problem may be particularly acute in direct physician surveys, because physicians may be tempted to exaggerate the impact of malpractice pressure in order to buttress the political argument in favor of liability reform (Klingman *et al*., 1996). Indeed, physicians estimated the probability of defending against a malpractice claim in any one year to be about three times higher than the actual probability of such a claim arising (Lawthers *et al*., 1992; Weiler *et al*., 1993).

Using state tort reforms that cap an injured patient's award as an exogenous change in malpractice risk, Kessler and McClellan (1996) showed that total expenditure declined for Medicare patients with acute myocardial infarction and ischemic heart disease in states that experienced tort reform. However, these states did not experience any statistically significant change in outcomes such as mortality. The authors were unable to tell which procedures were 'defensive' in nature (i.e., whether fewer diagnostic tests were prescribed or less aggressive treatment lowered costs).

Using data from the 1990-1992 periods, Dubay *et al*. (1999) analyzed a within-group model correlating changes in c-section rates and malpractice premiums. They found that higher OB/GYN malpractice premiums had a statistically insignificant impact on the rate of cesarean delivery among all births. In contrast, they found that among unmarried and less than high school graduate mothers, a group suspected to have a higher rate of being a malpractice plaintiff, malpractice premiums had a statistically significant positive impact on the c-section rates. While informative, there are two potential limitations of this study. First, given the short sample period, there is some question as to whether there was sufficient within-panel variation in premiums to successfully identify their model. Second, it is not clear whether higher premiums indicate an elevated malpractice risk for the doctor. While premiums do depend both on claim frequency and claim severity, they also depend on other market factors, such as interest rates and market competitiveness, and hence may measure the malpractice environment poorly (Grant and McInnes, 2004). Indeed, Black *et al*. (2005) did not find a strong correlation between paid claims and malpractice insurance premiums. A report by the Americans for Insurance Reform (2002) pointed out that medical insurance premiums are closely related to insurance market competition but not paid claims.[45]

Baicker and Chandra (2004) utilized state-level data on premiums and closed claims to examine the impact of malpractice risk on healthcare delivery. They examined average malpractice risk from 1992 to 1994 and compared it with the average malpractice risk from 1999 to 2002. They found no statistically significant positive relationship between c-section rates and OB/GYN claims. Their estimates are also possibly subject to an omitted variable bias problem that I mentioned earlier.

Using hospital discharge data and closed claims data from Florida over the 1992-1995 period, Grant and McInnes (2004) estimated the changes in doctor-specific c-section rates after physicians experienced malpractice litigation. They found that after litigation, physicians had a one-percentage point higher risk-adjusted c-section rate. If the malpractice risk that a doctor faces depends on not only his/her experience of being sued, but also on the malpractice claim history for the same specialty in their region, Grant and McInnes' estimates are a lower bound estimate of the true impact of malpractice risk. Taken together, the previous literature provides conflicting evidence regarding the impact of increased malpractice risk on physician behavior.

[45] http://www.insurance-reform.org/StableLosses.pdf

3.3 Empirical Methodology

3.3.1 Data

There are two major sources of data for this study: the National Practitioner Data Bank Public Use Data File and the Natality Detail File. The National Practitioner Data Bank (NPDB) is an extensive collection of data on malpractice payouts, including pre-trial settlements throughout the nation. If a malpractice insurance carrier pays on behalf of a practitioner, the carrier is required by the Healthcare Quality Improvement Act of 1986 to report data about the claim to the NPDB within 30 days of the payout. This public-use data file is updated at the end of each quarter.[46] For this project, I use the NPDB Public Use Data File containing reports received from September 1, 1990 through March 31, 2005. I measure malpractice risk from 1992 to 1998 after dropping cases which took more than 6 years to resolve from injury occurrence, for consistency throughout the data period, because some claims take several years to resolve.

The NPDB Public Use Data File records the year of injury and the year of report and has information about the size of the payment, related services (obstetric-related, medication-related, etc), practitioner's work state and practitioner's field of license (physician, pharmacist, dentist, etc.) for each case.[47] To maintain confidentiality of the data, payment amounts are recorded into ranges only.[48] Payments are also top coded at $105 million, but no payments exceeded this amount during my data period.

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[46] http://www.npdb-hipdb.com/publicdata.html

[47] For each claim, there are six potential dates of interest: date of injury, date of opening a legal case, date of reporting to insurance company by doctors, date of a case closing (by jury verdict or settlement), payment date, and date of report (when the NPDB received the record). Only year of injury and year of report are available in the data.

[48] For example, $10,000 increments are used for actual payments between $100,001 and $1 million, Payments between $1 million and $10 million are coded as the midpoint of $100,000 increments. Between $10 million and $20 million, a $1 million increment is used.

Even though the NPDB is the most extensive data set about closed malpractice cases, it has several limitations. First, it only lists closed cases with a positive payout. In the introduction, I argued that there may be both psychic and economic costs to defending against a malpractice claim. As a result, not counting cases with zero payout may be understating the malpractice risk faced by physicians. Second, tort cases are decided locally (e.g., juries are selected at the county level) so there may be variation within a state in malpractice risk. However, the NPDB does not identify sub-state geographic information, so I cannot measure any within-state variation in malpractice risk. Third, the NPDB cannot link multiple defendants for a single case together when they are reported separately. Fourth, hospitals are not included as providers. Therefore, hospitals that are the sole defendants in a case are not included in the data set. Likewise, closed cases in the NPDB that included both hospitals and physicians as defendants only list the physician defendants. Despite these limitations, it is the most accurate source of information for the entire U.S. over a long period regarding physician malpractice risk.

The Natality Detail File is a census of all live births in the U.S. and includes almost 24 million births for the 7 years (1992-1998) in which I have measures of malpractice risk. These data have demographic information about the mother (age, education, marital status, race, and ethnicity), the father (age, race, and ethnicity), characteristics of the pregnancy (parity, plurality, gestation, maternal weight gain, smoking and drinking during pregnancy, prenatal visits, breech presentation, high blood pressure and gestational diabetes), and method of delivery. The data also include information about who attended the delivery, such as a midwife or a medical doctor.[49]

3.3.2 Measuring Malpractice Risk

I measure malpractice risk using closed claims information in the NPDB Public Use Data File. Theoretical models of the tort liability system typically assume that agents respond to both the probability and the size of liability awards. Subsequently, I construct two measures of malpractice risk: one that measures the number of cases (frequency) and another that measures the size of liability payments (severity) per birth.

As I mentioned above, there is tremendous heterogeneity across medical specialties in the lifetime risk of being sued for malpractice. This is not surprising. OB/GYNs care for different types of patients and perform a very different service than dermatologists or psychiatrists. As a result, each specialty should have different underlying levels of malpractice risk. For this reason, I measure malpractice risk within each specialty.

The malpractice risk faced by doctors is also assumed to vary by state and year, based on several factors. Each state has a different tort environment (tort law and precedent by jury, etc.). Practice patterns also vary substantially for different regions (Nicholson and Epstein, 2003). For the most part, insurance companies also set malpractice premiums according to a physicians' specialty, type of practice, and

[49] In this analysis, I only use births delivered by medical doctors since midwives do not have the same procedure choices, such as cesarean section delivery, nor do they face the same malpractice risk. Less than 8 percent of births were delivered by midwives in the seven years worth of data I use.

geographical location (Quinn 1998).[50] For example, OB/GYNs practicing in New York have very different malpractice risk from OB/GYNs practicing in Wyoming because of different legal environments as well as different practice patterns.

The NPDB identifies both the year of injury and the year of report so the malpractice risk can be measured using one of these years as the frame of reference. The key question to address is this: if doctors are altering their practice style based on malpractice risk, are they altering their behavior after alleged malpractice occurs (date of injury) or when a malpractice suit is paid out and then reported to the NPDB (date of report)? Research on this question by Grant and McInnes (2004) suggests that behavior changes are associated with the incident that led to the malpractice claim, not with the closure of the claim. Subsequently, I look for evidence that OB/GYN's practice defensive medicine after an injury occurs. Unfortunately, injury claims only make it into the NPDB once a case has been closed, which many times can be years after the injury. Therefore, I must define a consistent window after which an injury occurs when cases will be reported in the NPDB.

Figure 3-1 reports the distribution of years when the case is reported to the NPDB for injuries that occurred in 1993 for all medical malpractice cases.[51] The mean year of report is 1997.3, the median is 1997 (the fourth year after the injury), and the mode is 1996 (the third year after the injury). Note, however, that a small fraction of cases are being reported ten to twelve years after a patient is injured. I find very similar results in

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[50] Type of practice means a hospital- or office-based practice. Insurance companies define their own geographical categories. Only nine big states such as New York, and California, have geographic variation in prices within a state. For example, depending on the insurance carrier, there are three to six geographical regions within California in 2002. The rest of the states tend to have one premium for each specialty. I use state as the geographical level since only state is observed in NPDB.

[51] OB/GYN cases have the same shape of distribution with bigger mean year of 1998, the fifth year after the injury.

Figure 3-2 in which I graph the distribution of total dollars paid (in real 2002 dollars) for reported cases resulting from injuries in 1993. Most cases are settled within a few years of the actual injury. In Figure 3-3, I report the cumulative distribution of closed claims for injuries occurring in 1993. Roughly 80 percent of cases are reported within six years of injury.

Although I have Natality data through the early 2000s and the NPDB data are reported through March of 2005, the long lag between injury and the claim report observed above means that I cannot use the latest years of data. If the distribution of paid claims in 2002 is similar to the distribution in 1993, then only about 15 percent of 2002 claims have been reported by March of 2005.[52] In order to have a consistent measure of malpractice risk across all years in the sample, I will use the same window of years after injury for cases to be reported. This will understate the total closed claims from earlier years in the sample, but all years will be treated equally.

The choice of the length of the window that I will use requires tradeoffs. Using a longer window will generate more accurate measurement of risk but will reduce the available years of data that I can use from the Natality detail data. For example, a two-year window would allow me to use data through 2002 and a four year window would allow me to use data through 2000.[53] Unfortunately, as the numbers in Figure 3-3 illustrate, the shorter the window, the fewer actual reported claims will be included in the malpractice risk index.

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[52] The distribution of lags between injury year and report year is indeed very stable throughout my data period.

[53] Although I have data reported by March 2005 I assume that I have data until 2004 as a complete year.

I use a window of 6 years after the injury to construct the malpractice risk measure.[54] Cases being reported within the same year that an injury happens are rare.[55] Therefore, I decided to drop cases reported in the same year when the injury occurred, basing this decision on the same logic I used to drop cases reported after 6 years from the injury.[56]

With the text above as a backdrop, we can define malpractice risk in the following manner. The variable C_{sjt} denotes the number of reported cases where the injury occurred in state *s* in year *t*, with the gap between injury and reporting measured in years by *j*. The numbers of births in thousands in state *s* and year *t* are written as B_{st}. These variables can be used to construct the malpractice risk for OB/GYNs in state *s* in year *t* R_{st}^{OB}.

$$(1)\ R_{st}^{OB} = (\sum_{j=1}^{6} C_{sjt}) / B_{st}$$

The number of malpractice injuries in a given year should be proportional to the size of the exposure, so I divide the number of OB/GYN malpractice cases by the number of births in thousands. For another measure of malpractice risk I use the amounts of OB/GYN claims paid after adjusting for inflation using the urban consumer price index for the year of the report in thousands of dollars and then dividing it by the number of births.[57]

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[54] I cover 81 percent of injuries in terms of frequency based on figure 5 with this window. Figure 6 shows slightly lower coverage which is 76 percent in terms of severity by using a 6 year window.
[55] Only around one percent of cases based on the number of claims or 0.3 percent of cases based on the amount of payout are closed within the same year from the year injury happened.
[56] Results are robust even if I use a 5-year window instead of a 6-year window.
[57] The paid year, not reported year, should be used. But paid year is not recorded in the data. Considering the rule that all paid claims should be reported within 30 days after payout reported year is a good proxy.

It is likely that doctors will consider not only this year's risk but also risk in recent years. To account for this, I will use a three-year moving average to measure the level of risk that doctors face.[58] The choice of a three year moving average is subject to discussion. Considering that the average litigation process takes three to four years, it is reasonable to assume that there is persistence in the malpractice risk from year to year. An additional benefit is that it will give a less noisy measure because the incidence rate is quite low. I denote the moving average of risk in OB/GYN as MAR_{st}^{OB}:

$$(2)\ MAR_{st}^{OB} = (\sum_{k=0}^{2} R_{st-k}^{OB})/3$$

To construct the malpractice risk for OB/GYN doctors in 1993, I use obstetric-related cases in which an injury happened in 1993, and the case was reported by 1999. Then the frequency of these included cases is divided by the number of births (in thousands) in 1993. The severity of these cases is measured in thousands of dollars divided by the number of births in 1993. Measured risk for OB/GYN doctors as in equation (1) in 1993, 1992 and 1991 was averaged to get the final measure of risk in equation (2) that OB/GYN doctors face in 1993. I have constructed malpractice risk in this way from 1992 to 1998. I cannot include data for 1999 because the six-year window for injuries occurring in this year is past the date of my last observation. The incorporation of the three-year moving average also forces me to drop the first two years of observations for which I have outcome data (injuries that happened in 1990 or 1991).

Table 3-1 presents descriptive statistics for the malpractice risk that OB/GYN doctors face. The first column contains the malpractice risk OB/GYNs face for the full sample, "all births", over 1992 - 1998. In the second column I present malpractice risk

[58] Results are robust to not using moving average for the risk measure.

for non-OB/GYN specialties which will be used as an instrumental variable. The amount of OB/GYN claims paid per birth in $1,000s and the amount of non-OB/GYN per population in $1,000s are presented in 2002 dollars. OB/GYNs face an average probability of a successful malpractice suit of 0.19 percent for every 1,000 deliveries. Based on the severity measure presented in the bottom of Table 3-1, OB/GYN doctors face an average risk of $73 in payout for each delivery. The numbers in the second column indicate that all of the non-OB/GYN specialties face a lower level of malpractice risk. They face 0.04 percent probability of a positive payout malpractice suit by frequency risk measure for every 1,000 procedures and 8 in 2002 dollars payout for each procedure.

3.3.3 Definition of Outcomes

The extensive data available in the Natality Detail File provide me with a number of outcomes that measure the practice patterns of physicians. The most frequent outcome analyzed is the method of delivery: vaginal or cesarean. There are different costs and benefits of each procedure. Women who delivered a baby by cesarean section can have a higher risk of hemorrhage, blood clots, and bowel obstruction as well as infection because it is a major abdominal surgery. They also have to be hospitalized longer and are more likely to be re-hospitalized subsequently. Women with a vaginal delivery are more likely to experience minor issues like urinary incontinence. However, a baby that is born vaginally is more likely to have a nerve injury (Maternity Center Association, 2004).

The following series of arguments suggest that cesarean section rates are employed as defensive medicine. Most obstetrical malpractice litigation is triggered by injuries to babies such as brain damage.[59] In both animal experimentation and epidemiological studies, it has been shown that total asphyxia in full-term infants leads to brain damage and in many cases to perinatal death (Sachs, 1989).[60] There is a greater chance of asphyxia when the baby is delivered vaginally. When an injury occurs and the baby is delivered vaginally, the plaintiff has a greater ability to allege a failure to perform a timely cesarean section or misinterpretation of the fetal heart rate tracing, or both, resulting in death or brain damage (of the baby) (Sachs, 1989).

There is also some suggestive evidence that c-section rates in the U.S. are responsive to malpractice risk. Cesarean section rates in the US (6 percent) were similar to that of Europe (England 5 percent, Hungary 6 percent) in the early 1970s. The U.S. rate increased to 20 percent between 1981 and 1983 while rates in England increased to only 10 percent (Watson *et al*., 1987). Some have suggested that one possible explanation for the divergence in c-section rates between the two countries is the difference in legal environments. The U.S. legal system, often described as litigious, could be driving the difference in c-section growth rates compared with other countries.[61]

The Natality Detail File includes information on prenatal care, including the number of doctor's office visits during the pregnancy and the use of diagnostic procedures such as ultrasound and amniocentesis. The data also indicate if equipment or technology such as fetal monitoring, forceps, or vacuum extraction were used during labor. Ultrasound is a commonly-used diagnostic procedure that allows the provider to

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[59] The primary allegation of obstetric claims is a neurologically impaired infant (34.3%) and still birth or neonatal death (15.3%) based on the 2003 ACOG survey.
[60] Asphyxia means a condition in which an extreme decrease in the concentration of oxygen in the body accompanied by an increase in the concentration of carbon dioxide leads to loss of consciousness or death (*The American Heritage Dictionary of the English Language, Fourth Edition)*. Perinatal means the five months before and one month after birth.
[61] Litigious America, Newsweek International, July 30, 2001

observe the development of a fetus. Amniocentesis is a procedure performed during the early stages of pregnancy to detect genetic or chromosomal disorders using sample fluid from the mother's womb. Fetal monitoring is typically performed during delivery to check the baby's heart rate. Steel forceps or soft cup vacuum extractors can be used to assist vaginal delivery when it does not progress spontaneously or when the baby must be delivered immediately due to either fetal distress or maternal fatigue.

Prenatal care visits are recorded as integer counts and values range from 0 to 49. All other outcomes are recorded as dummy variables where usage of the procedure, test, or device is given a value of 1.

3.3.4 Subsamples

I use a census of births in the U.S. from 1992-1998. This is referred to as the "All Births" sample in the Tables in the text. Some patients are more likely to be more affected by a physician's change in practice style stemming from an increase in malpractice risk. I divide the data into six different subsamples that might be more or less susceptible to practice pattern changes based on the mother's history of previous delivery, complications during birth, or socio-economic status.

The Centers for Disease Control and Prevention (CDC), and King and Lahiri (1994) claim that a c-section may represent defensive medicine for some patients and that this would be especially true for patients with a previous c-section.[62] Within the medical profession, there is substantial disagreement on the costs and benefits of vaginal

[62] The Centers for Disease Control and Prevention have targeted that by the year 2010 the U.S. cesarean delivery rate for women giving birth for the first time should decrease to 15 percent from the 1998 baseline rate of 18 percent (U.S. Public health service, 1991). They specifically wanted to increase vaginal birth after cesarean rates to 37 percent from 28 percent (the 1998 rate) by the year 2010.

deliveries after c-sections (VBACs) and as a result, a consensus guideline for treatment has not yet been reached. The old concept could be summarized by the phrase, "Once cesarean forever cesarean." The supporting idea was that a woman who has a scar in her uterus as a result of a previous cesarean section might have a higher probability of experiencing a rupture in a future labor. Repeated cesarean would then reduce the chance of separation of the uterus. However, c-section carries its own risks, such as blood clots, bowel obstruction, or infection, since it is a major abdominal surgery.

Due to the lack of consensus in the medical profession about the desirability of VBACs, and partly due to movements by such groups as the CDC and ACOG to lower the repeated cesarean section rate, VBAC rates have fluctuated significantly over time. Almost 19 percent of patients who had a previous c-section delivered their baby vaginally in 1989, but this rate increased sharply to 28.3 percent by 1996, and then declined rapidly to 20.6 percent by 2000. A high risk of trying vaginal labor for patients with a previous c-section, and a lack of consensus in the medical profession, may lead obstetricians to respond to the malpractice risk that they face when they practice for this subgroup of patients. This group is referred to as the "Previous cesarean section" sample in the tables and in the text.[63]

There are specific high-risk medical conditions such as breech presentation for pregnancy. Breech presentation means that a baby is in buttocks or feet-first position instead of a head first position. For some breech presentations, vaginal delivery can pose serious health risks for both the mother and the baby (Sachs, 1989). Other conditions that produce more frequent use of c-sections include gestational diabetes, multiple births and

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[63] Several studies found that Patients who tried VBAC but failed in the end have higher risks of uterine disruption and infectious morbidity compared with repeat cesarean delivery (Hibbard et al., 2001, Landon et al., 2004, Scott et al., 1998, Mozurkewich and Hutton, 2000).

high blood pressure. These potentially high-risk patients could lead doctors to choose more defensive procedure choices. People also have expressed concern that care for these high-risk pregnancies could be impacted by heightened professional liability risk.

Low socio-economic status patients are another subgroup of women that researchers believe are differentially affected by physicians' practice changes (Dubay *et al*., 1999). Some state governments have experienced reduced obstetrician participation in Medicaid.[64] One possible reason is the common notion that low-income patients are more litigious with physicians even though the data do not confirm this.[65] These low socio-economic status patients also have lower incomes and a higher probabilities of adverse outcomes which might lead to litigation. They have limited access to health care including prenatal care due to medical insurance status or time constraints. Therefore, obstetricians might choose more defensive procedures or perform more diagnostic tests when they care for this subgroup. In this chapter, I classify the low socio-economic status group as those with less than or equal to a high school degree.[66]

In Table 3-2, I present descriptive statistics for a variety of outcomes on the full set of patients in the first column. The "previous cesarean section" sample is presented in the second column. In the third column, I present the subgroup that has not had a previous c-section at the time of delivery, which is the residual population from the second column. I present the subsamples of patients with complications in columns 4, 5 and 6: breech presentation, gestational diabetes and multiple births. In the far right

[64] For example, providers participating in Medicaid maternity care declined by 4.3 percent in 1986 compared to the year before in Washington state.

[65] General Accounting Office (GAO), U.S. Congress. 1987. Medical Malpractice: Characteristics of Claims Closed in 1984. GAO/HRD-87-55

[66] I also tried the socio-economic classification which was used by Dubay *et al*.: unmarried, less than high school degree or unmarried, high school. Results are robust to different classifications of socio-economic status.

column I present births to mothers with a high school degree or less as the low socio-economic subsample.

Except for prenatal visits, the statistic presented is the percentage of patients receiving the given procedure. Given the large sample size, standard deviations for these discrete outcomes are approximately equal to $[\overline{X}(1-\overline{X})]^{0.5}$ where $\overline{X}$ is the sample mean. The primary outcome in the analysis is the choice between c-section and vaginal delivery, and this outcome is given in the first row. C-section rates vary considerably across subsamples as expected. Only sixteen percent of pregnancies are delivered by c-section among the patients who had not had a previous c-section. On the other hand, eighty-six percent of pregnancies are delivered by c-section in the case of breech presentation. The number of prenatal visits could measure the mother's access to health care. Interestingly, the number of prenatal visits does not vary much across subsamples. The use of diagnostic tests (ultrasound and amniocentesis) during pregnancy, and the use of equipment or technology like fetal monitoring, forceps and vacuum extraction during labor are my other outcome variables reported in rows three through seven. Use of ultrasound and amniocentesis are significantly higher for women with complications. Patients with less than or equal to a high school degree have the lowest rates of use of diagnostic tests among all subsamples. Note that since forceps and vacuum are assisting tools for vaginal deliveries, I include only vaginal deliveries in the sample for these rows.

3.3.5 Econometric Model

The basic question I examine is whether a higher malpractice risk faced by OB/GYNs alters procedure choices. As discussed above, I measure risk (MAR) in two ways: (1) the number of OB/GYN claims per 1,000 births over the last three years in a state, and (2) the amount of OB/GYN claims paid per birth in $1,000s over the last three years in a state. The basic econometric model is a within-group specification, in which I examine within-state changes in the use of procedures over time as malpractice risk changes. This model can be described by the following specification:

$$(3) \quad Y_{ist} = X_{ist}\beta + \lambda MAR_{st}^{OB} + \theta_t + \alpha_s + \nu_{ist}$$

where Y_{ist} measures the procedure choice (e.g., the binary variable equals 1 if the baby was delivered by c-section and equals 0 for vaginal deliveries) for patient *i* in state *s* in year *t*; X_{ist} denotes the mother's observable characteristics which include age, race and education; θ_t is a year fixed effect; α_s is a state fixed effect; and ν is an idiosyncratic error. In all models, I calculate variances allowing for arbitrary form of heteroskedasticity and allowing for arbitrary correlation in errors within a state.

The use of a within-group specification is critical for a variety of reasons. Much of the variation in malpractice risk is due to permanent differences across states. If I regress the state-level malpractice risk on state and year fixed effects, I obtain R-squared values of 0.73 and 0.68 for the frequency risk measure and the severity risk measure, respectively. This is not surprising. Each state has a different tort law system, leading some states to have a more favorable legal climate for plaintiffs than others.

We could capture some of these permanent differences by including a series of descriptive variables that characterize a state's tort law system. However, these variables would imperfectly measure these differences, especially in the case of medical malpractice, since tort law is based on both common and statutory law.

More importantly, I am concerned that the same factors that lead to a different legal environment may also reveal something about the medical environment. For example, suppose a state has lower quality medical services with a higher than average frequency of true medical malpractice. In this case, the medical tort system within a state may evolve reflecting the higher rate of malpractice. The state legal environment may alter the burden of proof for plaintiffs, or payments conditional on judgment may differ as well. To address these unobservable differences between states, I use a within-state model that uses variation over time in malpractice risk and procedure choices to identify the model. Therefore, any difference in practice style or malpractice risk that is permanent across states will be purged from the analysis by adding state fixed effects.

Even if I use a within-group model, malpractice risk may signal something unobserved about physician quality. Suppose that low-ability doctors are more prone to use c-sections so as to minimize the possibility of an emergency situation such as fetal distress or dystocia, common complications of vaginal deliveries. At the same time, low ability doctors are also more likely to be sued, since the quality of their service provided is substandard. If this is the case, then there is a positive correlation between the malpractice risk and error term in (3), biasing upward the coefficient λ.

Omitted variables bias might also be generated by reverse causality. As I mentioned above, deliveries by c-sections are less likely to be sued than vaginal deliveries. If doctors shed risk by performing more c-sections, this may decrease the malpractice risk as I've constructed it. If this is the case, then there is a negative correlation between the malpractice risk and error term in (3), biasing downwards the coefficient λ.

To reduce the possibility of omitted variable bias, I will use an instrumental variable (IV) procedure. A 2SLS model will produce a consistent estimate of λ if I can identify an instrumental variable that alters the OB/GYN malpractice risk but does not directly impact c-section rates. I will use a measure of malpractice risk for all other medical specialties except OB/GYN as an instrument.

As I demonstrate below, it is easy to establish the first criteria, namely, that within-state malpractice risk is correlated across specialties. This instrument captures medical malpractice risk that is specific to each state and year observation that is not based on practice style. Potential plaintiffs consider a number of factors when deciding whether or not they should seek a legal remedy for their injuries. One factor they consider is the legal climate within the state. The state tort laws or recent jury verdicts may encourage or discourage patients from seeking remedies regardless of medical specialty. Subsequently, in any given year, the malpractice risk for OB/GYNs and non-OB/GYNs in a state may be correlated since both risks are governed by the same legal climate.

The other criterion to be a valid instrument is that the instrument must not directly impact the outcome of interest. In other words, OB/GYNs should depend only on their own specialty's risk, not other specialties' risks for their procedure choices. For example, when the number of malpractice claims in cardiology increases due to medical services provided to their patients OB/GYN doctors will not change their procedure choice (e.g., c-section and vaginal delivery) based on increased malpractice risk in cardiology. Each specialty has its own underlying malpractice risk which does not depend on other specialties partly because each specialty, especially OB/GYN, provides unique medical services. Separate malpractice insurance premia by specialty reflects these factors. Therefore, changes in malpractice risk for non-OB/GYNs will not change the procedure choice of OB/GYNs unless it is subsumed in the legal environment.[67]

In summary, the legal environment may be reflected in both OB/GYN and non-OB/GYN malpractice risk. However, it is unlikely that OB/GYNs are responding to the higher risk levels in non-OB/GYN specialties. The higher risk that doctors face in all other specialties except OB/GYN should be subsumed into the OB/GYN risk through the legal environment. Therefore, my assumption that malpractice risk in non-OB/GYN affects OB/GYN procedure choices only through the legal climate seems reasonable.

I measure non-OB/GYN risk exactly the same way as OB/GYN risk with only one exception. I use the number of births as a denominator to calculate risk per case for OB/GYNs, but for non-OB/GYN cases, I use the resident population of the state in the relevant year as the denominator.

[67] The worst possible scenario for my instrument is that doctors' abilities in OB/GYN and non-OB/GYN in a state move together over time. However, my instrument will be valid unless doctors in all specialties have the same preference in geography (e.g., one region experience lower ability doctors throughout all specialties) and it looks like doctors are more likely to change preference on specialty based on future income over time since the opening for each specialty is very limited (Bhattacharya, 2005; Hurley, 1991).

3.4 Results

3.4.1 The Impacts of Malpractice Risk on the Use of Cesarean Section

In Table 3-3, I report OLS and two-stage-least-square (2SLS) estimates of equation (3) for one outcome variable, cesarean section, for the all births sample. I only report the estimated coefficient on the malpractice variable as that is the coefficient of primary interest. In the first panel, I present OLS estimates which are potentially biased for the reasons mentioned above. I use two measures of risk: the number of OB/GYN claims per 1,000 births and the amount of OB/GYN claims paid per birth in thousands of dollars. Standard errors are reported in parentheses. The negative sign of the coefficients for malpractice risk suggest that fewer cesarean sections are performed when risk increases, which is contrary to the conventional wisdom. However, none of the coefficients are estimated precisely. In addition, the elasticity for the number of OB/GYN claims per 1,000 births reported in square bracket is -0.0062, which is very small. The elasticity for the amount of OB/GYN claims paid per birth in \$1,000s is even smaller.

The second panel of Table 3-3 presents the first stage results for the 2SLS estimates. The coefficient estimates for both non-OB/GYN risk measures are positive and statistically significant at the 1 percent level, meaning that the instrument and OB/GYN malpractice risk are correlated. In the third panel, I report the 2SLS estimates for the malpractice risk measures.[68] The coefficient of risk measured as the number of

[68] Due to the large number of observations I cannot run the regression using each birth as the unit of observation. I collapsed the data into cell' based on the covariates which are all discrete (such as age, race, marital status, education, state and year) and use the cell size as a frequency. Unfortunately, there is no STATA procedure that allows calculating clustered standard errors in a 2SLS model with fixed-effects and using frequency weights. Therefore, I run the first stage, get the predicted value and use this predicted value in the second regression. In this case I will not get the correct standard error because the actual

OB/GYN claims per 1,000 births decreases greatly compared to the OLS estimate. However, not surprisingly, the standard errors increase substantially. As a result, the 2SLS estimates are statistically insignificant. For risk measured as the amount of OB/GYN claims paid per birth in $1,000s, the 2SLS estimate is much larger than the OLS estimate. It is positive, which supports the hypothesis that c-sections increase with higher malpractice risk. However, this estimate is also imprecise. Based on the p value of the Hausman test for exogeneity reported at the bottom of Table 3-3, I cannot reject the null that the OLS estimates are statistically equal to the 2SLS estimates. While the magnitude of the estimates changed substantially, the increased standard error makes it difficult to reject the null hypothesis.

The elasticity calculated from the 2SLS results using the number of OB/GYN claims per 1,000 births or the amount of OB/GYN claims paid per birth in $1,000s are both still very small. If the amount of OB/GYN claims paid per birth in $1,000s increased from the 25th percentile to the 75th percentile (which is a 54 dollar increase in risk per delivery) the rate of cesarean sections would increase by just 0.1 percentage points, which is 0.6 percent of the mean. In other words, it is the case that for every 10,000 babies delivered, 2,278 babies are delivered by c-section. A 129 percent increase in risk will increase the number of babies delivered by c-sections by 14. Even if the true impact of the malpractice risk were at the top end of the 95 percent estimated confidence interval, an increase in the amount of OB/GYN claims paid per birth in $1,000s from the 25th percentile to the 75th percentile would increase the c-section rate by only one

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malpractice risk is required to calculate the correct 2SLS standard errors. I am able to compare estimates from this procedure with estimates from a 2SLS model for breech presentation since it has only 940,378 observations. I do not need to use frequency weight and STATA provides for 2SLS without using frequency weight. The standard errors differ from each other in the fifth digit after the decimal point. So I am comfortable presenting standard error estimates based on this alternative procedure.

percentage point. Therefore, although the estimates are statistically insignificant, we can reject the hypothesis that the impact of malpractice risk on c-section choice is substantial.

In Table 3-4, I expand my analysis of cesarean sections into the six different subsamples discussed earlier. In the first column, I repeat the all births sample that I presented in Table 3-3 as a reference. The rest of the columns are classified into three categories: (1) based on pregnancy history of having had a previous c-section (candidates for VBAC in the 2nd column and the rest of sample in the 3rd column), (2) complications of pregnancy (breech presentation, gestational diabetes, and multiple births), and (3) low education (less than or equal to high school graduate) as socio-economic status.

In the first panel, I report OLS estimates. Standard errors that allow for heteroskedasticity and arbitrary covariance in errors within a state are reported in parentheses and the elasticity is reported in square brackets. The coefficients of interest are all negative except for the no previous c-section group in column 3 for both frequency (number of OB/GYN claims / 1,000 births) and severity (amount of OB/GYN claims paid / birth in $1,000s) but statistically insignificant. The first-stage estimates for 2SLS in the second panel are statistically significant at the 1 percent level throughout all of the subsamples. The 2SLS estimates reported in the third panel differed substantially from the OLS estimates. Some of the 2SLS estimates such as all births using amount of OB/GYN claims paid/birth in $1,000s even changed sign from the corresponding OLS estimates but the direction of movement from OLS is not consistent throughout the subsamples. However, all of the coefficients are estimated imprecisely and I cannot reject the null based on the p values of the Hausman test for exogeneity reported in the last panel.

Consider the magnitude of the estimate using the amount of OB/GYN claims paid per birth in \$1,000s for less than or equal to high school degree. If the malpractice risk were to increase from the 25th percentile to the 75th percentile the cesarean section rate would increase by just 0.2 percentage points, which is 0.8 percent of the mean. In other words, babies delivered by c-sections would increase by only 16 from 2,180 for every 10,000 babies born to mothers with less than or equal to a high school degree as a result of a 129 percent increase in risk.

3.4.2 The Impacts of Malpractice Risk on Other Outcomes

In Table 3-5, I report OLS estimates for various outcomes using the number of OB/GYN claims per 1,000 births as the risk measure. The number of prenatal visits as an outcome is presented in the second row. Higher malpractice risk could limit patients' access to healthcare if some doctors decided to stop practicing OB/GYN as malpractice risks increased. On the other hand, doctors might want to see patients more often to decrease the possibility of litigation. The estimated effect here is positive, which suggests that pregnant women see doctors more often when doctors face higher malpractice risk. For pregnant women who have gestational diabetes, higher malpractice risk produces a statistically significant increase in prenatal visits. In the next two rows, I report results for the ultrasound and amniocentesis outcomes. Some doctors were sued due to failure to detect certain genetic problems in advance which can give parents broader choice.[69] Therefore, doctors might want to perform diagnostic tests to detect any genetic problems more aggressively with increased malpractice risk. Indeed, these estimates suggest that doctors are more likely to use these diagnostic tests with a higher

[69] http://www.njatty.com/articles/medmal/cfsm03b.html

malpractice risk. For mothers who had a c-section previously and for mothers having multiple births, the coefficient on malpractice risk in the amniocentesis equation is estimated with statistical precision. The coefficients on malpractice risk for both subsamples are 0.05 and 0.08, respectively. I find a positive sign in all subgroups for use of vacuum except for the multiple births subgroup. No statistically significant change however is detected.

I next report 2SLS estimates in Table 3-6 for each subsample, using the number of OB/GYN claims per 1,000 births. The p-value from the exogeneity test is in curly brackets. For the use of amniocentesis, the 2SLS estimates are six times larger than the corresponding OLS estimates and these estimates indicate that amniocentesis rates increase as doctors face higher malpractice risk. I reject the null of exogeneity meaning that the 2SLS estimates are significantly different from their OLS counterparts. Looking at the multiple births sample, the elasticity of amniocentesis for this group is 1.3, which is very large. Increasing the malpractice risk from the 25th percentile to the 75th percentile would increase the rate of amniocentesis by 4.1 percentage points, which is 64.7 percent of the mean. For every 10,000 multiple birth babies, 626 have an amniocentesis test and 405 more babies would have amniocentesis when the risk increases 85 percent. For the rest of the subgroups, the impact of malpractice risk on the use of amniocentesis is somewhat smaller. There is one more statistically significant estimate for the gestational diabetes subsample. Use of vacuum extraction will increase 1.3 percentage points when malpractice risk increases from the 25th percentile to the 75th percentile.

Table 3-7 presents OLS results for risk measured as the amount of OB/GYN claims paid per birth in $1,000s. Use of amniocentesis is positive throughout all subgroups and statistically significant for the previous cesarean section, gestational diabetes, multiple births and less than or equal to high school degree subgroups. For the previous cesarean section subgroup, malpractice risk has a statistically significant positive impact on the use of amniocentesis, forceps and vacuum extraction.

In Table 3-8, I report 2SLS results using the amount of OB/GYN claims paid/birth in $1,000s as the risk measure. When the number of prenatal visits is the dependent variable, the coefficient is negative, which suggests there might be some problem with access to health care that changes the sign from OLS estimates. However, all of the estimates are statistically insignificant with relatively small elasticities.

Malpractice risk increases the use of amniocentesis by a statistically significantly amount in all subsamples except breech presentation. For the previous cesarean section subsample, if the malpractice risk increases from the 25th percentile to the 75th percentile the rates of amniocentesis increase by 3 percentage points, which is 54 percent of the mean. For every 10,000 multiple births babies, 536 babies have the amniocentesis test as a baseline. If malpractice risk were to increase by 77 percent, 290 more babies would have the amniocentesis test. For the same subsample, forceps and vacuum extraction usage also would increase by a statistically significant amount, but the value is smaller in magnitude with elasticities of 0.6 and 0.4, respectively.

3.4.3 Other Measures of Malpractice Risk

There are some other ways to measure malpractice risk. One is to use a different source of closed claims data that has some advantages over the NPDB. The state of Texas collects data on closed malpractice cases within their jurisdictions and this data is publicly available to researchers. Texas is the 2nd largest state measured by population and the 3rd largest in total health care spending (Black *et al*., 2005). The advantages of the Texas data are as follows. The Texas data have county level information as well as the month and year of injury, filing of the suit and payment. The Texas data also have a unique identifier that allows me to combine multiple defendant cases into a single case. It also includes the payment of cases on behalf of hospitals.

I have analyzed the Texas closed claims data collected from 1988 to the present using a within-county model. In this analysis, I found that malpractice risk has no statistically significant impact on procedure choices. I also find an insignificant first stage at the 5 percent level using an instrumental variable constructed in the same way as in this chapter. It might be because the judicial area does not completely match with the county or, as we see in the malpractice insurance market, it has a different market area from the county border. In any case, the model does not have a sufficiently large sample size to be successfully identified.

The other possible source of a malpractice risk measure is to use malpractice premiums, which have been used in several previous studies. The Medical Liability Monitor surveys malpractice insurance premiums annually at the state level or, in some cases, at the sub-state level.[70] With these data, I used the log of the average annual premium for OB/GYNs from 1994 to 2002 as a measure of the malpractice risk that OB/GYNs face. I then used the log of premiums for general surgeons as an instrumental variable. The estimates from the first stage of the 2SLS estimates were significant at the 1 percent level, but the 2SLS estimates were insignificant for most outcomes, including

[70] Respondents report the base premium for coverage providing $1 million per claim and $3 million in aggregate for a year, which is considered standard coverage. Survey respondents report premiums for three specialties (internal medicine, general surgery and OB/GYN) and company-specific premiums vary by state-specific sub-markets.

c-section rates. As other research has found, my results show very little correlation between premiums and either frequency or severity of malpractice claims.

3.4.4 Marginal Patient Sample

When OB/GYNs choose the delivery method between a c-section and a vaginal delivery, they consider various medical and physical conditions. Therefore, not all women are equally likely to have a c-section. Some conditions, such as breech presentation, increase the chance of a c-section greatly. When the patient's medical condition makes the choice obvious, malpractice risk is less likely to affect a doctor’s behavior. However, for some patients where the method of delivery is not as certain based on observed characteristics, malpractice risk could be a larger factor in the doctor’s decision. For example, a patient with certain conditions might be treated using a c-section by some doctors and using vaginal delivery by others.

In this section, I develop a model that attempts to telescope in on those patients who are most likely to be affected by malpractice risk. I will call this sample “the marginal patient sample” because it excludes patients with the lowest and highest probabilities of having a c-section. To find the marginal patient sample, I regressed medical and physical information on a dummy variable that equals one if a cesarean section was preformed, with state and year fixed effects. The model fits quite well – the R^2 for this regression is 0.17. Next I rank patients by their predicted probability of having a cesarean section by descending order. Patients who fall between the 12.5th percentile and the 37.5th percentile of the descending order of predicted c-section probability are the marginal patient sample considering the roughly 23 percent c-section delivery rate for all births in my data. The marginal patient sample has 5.8 million observations.

Table 3-9 displays descriptive statistics on the two measures of risk for this marginal patient sample. It is very similar to that of previous subgroups reported in Table 3-1. In Table 3-10, the impact of malpractice risk on cesarean section for the marginal patient sample is reported. In the first panel I present estimates from OLS specifications. For both measures of risk I find negative and insignificant estimates with very small elasticities. The first stage of the 2SLS regression is significant as reported in the second panel. 2SLS estimates reported in the third panel are still negative and insignificant. Even if the true impact of the malpractice risk were at the top end of the 95 percent estimated confidence interval, an increase in the number of OB/GYN claims per 1,000 births from the 25th percentile to the 75th percentile would increase c-section rates by only 0.6 percentage points. When I use the amount of OB/GYN claims paid per birth in $1,000s the malpractice risk at the top end of the 95 percent estimated confidence interval would increase c-section rates by 1.6 percentage points, which is small. In the last panel, I report the p-value from an exogeneity test and I cannot reject the null that OLS estimates are equal to the 2SLS estimates.

3.5 Conclusion

During the past year, President Bush has argued for 'common-sense' medical liability reform to protect patients, to stop sky-rocketing costs associated with frivolous lawsuits, to make health care more affordable and accessible for all Americans, and to keep necessary services in communities that need them most.[71] A frequent justification

[71] http://www.whitehouse.gov/news/releases/2005/01/20050105-2.html

for tort reform is the concern that malpractice risk may encourage doctors to alter their practice style. To date, there is little evidence supporting this point. In this chapter, I examined whether a higher risk of malpractice awards alters procedure choice in obstetrics. I focused on the obstetric specialty because it is exposed to one of the highest malpractice risks in medicine and it is often considered to be a specialty in which defensive medicine is particularly prevalent.

In a sample spanning 7 years and containing more than 24 million observations, I find that doctors' procedure choice is insensitive to the risk that they face. I also find that increased malpractice risk has little if any impact on health care access, as measured by the number of prenatal doctor's office visits. I also find no statistically significant change in other measures of treatment such as ultrasound, forceps, and vacuum as malpractice risk changes. Even though I find some significant increase in the use of amniocentesis when malpractice risk increases, overall I do not find substantial changes in behavior by obstetricians as malpractice risk increases.

There are some limitations of this chapter in terms of data. One is that the NPDB data is not complete because it does not cover payouts on behalf of hospitals (Smarr, 1997). It also did not include cases that ended without any positive payment. However, the NPDB data is the most extensive existing data set and the results are not different even if I use a state of Texas data set, which has some advantages such as including claims against hospitals.

Another limitation is that there is more than one possible explanation for my findings. For example, there may be no significant principal agent concerns that lead to defensive medicine because doctors only care about the patients' outcomes. The other possibility is that malpractice risk is still too small for doctors to change procedure choices. In addition, the measure of risk that I construct is still only a proxy for malpractice risk even though it is the best one given the available data. Therefore, it might not capture perfectly the risk as it is perceived by physicians. Unfortunately, this chapter cannot distinguish between these explanations and thus further research is needed.

In the 1970s and 1980s, many states enacted tort reform in order to control malpractice insurance premiums. However, the issue of malpractice premiums has recently returned as an object of public concern. One of the most important reasons for further reform at the federal level is the potential adverse impact of increasing malpractice premiums on health care delivery through changes in doctors' behavior. Based on my findings, it appears that federal level tort reform will have at most a minimal impact on the way doctors practice medicine.

Chapter 4: Concluding Remark

Legislation can be an effective way to overcome market failure and the health care market has been subject to frequent regulatory changes at the federal, state and local level. In the two cases I've considered in this dissertation, I examine the evidentiary backing for two important pieces of health care quality legislation. The first was a California state law requiring minimum nurse/patient staffing levels in hospitals. The second is federal tort reform. In both cases, there was limited evidence justifying legislative intervention. Using the techniques used by economists, I evaluate these claims using large representative samples of data and in both cases, I find the evidence lacking.

Some researchers have documented a correlation between nurse to patient ratio and patient outcomes. As I argue above, however, there is reason to believe these results represent correlation and not a causal relationship. In this dissertation, I attempt to add to the literature by estimating the impact of hospital staff levels on adverse events by examining whether outcomes are correlated with the number of admissions in the hospital over the next two days. The variation I exploit to identify the models is exactly the type of variation the California law is designed to eliminate. Specifically, the law requires minimum staff levels at all points in time. I find quantitatively small and statistically insignificant effects of Friday and Saturday admission shocks on mortality rates of patients admitted on Thursdays. These results suggest that the portion of the California law designed to guarantee adequate staffing when the patient census increases unexpectedly should have little impact on patient outcomes. However, there are some important limitations to this study. For example, the California nurse staffing law will increase the size of hospital staffs. This change can potentially raise overall care quality by providing patients with a more attentive staff. It can however decrease quality if the new nurses hired because of the legislation are of lower quality that existing employees.

Another re-occurring legislative issue in the health care market is tort reform. Policy makes argue that high malpractice risks encourage doctors to practicing defensive medicine. However, the evidence establishing this claim is, at best, limited. In the third chapter of my dissertation I examine whether malpractice risk alters the procedure choices of obstetricians. I focus on obstetricians because they face one of the highest rates of malpractice lawsuits and pay much larger malpractice premiums than most other specialties. The high rate of c-sections in this country is also frequently cited as an example of defensive medicine. Because the measured malpractice risk may signal something unobserved about physician quality or practice style, I use malpractice claims against non-OB/GYNs as an instrument for OB/GYN claims. Finally, my sample has over 23 million observations. Although I focus on a specialty with a high malpractice risk, examining a procedure that is thought to be sensitive to malpractice risk, and using the largest sample ever to examine this question, I find there is little evidence that malpractice risk alters procedure choice of doctors.

As with Chapter 3, there are some limitations to this work. My instrument was constructed from the same data that I construct malpractice risk for OB/GYN claims. If there is measurement error in the endogenous variable, the instrumental variable will face exactly the same problem. One limitation of the work is that I cannot explain why the cesarean section rate is not sensitive to medical malpractice risk. I have a few conjectures. If doctors are only concern about their patients then there will not be any defensive medicine. If malpractice risk that doctors perceive is different from my measure of malpractice risk then I will not find any changes of procedure choice based on my measure of malpractice risk. For example, doctors are not only concern their own state's precedents but neighbor state's precedents. Malpractice risk is a perceived risk which can't be measured perfectly in any way. What I tried in this dissertation is to measure malpractice risk as objectively as possible and my objective measure is one contribution to the literature.

Table 2-1. Hospital Workers and Admitted Patients by the Day of the Week

	Percent of people that reported working on a particular day from the CPS Work Schedule Supplements [a]				Percent of admissions to California hospitals by the day of the week [b]	
	Registered nurses in hospitals		Nurse aides or orderlies in hospitals			
	1997	2001	1997	2001	1997	2000
Sunday	41.9	14.8	42.9	8.9	9.2	9.3
Monday	81.2	85.1	81.7	89.9	17.4	17.2
Tuesday	82.1	82.8	82.4	93.7	17.1	17.0
Wednesday	79.8	83.1	82.5	93.7	16.4	16.3
Thursday	76.9	82.9	78.0	92.5	15.6	15.6
Friday	71.0	80.5	73.4	91.2	14.9	14.8
Saturday	38.4	15.5	40.3	14.9	9.5	9.7

[a] The May 1997 and 2001 CPS Work Schedule Supplements asked the following questions. In 1997: Which days of the week do you work? Check all that apply. In 2001: Which days of the week do you usually work? Check all that apply.

[b] Based on a adult medical and surgical admissions of the California hospital discharge data(1996-2000) that we use below.

Table 2-2. Descriptive Statistics, Daily Hospital Admission Index Based on Eight-week Moving Average

	Mean	Standard deviation	10th percentile	25th percentile	75th percentile	90th percentile	90/10 ratio
Sunday	1.0082	0.2768	0.6990	0.8571	1.1428	1.3114	1.8761
Monday	1.0073	0.2779	0.7046	0.8587	1.1386	1.3114	1.8611
Tuesday	1.0079	0.2880	0.6871	0.8520	1.1428	1.3251	1.9285
Wednesday	1.0079	0.2952	0.6797	0.8458	1.1497	1.3333	1.9616
Thursday	1.0087	0.3234	0.6575	0.8315	1.1626	1.3675	2.0798
Friday	1.0070	0.3834	0.6050	0.8000	1.1818	1.4222	2.3507
Saturday	1.0099	0.3207	0.6518	0.8358	1.1636	1.3584	2.0840

Based on adult medical and surgical admissions of the California hospital discharge data (1996-2000) that we use below.

Table 2-3. Descriptive Statistics, California Hospital Discharge Data, 1996-2000

		Patients Admitted on Thursdays			
	Adult medical and surgical admissions	100 diseases with the highest mortality counts	50 diseases with the highest mortality counts	50 diseases with the highest mortality rates[a]	Failure to rescue sample
Outcomes					
Mean length of stay	4.91 (6.41)	5.77 (7.00)	5.79 (6.97)	8.18 (9.46)	11.40 (14.20)
% died in hospital	2.96	5.77	6.51	15.32	16.95
% died within 7 days of admission	2.12	4.12	4.74	10.63	8.08
% died within 14 days of admission	3.22	6.22	7.07	15.56	12.86
% readmitted w/in 7 days[b]	3.01	4.36	4.82	4.80	1.29
% readmitted w/in 30 days[b]	9.92	13.29	13.89	16.35	11.61
Characteristics					
Mean age	62.21 (18.58)	68.41 (15.54)	68.84 (15.48)	66.90 (16.77)	57.22 (13.64)
% female	53.68	49.25	49.1	47.16	46.25
% Black	9.08	8.65	8.53	9.27	13.09
% Hispanic	15.28	13.50	13.33	15.04	18.31
% White	67.55	69.57	69.65	65.99	59.79
% Medicaid	14.01	11.39	11.67	14.27	22.02
% Medicare	49.50	61.20	61.61	59.34	44.31
% Self-pay	2.55	1.54	1.58	1.89	2.71
% Private insurance	31.09	24.42	23.77	23.11	28.61
Hospitals	491	399	399	395	397
Observations	9,912,889	566,058	414,049	120,976	48,223

We present standard deviations for continuous variables and these numbers are in parenthesis. Give our large sample sizes, standard deviations for discrete outcomes are approximately equal to $[\bar{x}(1-\bar{x})]^{0.5}$ where $\bar{x}$ is the sample mean.

[a] These diseases are selected only from the 100 diseases with the highest mortality counts.

[b] Patients who died during their hospitalization were dropped from this sample.

Table 2-4. Descriptive Statistics, Moving Average and Regression-Based Friday/Saturday Admission Index, 100 Diseases with the Highest Mortality Counts Sample

	Friday/Saturday Moving Average Admission Index	Friday/Saturday Regression-Based Admission Index
1%	0.4897	0.6256
5%	0.6808	0.7515
25%	0.8787	0.9042
50%	1.0000	1.0046
75%	1.1267	1.1072
95%	1.3658	1.2745
99%	1.6315	1.4300
Mean	1.0090	1.0082
<20^{th} percentile index	0.8486	0.8799
20^{th} - 40^{th} percentile index	0.9545	0.9662
60^{th} - 80^{th} percentile index	1.0463	1.0418
>80^{th} percentile index	1.1627	1.1338
Observations	566,058	335,419

Table 2-5. Impact of Friday/Saturday Hospital Admissions on Outcomes of Patients Admitted on Thursdays, 100 Diseases with the Highest Mortality Counts Sample, Using the Friday/Saturday Moving Average Admission Index

Independent variable	Length of stay	Died in hospital	Died within 7 days of admission	Died within 14 days of admission	Re-admitted within 7 days[a]	Re-admitted within 30 days[a]
			Model (1) Parameter estimate (standard error) [elasticity]			
Friday/Saturday Moving Average Admission Index	-0.0828 (0.0404) [-0.0144]	0.0002 (0.0013) [0.0034]	-0.0004 (0.0011) [-0.0098]	0.0001 (0.0014) [0.0016]	0.0018 (0.0012) [0.0416]	0.0037 (0.0021) [0.0280]
R^2	0.1405	0.0914	0.0708	0.0892	0.0503	0.0261
			Model (2) Parameter estimate (standard error)			
<20th percentile index	0.0033 (0.0282)	0.0011 (0.0009)	0.0011 (0.0008)	0.0013 (0.0010)	-0.0016 (0.0008)	-0.0035 (0.0015)
20th - 40th percentile index	-0.0384 (0.0275)	0.0012 (0.0009)	0.0012 (0.0008)	0.0009 (0.0009)	-0.0014 (0.0008)	-0.0012 (0.0014)
60th - 80th percentile index	-0.0257 (0.0275)	0.0004 (0.0009)	0.0000 (0.0008)	-0.0001 (0.0009)	-0.0013 (0.0008)	-0.0003 (0.0014)
>80th percentile index	-0.0388 (0.0282)	0.0016 (0.0009)	0.0013 (0.0008)	0.0016 (0.0010)	-0.0002 (0.0008)	-0.0010 (0.0015)
R^2	0.1405	0.0884	0.0708	0.0893	0.0503	0.0261
Mean of outcome	5.77	0.0578	0.0412	0.0622	0.0436	0.1329

Standard errors in parentheses.
[a] Patients who died during their hospitalization were dropped from this sample.
The independent variables include a complete set of hospital fixed-effects, month effects that vary by hospital size and region, a complete set of four-digit ICD-9CM effects and age effects, plus effects for sex, race and ethnicity (white, black, Hispanic and other), Federal holidays, insurance type (Medicare, Medicaid, private and self pay).

Table 2-6. Impact of Friday/Saturday Hospital Admissions on Outcomes of Patients Admitted on Thursday, Various Samples, Using the Friday/Saturday Moving Average Admission Index

Independent variable	Length of stay	Died in hospital	Died within 7 days of admission	Died within 14 days of admission	Re-admitted within 7 days[a]	Re-admitted within 30 days[a]
100 diseases with the highest mortality counts sample, 566,058 observations, parameter estimate (standard error) [elasticity]						
Friday/Saturday Moving Average Admission Index	-0.0828 (0.0404) [-0.0144]	0.0002 (0.0013) [0.0034]	-0.0004 (0.0011) [-0.0098]	0.0001 (0.0014) [0.0016]	0.0018 (0.0012) [0.0416]	0.0037 (0.0021) [0.0280]
Mean of outcome	5.77	0.0578	0.0412	0.0622	0.0436	0.1329
50 diseases with the highest mortality counts sample, 414,049 observations, parameter estimate (standard error) [elasticity]						
Friday/Saturday Moving Average Admission Index	-0.1126 (0.0467) [-0.0196]	-0.0000 (0.0017) [-0.0003]	-0.0008 (0.0014) [-0.0170]	-0.0004 (0.0017) [-0.0057]	0.0033 (0.0015) [0.0691]	0.0050 (0.0025) [0.0363]
Mean of outcome	5.79	0.0651	0.0474	0.0707	0.0482	0.1389
50 diseases with the highest mortality rate sample, 120,976 observations, parameter estimate (standard error) [elasticity]						
Friday/Saturday Moving Average Admission Index	-0.2611 (0.1167) [-0.0321]	0.0008 (0.0046) [0.0052]	-0.0008 (0.0039) [-0.0075]	0.0022 (0.0046) [0.0142]	0.0027 (0.0029) [0.0566]	0.0066 (0.0052) [0.0406]
Mean of outcome	8.18	0.1532	0.1063	0.1556	0.0480	0.1635
Failure to rescue sample, 48,223 observations, parameter estimate (standard error) [elasticity]						
Friday/Saturday Moving Average Admission Index	-0.1330 (0.2978) [-0.0117]	-0.0077 (0.0078) [-0.0457]	-0.0034 (0.0059) [-0.0423]	-0.0053 (0.0071) [-0.4148]	-0.0006 (0.0028) [-0.0125]	0.0229 (0.0080) [0.1985]
Mean of outcome	11.40	0.1695	0.0808	0.1286	0.0129	0.1161

Standard errors in parentheses.
[a] Patients who died during their hospitalization were dropped from this sample.
See footnotes to Table 2-5 for a list of other covariates in the regression.

Table 2-7. Various Moving Average Admission Indexes and Different Sample Admission Day of the Week

Independent variable	Length of stay	Died in hospital	Died within 7 days of admission	Died within 14 days of admission	Re-admitted within 7 days[a]	Re-admitted within 30 days[a]
Thursday admissions sample, using Friday/Saturday Moving Average Admission Index, 566,058 observations, parameter estimate (standard error) [elasticity]						
Moving Average Admission Index	-0.0828	0.0002	-0.0004	0.0001	0.0018	0.0037
	(0.0404)	(0.0013)	(0.0011)	(0.0014)	(0.0012)	(0.0021)
	[-0.0144]	[0.0034]	[-0.0098]	[0.0016]	[0.0416]	[0.0280]
Mean of outcome	5.77	0.0578	0.0412	0.0622	0.0436	0.1329
Thursday admissions sample, using Friday Moving Average Admission Index, 565,896 observations, parameter estimate (standard error) [elasticity]						
Moving Average Admission Index	-0.0521	0.0004	-0.0002	-0.0003	0.0012	0.0027
	(0.0318)	(0.0010)	(0.0009)	(0.0011)	(0.0010)	(0.0016)
	[-0.0091]	[0.0070]	[-0.0049]	[-0.0048]	[0.0278]	[0.0205]
Mean of outcome	5.78	0.0577	0.0412	0.0622	0.0436	0.1328
Thursday admissions sample, using Friday/Saturday/Sunday Moving Average Admission Index, 565,082 observations, parameter estimate (standard error) [elasticity]						
Moving Average Admission Index	-0.1071	0.0007	0.0005	0.0015	0.0034	0.0072
	(0.0471)	(0.0016)	(0.0013)	(0.0016)	(0.0014)	(0.0025)
	[-0.0186]	[0.0122]	[0.0122]	[0.0243]	[0.0785]	[0.0546]
Mean of outcome	5.78	0.0577	0.0412	0.0622	0.0436	0.1328
Friday admissions model, using Saturday Moving Average Admission Index, 548,579 Observations, Parameter estimate (standard error) [elasticity]						
Moving Average Admission Index	0.0259	-0.0007	-0.0007	-0.0014	0.0007	0.0003
	(0.0255)	(0.0009)	(0.0007)	(0.0009)	(0.0008)	(0.0013)
	[0.0044]	[-0.0116]	[-0.0164]	[-0.0218]	[0.0162]	[0.0022]
Mean of outcome	5.89	0.0608	0.0432	0.0649	0.0436	0.1334

Standard errors in parentheses.
[a] Patients who died during their hospitalization were dropped from this sample.
See footnotes to Table 2-5 for a list of other covariates in the regression.

Table 2-8. Impact of Friday/Saturday Hospital Admissions on Outcomes of Patients Admitted on Thursday, Various Samples, Using the *Friday/Saturday Regression-Based Weekend Admission Index*

Independent Variable	Length of stay	Died in Hospital	Died within 7 days of admission	Died within 14 days of admission	Re-admitted within 7 days*	Re-admitted within 30 days*
	100 diseases with the highest mortality counts sample, 335,419 observations, Parameter estimate (standard error) [elasticity]					
Friday/Saturday Regression-based Admission Index	-0.1414 (0.0686) [-0.0247]	0.0005 (0.0023) [0.0094]	0.0007 (0.0019) [0.0188]	0.0009 (0.0024) [0.0159]	0.0025 (0.0019) [0.0785]	0.0009 (0.0035) [0.0076]
Mean of outcome	5.77	0.0535	0.0375	0.0570	0.0321	0.1189
	50 diseases with the highest mortality counts sample, 241,841 observations, Parameter estimate (standard error) [elasticity]					
Friday/Saturday Regression-based Admission Index	-0.1285 (0.0796) [-0.0224]	0.0003 (0.0029) [0.0049]	0.0006 (0.0025) [0.0139]	0.0014 (0.0030) [0.0216]	0.0060 (0.0023) [0.1738]	0.0040 (0.0043) [0.0327]
Mean of outcome	5.78	0.0605	0.0434	0.0652	0.0348	0.1232
	50 diseases with the highest mortality rate sample, 67,940 observations, Parameter estimate (standard error) [elasticity]					
Friday/Saturday Regression-based Admission Index	-0.4958 (0.2118) [-0.0606]	0.0024 (0.0082) [0.0165]	0.0046 (0.0070) [0.0462]	0.0078 (0.0082) [0.0529]	0.0061 (0.0047) [0.1760]	-0.0001 (0.0092) [-0.0006]
Mean of outcome	8.23	0.1465	0.1003	0.1483	0.0349	0.1498
	Failure to rescue sample, 28,561 observations, Parameter estimate (standard error) [elasticity]					
Friday/Saturday Regression-based Admission Index	0.0731 (0.5050) [0.0061]	-0.0165 (0.0134) [-0.1006]	-0.0040 (0.0099) [-0.0549]	-0.0211 (0.0120) [-0.1754]	-0.0025 (0.0047) [-0.2131]	0.0101 (0.0138) [0.0873]
Mean of outcome	12.00	0.1649	0.0732	0.1210	0.0118	0.1163

Standard errors in parentheses.
[a] Patients who died during their hospitalization were dropped from this sample.
See footnotes to Table 2-5 for a list of other covariates in the regression.

Table 2-9. Impact of the Next Two Days Hospital Admissions on Outcomes of Patients, Various Samples, Using the *Two-Day Moving Average Admission Index*

Independent Variable	Length of stay	Died in Hospital	Died within 7 days of admission	Died within 14 days of admission	Re-admitted within 7 days [a]	Re-admitted within 30 days [a]
100 diseases with the highest mortality counts sample, all days, using the next two days Moving Average Admission Index, 3,697,293 observations, parameter estimate (standard error) [elasticity]						
Two-Day	-0.0421	-0.0003	0.0000	-0.0001	0.0009	0.0009
Moving Average	(0.0166)	(0.0005)	(0.0005)	(0.0006)	(0.0004)	(0.0007)
Admission Index	[-0.0073]	[-0.0050]	[0.0015]	[-0.0015]	[0.0217]	[0.0071]
Mean of outcome	5.74	0.0604	0.0444	0.0661	0.0416	0.1275
All admissions excluding rare diseases and diseases with no in-hospital mortality[b] using the next two days Moving Average Admission Index, 8,674,446 observations, parameter estimate (standard error) [elasticity]						
Two-Day	-0.0269	-0.0003	$-2*10^{-5}$	-0.0001	0.0007	0.0006
Moving Average	(0.0096)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0004)
Admission Index	[-0.0055]	[-0.0089]	[-0.0008]	[-0.0027]	[0.0279]	[0.0074]
Mean of outcome	4.91	0.0337	0.0241	0.0366	0.0252	0.0815

Standard errors in parenthesis.

[a] Patients who died during their hospitalization were dropped from this sample.

[b] Diseases with less than 100 admissions and diseases with no mortality at all over the five years were dropped.

See footnotes to Table 2-5 for a list of other covariates in the regression. In these models, we also include dummy variables for the day of the week the patient was admitted.

Table 3-1. Descriptive statistics of measured risks in OB/GYN and non-OB/GYN

	Number of OB/GYN Claims/1000births	Number of non-OB/GYN Claims/1000population
1%	0.0620	0.0105
5%	0.0936	0.0181
25%	0.1357	0.0291
50%	0.1762	0.0394
75%	0.2292	0.0439
95%	0.3136	0.0586
99%	0.4318	0.0645
Mean	0.1874	0.0382

	Amount of OB/GYN Claims Paid/Birth	Amount of non-OB/GYN Claims Paid/Population
	unit: $1,000 in 2002 dollars	
1%	0.0151	0.0030
5%	0.0272	0.0039
25%	0.0408	0.0049
50%	0.0635	0.0072
75%	0.0943	0.0104
95%	0.1591	0.0152
99%	0.2120	0.0186
Mean	0.0733	0.0082

Table 3-2. Descriptive Statistics of Outcome Variables, Various Samples

		Subsamples					
		History		Complications			S.E.S
	All births	Previous cesarean section	No previous C-sec.	Breech	Gest. Diabetes	Multiple Birth	≤ HS Degree
C-section delivery	22.78	75.59	16.09	85.87	37.27	56.44	21.80
Prenatal visits	11.45 (1.28)	11.59 (1.35)	11.43 (1.29)	11.55 (1.76)	13.05 (1.77)	12.95 (2.54)	10.87 (1.22)
At least one ultrasound	62.43	67.38	61.80	72.80	70.97	70.63	60.47
Had amnio-centesis	3.22	5.36	2.94	5.38	9.18	6.26	1.90
Had fetal monitoring	81.64	78.00	82.10	81.50	87.08	81.65	80.71
Used Forceps*	4.87	5.78	4.83	11.16	5.98	5.86	4.21
Used Vacuum*	7.75	9.36	7.69	9.02	8.57	9.26	6.93
N	23,639,438	2,657,393	20,982,045	940,378	622,569	644,402	13,108,946

I present standard deviations for continuous variables and these numbers are in parenthesis. All outcomes except prenatal visits are presented by percentage. Prenatal visits recorded the number of visits to doctor's office from 0 to 49.

Give our large sample sizes, standard deviations for discrete outcomes are approximately equal to $[\bar{x}(1-\bar{x})]^{0.5}$ where $\bar{x}$ is the sample mean.

* Cesarean section population is dropped since these procedures are applied only for vaginal delivery.

Table 3-3. Impact of Malpractice Risk on Cesarean Section, All Births Sample

Risk Measure	All Births
OLS Estimates Dependent variable: Cesarean Section	
Number of OB/GYN Claims /1000births	-0.0076 (0.0181) [-0.0062]
Amount of OB/GYN Claims Paid /Birth in $1,000	-0.0101 (0.0221) [-0.0032]
First Stage Estimates	
Number of non-OB/GYN Claims /1000population	3.97 (0.92)
Amount of non-OB/GYN Claims Paid/Population in $1,000	5.96 (1.76)
2SLS Estimates Dependent variable: Cesarean Section	
Number of OB/GYN Claims /1000births	-0.0412 (0.0727) [-0.0338]
Amount of OB/GYN Claims Paid /Birth in $1,000	0.0262 (0.1108) [0.0084]
Exogeneity Test (P value)	
Number of OB/GYN Claims /1000births	0.609
Amount of OB/GYN Claims Paid /Birth in $1,000	0.724
N	23,639,438

Standard errors in parenthesis are clustered by state. Elasticities are in brackets.
State, year fixed effects are included.
The independent variables for regression are age, race(White, Black, Hispanic and other), marital status(1 if married), and education(less than high school, high school graduate, some university, and university graduate).

Table 3-4. Impact of Malpractice Risk on Cesarean section, Various Samples

	All births	Previous cesarean section	No previous C-sec.	Breech	Gest. Diabetes	Multiple Birth	≤ HS Degree
		Subsamples					
		History		Complications			S.E.S
		OLS Estimates, Dependent variable: Cesarean Section					
Number of OB/GYN Claims/1000births	-0.0076 (0.0181) [-0.0062]	-0.0391 (0.0640) [-0.0098]	0.0022 (0.0146) [0.0025]	-0.0694 (0.1096) [-0.1528]	-0.0554 (0.0306) [-0.0285]	-0.0197 (0.0420) [-0.0666]	-0.0033 (0.0173) [-0.0027]
Amount of OB/GYN Claims Paid/Birth in $1,000	-0.0101 (0.0221) [-0.0032]	-0.0750 (0.0784) [-0.0073]	0.0127 (0.0182) [0.0057]	-0.1121 (0.1873) [-0.0976]	-0.0749 (0.0504) [-0.0155]	-0.0393 (0.0655) [-0.0532]	-0.0031 (0.0232) [-0.0010]
		First Stage Estimates					
Number of non-OB/GYN Claims/1000population	3.97 (0.92)	3.98 (0.89)	3.97 (0.92)	4.00 (0.99)	3.99 (0.93)	4.02 (0.91)	3.81 (0.86)
Amount of non-OB/GYN Claims Paid/Population in $1,000	5.96 (1.76)	5.82 (1.69)	5.97 (1.77)	5.98 (1.98)	5.81 (1.64)	6.01 (1.69)	5.63 (1.73)
		2SLS Estimates, Dependent variable: Cesarean Section					
Number of OB/GYN Claims/1000births	-0.0412 (0.0727) [-0.0338]	-0.0895 (0.2092) [-0.0225]	-0.0218 (0.0598) [-0.0253]	-0.0398 (0.1808) [-0.0876]	-0.0170 (0.0915) [-0.0087]	-0.1924 (0.1285) [-0.6511]	-0.0338 (0.0737) [-0.0286]
Amount of OB/GYN Claims Paid/Birth in $1,000	0.0262 (0.1108) [0.0084]	-0.3606 (0.4168) [-0.0355]	0.0575 (0.0935) [0.0261]	-0.1971 (0.4329) [-0.1716]	-0.0718 (0.1601) [-0.0149]	-0.0887 (0.1837) [-0.1202]	0.0319 (0.1240) [0.0103]
		Exogeneity Test (P value)					
Number of OB/GYN Claims/1000births	0.609	0.765	0.659	0.847	0.640	0.143	0.648
Amount of OB/GYN Claims Paid/Birth in $1,000	0.724	0.441	0.612	0.812	0.983	0.775	0.763
N	23,639,438	2,657,393	20,982,045	940,378	622,569	644,402	13,108,946

See foot note for Table 3-3.

Table 3-5. Impact of Malpractice Risk on Various Outcomes, Various Samples, Using the Number of OB/GYN Claims per Births as a Measure of Risk

		History		Complications			S.E.S
Outcomes	All births	Previous cesarean section	No previous C-sec.	Breech	Gest. Diabetes	Multiple Birth	≤ HS Degree
	OLS Estimates, Number of OB/GYN Claims/1000births						
C-section delivery	-0.0076 (0.0181) [-0.0062]	-0.0391 (0.0640) [-0.0098]	0.0022 (0.0146) [0.0025]	-0.0694 (0.1096) [-0.1528]	-0.0554 (0.0306) [-0.0285]	-0.0197 (0.0420) [-0.0666]	-0.0033 (0.0173) [-0.0027]
Prenatal visits	0.2561 (0.5023) [0.0041]	0.0714 (0.4850) [0.0011]	0.2834 (0.5070) [0.0046]	0.8846 (0.5198) [0.0144]	1.2485 (0.6100) [0.0183]	0.8578 (0.8005) [0.0126]	0.4176 (0.5517) [0.0070]
At least one ultrasound	0.0106 (0.1026) [0.0031]	0.0570 (0.1174) [0.0160]	0.0053 (0.1017) [0.0016]	0.0533 (0.1099) [0.0138]	0.0600 (0.1066) [0.0162]	0.0738 (0.1363) [0.0199]	0.0132 (0.0880) [0.0040]
Had amnio-centesis	0.0228 (0.0133) [0.1326]	0.0484 (0.0206) [0.1717]	0.0198 (0.0124) [0.1259]	0.0358 (0.0218) [0.1258]	0.0357 (0.0274) [0.0746]	0.0779 (0.0304) [0.2376]	0.0145 (0.0083) [0.1408]
Had fetal monitoring	0.0232 (0.0552) [0.0053]	0.0025 (0.0644) [0.0006]	0.0239 (0.0554) [0.0054]	0.0261 (0.0533) [0.0060]	0.0112 (0.0501) [0.0024]	0.0055 (0.0683) [0.0012]	0.0310 (0.0603) [0.0070]
Used Forceps*	0.0190 (0.0164) [0.0731]	0.0340 (0.0249) [0.1118]	0.0182 (0.0162) [0.0704]	0.0710 (0.0573) [0.1203]	0.0013 (0.0272) [0.0041]	0.0454 (0.0244) [0.1479]	0.0188 (0.0165) [0.0823]
Used Vacuum*	0.0078 (0.0176) [0.0188]	0.0520 (0.0276) [0.1056]	0.0060 (0.0174) [0.0145]	0.0063 (0.0718) [0.0132]	0.0364 (0.0243) [0.0815]	-0.0261 (0.0256) [-0.0538]	0.0070 (0.0185) [0.0186]
N	23,639,438	2,657,393	20,982,045	940,378	622,569	664,402	13,108,946

* Cesarean population was dropped for this dependent variable analysis since this procedure is not used in case of cesarean section

See foot note for Table 3-3.

Table 3-6. Impact of Malpractice Risk on Various Outcomes, Various Samples, Using the Number of OB/GYN Claims per Births as a Measure of Risk

		History		Complications			S.E.S
Outcomes	All births	Previous cesarean section	No previous C-sec.	Breech	Gest. Diabetes	Multiple Birth	≤ HS Degree
		2SLS Estimates, Number of OB/GYN Claims/1000births					
C-section delivery	-0.0412	-0.0895	-0.0218	-0.0398	-0.0170	-0.1924	-0.0338
	(0.0727)	(0.2092)	(0.0598)	(0.1808)	(0.0915)	(0.1285)	(0.0737)
	[-0.0338]	[-0.0225]	[-0.0253]	[-0.0876]	[-0.0087]	[-0.6511]	[-0.0286]
	{0.609}	{0.765}	{0.659}	{0.847}	{0.640}	{0.143}	{0.648}
Prenatal visits	0.4450	0.2512	0.4928	0.8028	-1.0668	-1.1059	1.1778
	(2.2200)	(2.1869)	(2.2272)	(2.4892)	(2.6407)	(3.5442)	(2.5133)
	[0.0072]	[0.0041]	[0.0080]	[0.0131]	[-0.0156]	[-0.0163]	[0.0199]
	{0.922}	{0.926}	{0.914}	{0.970}	{0.314}	{0.540}	{0.729}
At least one ultrasound	-0.1625	0.1951	-0.2059	0.3034	0.3900	0.1524	-0.1427
	(0.2495)	(0.2954)	(0.2480)	(0.3022)	(0.2823)	(0.2828)	(0.2483)
	[-0.0487]	[0.0550]	[-0.0623]	[0.0788]	[0.1054]	[0.0412]	[-0.0435]
	{0.408}	{0.558}	{0.313}	{0.324}	{0.172}	{0.734}	{0.468}
Had amnio-centesis	0.1300	0.2837	0.1103	0.2095	0.3021	0.4134	0.1020
	(0.0500)	(0.0849)	(0.0460)	(0.0575)	(0.0747)	(0.0927)	(0.0328)
	[0.7565]	[1.0067]	[0.7015]	[0.7363]	[0.6315]	[1.2613]	[0.9904]
	{0.020}	{0.003}	{0.032}	{0.002}	{0.000}	{0.000}	{0.004}
Had fetal monitoring	0.1757	0.1487	0.1749	0.3284	0.2023	0.0218	0.2130
	(0.2697)	(0.2885)	(0.2699)	(0.2655)	(0.2962)	(0.3009)	(0.2912)
	[0.0403]	[0.0362]	[0.0398]	[0.0761]	[0.0445]	[0.0051]	[0.0486]
	{0.525}	{0.568}	{0.530}	{0.207}	{0.473}	{0.951}	{0.484}
Used Forceps*	0.0578	0.1561	0.0532	0.4373	0.1424	0.0451	0.0561
	(0.0862)	(0.1231)	(0.0855)	(0.2822)	(0.1135)	(0.0957)	(0.0891)
	[0.2224]	[0.5136]	[0.2059]	[0.7409]	[0.4569]	[0.1469]	[0.2458]
	{0.639}	{0.286}	{0.670}	{0.152}	{0.185}	{0.997}	{0.665}
Used Vacuum*	0.0378	0.1959	0.0301	-0.1947	0.1214	0.0029	0.0173
	(0.0619)	(0.1172)	(0.0602)	(0.1918)	(0.0535)	(0.0905)	(0.0618)
	[0.0914]	[0.3980]	[0.0731]	[-0.4081]	[0.2718]	[0.0059]	[0.0460]
	{0.589}	{0.185}	{0.656}	{0.248}	{0.076}	{0.740}	{0.849}
N	23,639,438	2,657,393	20,982,045	940,378	622,569	664,402	13,108,946

* Cesarean population was dropped for this dependent variable analysis since this procedure is not used in case of cesarean section

Se foot note for Table 3-3 { } p value of exogeneity test

Table 3-7. Impact of Malpractice Risk on Various Outcomes, Various Samples, Using the Amount of OB/GYN Claims Paid per Births as a Measure of Risk

		History		Complications			S.E.S
Outcome	All births	Previous cesarean section	No previous C-sec.	Breech	Gest. Diabetes	Multiple Birth	≤ HS Degree
	OLS Estimates, Amount of OB/GYN Claims Paid/Birth in $1,000						
C-section delivery	-0.0101	-0.0750	0.0127	-0.1121	-0.0749	-0.0393	-0.0031
	(0.0221)	(0.0784)	(0.0182)	(0.1873)	(0.0504)	(0.0655)	(0.0232)
	[-0.0032]	[-0.0073]	[0.0057]	[-0.0976]	[-0.0155]	[-0.0532]	[-0.0010]
Prenatal visits	0.0780	0.0562	0.0857	1.1031	0.6517	0.9994	0.5128
	(0.5577)	(0.5478)	(0.5626)	(0.6797)	(1.0046)	(0.8757)	(0.6557)
	[0.0004]	[0.0003]	[0.0005]	[0.0071]	[0.0038]	[0.0059]	[0.0033]
At least one ultrasound	0.5577	0.6749	0.5443	0.5595	0.4475	0.6123	0.4802
	(0.3518)	(0.3489)	(0.3529)	(0.3261)	(0.2521)	(0.3603)	(0.3038)
	[0.0655]	[0.0746]	[0.0644]	[0.0574]	[0.0488]	[0.0663]	[0.0562]
Had amnio-centesis	0.0750	0.1206	0.0697	0.0960	0.0973	0.1354	0.0405
	(0.0418)	(0.0558)	(0.0401)	(0.0617)	(0.0452)	(0.0517)	(0.0204)
	[0.1707]	[0.1676]	[0.1735]	[0.1334]	[0.0820]	[0.1654]	[0.1509]
Had fetal monitoring	-0.0215	-0.0606	-0.0194	-0.0588	-0.0233	-0.0420	-0.0283
	(0.0905)	(0.1016)	(0.0908)	(0.0873)	(0.0828)	(0.1099)	(0.0833)
	[-0.0019]	[-0.0057]	[-0.0017]	[-0.0053]	[-0.0020]	[-0.0039]	[-0.0024]
Used Forceps*	0.0440	0.1117	0.0413	0.1569	0.0220	0.0553	0.0397
	(0.0216)	(0.0407)	(0.0213)	(0.0624)	(0.0504)	(0.0319)	(0.0212)
	[0.0662]	[0.1439]	[0.0625]	[0.1051]	[0.0284]	[0.0721]	[0.0667]
Used Vacuum*	0.0097	0.1251	0.0052	-0.0045	0.0321	-0.0183	0.0130
	(0.0215)	(0.0472)	(0.0216)	(0.1257)	(0.0294)	(0.0341)	(0.0214)
	[0.0091]	[0.0995]	[0.0049]	[-0.0037]	[0.0289]	[-0.0151]	[0.0132]
N	23,639,438	2,657,393	20,982,045	940,378	622,569	664,402	13,108,946

* Cesarean population was dropped for this dependent variable analysis since this procedure is not used in case of cesarean section

See foot note for Table 3-3.

Table 3-8. Impact of Malpractice Risk on Various Outcomes, Various Samples, Using the Amount of OB/GYN Claims Paid per Births as a Measure of Risk

Outcome	All births	History: Previous cesarean section	History: No previous C-sec.	Complications: Breech	Complications: Gest. Diabetes	Complications: Multiple Birth	S.E.S: ≤ HS Degree
2SLS Estimates, Amount of OB/GYN Claims Paid/Birth in $1,000							
C-section delivery	0.0262	-0.3606	0.0575	-0.1971	-0.0718	-0.0887	0.0319
	(0.1108)	(0.4168)	(0.0935)	(0.4329)	(0.1601)	(0.1837)	(0.1240)
	[0.0084]	[-0.0355]	[0.0261]	[-0.1716]	[-0.0149]	[-0.1202]	[0.0103]
	{0.724}	{0.441}	{0.612}	{0.812}	{0.983}	{0.775}	{0.763}
Prenatal visits	-4.0906	-4.1888	-4.0494	-4.3970	-8.2019	-7.5293	-1.7111
	(2.6960)	(2.7560)	(2.6969)	(2.9430)	(3.6181)	(5.7066)	(3.0602)
	[-0.0261]	[-0.0269]	[-0.0259]	[-0.0284]	[-0.0486]	[-0.0444]	[-0.0111]
	{0.095}	{0.100}	{0.098}	{0.041}	{0.008}	{0.104}	{0.430}
At least one ultrasound	0.4794	1.2750	0.3792	1.1461	1.0582	1.2509	0.6041
	(0.4856)	(0.6359)	(0.4796)	(0.6779)	(0.6253)	(0.6590)	(0.5284)
	[0.0563]	[0.1409]	[0.0449]	[0.1177]	[0.1154]	[0.1354]	[0.0707]
	{0.870}	{0.323}	{0.731}	{0.367}	{0.292}	{0.333}	{0.812}
Had amnio-centesis	0.2786	0.5392	0.2448	0.3271	0.5398	0.6053	0.2161
	(0.1317)	(0.2003)	(0.1217)	(0.1774)	(0.2125)	(0.2511)	(0.0875)
	[0.6342]	[0.7494]	[0.6095]	[0.4547]	[0.4551]	[0.7397]	[0.8052]
	{0.104}	{0.031}	{0.130}	{0.182}	{0.030}	{0.051}	{0.036}
Had fetal monitorin g	0.3096	0.1802	0.3219	0.2819	0.1415	-0.0100	0.4394
	(0.5231)	(0.6453)	(0.5124)	(0.4985)	(0.4671)	(0.5065)	(0.5122)
	[0.0277]	[0.0172]	[0.0287]	[0.0258]	[0.0125]	[-0.0009]	[0.0385]
	{0.498}	{0.691}	{0.475}	{0.471}	{0.707}	{0.945}	{0.334}
Used Forceps*	0.2093	0.4907	0.1960	0.4023	0.2731	0.0949	0.2146
	(0.1450)	(0.1895)	(0.1426)	(0.2514)	(0.2061)	(0.1739)	(0.1488)
	[0.3150]	[0.6324]	[0.2970]	[0.2696]	[0.3534]	[0.1238]	[0.3608]
	{0.237}	{0.042}	{0.260}	{0.305}	{0.202}	{0.816}	{0.221}
Used Vacuum*	0.0588	0.4424	0.0403	-0.1679	0.1118	0.0114	0.0187
	(0.0752)	(0.1675)	(0.0714)	(0.2851)	(0.1043)	(0.1168)	(0.0722)
	[0.0556]	[0.3521]	[0.0383]	[-0.1392]	[0.1009]	[0.0094]	[0.0191]
	{0.472}	{0.050}	{0.589}	{0.522}	{0.410}	{0.769}	{0.936}
N	23,639,438	2,657,393	20,982,045	940,378	622,569	664,402	13,108,946

* Cesarean population was dropped for this dependent variable analysis since this procedure is not used in case of cesarean section

See foot note for Table 3-3. { } p value of exogeneity test

Table 3-9. Descriptive statistics of measured risks in Obstetrics, Marginal Patient Sample

	Number of OB/GYN Claims/1000births	Amount of OB/GYN Claims Paid/Birth in $1,000
1%	0.0669	0.0155
5%	0.0990	0.0272
25%	0.1402	0.0390
50%	0.1687	0.0607
75%	0.2175	0.0906
95%	0.3081	0.1591
99%	0.4318	0.2120
Mean	0.1860	0.0712

Table 3-10. Impact of Malpractice Risk on Cesarean section, Marginal Patient Sample

Risk Measure	Cesarean Section
OLS Estimates Dependent variable: Cesarean Section	
Number of OB/GYN Claims/1000births	-0.0127 (0.0420) [-0.0094]
Amount of OB/GYN Claims Paid/Birth in $1,000	-0.0023 (0.0651) [-0.0006]
First Stage Estimates	
Number of non-OB/GYN Claims/1000population	3.75 (0.77)
Amount of non-OB/GYN Claims Paid/Population in $1,000	4.87 (1.26)
2SLS Estimates Dependent variable: Cesarean Section	
Number of OB/GYN Claims/1000births	-0.1649 (0.1234) [-0.1225]
Amount of OB/GYN Claims Paid/Birth in $1,000	-0.2395 (0.2777) [-0.0681]
Exogeneity Test (P value)	
Number of OB/GYN Claims/1000births	0.149
Amount of OB/GYN Claims Paid/Birth in $1,000	0.355
N	5,834,291

Predicted cesarean section rate is calculated for each individual using 32 pregnancy related conditions such as blood pressure etc. I rank each individual based on predicted cesarean section rate by descending order. I use sub sample of 12.5th percentile to 37.5th percentile taking into account that c-section rate of around 23 percent for all births as a marginal patients whose procedure choice are more responsive when malpractice risks for doctors are changed.
Standard errors are clustered by state.

Figure 3-1:

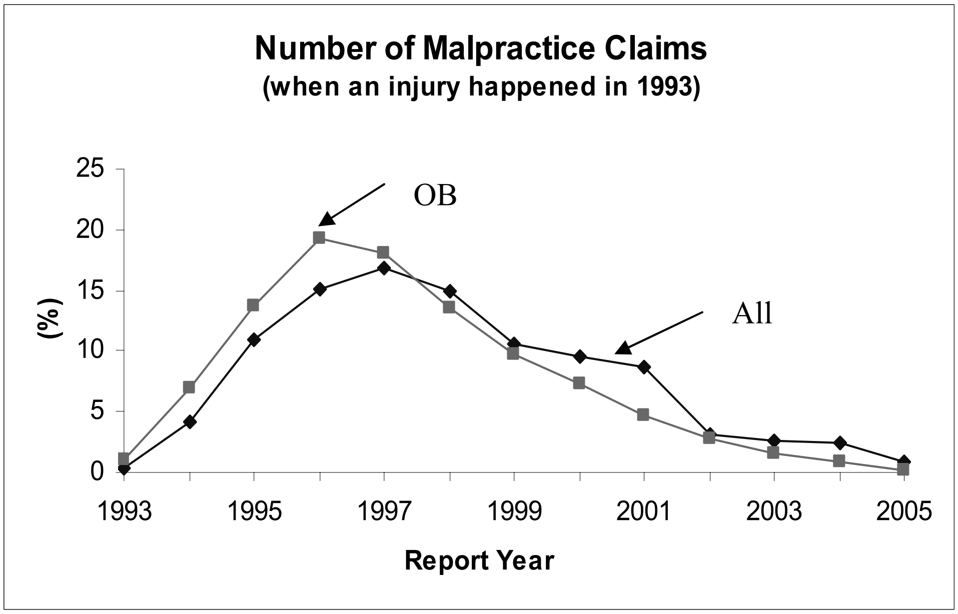


Figure 3-2:

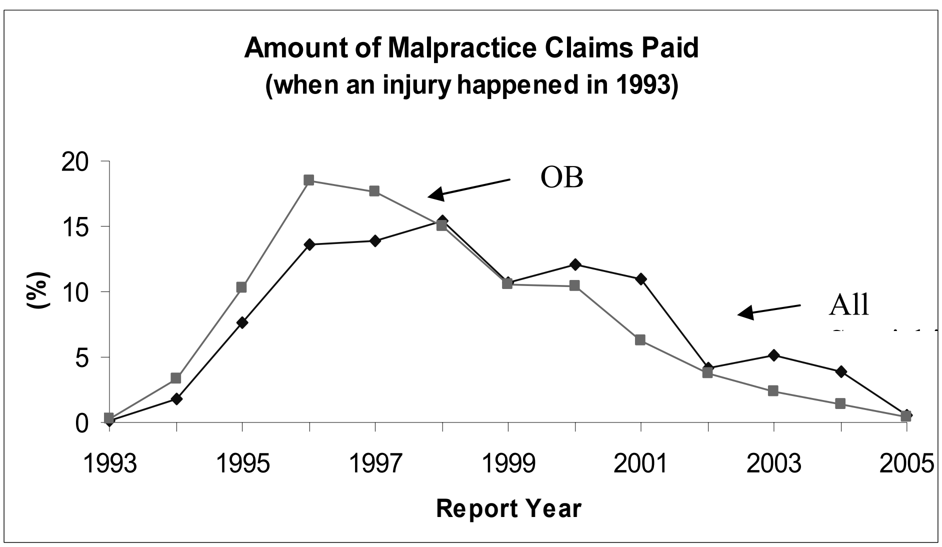


Figure 3-3:

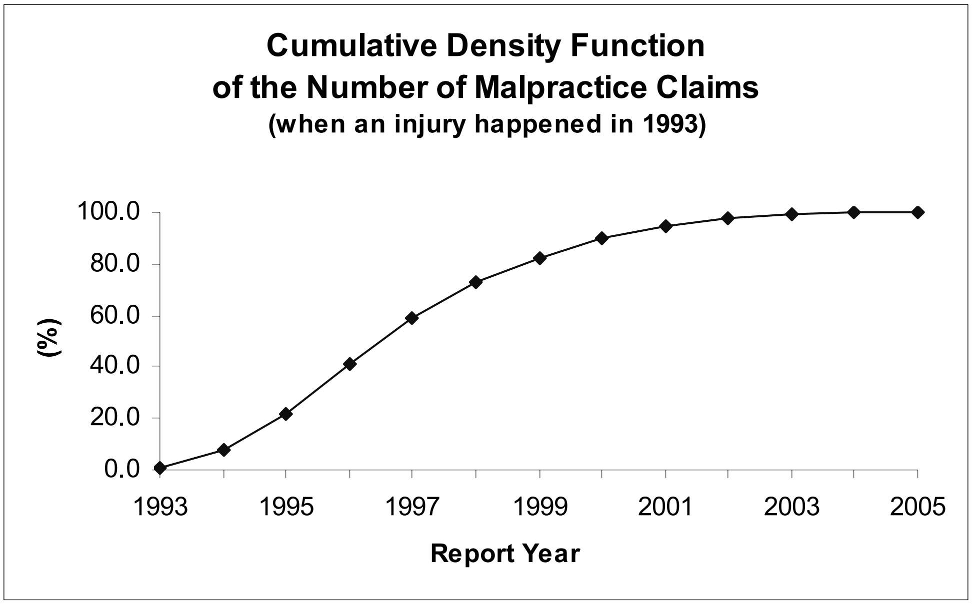
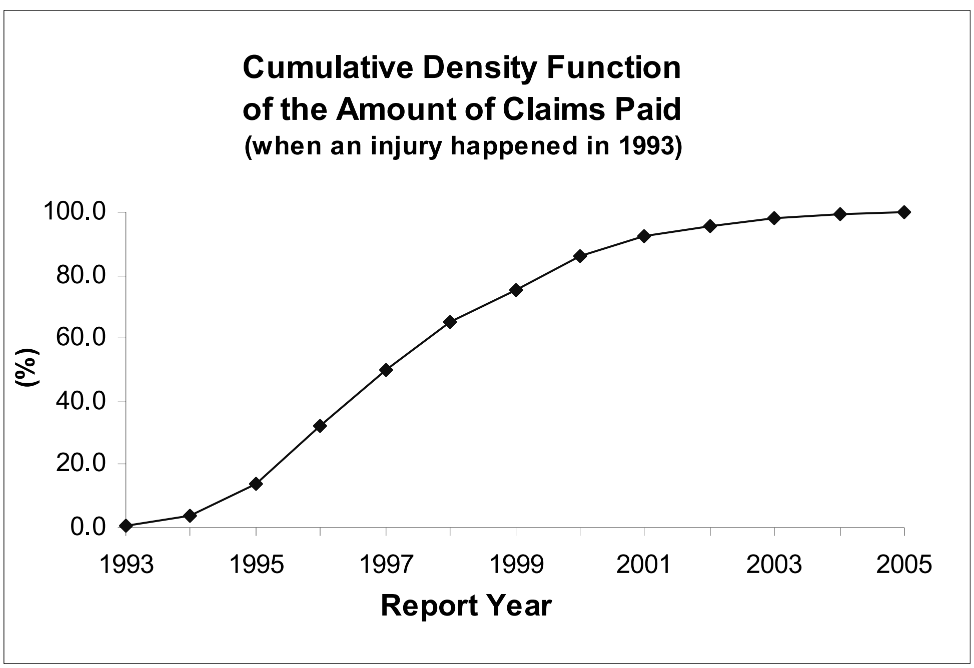


Figure 3-4:



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