ABSTRACT

Title of Dissertation: AN ECONOMIC ANALYSIS OF FACTORS IMPACTING HOSPITAL PATIENT OUTCOMES IN THE UNITED STATES.

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This dissertation is two essays that examine the impact of two distinct structural changes in hospitals. The first essay examines whether legislated changes in the minimum postpartum length of stay improve health outcomes for newborns. The second essay examines the consequences of nurse unions in hospitals.

Much of the previous research about the relationship between postpartum length of stay and patient outcomes are potentially subject to an omitted variable bias because sicker newborns usually stay longer in the hospital. We overcome this problem by using passages of early discharge laws as a quasi experiment that generated exogenous increase in length of postpartum stays. The California Newborn's and Mother's Health Act of 1997 effective on August 26, 1997, mandated that private insurance carriers provide coverage for at least 48-hour hospital stays for normal deliveries and 96-hour hospital stays for cesarean deliveries. A similar federal law went into effect on January1, 1998. Using an interrupted time series design, we demonstrate that early discharge laws reduced considerably the fraction of

newborns and mothers who were discharged early. In two-stage least square models using the state and Federal law as instruments for the length of hospital stay, we find that an additional day in the hospital reduces the probability of readmission by about one percentage point for complicated vaginal deliveries and c-sections of all types. For uncomplicated vaginal deliveries, we find there was no statistically significant change in 28-day newborn readmission rates.

The second essay examines the impact of nurse unionization in hospitals on wages, hours, staffing ratio of nursing personnel. Using hospital-level panel data merged with data on union elections from the National Labor Relations Board, we compare the outcomes of nursing personnel in hospitals that became unionized during the sample period with hospitals that did not change union status. The results indicate that unions have a small negative impact on nurse wage rates and they encourage hospitals to use a larger fraction of contract employees. These difference-in-difference estimates also indicate that cross-sectional regressions tend to overstate the wage gains of union because unions are more likely to appear in higher-wage hospitals.

AN ECONOMIC ANALYSIS OF FACTORS IMPACTING HOSPITAL PATIENT OUTCOMES IN THE UNITED STATES

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Chapter 1: Overview

The determinants of inpatient outcomes are an important research topic in the medical and social sciences. Patient characteristics, such as age, sex, race, occupation, socioeconomic status, the type of insurance, and the characteristics of illness are but a few of the factors that predict inpatient outcomes. Likewise, characteristics of the hospital and the type of care such as the intensity of health care use, hospital ownership, staff levels as well as other organizational features of the hospital also predict outcomes. In the dissertation, we contribute to this literature by examining two characteristics of hospital care that may impact outcomes: longer postpartum length of stays and the union status of nurses.

1.1 The Length of Postpartum Hospital Stays and Patient Outcomes

The standard health production function is assumed to be a concave function of health inputs but there is a question about whether expenditures have positive returns at higher levels of spending. The US in 2004, for example, spent roughly 16 percent of its GDP on health care or approximately $5,267 per person, an amount substantially higher than any other country in the OECD. Even with these startling numbers, the US ranks near the bottom among industrialized countries in health outcomes such as life expectancy and infant mortality. Subsequently, researchers in a number of disciplines have attempted to estimate the marginal productivity of health care in a variety of contexts.

In the first essay of this dissertation, we contribute to this literature by examining whether longer postpartum hospital stays improve patient outcomes. During the 1980s and 1990s, the lengths of postpartum hospital stays declined for both vaginal and cesarean births. Various factors were responsible for this decline including the rise of managed care and an effort to de-medicalize the birth process. In recent years, however, a number of authors began to question whether the decline had been too large and policy makers began to wonder whether infants and mothers were discharged too early. The main adverse effects of an early discharge are that mothers and newborns are no longer under clinical observation during the early postpartum period, increasing the risk of a subsequent re-admission and possibly mortality. In this context, the key question is what an additional day in the hospital has on the marginal effect of readmission and mortality rates of mothers and newborns.

Many previous researches have examined this question by comparing, in a cross section, length of stay and hospital re-admission rates. These studies are however subject to three persistent problems. First, samples sizes are often of an insufficient size to detect effects on rare outcomes such as readmission and mortality rates. Second, many of these studies lacked detailed control variables, especially measures of pregnancy complications. And third, infants assigned longer postpartum hospital stays are not a random selection of newborns but rather, tend to be children with more complications. Few studies had exploited experimental variation in the covariate of interest (postpartum length of stay) to solve the problem of selection bias. Our work addresses all three of these shortcomings.

In the mid 1990s, health professionals and policy makers expressed concern that shorter hospital stays might jeopardize the health of both mothers and newborns. Tragic stories of mothers and newborns discharged early who later developed life-threatening but preventable conditions fueled the desire of legislatures to address this issue. The federal government and states responded by passing laws requiring insurance carriers provide coverage for longer postpartum stays. One such law was the California Newborn's and Mother's Health Act of 1997, which went into effect on August 26, 1997, and mandated that insurance carriers provide coverage for at least 48-hour hospital stays for normal deliveries and at least 96-hour hospital stays for cesarean deliveries. If the physician, in consultation with the mother, discharges the patient before these time limits, the law requires that insurers provide coverage for a home or office follow-up visit for these women. A similar federal law called the Newborns' and Mothers' Health Protection Act of 1996 went into effect on January1, 1998.

We use a restricted-use data set of all births in California over a six-year period to examine the effect of these early discharge laws. Our sample has a total of 2.4 million births over this period, reducing the problems of Type II errors present in some previous studies. Using an interrupted time series design, we demonstrate that early discharge laws reduced considerably the fraction of newborns and mothers who were discharged early. In two-stage least squares models, we used these laws as exogenous changes in the length of postpartum stays as instruments for the length of stay. In these models we find that an additional day in the hospital reduced the probability of readmission by about one percentage point for vaginal deliveries with complications and c-sections of all types. The former result is statistically significant at conventional levels but the latter result is only significant at a p-value of around 0.10. We find there was no statistically significant change in 28-day newborn readmission rates for babies whose mothers had uncomplicated vaginal deliveries. Finally, although the statutes did not cover Medicaid patients and patients with no insurance, we find that their postpartum length of stay was affected by the law changes as well. This is primarily driven by a unique feature of the California Medicaid system that many in Medicaid managed care plans received coverage from private plans that were subject to the law.

1.2 Hospital Unionization

Patient outcomes are also affected by the organizational characteristics of hospitals, such as nursing staffing ratio, and skill mix of nurses, the profit structure of the hospital, etc. Blegen *et al.* (1998) found that the higher the registered nurse skill mix, the lower the incidence of adverse occurrences on inpatient care units. Aiken *et al.* (2002) found higher adverse events such as death within 30 days of admission and failure to rescue in hospitals with lower nurse staffing levels. A review of the literature by Lang *et al.* (2004) concluded that although the literature offers no support for specific minimum nurse-patient ratios for acute care hospitals, total nursing hours and skill mix do appear to affect some important patient outcomes.

Over the past 40 years, the health care sector has experienced rising healthcare costs, growing government involvement in health care through the Medicare and Medicaid program, the emergence of managed health care, and accelerating technological advancement. Given these complicated changes to the health care delivery system, nursing personnel indicated the desire for a greater voice in hospital organization and operations. From the 1970s to the 1990s, in contrast to the widespread decline in unionization in the economy as a whole, hospitals experienced a rapid increase in unionization. With collective bargaining, unions negotiate with hospitals not only on wage and benefits, but also on minimum staffing levels, overtime hours and employment of nursing personnel such as, the ratio of registered nurses (RNs) to Licensed Practical Nurses (LPNs) and aides. Needleman *et al.* (2002) found that the outcomes of patients are likely impacted by both the quantity and quality of nursing care. Subsequently, it is very important to examine the effect of unionization on wage and employment of nursing personnel.

There have been few studies that have examined how unionization affects the employment of nursing personnel, and to validate the benefits or hindrances of unionization on nursing personnel and patients. Most of the studies have been cross-sectional in natures that have treated union status as exogenous. Therefore, if unions are more likely to appear in hospitals with particular characteristics (e.g., hospitals with already higher wages) then these cross-sectional models will provide inconsistent estimates of the impact of unions. As we demonstrate below, hospitals experiencing a union certification election and those covered by collective bargaining are very different from other hospitals.

With a hospital-level panel dataset merged with unionization information, we use a basic difference-in-difference model to examine the impact of unions on employment outcomes such as nurse staff levels, wages, hours and the use of contract employees. The model identifies the impact of new union activity on the labor outcomes for the hospital using hospitals that do not change union status as a control. After unionization, we find greater utilization of aides who are lower-skilled nursing personnel compared to RNs and LPNs and large increase in the use of contract nursing personnel, who are not usually union members, relative to regular nursing personnel after hospital employees lost unionization elections. The staffing ratio of nursing personnel per discharge or per bed days were not statistically significantly affected by unionization.

In contrast to the vast literature on unionization in many other sectors of the economy, we find that the average hourly earnings of different nursing personnel decreased after hospital unionized. This result could be caused by less usage of expensive overtime hours after hospital won the unionization elections.

We also examine the impact of unions on wages using individual-level data from the Out-going rotation samples of the Current Population Survey (CPS). We find small positive effects of unions on wages and little if any impact on hours of work. These results are very similar to single-equation estimates that treat union status as exogenous. These results suggest that nurses are not selected into unions based on their earnings potential, but, unions are more likely to appear in hospitals with higher-than average earnings already.

Chapter 2: Postpartum Hospital Stay and the Outcomes of Mothers and their Newborns

2.1 Overview

Between 1970 and 1992, the average postpartum length of stay for mothers who delivered vaginally declined by 46 percent, from 3.9 to 2.1 days. Over the same period, the length of stay for those delivering by cesarean section fell from 7.8 to 4.0 days, a drop of 49 percent (Thilo *et al.* 1998; Hyman 1999). As a result of these trends, health professionals and policy makers expressed concern that shorter hospital stays might jeopardize the health of both mothers and newborns. A number of tragic stories about mothers and newborns discharged early who later developed life-threatening but preventable conditions fueled the desire of legislatures to address this issue. Between 1995 and 1998, 42 states passed laws requiring insurance carriers to provide minimum postpartum length of stays and a similar federal law called the Newborns' and Mothers' Health Protection Act of 1996 went into effect on January 1, 1998.

A number of authors have demonstrated that these laws increased average postpartum hospital length of stay, decreased the fraction of mothers and newborns discharged 'early', and increased hospitalization costs. There is however limited evidence about the impact of these laws on the health of the mothers and their newborns, and estimates from these studies provide conflicting results.

In this chapter, we use a restricted-use data set of California births over the 1995-2000 periods to examine the impact of both the California and federal early discharge laws. The California Newborns' and Mothers' Health Act of 1997 (NMHA), which went into effect on August 26, 1997, mandated that insurance carriers provide coverage for at least 48 hour hospital stays for normal vaginal deliveries and at least 96 hour hospital stays for cesarean deliveries. If the mother, in consultation with the physician, agreed to be discharged before the state minimum time limits, the law also required that insurers provide coverage for an early home or office follow-up visit for the new mother and her newborn. The federal law is similar to the California statute but it does not require that insurance carriers provide coverage for follow-up visits. Both the California and Federal laws explicitly excluded Medicaid births from coverage.

Using an interrupted time series model, we find that the California and federal law generated substantial and abrupt drops in the early discharge rate for vaginal and cesarean deliveries for births insured by private carriers. Although the law did not cover Medicaid patients, their lengths of stay were affected by the laws as well. We find that the law had no statistically significant impact on the 28-day readmission rates for newborns from uncomplicated vaginal deliveries, but there were statistically precise drops in readmission rates for newborns with complicated vaginal deliveries and c-sections without complications. There was no statistically significant change in 28-day mortality rates for newborns in any sub sample.

Because early discharge laws exogenously increased the postpartum length of stay, we use their passages as instrumental variables for length of stay in a two-stage least squares (2SLS) model to obtain consistent estimates of the impact of length of stay on medical outcomes. In the sample of vaginal deliveries with complications and in both c-section deliveries sub samples, we find that an additional postpartum day in the hospital reduces 28-day readmission rates by 1 percentage point. The former result is statistically significant at conventional levels but the latter result is only significant at a p-value of around 0.10. In most cases, 2SLS results are larger in absolute value than their ordinary least-squares counterparts, suggesting OLS estimates are biased due to positive selection: those most likely to be readmitted are also those most likely to have longer stays.

2.2 Declining Postpartum Length of Stay and Passage of Early Discharge Laws

The trend towards shorter postpartum hospital stays outlined above was brought about by a number of factors including a shortage of hospital beds, cost containment efforts by managed care organizations, and an effort to 'de-medicalize childbirth' (Braverman *et al.* 1995, Eaton, 2000). As an increasing number of new mothers were discharged prior to the medically recommended length of stay, the press took notice and increasingly used terms such as "drive through deliveries" or "drive by deliveries" to describe early discharges.[1]

In the middle of the 1990s, the medical profession and a number of state and federal legislators began recognizing the potential problems of shorter postpartum hospital length of stay. Tragic stories of mothers and newborns discharged early[2]

[1] According to Declercq (1999), "…a June 1991 article in the Philadelphia Inquirer was the first reference to early postpartum discharge and the phrase "drive through deliveries" first appeared in the headline in a February 14, 1994, editorial in the *New York Times*."

[2] The American College of Obstetrics and Gynecology (ACOG) and the American Academy of Pediatrics (AAP) define "early discharge" as a postpartum stay of less than 48 hours for uncomplicated vaginal births and a stay of less than 96 hours for cesarean deliveries (American College of Obstetrics and Gynecology 1992).

who later developed life-threatening but preventable conditions fueled the desire of legislatures to address this issue. For legislatures, mandating a minimum postpartum hospital length of stay seemed to be a reasonable and direct solution at that time. Declercq (1999) notes "early discharge laws involved incremental changes to an existing policy, a simple solution to a problem whose health consequences are unclear…." The first bill regulating early discharge was passed by the Maryland legislature in 1995. By 1998, early discharge laws had been adopted by 42 states.

The Newborns' and Mothers' Health Protection Act of 1996 (NMHPA) was signed by President Clinton on September 26, 1996 and became effective on January 1, 1998. The federal law mandated insurance carriers provide coverage for at least 48-hour hospital stays for vaginal deliveries and at least 96-hour hospital stays for cesarean deliveries. A decision to discharge a patient before these time limits could be made by a physician only after consulting with the mother.

The California Newborns' and Mothers' Health Act of 1997 (NMHA), which was passed and went into effect on the same day, August 26, 1997, adopted similar mandatory minimum stays as the NMHPA. The California law did however require that if the physician, in consultation with the mother, discharged the patient before these time limits, insurers must provide coverage for an early home or office follow-up visit for both the newborn and mother. Both the federal and California statutes specifically exempted Medicaid patients from coverage.[3]

[3] Declercq (1999) suggests that by exempting Medicaid, the laws required minimal public funds and assured quick passage of the statutes.

2.3 Literature Review

2.3.1 The Health and Medical Consequences of Short Postpartum Hospital Stays

Research results vary widely regarding the consequences of an early postpartum discharge for the mother and newborn. Most of the research on this topic correlates medical outcomes with the length of stay, controlling for observed characteristics of the patient and hospital. Many of these studies have demonstrated that shorter hospital stays for newborns are associated with higher probabilities of hospital re-admissions (Lee *et al.*, 1995; Liu *et al.*, 1997; Malkin *et al.*, 2000a), increased non-urgent visits to health centers and primary care providers (Madden *et al.*, 2002; Mandl *et al.*, 2000; Kotagal *et al.*, 1999), increased the risk of jaundice (Liu *et al.*, 1997; Grupp-Phelan *et al.*, 1999) and increased neonatal mortality (Malkin *et al.*, 2000). The magnitudes of these effects are sometimes quite large. Using Washington State linked birth certificate and hospital discharge abstracts covering 310,578 live births from 1991 to 1994, Liu and Davis (1997) used logistic regressions to assess the impact of an early discharge (a discharge less than 30 hours after birth) on the risk of rehospitalization within one month of birth. They found that newborns discharged early had a 28 percent higher 7-day re-admission rate and a 12 percent higher 28-day rate. Using linked birth and death certificates, plus hospital discharge records for 48,000 births from Washington state in 1989 and 1990, Malkin *et al.*, (2000b) found that neonatal mortality rates (death within 28 days) were 265 percent higher for infants discharged early compared to those with longer hospital stays.

In contrast to these findings, Dalby *et al.* (1996), Kotagul and Tsang (1996), Brumfield *et al.* (1996), Cooper *et al.*, (1996), Gagnon *et al.* (1997), Bragg *et al.* (1997), Mandl *et al.* (1997), Kotagal *et al.* (1999), Danielsen *et al.*, (2000), Johnson *et al.* (2002), and Madden *et al.* (2002), all show little or no relationship between postpartum hospital stays and hospital re-admission rates. Beebe *et al.* (1996) found no relationship between postpartum length of stay and neonatal mortality. Finally, Mandl *et al.* (2000) and Madden *et al.* (2002) found no impact of early discharges on emergency room or urgent care visits while Bossert *et al.* (2001) and Madden *et al.* (2004) found no link between early discharge and treatment for jaundice. Although many of these studies have smaller samples than the Liu and Davis (1997) paper cited above, not all of the results are simply Type II errors. Using Ohio Medicaid Claims data linked to vital statistics files for 102,678 full-term births from July 1, 1991 to June 15, 1995, Kotagal *et al.* (1999) found that the fraction of newborns discharged early increased 185 percent over the period (from 21 to almost 60 percent). However, there was no corresponding increase in the re-hospitalization rates in the same period. In a sample of 1.2 million vaginally-delivered newborns in California over the 1992-1995 period, Danielson *et al.* (2000) found no statistically significant difference in 28-day hospital readmission rates for babies released after a one-night stay compared to those with two or more nights stay.

It may be no surprise that the results of the single-equation models vary considerably from study to study. Infants assigned longer postpartum hospital stays are not a random selection of newborns but rather, tend to be children with more complications. Therefore, if we expect longer stays to reduce readmissions and there is positive selection on unobserved variables as well, then the expected negative relationship between length of stay and hospital readmission rates should be biased towards zero. Positive selection bias on observed characteristics is easy to establish in our data set. As we outline below, the data for this project includes all hospital discharges for childbirth in California over the 1995-2000 period. Using data from the pre-California law period (1995 and 1996), in a simple OLS model, we regress postpartum length of stay (LOS) on observed characteristics. Likewise, we estimate a logistic model with the same covariates but use as the dependent variable a dummy indicating whether the newborn was readmitted within 28 days. We estimate these models for all vaginal and c-section births covered by private insurance and Medicaid and results for these models are reported in Table 2-1. We report coefficients from the length of stay regression and marginal effects from the 28-day readmission logit regression.

The results from these models indicate that for both vaginal and c-section deliveries, there is positive selection, that is, children we expect to have longer hospital stays tend to have observed characteristics that predict higher readmission rates. Focusing on the results for vaginal deliveries, those with private insurance, those admitted to non-profit and for profit hospitals (compared to government-owned hospitals), children whose mothers had fewer complications during pregnancy and delivery, those with one or two previous births and girls, all have lower length of stays and lower odds of being readmitted to the hospital. There is less of a consistent story about the selection bias for some of the demographic variables such as the marriage, race/ethnicity, age, and education. Children with married mothers have shorter stays but the coefficient on marriage in the readmission logit is statistically insignificant. Likewise, the coefficients on white and black children are of opposite signs in the two models.

2.3.2 Analyses of Early Discharge Laws

Udom and Betley (1998), Dato *et al.* (1996), and Liu *et al.* (2004) examined the impact of early discharge laws on the postpartum length of stay and costs. All three studies show the laws increased the postpartum length of stay and increased costs, while the final two studies demonstrate the laws increased length of stays for those not impacted by the law such as Medicaid recipients and the self-insured.

Madden *et al.* (2004) examined the effects of two policies affecting length of stay of mothers and newborns in Massachusetts: An HMO protocol adopted in 1994 requiring a one-night hospital stay plus a nurse home visit after a vaginal delivery, and a 1996 Massachusetts early discharge law that was similar in scope to the 1998 federal statute. The authors used data on 20,366 mother-newborn pairs with normal vaginal deliveries between October 1990 and March 1998. They found that the reduced length of stay in this HMO and the increase in stay generated by the state law had little impact on subsequent medical encounters for jaundice or newborn feeding problems.[4]

In one of the most detailed studies to date, Meara *et al.* (2004) examined the impact of the Ohio early discharge law on the health of newborns in Medicaid.

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[4] Madden *et al.* (2002) examined the impacts of the same two interventions as in the previous paragraph, but this article considered the impacts for more vulnerable subgroups such as mothers enrolled in Medicaid and mothers from low-income or low-education census tracts. This work generated results similar to their earlier work.

Unlike the California and federal statutes, the Ohio law covered Medicaid patients. Using Medicaid claims over the 1991 through 1998 period, the authors establish that the law decreased considerably the fraction of short postpartum stays but they showed a noticeable but not statistically significant drop in hospital readmission rates. The most innovative portion of this study was an examination of the efficacy of early post-discharge office visits. Using the fact that there is variation in the delay of an office follow-up visit generated by the day of the week the birth occurred, the authors find that a follow-up visit that occurred within three days of discharge generated statistically significant reductions in hospital re-admission probabilities.

Despite the large volume of research, there is still no consensus regarding the impact of short hospital stays on mothers and their newborns. A review of the literature by Britton *et al.* (1994) concludes that "heterogeneity and limitations of methodology and study design substantially limit conclusions that may be drawn from published studies (p. 291)." Braverman *et al.* (1995) concludes that "there is no clear evidence for the safety, efficacy, and effectiveness of the hospital and post-hospital practices that were previously standard. The current available literature provides little scientific evidence to guide discharge planning for most apparently well newborns and their mothers (p. 724)". A third review by Grullon and Grimes (1997) concludes that "The safety of early discharge is unclear (p. 860)" and "the current data do not support or condemn widespread use of early postpartum discharge in the general population (p. 860)."

There are three persistent problems noted by the authors of the literature reviews discussed above. First, samples sizes are often of an insufficient size to detect effects on outcomes that are rare in the population. Second, many of these studies lacked detailed control variables, especially measures of pregnancy complications. And third, few studies had experimental variation in the covariate of interest (postpartum length of stay). Our work addresses all three of these shortcomings.

Although there have been numerous studies on the impacts of short postpartum hospital stays, much of the literature has one or more of the shortcomings listed above. Studies such as Marbella *et al.* (1998) have large samples but limited controls and no quasi-experimental variation. Studies such as Malkin *et al.* (2000), Danielson *et al.* (2000), Kotagal *et al.* (1999), and Liu and Davis (1997), have large samples, excellent control variables, but no quasi experimental variation in postpartum length of stay. Studies such as Meara *et al.* (2004), Madden *et al.* (2002) or Madden *et al.* (2004) have quasi-experimental variation and excellent control variables but samples that may be too small to make meaningful predictions about rare events like hospital re-admissions and neonatal mortality.

Our study deals with the three major concerns listed above. First, the law change occurred in California, a state with a large population, and we utilize data for approximately 3 million births in total, with more than half of these births occurring in the treatment period. Our study will therefore be the largest sample ever used to analyze the impact of postpartum length of stay on health outcomes. Second, although random assignment clinical trials are the gold-standard for inferring causal relationships, the number of observations necessary to eliminate a high Type II error rate for many low incidence outcomes makes clinical trails impractical for some questions. The best that one can hope to obtain is quasi-experimental variation in field data that mimics a clinical trial. As we demonstrate below, the California law change generated a large and immediate decline in the fraction of newborns and mothers discharged early provides just such variance. Third, in contrast to some previous studies, we examine the impact of early discharge for infants whose mothers experienced complications either during pregnancy or labor.

The most recent paper studying the effect of early discharge law is Datar and Sood (2006). They used the public-use version of the data set we use: linked California hospital discharge data for the years 1991 – 2000. They also estimate an interrupted time series design to examine the effect of federal early discharge law on three outcomes, hours in the hospital, 28 day readmission rate and 1 year mortality rate of newborns. They pooled all births together into one model: vaginal and cesarean births, private insured, Medicaid insured and uninsured births, and uncomplicated and complicated births. Using data for year 1995 – 2000 for 28 day readmission regressions, they found a large impact of the Federal law on 28 days readmission rates. In the first three years the law was in effect, the authors estimated the log odds of the 28-day readmission rate fell by 10, 20 and 30 percent respectively. Since the probability of a readmission is so close to 0, the percent change in the probability of a readmission is roughly the same magnitude. Although they use similar samples and a similar econometric model, their results are different from ours. We have investigated in detail why the results differ and in Appendix A, we demonstrate that this is due primarily to their inability to measure the month of birth.

2.4 Constructing the Analytic File

2.4.1 Data

The data set for this analysis is a specially linked administrative record data sets of all mothers and newborns discharged from non-federal hospitals in California from January 1, 1995 to December 31, 2000. The data set is generated and maintained by the State of California Office of Statewide Health Planning and Development (OSHPD) and created by linking patient discharge data sets with birth, death and fetal death certificate information.

Public-use versions of the patient discharge dataset contain demographic information such as the age, race, and sex of the mothers and newborns, information about the admission such as the length of stay, procedures used, diagnoses codes, the hospital charges, the type of insurance, and whether the patient died in the hospital. These discharge data sets also contain a code that identifies the hospital.

The linked patient discharge dataset with vital statistics birth file is a restricted-use version of the discharge data that contains all the information in the public use discharge record, plus the exact date and time of birth (and therefore the newborn's admission to the hospital), the zip code of residence, a scrambled Social Security number, information from the birth file that identifies when and where a baby was born and information from the death file that identifies when and where a newborn died for up to one year after discharged. The scrambled Social Security number can be used to link the discharge record over time so as to construct re-admission rates for both mothers and newborns. We have the ability to measure re-admission rates for up to one year after discharge. The information from the birth and death files will allow us to identify whether a newborn died within a fixed time period after admission and discharge, not just whether they died in the hospital. Also, the developers of the data file have matched mothers to newborns allowing us to use characteristics of both mother and the newborn as covariates in multivariate regressions. During the six years in our data set, there are approximately 3 million births in total, with 1.68 million births occurring after the passage of the California law.

Although the early discharge laws impact the length of stay for both mothers and their newborns, in this analysis, we focus primarily on the outcomes of infants. We do this because adverse outcomes like readmission and mortality rates are higher for infants than mothers and as we demonstrate below, samples are in most cases just large enough to produce statistical significance with these larger adverse outcome rates.

2.4.2 Outcome Variables

There are several outcome variables that we can utilize in the linked Hospital Discharge Data/Vital Statistics birth files that directly or indirectly measure the health of mothers and their newborns. The two most obvious outcomes are: the postpartum hospital length of stay and whether the mother and newborn were discharged early from the hospital. These variables can be used to directly measure the impact of the California law and federal law.

The hospital discharge data set measures length of hospital stay in days. Although the California and federal law requires that insurers pay for a minimum length of stay in hours, our data set does not have the hour of discharge and therefore, we cannot calculate this value. The intent of the law was to provide mothers with the ability to stay an extra night in the hospital if they desired. Subsequently, the key outcome in our analysis is whether the infant was discharged early which is less than two nights for vaginal deliveries and less than four nights for c-sections.

As we noted in the literature review, one concern with early discharges is that health care providers may not have had sufficient time to detect certain conditions. We will exploit the linked nature of our data and construct a measure of whether the newborns were re-admitted to the hospital within a specified time period. In the Hospital Discharge Data/Vital Statistics birth files, the scrambled Social Security number can be used to link the discharge records of newborns over time. Researchers typically measure re-admissions within 7, 14 and 28-days of birth, and we will follow this convention. We will measure the re-admission rates with a dummy variable that equals 1 if a person is re-admitted within a particular number of days.

We will also use neonatal mortality rates as an outcome. In the linked data set, death records for the newborn have been linked to the discharge and birth record. For each newborn, we know whether they died within one year of birth, plus the cause and place of death. Following previous literature, we will use 28-day mortality rates for newborns.

The final outcome measure we will examine is total charges for both the mother and infant's hospitalization.[5] This variable is closely connected with postpartum hospital length of stay of mothers and newborns and the results will allow us to conduct a cost-benefit analysis of the early discharge law. In regression models,

[5] Total charges were converted into real 2000 dollars using the Consumer Price Index.

given the skewness of hospital charges, we will examine models where the dependent variable is measured in natural logs.[6]

2.4.3 Analysis Samples

Many previous analyses of the impact of early discharge on the health of newborns have restricted their attention to uncomplicated vaginal and c-section deliveries. The authors surmise that few complicated deliveries will be discharged early so they focus on the deliveries most likely impacted by laws such as the federal statute. However, as we demonstrate below, we found that these laws significantly impacted the length of stay for complicated deliveries as well. Therefore, we work with four distinct sub samples: complicated and uncomplicated vaginal and c-section deliveries. There are a variety of ways to define complicated deliveries. One popular way is to use a specific DRG code for uncomplicated deliveries and there are codes for both mothers and newborns.[7] In our data set, we also have data from the birth record that can be used to define a complicated pregnancy/delivery as any one where the mother presented any one of 24 complications during pregnancy[8] or 23 complications during labor.[9] Although many patients whose DRG codes indicating a complicated delivery were also identified complications on the birth record, the overlap was not perfect. Subsequently, we use both DRG code and detailed

6 Because some hospital stays are incredibly long and expensive, we will also experiment with using median regressions to eliminate the influence of outliers in the data set.
7 DRG 370 represents cesarean deliveries with complications, and DRG 372 represents vaginal deliveries with complications.
8 These include such factors as pre-eclampsia, chronic hypertension, renal disease, Rh sensitivity, premature labor, sexually transmitted disease, Hepatitis B, low or high birth weight, less than 37 weeks gestation, plus others.
9 These include such factors as seizure during labor, premature rupture of membrane, breech presentation, excessive bleeding, sepsis, cord prolapse, fetal distress, anesthetic complications, maternal blood transfusion, plus others.

complication information from birth record to define complicated or uncomplicated birth. A birth is defined as uncomplicated if neither the DRG code nor the birth record identifies a complication.

We use ICD-9 procedure codes and data on the birth record to identify whether the mother delivered vaginally or by c-section. We restrict our attention to births covered by Medicaid or private insurance carriers, which captures about 95 percent of all births in the state over the period of analysis. Table 2-2 reports basic demographic information of mothers in our four samples, vaginal and c-section deliveries with and without complications. On average, based on our samples, women who had a cesarean delivery were older, more educated, and more likely to be black than women who had a vaginal delivery.

Table 2-3 reports distribution of postpartum length of stay of newborns in July 1995 through August 1997 period when federal law and California law has not been taken into effect. For uncomplicated vaginal deliveries, 76.17 percent women stayed only one day in hospital after giving birth and this number was 62.12 percent for women who had complicated vaginal deliveries. For cesarean deliveries, 86.74 percent women stayed two or three days in hospital after having uncomplicated cesarean deliveries and it was 74.98 percent for women who had complicated cesarean deliveries. So before the early discharge laws, most women stayed less than the mandated time which are 48 hours for vaginal deliveries and 96 hours for cesarean deliveries.

2.5 Graphical Analysis of California Law and Federal Law

2.5.1 The Change in Postpartum Length of Stay

In Figure 2-1, we plot the monthly fraction of newborns delivered vaginally without complications who were released early in each month. The first vertical line in September of 1997 indicates the first full month the California law was in effect and the second line at January 1998 indicates when the federal law became effective. Note that in Figure 2-1, there was a substantial and abrupt change in the private insurance time series during a short period between September 1997 and January of 1998. Before September of 1997, the fraction of newborns with private insurance that had a length of stay less than 2 days was relatively stable with a small drift downward. In August of 1997, 82 percent of newborns whose deliveries were paid for by private insurance had a postpartum length of stay less than 2 days. By February of the next year, just six months later, this number had fallen to 50 percent, a 32 percentage point decline and a 39 percent reduction. Early on in our sample, most insurance carriers knew the federal law would take effect in 1998, but from Figures 2-1, it appears that few were adjusting 6 months prior to the law change. Therefore, the state law change caught insurance carriers by surprise and as a result, it took some time to adjust. Most carriers had a long lead time to prepare for the federal law and it appears from Figure 2-1 (and subsequent figures), that the adjustments to a longer length of stay occurred by the 1st quarter of 1998.

Notice also in Figure 2-1 that although neither the California nor the federal law covered births paid by Medicaid, there was a sudden and persistent drop in the fraction of newborns with stays of less than 2 days in this group as well. The drop was less dramatic, but the post-law rates for both insurance types converged rapidly after the law was passed. In chapter 3, we examine in detail why there was a decline in the fraction of short postpartum stays in Medicaid and much of the decline appears to be due to the specific type of Medicaid managed care plans in California.

In Figure 2-2 we graph the early discharge rate for newborns who were delivered via uncomplicated cesarean section. In this figure, an early discharge is defined as a postpartum length of stay that was less than four days. Notice that starting in September of 1997 for those covered by private insurance, there was a noticeable drop in the fraction of early discharges. In August of 1997, 90 percent of newborns delivered by cesarean section were discharged in less than 4 days. By the middle of 1999, this number had fallen anywhere from 14 to 16 percentage points. For cesarean deliveries, although the drop was much less dramatic, we do see a drop in the fraction of Medicaid births discharged early.

Figures 2-3 and 2-4 plot the early discharge rate for newborns after complicated vaginal and c-section deliveries, respectively. From Figure 2-3, we can see that before the laws, on average there were 67 percent mothers and newborns that were discharged early even for vaginal deliveries with complications. As was true for deliveries without complications, there was a substantial and abrupt drop in the early discharge rate in both September 1997 and January 1998 that are roughly the same size as the drops for uncomplicated deliveries. Complicated vaginal deliveries covered by Medicaid showed a similar but less dramatic drop in January 1998. In Figure 2-4, we find that for privately insured complicated c-sections, there was a 17 percentage point drop in the early discharge rate between August 1997 and February 1998.

These figures highlight a number of important facts. First, if a short postpartum stay does affect some outcomes such as hospital re-admissions, then the sharp and dramatic drop in short stays generated by the California law should provide an excellent opportunity to precisely estimate the magnitude of this effect. Second, deliveries covered by Medicaid were affected by the early discharge laws as well, eliminating this group as a potential control. Third, the timing of the change in time trends corresponded exactly with the effective dates of the state and federal law. Fourth, the federal law reduced early discharge rates for complicated deliveries by the same rate as for uncomplicated deliveries, even though the former group has substantially lower early discharge rates prior to passage of the federal law.

2.5.2 Change in Delivery Method

We want to use the large change in postpartum hospital stays after the passage of the California and Federal laws to examine the link between length of stays and hospital re-admission rates. As we noted above, we will perform separate analyses for women with different insurance types, deliveries and the presence of complications. This analysis will only be accurate if the law did not change the distribution of births across these categories. For example, if hospitals responded to the greater length by reducing c-section rates or defining fewer mothers as having complications, then the sub samples would not be comparable over time. In this section, we examine whether insurance status, c-section rates and the fraction of complications change when the California and Federal laws come into effect.

In Figure 2-5, we graph the percent of complicated deliveries for private insurance and Medicaid. From Figure 2-5, there were no noticeable changes in the percent of complicated after January 1998 when the federal law became effective. We do however see an increase in the fraction of births with complicated deliveries in the September 1997 to December 1997 which was when the California law first became effective. We will return to this point in a future section but our analyses suggest that this was primarily due to a heavy flu season during this period.

In Figure 2-6, we plot the fraction of deliveries that were by c-section for both private insurance and Medicaid coverage. For both types of coverage, there was a slow upward movement in the fraction c-sections, but in general, there was no abrupt change in either September of 1996 or in January of 1998.

Finally, Figure 2-7 plots the percent of births covered by insurance over time. The law will increase the costs of deliveries, possibly increasing the costs of insurance and as a result, possibly pricing some new mothers out of health insurance or encouraging them to move to Medicaid. On average 97.5 percent of deliveries were covered by insurance in the long run and this ratio was very constant. There were no significant changes before and after early discharge laws became effective.

Based on the results from Figure 2-5 through 2-7, early discharge laws did not affect the percent of births recorded as complicated, the percent of cesarean deliveries and the percent of deliveries covered by insurance.

2.5.3 Changes in Health Outcomes

In the next four figures, we plot the 28-day readmission rate of newborns for vaginal and c-section deliveries without complications (Figures 2-8 and 2-9), and the same deliveries with complications (Figures 2-10 and 2-11). In each of these figures, the month-to-month variation in 28-day readmission rate dwarfs any systematic change in readmission rates produced by the law, so it is difficult in these graphs to tell whether the law improved birth outcomes. There was however a noticeable increase in readmission rates in December of 1997, the fourth full month that the California law was in effect and the last month before the federal law took effect. One may be tempted to attribute this sharp increase in readmissions to the California law. But a closer inspection of the data suggests that something else was occurring. Notice that readmission rates always spike in the late autumn and early winter months during the flu season. The winter of 1997/98 was a particularly heavy flu season in California as was pointed out in a report released by OSHPD.[10] Flu complications are a common reason for readmission among infants.

The cyclic nature on newborn readmissions suggests that we must control for day-to-day environmental conditions that may alter readmission rates. For infants born on a particular day, we calculate the 28-day admission rate (defined as admissions per number of children) for those born 90 to 180 days ago. We allow this

[10] The title of this report is *CALIFORNIA HEALTH CARE SYSTEM: OVERVIEW OF THE HOSPITAL/EMS CRISIS WINTER OF 1997-98*

readmission rate to vary by type of insurance. Our use of children 90-180 days old does however mean that we lose the first six months of data in our analysis. [11]

In Figures 2-12 and 2-13, we average the 28-day readmission rate of newborns and older infants up to the month level and plot the series over time for vaginal deliveries under private insurance and Medicaid. These Figures show that the admission rates of slightly older infants is highly correlated with the 28 day readmission rate of newborns and both indexes demonstrates a sharp rise in admissions in December of 1997. We will use the admission index for older infants as a control variable in our model to capture the day to day variation in unobserved conditions that lead to changes in readmission rates.

2.6 Econometric Model

2.6.1 A Reduced-Form Model

To estimate the impact of the law on birth outcomes, we would ideally use a difference-in-difference model, where we could compare the average length of stay of mothers and newborns before and after the California early discharge law went into effect, using a comparison group to identify what the time path of outcomes would have been in the absence of the intervention. Unfortunately for our research purposes, all states were treated in roughly the same time period by the Federal law. Likewise, the preliminary data above suggest that within California, the law impacted those not covered by the statute. Given the lack of any control group, we therefore

[11] Use of the index as a covariate assumes that changes in early discharge rates produced by the state and federal law will have no impact on admission rates after 90 days of age. Results in Table 5 provide some evidence in support of this hypothesis.

use an interrupted time series model instead. This technique requires the modeling of a process underlying a time series of data and is used as a means of assessing the impact of a discrete intervention on a social process.

Interrupted time series models are sometimes difficult to implement for a variety of reasons. First, it is often difficult to measure precisely when an intervention became effective. Second, other events may contaminate the treatment effect. For this particular study, we are fortunate that the timing of the law change is exact, and the immediate and large impact of the law makes it difficult to argue that some other event explains the sudden and precipitous change in hospital length of stays for newborns.

The unit of analysis for this study will be a mother/newborn pair, and the key patient of interest is the newborn because prior studies have suggested their health is more likely to be affected by an early discharge. In the newborn models, we label the outcome variable of interest Y. Outcomes vary across patients, hospitals and time, which are indexed by i, k and t, respectively. The basic interrupted time series model is of the form

$$
\begin{aligned}
(1)\ Y_{ikt} = {} & NEWBORN_{ikt}\beta_1 + PARENTS_{ikt}\beta_2 + HOSP_{ikt}\beta_3 + \\
& STATELAW_t{*}PAYER_i\alpha_1 + FEDLAW_t{*}PAYER_i\,\alpha_2 + \\
& HSAREA_k{*}HOSP\_SIZE_k{*}PAYER_i{*}MTREND_t\beta_4 + \\
& HSAREA_k{*}HOSP\_SIZE_k{*}PAYER_i\,\beta_5 + INDEX90180_t\beta_6 + MONTH_t + \\
& DAY_t + \varepsilon_{ikt}
\end{aligned}
$$

NEWBORN and PARENTS are vectors of control variables that describe characteristics of the newborn (sex, race, ethnicity, the hour of birth), and the mother and father (such as age, education, and the number of previous births) plus insurance status (PAYER). To control for seasonal patterns in re-admission rates, we include a set of twelve monthly fixed-effects, labeled as MONTH. There are also persistent differences in outcomes across hospitals with particular characteristics and we capture these with the vector HOSP. In this vector, we include dummy variables for hospital size (HOSP_SIZE),[12] ownership status[13], hospital service area (HSAREA)[14]. There may be variation within the week in some outcomes based on the day of admission, so we include a set of day of the week fixed-effects, labeled as DAY_t. Finally, to control for day-to-day conditions that may generate readmissions, we include the admission rate for infants 90 to 180 days of age. For an infant born on day t, we use as an index the number of admissions per child over the next 28 days for all children aged 90-180 days who were alive on that birth date. The variable ε_{ikt} is an additive error.

Because we do not have a natural control group that was unaffected by the law changes, we must capture the time series in postpartum length that would have occurred in the absence of the California with time trends. We are aided by the fact

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[12] We break hospitals up into 6 groups based on average monthly number of delivery cases. HOSP_SIZE=1 if average monthly case<=20; HOSP_SIZE=2 if 20 <average monthly case<= 50; HOSP_SIZE=3 if 50 <average monthly case<= 100; HOSP_SIZE= 4 if 100 <average monthly case<= 150; HOSP_SIZE=5 if 150 <average monthly case<= 300; HOSP_SIZE=6 if average monthly case>300.

[13] For hospital ownership variable, it is classified into 11 categories: 1=church, 2=non profit corporation, 3=no profit other, 4=individual investor, 5=partnership investor, 6=corporation investor, 7=state, 8=county, 9=city/county, 10=city, 11=district.

[14] We use 14 health service areas defined by the U.S. Department of Health and Human Services for the state of California. Health service areas are sometimes single counties (e.g., Los Angeles and Orange county) but in many cases, areas include multiple counties.

that the pre-law and post-law trends are very similar as Figures 1-4 demonstrate. Taking the aggregate data for Figure 1 and regressing the fraction of early discharges in private insurance on a time trend, dummies for the state and federal law, plus a time trend for the four months of the state law, we obtain an R^2 of 0.98. To eliminate any secular trend in the data, we include a monthly trend $MTREND_t$ that equals 1 if the birthday of newborn is in January of 1995, 2 in February, etc. We allow the monthly trend to vary by the health service area, hospital size, and the source of payment of delivery.

The key variables in the model are the vectors α_1 and α_2 that measure the impact of the state and federal respectively. The variable STATELAW equals 1 from August 27, 1997 through December 31, 1997, while FEDLAW equals 1 from January 1, 1998 and on. In each regression, we allow the effects of the law to vary across patients with private insurance and Medicaid.

There are two continuous outcomes (length of stay in days and dollars of total charges) and three sets of outcomes that are dichotomous (early discharge, re-admission and neonatal mortality). For models with continuous outcomes, we will estimate ordinary least squares, and for discrete models, we will estimate logistic regression models.[15] In all models except quantile regression estimates below, we control for possible autocorrelation in errors by allowing for arbitrary correlation in errors within a hospital over time.

[15] We can also add hospital-specific fixed-effects to the model. In the initial regression models, we estimate all models via OLS even though some outcomes are dichotomous. However, later on in the paper, we compare OLS results to logistic regression results for these discrete outcomes. There are many hospitals in the data set and estimating a nonlinear model like a logistic regression with hospital fixed effects is subject to the problem of incidental parameters (Chamberlain, 1984). The observations per hospital preclude us from using a conditional logistic regression. Therefore, to directly compare results, we do not include hospital fixed-effects in the basic regression models. The linear probability estimates are however not sensitive to whether we include hospital fixed-effects or not.

We titled this section 'a reduced-form model' for a particular reason. The California law changed two things at once. First, it required insurance carriers to provide coverage for longer postpartum hospital stays. Second, it required insurance carriers to provide coverage for a follow-up visit for mothers who, after consulting with their physician, were discharged from the hospitals early. Therefore, the estimated impact of the law contained in coefficients (α_1 and α_2) captures both of these changes. This distinction is potentially important. Suppose that a) early discharge increases the chance of a hospital readmission, b) early follow-ups of patients released early eliminate this risk, and c) everyone released early has a follow-up visit. In this case, the coefficients on α_1 and α_2 will both be zero since the harm from an early discharge was compensated for by the office follow-up visit. Previous research from California has demonstrated, however, that this is probably not a concern.[16] Although in principal α_1 and α_2 capture both effects, any impact we estimate is likely to be driven primarily by the change in length of stay and not an increase in early follow-up visits.

2.6.2 Two-Stage Least-Squares Estimate

We argued in the previous section that the health benefits from the law are likely to be driven by longer postpartum length of stays and not the mandated coverage for early follow-up visits after an early discharge. We quantify the size of the relationship between postpartum length of stay and adverse outcomes by using the information from the reduced-form models in a more structured way. Specifically,

[16] Galbraith *et al*. (2003) surveyed 2,828 mothers in California in 1999 and found there was no difference in the percentage of newborns with an early follow-up (within 2 days of discharge) between those discharge early and those discharged later.

we use the adoptions of the federal and state laws as instrumental variables for length of stay in a two-stage least squares (2SLS) model to obtain consistent estimates of the impact of length of stay on medical outcomes.

To more formally outline this model, note that one question of interest is the impact of length of stay on 28-day readmission rates. We can model this statistically with the following 'structural' equation of interest:

$$(2)\ 28DAY_{ikt} = X_{ikt}\beta_1 + LOS_{ikt}\,\delta_1 + \varepsilon_{1ikt}$$

Where for simplicity, we include all the covariates from equation (1) into one vector X. The key covariate in this regression is LOS which measures postpartum length of stay of newborns in days. As we noted above, equations such as (2) have been estimated by a number of authors but we suspect that $cov(LOS_{ikt}, \varepsilon_{1ikt}) > 0$ so OLS estimates of δ_1 will be biased towards zero.

Two-stage least squares estimation requires that a researcher identify a variable that exogenously changes the endogenous covariate of interest (LOS) but has no direct impact on health (28DAY). In this case, the instruments are the enforcement dates for the state and federal law. As Figures 2-1 – 2-4 indicate, the federal law clearly changed hospital length of stay and for the reasons mentioned above it is plausible that this change was exogenous. For this particular study, we are fortunate that the timing of the law change is exact, and the immediate and large impact of the law makes it difficult to argue that some other event was explaining the sudden and precipitous change in hospital length of stays for newborns. Likewise, so long as we properly control for the secular trends in the outcome, the instruments should not generate any omitted variables bias in the outcome equation of interest.[17]

2.7 Results

2.7.1 Reduced Form Model Regression Results

Table 2-4 presents the results of equation (1) – the reduced-form model for infants using four sub samples, vaginal and cesarean deliveries without and with complications. We initially report results for four outcomes: percent discharged early, length of stay in days, 28 days readmission, and log total inpatient charges (charges for the infants plus the mother). We estimate all models as linear regressions although these two outcomes – discharged early and 28 day readmission – are dichotomous.

For privately insured vaginal deliveries without complications, we find that the California and federal law reduced early discharge rates of newborns by 16 percentage and 31 percentage points, respectively, with the later result being 38 percent of the pre-law rates. The standard errors on these estimates are incredibly small and the results are statistically significant at conventional levels.[18] Interestingly, the federal law had only a slightly smaller impact on vaginal deliveries with complications although the mean discharged early is 14 percentage points lower

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[17] Our key outcomes (re-admissions and neonatal mortality) are both discrete and the key covariate of interest (length of stay in days) is continuous. A 2SLS model where we instrument for LOS will therefore not mimic the data generating process well. This model could be estimated by maximum likelihood models where the outcome is discrete and the endogenous variable of interest is continuous (Evans, Oates and Schwab, 2002; Evans, Farrelly and Montgomery, 1998). However, given the size of the data set and the number of covariates in the model, this model will be difficult to estimate. Angrist (2001) has however demonstrated that two-stage least squares models applied to limited and discrete dependent variables replicate treatment effect parameters from more complicated maximum likelihood models.

[18] References to statistically significant estimates assume a p-value of 0.05 or below.

in this later group. For c-section deliveries, the federal law is estimated to reduce early discharge rates by 12 and 14 percentage points respectively, privately insured c-section deliveries with and without complications, respectively. In general, the effect of the state law is about half the impact of the federal law for those with private insurance. Although neither law applies to Medicaid births, the federal law reduced early discharges of vaginal births by 12 percentage points in both samples, and again, both of these results are statistically significant at conventional levels, The state law is estimated to not have reduced early discharges for c-sections by a statistically significantly amount in either sample.

In the next column of results, we report average change in length of stay measured in days and the results in this column parallel those for the discharged early outcome. For vaginal deliveries covered by private insurance, the federal laws increased average length of stay by 0.36 days among uncomplicated deliveries and by 0.44 days for those with complicated deliveries, and both estimates are statistically significant. The corresponding numbers for the Medicaid population are 0.15 and 0.21 days (both statistically significant), which are roughly 40 percent of the values of the federal law. Among c-section deliveries covered by private insurance, the federal law increased average stays by a statistically significant 0.43 days for uncomplicated deliveries and 0.73 days for complicated deliveries, while the impact of the state law was half as large for uncomplicated deliveries and two-thirds as large for complicated c-sections.

In the third set of results for each sub sample, we report results for the 28-day readmission equation. If longer stays reduce readmission rates, then we should see reductions in these rates after passage of the state and federal laws. For vaginal deliveries without complications, we observe a statistically insignificant drop in readmissions rates of 0.06 percentage point after the passage of the federal law for privately insured and 0.03 percentage point for those covered by Medicaid. In contrast, for the same subsample, we observe a statistically significant *increase* in readmissions after passage of the state law for both insurance types. This could be attributed to the law but alternatively, this could be due to the fact we cannot control perfectly for the large spike in admissions that is observed in December of 1997 during the severe flu season in California. What do those results mean? For complicated vaginal deliveries and both cesarean deliveries samples, totally there were 594,049 deliveries in January 1998 through December 2000 period. For those observations, if we multiply the estimated parameters for federal law*Private Insurance and federal law*Medicaid to the corresponding number of observations in that sub sample and get the sum of the results, we could see for complicated vaginal deliveries and cesarean deliveries samples all together, the federal law reduced 28 days readmission by 2,700 cases, that is, 2,700 readmissions of newborns were avoided due to the federal law.

For vaginal deliveries with complications, we observe reductions in readmission rates after the state and federal law and for both insurance types, but the results are only statistically significant for Medicaid patients after the federal law. Among those with private insurance delivered by c-section, there was a statistically significant drop in readmission rates of a half of a percentage point after the passage of the federal law, and the coefficient is statistically significant in the uncomplicated sample but the estimate in the complicated sample has a t-statistic of -1.42.

In the final column of results for each subsample, we estimate a model of log charges which is the total charges paid for both the mother and her infant. Among those with private insurance, the passage of the federal law is estimated to increase charges by eight to 10 percent for vaginal births, 4.7 percent for uncomplicated c-section deliveries and 6.1 percent for complicated c-section deliveries. The federal law increased Medicaid charges by 2 percent for vaginal deliveries and the state law increased payments for privately insured patients by a similar amount.

Given the skewness in total charges, the least square regression results in Table 2-4 can be distorted by some large outliers in the charges distribution. For the reduced form model having log total charge as dependent variable, we also estimate quantile regressions models at the 25^{th}, 50^{th}, and 75^{th} percentile of the conditional distribution in log charges. The quantile regression results are reported in Table 2-5.[19] In general, because the law is impacting the lower tail of the length of stay distribution, we would expect larger reduced-form estimates on the treatment effect dummies for lower conditional quantiles and this is exactly what we find. Looking at the results for those with private insurance among vaginal deliveries without complication, the federal law is estimated to increase costs by 9.6, 9 and 8 percent at the 25^{th}, 50^{th}, and 75^{th} percentile, respectively. In all three cases, the parameter estimates are forty to fifty times larger than standard errors. For uncomplicated c-section deliveries paid by private insurance, the federal law increased costs by 5, 4 and 2 percent across the three conditional quantiles. Notice that mean estimates from

[19] We are unable to allow for within-hospital correlation in errors in the quantile regression models.

Table 2-4 for the federal law among the privately insured is slightly lower than the median estimates in Table 2-5 among vaginal deliveries, but this ordering is reversed for c-section deliveries.

Much of the literature in this area examines 28-day readmission rates as a key outcome but we can calculate 7 and 14 day readmission rates as well. As we shorten the follow up after birth, the incidence rate falls considerably. Linear probability models tend to generate marginal effects similar to estimates from logit models but only when the mean outcome is some distance from zero or one. In the case, the small rate of 7 and 14 day readmissions necessitates that we estimate a logit model for these outcomes.

Table 2-6 reports the reduced-form logistic regression models where the outcomes of interest are initially the 7, 14, and 28 day readmission rate for infants and in the table, we report the 'average treatment effect' which is the estimated change in the logistic CDF when the law dummies are turned on and off. The results in Table 2-6 indicate the federal early discharge law had no statistically significant effect on readmission rates for either privately insured or Medicaid vaginal delivery patients. In contrast, the state law is estimated to generate statistically significant increases in readmission rates for these same patients. As we explained before, this could be fact that we are not controlling completely for the day-to-day fluctuations in admission rates with our admission index.

Looking at the impacts of the federal law for privately insured vaginal deliveries with complications and c-sections without complications, we see a number of interesting results. First, the logit marginal effect in the 28-day readmission equation is nearly identical to the linear probability estimates in Table 2-4. Second, the coefficients for the 7, 14 and 28-day readmission rates are nearly identical meaning that for these samples, ALL of the problems generated by short hospital stays will manifest themselves within seven days. However, in all three samples, especially for c-sections with complications, the coefficient on the (federal law x private insurance) variable increases monotonically as one move from the 7-day, 14-day and 28-day equations. In this final model, we generate a coefficient that is statistically significant at the 90 percent confidence level.

In the final column of each section of results, we also report logit estimates where the outcome of interest is the 28-day mortality rate for newborns. Across the four samples, neonatal mortality is anywhere from 6 per 10,000 to 45 per 10,000 so this adverse outcome is very rare. We see some evidence that for those experiencing the largest increase in length of stay (privately insured patients impacted by the federal law), there is a decline in mortality in two of four samples but none of the estimates are statistically significant at conventional levels.

2.7.2 2SLS Regression Results

The reduced-form estimates presented in the previous three tables represent the 'intention to treat' impacts of the state and federal law. In this section, we generate estimates of the impact of the law on those treated by calculating 2SLS estimates of equation (2). As we noted above in Table 2-1, those most likely to have longer hospital stays are also those most likely to experience a 28-day readmission. If the unobserved characteristics of mothers and their infants that predict 28-day readmission rates are correlated with length of stay in the same direction as these observed characteristics, then OLS estimates of equation (2) are biased towards zero.

In the first third of Table 2-7, we report OLS estimates of equation (2) to form a baseline to which we can compare 2SLS results. In this model, the 28-day readmission dummy variable is the outcome of interest and key covariate is length of stay measured in days. The estimated impact of an additional day in the hospital is fairly uniform across the samples, ranging from a low of .12 percentage points for vaginal deliveries with complications to .22 percentage points for c-sections without complications.

In the next third of the table, we report 2SLS estimates that use four instruments for postpartum length of stay: the federal and state laws interacted with Medicaid and private insurance. In three of our four samples, the results are negative and the estimates are larger in absolute value, which is consistent with the selection bias story we outlined above. In the vaginal delivery sample without complications, the 2SLS estimates suggest that an additional day in the hospital will reduce 28-day readmission rates by 0.09 percentage points, but by OLS, an additional day in the hospital will reduce 28-day readmission rate by 0.13 percentage point. For this sample, the results are not consistent with our story that OLS estimates are biased towards zero. This could be caused by the fact that we are using both state and federal law as instrumental variables, and there are significant positive effects of state law on readmission rates. For vaginal deliveries without complications, the 0.764 p value of 2SLS regression estimate means that, at conventional statistical levels, what we can conclude is that even with 1.3 million observations, we do not have enough power to reject the null hypothesis that the length of stay coefficient is zero.

In contrast, the coefficients on length of stay in the other three samples are all large and statistically precise at much lower p-values. For vaginal deliveries with complications and both c-section samples, we estimate that an additional day of stay reduces 28-day readmission rates by about 1 percentage point. These are large numbers. In the vaginal delivery with complication sample and in the c-sections samples without and with complications, the ratio of the 2SLS estimate divided by the sample mean is 0.297, 0.20, and 0.119, respectively. Across these same samples, these estimates are statistically different from zero at the p-values of 0.054, 0.093, and 0.158, respectively.

The p-values on the test of over-identifying restrictions in the final three models are all comfortably in excess of 0.05 but the estimate for the uncomplicated vaginal delivery sample is very small. This is to be expected given the reduced-form estimates in Table 2-4. Given the imperfect controls for cyclic variation in readmission rates, we find in reduced-form models that the state law actually increased readmission rates. Since the test of over-identifying restrictions can also be thought of as a test of the null hypothesis that the 2SLS estimates are identical regardless of the instrument set used, we would expect to reject the null with test since the federal instruments is predicting a positive benefit of an additional day of stay whereas the other instruments predict a negative effect.

The problems with the four months when only the state laws were in effect is illustrated with the final column of Table 2-7 where we delete these four months from the sample and only use the federal law interacted with insurance coverage as instruments. Now in all four samples, the estimates are negative and larger in absolute value, which is consistent with the selection bias story we outlined above. In these models, the p-values on the 2SLS coefficients across the four samples are 0.647, 0.05, 0.115 and 0.086, respectively. Also, the p-values on the test of over-identifying restrictions are now all incredibly big with three in excess of 0.95. For vaginal deliveries without complications, we still do not have enough power to reject the null hypothesis that the length of stay coefficient is zero.

2.8 Why were Medicaid Deliveries Impacted by the Early Discharge Laws?

The federal and California early discharge statutes specifically excluded Medicaid patients from coverage. Declercq (1999) suggests that by exempting Medicaid covered deliveries, the laws required minimal public funds and assured quick passage of the statutes. It is therefore somewhat surprising that although the law did not cover Medicaid patients, their lengths of stay were affected by the laws as well. In this section, we attempt to explain why deliveries covered by Medicaid were also affected by the early discharge laws. We do so by examining two possible explanations. First, there are two types of Medicaid covered deliveries: deliveries covered by traditional Medicaid fee-for-service, and deliveries covered by Medicaid managed care. For Medicaid fee-for-service deliveries, they were not covered by the early discharge laws, so they should not be affected by the laws. Some Medicaid managed care plans received coverage from private plans that were subject to the law.

Enrollees in these managed care plans in many cases have the same type of insurance as a person enrolled in an employer-provided managed care plan, so those deliveries should be affected by the early discharge laws. We pool Medicaid deliveries together and do not differentiate different types of Medicaid coverage in the regressions. Subsequently we get the results that Medicaid deliveries were affected. Second, the drop in early discharge Medicaid might be caused by the behavior of physicians. As we demonstrated above, prior to passage of the California early discharge law, the majority of private and Medicaid deliveries were discharged early. After the passage of the law, it is clear that physicians responded quickly to the change in private insurance coverage. Among Medicaid patients, the response was not nearly as large. It may be the case that in hospitals with a majority of private insurance deliveries, it was easier for physicians to treat the minority of Medicaid cases like their privately insured patients.

2.8.1 Medicaid Managed Care in California

Prior to 1994, few Medicaid recipients in the state of California were in any type of managed care plan. As a result, most Medicaid deliveries were covered by traditional fee-for-service Medicaid where the state government directly reimbursed hospitals, physicians, pharmacies and other health care providers for the costs associated with the medical care they provided.

In 1993, the director of the state's Department of Health Services, in response to rising health care costs, devised a plan to increase the share of Medicaid patients on managed care plans. The goal of the plan was to increase to 50 percent the fraction of Medicaid recipients in managed care plans by the end of 1996.

Under this new initiative, enrollment in managed care plans would be done at the county level and each county would be part of one of three types of managed care structures: a County Organized Health System (COHS), a two-plan model, or a geographic managed care model (GMC). Under COHS, managed care plans are run by the county. In the Two-Plan model, one of the options would be a county plan and the other would be a private insurance plan that has a contractual obligation to insure county residents in their plan. In the GMC option, there is no county-provided managed care option and residents can only choose a privately contracted Medicaid managed care plan.

Prior to 1993, two counties (Santa Barbara and San Mateo) had COHS options and under the new plan, three more counties would adopt this system (Orange, Santa Cruz and Solano). A total of 13 counties would adopt the two-plan models and the remainder of the counties would be assigned the GMC type. Based on Duggan (2004), the share of California Medicaid recipients enrolled in a managed care plan increased from 10% in 1991 to 51% in 1999.

Medicaid deliveries are excluded from the coverage of early discharge law based on California Newborns' and Mothers' Health Act of 1997 (NMHA). A memo from Department of Health Services on January 16, 1998 specifies the application of NMHA to the three models of Medicaid managed care. Given the legislation that defined the charters for managed care plans, the memo concludes that Medicaid managed care plans are covered by NMHA if the managed care plan is a private plan. This includes Medicaid patients under the private insurance plan of Two-Plan model and all Medicaid patients under GMC model since this model contracts with different private plans to provide services to Medicaid patients. The memo further concludes that county-run Medicaid managed care plans in the COHS and Two-Plan models are *not* covered unless a county-run Medicaid managed care plan contracts with private plans to provide care.

The California State Inpatient dataset that we use identifies which deliveries are covered by Medicaid, but unfortunately, the data set did not start to report whether Medicaid deliveries were under the coverage of Medicaid fee-for-service or Medicaid managed care till year 1999. So for the critical period between September 1997 and January of 1998 when the state law and federal law became effective, we do not have information about the specific Medicaid type of Medicaid deliveries. More importantly, when we do have data on managed care enrollment in the 1999 and after period, we do not know in two-plan counties whether the patient was enrolled in the county-based or private managed care plan.

We do not have individual level data about their specific type of Medicaid insurance, but we know the Medicaid insurance model of each county. Based on this information, we might be able to test our hypothesis by checking effect of laws on counties with different types of Medicaid insurance. Table 3B in Duggan (2004) and Table 1 in Aizer *et al.* (2004) list the start dates of different Medicaid Managed care models in different counties. Based on these lists, counties could be classified as three types, counties covered by Medicaid fee-for-service plus counties with other Models of Medicaid coverage but percent of Medicaid Managed care counties less than 10 percent, counties covered by Medicaid COHS model and with higher than 10 percent of Medicaid managed care coverage, and counties covered by Two-Plan and

GMC models and with higher than 10 percent of Medicaid managed care coverage. Since we are studying the effect of early discharge laws on different types of Medicaid, we make sure the start date of Medicaid managed care plans are earlier than September 1997. Also, our data reports whether Medicaid patients are under the coverage of Medicaid fee-for-service or Medicaid managed care from 1999, we check this information for different counties and conform that our Medicaid fee-for-service counties have no patients under the coverage of Medicaid managed care in year 1999.

For the reduced model (1), we do separate regressions to deliveries in the three types of counties, Medicaid fee-for-service counties and counties with percent of Medicaid managed care less than 10 percent, Medicaid COHS counties (with higher than 10 percent of Medicaid managed care coverage), and Medicaid Two-Plan and GMC counties (with higher than 10 percent of Medicaid managed care coverage).

Table 2-8 presents regression results of model (1) using four sub samples but only for Medicaid fee-for-service counties, vaginal and cesarean deliveries without and with complications. Results for two outcomes, percent discharged early, and length of stay in days are reported.

We are focusing on the effect of laws on Medicaid deliveries. For Medicaid fee-for-service covered uncomplicated vaginal deliveries, we find that the California law reduced early discharge rates of newborns by 7.09 percentage points, and reduced early discharge rates of newborns by 6.44 percentage points for complicated vaginal deliveries. The standard errors on these estimates are incredibly small and the results are statistically significant at conventional levels. For state law, we find that the reduction of early discharge rates of newborns was 2.62 percentage points for uncomplicated vaginal deliveries and 1.01 percentage points for complicated vaginal deliveries and both results are not statistically significant.

For cesarean deliveries, neither federal law nor state law had statistically significant effect on Medicaid fee-for-service deliveries. The federal law and the state law even increased early discharge rate of both complicated and uncomplicated deliveries although the results are not statistically significant.

In the next column of results, we report length of stay measured in days for Medicaid fee-for-service counties as an outcome variable and the results in this column parallel those for the discharged early outcome. For Medicaid fee-for-service covered deliveries, on average the federal law increased length of stay by 0.08 days among uncomplicated vaginal deliveries and had no statistically significant effect on length of stay of complicated vaginal deliveries. For Medicaid fee-for-service covered cesarean deliveries, neither federal law nor state law had a statistically significant effect on postpartum length of stay of newborns.

So for Medicaid fee-for-service covered cesarean deliveries, the results are as our hypothesis, they were not covered by the early discharge laws, so the early discharge laws had no statistically significant effect on the postpartum length of stay of those newborns. However, for Medicaid fee-for-service covered vaginal deliveries, they were affected by the law although the laws excluded them from coverage. This could not be explained by our hypothesis.

Table 2-9 reports regression results of model (1) for Medicaid COHS counties. We are still focusing on effect of early discharge laws on Medicaid deliveries. For results of outcome variable – whether length of stay is less than mandated time, the federal law reduced early discharge rates of newborns by 20 percentage and 24 percentage points respectively for uncomplicated and complicated vaginal deliveries, and the impact of state law was half as large for both samples. The federal law also had statistically significant effects on Medicaid COHS covered cesarean deliveries and reduced early discharge rate by 4.53 percentage and 10.26 percentage points respectively for uncomplicated and complicated cesarean deliveries. For results of the outcome variable – length of stay in days, for all samples the federal and state law increased length of stay in days by a statistically significant amount except that for uncomplicated cesarean deliveries, the state law had no statistically significant on length of stay of Medicaid deliveries.

Although for Medicaid COHS model covered deliveries, we find the laws had statistically significant effect on their length of stay. Our hypothesis that the reason Medicaid deliverers were affected is that Medicaid managed care covered deliveries were treated as privately insured deliveries could not be proved by the results, since if county-run Medicaid managed care plans in COHS model contracts with private plans to provide care, they would be covered by early discharge laws. We do not have detailed information about how each Medicaid COHS model county contracts with private plans, so with contracted private plans covered by the law, we could not differentiate whether the effect of the laws on length of stay was due to the coverage of law on contracted private plan or due to that the possibility deliveries insured by Medicaid county-run plan were treated as privately insured deliveries although they are not covered by the law.

Table 2-10 reports regression results of model (1) for Medicaid Two-Plan and GMC counties. We are still focusing on effect of early discharge laws on Medicaid deliveries. For results of outcome variable – whether length of stay is less than mandated time, the federal law reduced early discharge rates of newborns by 10 percentage points for uncomplicated and complicated vaginal deliveries and reduced early discharge rate of newborns by 1 percentage points for complicated cesarean deliveries, while the federal law had no statistically significant effect on early discharge rate of uncomplicated cesarean deliveries. For results of outcome variable – length of stay in days, for all samples the federal law increased length of stay in days by a statistically significant amount and the state law had no statistically significant on length of stay of Medicaid deliveries except for complicated cesarean deliveries.

Since Medicaid managed care plans are covered by NMHA if they are private, and this includes Medicaid patients under the private insurance plan of Two-Plan model and all Medicaid patients under GMC model since the GMC model contracts with different private plans to provide services to Medicaid patients, although for Medicaid Two-Plan and GMC model covered deliveries, we find the laws had statistically significant effect on their length of stay. The results we get are total effect of different plans, so as Medicaid COHS, our hypothesis that the only reason Medicaid deliveries were affected is that some Medicaid managed care plan were covered by the law still could not be proved. Comparing results in Table 2-9 with Table 2-10, the early discharge laws had a bigger effect on Medicaid COHS covered deliveries than Medicaid Two-Plan and GMC covered deliveries. Since we do not have the proportion of different Medicaid managed care models in each county, we could not explain why the result happened.

There is one thing to note about results in Table 2-9 and 2-10. For Medicaid COHS counties, and Medicaid Two-plan and GMC counties, there were still high proportions of Medicaid fee-for-service deliveries since the transition from Medicaid fee-for-service to Medicaid managed care was a long process. Based on results from regressions of model (1) on counties with different models of Medicaid deliveries, we could not fully prove our hypothesis. To prove our hypothesis, we need to see effect of law only on samples with Medicaid managed care that they are covered by early discharge laws. We could get a definite conclusion from our results that for Medicaid fee-for-service counties, Medicaid vaginal deliveries were affected by the law statistically significantly although they were not covered by the law. This result could not be explained by our hypothesis. We are trying to explain this by the hypothesis about behavior of physicians.

2.8.2 Behavior of Physicians

The early discharge laws only apply to privately insured deliveries. The distributions of Medicaid deliveries are various across hospitals. For hospitals with very low fraction of Medicaid deliveries, the ability of physicians to modify their behavior based on insurance type might be limited, and physicians might treat Medicaid deliveries as privately insured deliveries and hold mothers in hospital as the mandated time of the early discharge laws after early discharge laws became effective. So their postpartum lengths of stay were affected by the laws. For hospitals with very high fraction of Medicaid deliveries, physicians might acknowledge the fact that Medicaid deliveries are not covered by California and Federal early discharge laws and did not adjust their treatment to Medicaid deliveries, and the Medicaid deliveries were not affected by the laws. We would like to test this hypothesis by estimating the following model:

$$(3)\ Y_{ikt} = NEWBORN_{ikt}\beta_1 + PARENTS_{ikt}\beta_2 + HOSP_{ikt}\beta_3 + STATELAW_t{*}PAYER_i\alpha_1\ FEDLAW_t{*}PAYER_i\ \alpha_2 + HSAREA_k{*}HOSP\_SIZE_k{*}PAYER_i{*}MTREND_t\beta_4 + HSAREA_k{*}HOSP\_SIZE_k{*}PAYER_i\ \beta_5 + INDEX90180_t\beta_6 + MONTH_t + DAY_t + \varepsilon_{ikt}$$

We still restrict our regressions to Medicaid deliveries. PAYER is 1 where faction of Medicaid deliveries is less than 29.5%, the 25 percentile of faction of Medicaid deliveries; PAYER is equal to 2 when faction of Medicaid deliveries is bigger than 29.5% and smaller than 53.7%; PAYER is equal to 3 when faction of Medicaid deliveries is within 53.7% and 74.9%, and PAYER is equal to 4 when faction of Medicaid deliveries is higher than 74.9%.

Table 2-11 reports the regression results of model (3). For Medicaid covered uncomplicated vaginal deliveries, we find that the federal law reduced early discharge rates of newborns by 15.17 percentage points, 12.35 percentage points, 8.08 percentage points and 10.74 percentage points respectively for PAYER 1, 2, 3 and 4. The standard errors on these estimates are incredibly small and the results are statistically significant at conventional levels. For payer is 1, 2 and 3, the results are as our expectation, with the increase of faction of Medicaid deliveries in a hospital; the Medicaid deliveries were less affected by early discharge laws. For payer 4, the results is not as our expectation, and the law still has effects on early discharge rates of Medicaid deliveries and this may be caused by the effect of early discharge laws on Medicaid managed care covered deliveries. For complicated vaginal deliveries, we could see the similar pattern of effect of federal law.

For cesarean delivery samples, federal early discharge law reduced early discharge rates of newborns by 6.37 percentage points, 5.42 percentage points, and 0.38 percentage points respectively for PAYER 1, 2 and 3. For payer 4 where there were very high factions of Medicaid deliveries, instead of decreasing early discharge rate, the federal law increased early discharge rate by 2.31 percentage points and the results are not statistically significant. For cesarean deliveries with complications, the effect of federal law was smaller, and we could see the similar pattern that the coefficient on the (Federal Law x Payer) variable increases monotonically as Payer variable moves from 1 to 4. The state law is estimated to not have reduced early discharges for c-sections by a statistically significantly amount in either sample.

Table 2-11 also reports results with length of stay of newborns as outcome variable. For vaginal deliveries, federal law increased average length of stay of newborns although the results do not decrease monotonically as Payer variable moves from 1 to 4. For cesarean deliveries, the federal law increased average length of stay of newborns for payer 1, 2, and 3. For payer 4, the federal law decreased average length of stay of newborns although the results are not statistically significant. That is, we get the similar results as regressions with early discharge rate as outcome variable. For cesarean deliveries in hospitals with very high fraction of Medicaid deliveries, they were not impacted by the law

From the results, we do get evidence that behavior of physicians were affected by fraction of Medicaid deliveries in the hospital. The absolute effect of laws decreases monotonically as fraction of Medicaid deliveries increased. Especially for cesarean deliveries, in hospitals with very high fraction of Medicaid deliveries, early discharge laws even increased early discharge rates and decreased average length of stay although the results are not statistically significant. However, the hypothesis of behavior of physicians could not explain why for vaginal deliveries, Medicaid deliveries were also affected by the law when Payer is equal to 4, that is, faction of Medicaid deliveries were very high.

2.9 Conclusion

In this chapter, we use a large change in postpartum length of stay generated by the passage of a state and federal law to examine the effect of longer stays on the health of newborns. We show that the impact of the induced increase in length-of-stay varied substantially across patients with the increase largest for vaginal deliveries and patients covered by private insurance. However, we find large changes in length of stay for Medicaid patients who were not covered by the law and for patients whose mothers experienced complications during pregnancy or labor. In all, the federal law is estimated to reduce the fraction of vaginally delivery newborns covered by private insurance by 27 to 30 percentage points privately insured c-section delivered infants by 12 to 14 percentage points.

At the same time, we witnessed reductions in the fraction of babies with 28-day readmission rates. 2SLS estimates suggest that for children whose mothers had cesarean deliveries or had complications during pregnancy or labor, an extra day in the hospital will reduce readmission rates by one percentage point which is 30 to 10 percent of sample means. In contrast, we do not find any statistically significant medical benefit for infants whose mothers had vaginal deliveries without complications.

It is worth noting that there are a number of limitations to the current study. First, we are studying only one state. To the extent that the effect of these laws differed in other states the results presented here may not generalize. Second, we do not have the detailed information about follow-up visits for early discharge deliveries, and thus can only speculate that the reductions in readmission rates are produced by longer stays and not the subsequent follow-up visit.

Chapter 3: Hospital Union Certification Elections and Outcomes of Nursing Personnel

3.1 Overview

Between 1954 and 1998, the fraction of workers that were union members declined from 33.5 to 13.3 percent (Farber and Western 2000). The decline in unionization is fairly widespread in the economy with rates falling in the manufacturing, construction, transportation, communication, and public utility industries. One industry that has bucked this trend is however hospitals. In 1961 only 3.2 percent of all nonfederal hospitals had formal collective bargaining agreements. This low unionization rate in this sector was determined in part by Federal law that prohibited unions from organizing in not-for-profit hospitals. In 1974, this restriction was relaxed and unionization rates have increased dramatically. By the middle of 1970s, one of every five hospitals had collective bargaining contracts and the percent of the hospital labor force unionized stood at 22.4 percent (Becker and Miller 1981). By the end of 1990s, roughly 35 percent of hospitals had formal registered nurse collective bargaining agreements (Ash and Seago 2002).

Although the legal environment allowed many more hospitals to be represented by collective bargaining, changes in the structure of the health care market have also encouraged hospital nurses to seek collective bargaining. Over the past 40 years, the health care sector has experienced rising healthcare costs, growing government involvement in health care through the Medicare and Medicaid program, the emergence of managed health care, and accelerating technological advancement. These forces have produced remarkable changes in the market structure of the hospital sector, with many hospital mergers and restructurings. For nursing personnel, especially Registered nurses (RNs), the changing market structure also produce job restructuring which usually included reduced staffing levels, increased workloads, and expanded responsibilities (Greiner 1996). As a result of these workplace changes, nursing personnel indicated the desire for a greater voice in the hospital organization (Clark 2001).

With collective bargaining, unions negotiate with hospitals not only on wage and benefits, but also on minimum staffing levels, overtime hours and employment of nursing personnel such as, the ratio of RNs to Licensed Practical Nurses (LPNs) and aides.[20] Since labor costs are the largest components of hospitals costs, and Needleman *et al.* (2002) found that the quantity and quality of nursing care are likely to be highly related to outcomes of patients, it is very important to examine the effect of unionization on wage and employment of nursing personnel and outcomes of patients.

Although there is a large literature in economics that examines the economic impact of unions on a variety of outcomes, such as wages and productivity, there is a fairly small literature examining the same issues for hospitals. A few papers have examined the impact of hospital unions on wages, and almost all of them are cross

[20] Registered nurse (RN) is a graduate nurse who has passed examinations for registration; Licensed Practical nurse (LPN) is a nurse who has enough training to be licensed by a state to provide routine care for the sick; Aides is someone who assists a nurse in tasks that require little formal training. So RN is better educated than LPN, and LPN is better educated than aides.

sectional-studies using individual-level data. Only two papers have examined the impact of unions on product quality. Seago and Ash (2002) argue that union representation may affect quality of care to patients through several mechanisms. Unions could improve the quality of care by raising wages, decreasing turnover, negotiating increased staffing levels, or facilitating communication between nursing personnel and management. However, unions could jeopardize patient safety by creating an adversarial work environment, decreasing management flexibilities and raising wages.

All of the previous published papers that examined the impact of nurse unions on labor outcomes are cross sectional studies that treated the union variable as exogenous in regression models. As we demonstrate below, this is a questionable assumption. Hospitals experiencing a union certification election and those covered by collective bargaining are very different from other hospitals. Based on union certification election reports from National Labor Relation Board (NLRB), union certification elections were more likely to occur in non-profit hospitals and hospitals with larger numbers of discharges. Among hospitals holding union certification elections, smaller hospitals are more likely to win the election. Union size has also shown in other contexts to predict a number of outcomes. At the individual level, nurses with certain characteristics such as higher wages may be more likely to select into unions. Therefore, an econometric model that examines this question must address the possibility that nurses and hospitals covered by collective bargaining are not a representative sample of hospital employees.

In this paper, we examine the impact of unionization on the wages and employment of nursing personnel, controlling for the possible non-random selection of people into unions and hospitals into union status. We merge unionization and bargaining contract information from National Labor Relation Board (NLRB) Federal Mediation and Conciliation Service (FMCS) to hospital level data from the California Hospital Annual Financial. With these data sets, we use a basic difference-in-difference model to examine the impact of unions on employment outcomes such as nurse staff levels, average hourly earnings, hours and the use of contract employees. The panel nature of the data allows us to examine hospitals before and after union certification, using hospitals not subject to collective bargaining as controls. The use of hospital fixed-effects allows us to purge the unobserved hospital-specific characteristics that may have led to union certification in the first place.

After unionization, we find great utilization of aides who are lower-skilled nursing personnel compared to RNs and LPNs and large increase in the use of contract nursing personnel, who are not usually union members, relative to regular nursing personnel after hospital employees lost unionization elections. The staffing ratio of nursing personnel per discharge or per bed days were not statistically significantly affected by unionization. By comparing results from difference in difference regressions with cross sectional regressions for average hourly earnings of nursing personnel, we do see evidence of selection that unions are more likely organized in higher paid hospitals. In contrast to the vast literature on unionization in many other sections, we find that the average hourly earnings of different nursing personnel decreased after hospital unionized. This result could be caused by less usage of expensive overtime hours after hospital won the unionization elections.

We also examine the impact of unions using individual-level data from the Out-going rotation samples of the Current Population Survey (CPS). Respondents in the CPS are interviewed for the same four months over a two year period. During the fourth and eighth months in the survey, which are exactly a year apart, respondents are asked detailed questions about employment including hours of work, weekly earnings and union-status. By merging observations across the year, we construct a one-year panel for CPS respondents. Using respondents who change unionization status during the year as a treatment group, we estimate a difference-in-difference with people not changing union status as a control group. In this analysis, we do not see evidence of selection by nurses into union status in that OLS and fixed-effect estimates are very similar. We find small positive effects of unions on wages and little of any impact on hours of work. We attempt to reconcile the difference in the results across the two samples.

3.2 Literature Review

3.2.1 Literature on Hospital Unionization

The rapid increase in hospital unions occurs primarily in the late 1960s and 1970s. In 1961, estimates indicate that only three percent of all hospitals had labor agreement with their employees (AHA 1972). By 1970, the figure approached 15 percent and for certain sectors, such as federal government hospitals, the corresponding number were in excess of 50 percent. By the mid-1970s, one in every five hospitals had contracts and the percent of the hospital labor force unionized was on the order of 22.4 percent. By 1980, 27.4 percent of the nation's hospitals (Beckerm 1982) and over 20 percent of registered nurses (Brint and Dodd 1984) had collective bargaining contracts. By 2000, based on collective bargaining contract expiration notices from Federal Mediation and Conciliation Service (FMCS), around 35 percent of the nation's hospitals have collective bargaining contracts.

Part of the growth in hospital unions has been generated by changes in Federal legislation. In 1947, the Taft-Hartley Act excluded nonprofit hospitals from the definition of employer in the National Labor Relations Act (NLRA). In 1974 amendments to the Taft-Hartley Act, private non-profit hospitals and other healthcare institutions were brought under the Jurisdiction of the National Labor Relations Board (Farkas 1978). Thus, nearly half of the industry's 7,000 hospitals (1976) employing 1.6 million workers were subject to the representation machinery and organizing protection of federal law.

There are three basic ways that hospital employees can obtain union representation (Becker and Miller 1981). First, an employer can voluntarily consent to recognize a labor organization. Second, an already-unionized industry or company expands its employment. Third, a union can be successful in a representation election. Voluntary recognition is nonexistent in the hospital industry, and, although the industry has been expanding, the healthcare industry had been lightly unionized in the recent past. Moreover, the incidence of union security agreements that would provide a device for automatically recruiting members is also low (BNA, 1976). As a consequence, membership has grown primarily through representation elections.

If a group of workers gain legal recognition as provided for by the National Labor Relation Act (NLRA), they are legally protected from being fired for association with a union and can only be "replaced" under specified conditions; most importantly, the law dictates that the employer bargain with the union "in good faith".

In the typical situation where union recognition is achieved through an election, employers are thought to generally oppose the organization drive (Kleiner 2001) because the union could have been voluntarily recognized by the employer. At the same time, there is a sense that in the healthcare industry, efforts to unionize are driven primarily by employees' low job satisfaction, especially among nursing personnel. For example, Clark *et al.* (2001) reported the results of a survey of the workplace experiences of hospital based RNs under healthcare reform and found that nurses who had experienced reform-related mergers or job restructuring held a more negative perception of the climate for patient care, and indicated a greater readiness than other nurses to vote for a union. Breda (1997) showed that the main impetus of unionization were factors such as poor working conditions, frustration with hospital management, low staff patients ratio and denial of an autonomous professional practice, not poor wages.

The notion that union certification elections are driven solely by worker disaffection is however a rather narrow view of the process. Ashenfelter and Pencavel (1969) adopted an alternative and more parsimonious approach suggesting that, 'an employee's decision to join a union will depend upon his subjective assessment of the expected benefits to be obtained from union membership against the subjective assessment of the expected costs of membership'. Consistent with this notion of union elections, nursing personnel (RNs, LPNs and aides) who comprise a large and significant employee group of hospitals are turning more to unionization as means of upgrading their position in hospitals (Wilson *et al.* 1990). Aside from allowing collective bargaining in nonprofit institutions after 1974, a number of other forces such as the rise of managed care especially health maintenance organizations (HMOs), legislative action to contain costs through Diagnostic Related Groups (DRGs), and hospital merger and hospital restructuring, have raised concerns among hospital personnel about compensation and job security issues. More and more frequently, hospital employees are resorting to union organizing efforts as a means to address these concerns.

3.2.2 The Effect of Hospital Union: A Literature Review

Only a few researchers have examined the effects of union on the wages of registered nurse (RN). Most of these have used cross sectional samples and these studies typically demonstrate that hospital unions have a modest impact on wages of registered nurses. Feldman and Scheffler (1982), for example, used data from a national probability sample of 1,200 hospitals in 1977 to estimate the effect of unions on wage and fringe benefits in four occupations: registered nurses, practical nurses, secretaries, and housekeepers. Unions are estimated to increase wages by about 8 percent for both types of nurses and 11 to 12 percent for secretaries and housekeepers employed in hospitals.

Using data from questionnaires sent to 3,982 short-term general medical-surgical hospitals, NLRB election reports and other union records, Schwarz and

Koziara (1992) examined how the presence and number of bargaining units within a hospital affect wages and the frequency of strikes in hospitals. They found little evidence that multiple bargaining units raised wages and strikes.

Using data extracted from the 1985 through 1993 Current Population Survey (CPS), Hirsch and Schumacher (1995) obtained union premium for all RNs of about 3 percent (and for hospital RNs about 2 percent), while for LPNs and nursing aides are 4.6 and 12.4 percent. Also using 1973 through 1994 CPS dataset, Hirsch and Schumacher (1998) found that standard union premium estimates are substantially lower among workers in health care than in other sectors of the economy, and smaller among higher-skill than among lower-skill occupations.

Results from studies that have examined the impacts of unions on benefits and working environment of nursing personnel are also mixed. Using data from questionnaires returned from 144 short term general care hospitals in Illinois, Minnesota, and Wisconsin, Becker (1979) found that direct union effects on fringe benefits (value of all fringe benefits, including pension, 'time off' with pay, etc.) were around 8.8 percent while indirect or spillover effects are on the order of 3.2 percent. Sloan and Elnicki (1978) found neither RN staffing nor RN turnover were significantly affected by unionizations. Salkever (1982) found that unionization increased production costs by 5 to 9 percent, with the bulk of this increase resulting from factors other than wage increases, however, Salkever (1982) did not examine the mechanism how unionization affects production costs other than through wage increase. Cost impacts were also found to be greater for national unions and where cost-based payment is more prevalent, and smaller for RNs and other service employees.

There are several mechanisms through which unions may affect the quality of care received by patients. Results from Aiken *et al.* (1999) showed that higher nurse to patient ratios and AIDS physician specialty services were strongly associated with lower mortality. A review of the literature by Lang *et al.* (2004) concluded that although the literature offers no support for specific minimum nurse-patient ratios for acute care hospitals, total nursing hours and skill mix do appear to affect some important patient outcomes. Organization of care affects patient outcomes and the presence or absence of a union may affect the organization of care, unions may impact patient outcomes. Also noted above, the main impetus of unionization were not low wages but instead of factors such as poor working conditions and low staff patients ratio, and therefore, it is reasonable to expect that unionization might impact patient outcomes if unions could alter these hospital characteristics.

Little systematic research has examined whether the presence or absence of nurse unions affect patient outcomes. Seago and Ash (2002) examined the relationship between the presence of a bargaining unit for registered nurses and the acute myocardial infarction (AMI) mortality rate for acute care hospitals in California. Their study found that having an RN union significantly lowered the risk-adjusted AMI mortality rate. The authors did not however identify the mechanism how unions improved outcomes.

In a more recent paper, Ash and Seago (2004) examined the same question as in their previous study but in this more recent paper, they account for the possibility that unionized hospitals have certain important but unobservable characteristics, independent of unionization, that affect patient care. By doing specification tests like checking the effect of non-healthcare union or the effect of future unionization, they found results similar to those in their previous paper, that hospitals with unionized RN's have 5.5 percent lower heart attack mortality than do non-union hospitals. The variable for union in the data set they used is registered nurse union status, so this variable contains little information on how and when the union was formed. With the cross-sectional data instead of panel data, they could not compare the effect of union before and after the hospital became unionized.

To summarize, the bulk of the estimates to date suggest that nurse unions raise wages but the relative wage impact is rather small, on the order of a few percentage point changes in wages. There have been few studies that have examined how unionization affects the employment of nursing personnel, and to validate the benefits or hindrances of unionization on nursing personnel and patients. All of the studies mentioned above have however been cross-sectional in natures that have treated union status as exogenous. Therefore, if unions are more likely to appear in hospitals with particular characteristics (e.g., hospitals with already higher wages) then these cross-sectional models will provide inconsistent estimates of the impact of unions.

3.3 Empirical Methodology

3.3.1 Process of Hospital Union Certification Elections

Moving from initial discussions about union organization until certification is a long and difficult process. Dinardo and Lee (2004, p. 5), provide a brief summary of prototypical timeline of the union representation process.

“1. A group of workers who interested in being represented by a union contact a labor union and ask for assistance in beginning an organizing drive.

2. The employees begin a “card drive” to petition the NLRB to hold an election. Unions need to get cards from at least 30% of the workers to be granted an election by the NLRB.

3. After the card have been submitted, the NLRB makes ruling on whether the people the union seeks to represent have a “community of interest”, a coherent group for the purposes of bargaining and makes a determination of which categories of employees fall within the union’s “bargaining unit”.

4. Then, the NLRB holds an election at the work site. A simple majority (50 percent plus 1 vote) is required for the union to win.

5. Within 7 days after the final tally of the ballots, parties can file objections to how the election was conducted. With sufficient evidence that the election was not carried out properly, the NLRB can

rule to invalidate the outcome of an election and conduct another one thereafter.

6. If after this, a union still has a simple majority, then the union is certified as the exclusive bargaining agent for the unit, and the employer is obligated to negotiate "in good faith" with that union."

At any point in the certification process, the union organization effort could be terminated. After a group of hospital employees decide to start the certification process, in most cases, employers and management would resist union organizing drives. Based on Bronfenbrenner (1994), multiple tactics could be adopted by employers to delay or deny a collective bargaining agreement, with the most often used being "captive meeting,"[21] "firing union activists", "hiring management consultants" and "alleging other unfair labor practices". "Firing union activists" refers to the situation that large number of campaigns never made it to an election because the employer discharged workers early in the union campaign even though the NLRB has the power to order reinstatement of those discharged workers. Bronfenbrenner (1994, p.81), notes "the workers were reinstated before the election in only 34% of the campaigns in which there were discharges for union activity." Examples of "alleging other unfair labor practices" are interfering with the formation of labor union, and discriminate against employees for engaging in union activities, etc. Many union organizing attempts would be terminated at this point.

[21] A captive meeting is an "all hands" meeting where the employer bring all the eligible employees in a union vote together to do the following: tell the employees how disappoint he is in this union effort; tell the employees that a union – third party between the employer and employees are not necessary, tell the employees some 'facts' about union and ask for another chance to make things better.

Even if a union certification election is held, the outcome of the election would be determined by multiple factors. Becker and Miller (1981) divided the possible influences into organizational characteristics such as hospital sizes, and employee-management relations, environmental characteristics such as the unionization status of nearby hospitals, the nature of the outside union organization, etc., and the election process. Becker and Miller also found that unions were more likely to win the elections in hospitals with a winning history, hospitals with lower voter turnout (since there is not a great deal of leeway for management action to get out the vote) and smaller unit size.

As the previous paragraphs indicate, the union certification process is long and difficult and the results are very uncertain from the start. The probability of holding a union certification election and a union election victory reflects the interests of three actors, the employees, the outside union organization, and management, and their respective power and ability to implement those interests. Given these diverse interests and the difficulty of the process, it should be no surprise that unions are more likely to appear in some types of hospitals.

Are there ex ante systematic differences among union election winners, losers and hospitals with no union elections? The size of hospitals and the organizational characteristics of hospitals like whether the hospitals are for profit or not for profit have been examined, and they do matter. Table 3-1 lists the characteristics of hospitals in California who are unionized or not in year 1999 by using California Hospital Annual Financial data linked with National Labor Relation Board (NLRB) and contract expiration notices from Federal Mediation and Conciliation Service

(FMCS). This data is outlined in more detail below. Hospitals with more beds and more discharges were more likely to be unionized. Hospitals owned by church or other non-profit organization were less likely to be unionized, and hospitals owned by a county government were more likely to be unionized.

Table 3-2 lists statistics of hospitals in 1998 that won or lost union certification election during the 1990 to 2000 period. In contrast to the results from the previous table, the characteristics of winners and lowers were much more similar. Among hospitals that held union certification elections, hospitals with fewer beds and fewer discharges were, on average, more likely to win the elections, although the differences in the beds and number of discharges between those two kinds of hospitals are not statistically significant. In general, there are far fewer differences between hospitals with winning and losing elections than there are when comparing union and non-union hospitals. Subsequently, disparities in characteristics between union and non-union hospitals are generated by which hospitals start and make it to the certification process, not which hospitals win union elections.

These results suggest hospital unions appear in a select type of hospital and therefore, care must be taken to control for hospital characteristics in OLS regressions of wages on union status. For example, Table 3-1 indicates that unions are more likely to appear in larger hospitals and for profit hospitals. Previous research has also established that wages tend to be higher in exactly these types of hospitals. Inadequate controls for hospital characteristics may then lead to an overestimation of the union wage effect. If the union status of hospitals is not random and certain types of hospitals are more likely to be unionized, then the OLS regression may be biased and inconsistent. For this reason, we choose to exploit the longitudinal nature and use a fixed effect and difference-in-difference model to estimate the union impacts. The fixed effect model purges the difference between unionized and non-unionized hospitals,

3.3.2 Data Description

The econometric model we use throughout this paper is a standard difference-in-difference model where we compare outcomes of hospitals before and after union certification elections to the same differences for hospitals that do not change their union status. The panel nature of the data allows us to hold constant unmeasured hospital specific factors that may be correlated with both the propensity to organize and labor outcomes.

The econometric model requires a panel data set of hospitals and time-varying information on union status. We constructed just such a data set by combining several data sets including the National Labor Relation Board (NLRB) election reports for hospital union certification elections held from 1984 to 2000, contract expiration notices from Federal Mediation and Conciliation Service (FMCS) from 1978 to 2001 through a Freedom of Information Act (FOIA) request, and the California Hospital Annual Financial data, 1980-2002.

The records from the NLRB have information such as the dates of the filing of the petition, the election, and the closing of the case, eventual vote tallies, as well as the employer characteristics such as the size of the voting unit, and the primary industry of the establishment in question. Also, the records contain the establishment name and exact address.

From the FMCS data, we obtain information on contract expiration notices which are required by Federal law to be filed. According to the U.S. Code of Federal Regulations (29 CFR 1425.2), "In order that the Service may provide assistance to the parties, the party initiating negotiations shall file a notice with the FMCS Notice Processing Unit … as least 30 days prior to the expiration or modification date of an existing agreement, or 30 days prior to the re opener date of an existing agreement…". Thus, the contract expiration notices filed from the establishment provide us with a measure of collective bargaining "activity" both before and after the election. More importantly, this data identifies for us hospitals that are unionized before the start of our sample period.

Within four months of their fiscal (accounting) year end, California licensed hospitals must submit an annual financial report that includes a detailed income statement, balance sheet, statements of revenue and expense, and supporting schedules. These financial reports are based on a uniform accounting and reporting system developed and maintained by the California Office of Statewide Health Planning and Development (OSHPD) and are undergone a thorough desk audit. The report includes the average hourly earnings, productive hours (hours actually worked) and nonproductive hours (paid time off, including vacation, sick-leave, and holidays) for different categories of personnel, such as Registered Nurses (RNs), Licensed Practical Nurses (LPNs), aides and orderlies which are the groups of nursing staff we focus on in this study.

As Spetz *et al.* (1999, 2001) and Currie *et al.* (2005) reported, the OSHPD financial data are quite noisy. Non-standard reporting periods and multiple reports in a single year are some of the most important problems. In this study, we solved the problem of noisy data by deleting the observations with non-standard reporting period and multiple reports in a single year and only leaving the full year observation. Over 20% observations were deleted with this step.

Based on the information of election reports from NLRB, Figure 3-1 depicts the change of total union election cases and union election cases that won or lost elections in U.S. hospitals from year 1984 to 2000. The numbers in Figure 3-1 indicate that the annual number of union elections held was relatively stable throughout the 1980s and 1990s with a slow drift downwards in total counts during the period. Starting in the early 1990s, the fraction of cases won by the union rose slightly.

The unionization information of hospitals from NLRB election reports and FMCS contract expiration notices are merged with California Hospital Annual Financial data by hospital name and address. From 1992 to 2000, 31 hospitals held union certification elections, of which, 20 hospitals won the elections and successfully organized union. At the end of 1999, there were 416 hospitals reporting financial data, and among them, 123 hospitals or 30% hospitals were unionized hospitals. This unionization rate is lower than 35% hospitals unionization rate from Ash and Seago (2002). The reasons are that rehabilitation hospitals, long term care hospitals and Kaiser Foundation hospitals which have higher unionization rates, do not report their financial data. It may also be the case that some hospitals might fail to report contract expiration notices to FMCS. We end up with annual level observations for each hospital level data set with unionization information and measures of outcomes of nursing personnel.

3.3.3 Outcome Variables

There are several variables that we use to measure the earnings and employment of nursing personnel. The first group is the average hourly earnings of RN, LPN and aides. The earning information in the California Hospital Financial data set is the average hourly earnings paid that is constructed by summing total wages paid and dividing by all productive hours. Overtime pay and premium pay for on-call or stand-by time are also included in salaries and wages. Productive hours are the actual hours worked which equals total hours paid (including overtime) less the hours not on the job. Hours not on the job include vacation time, sick time, holidays and other paid time-off. Non-productive hours are counted as employee benefits and are hours not on the job such as vacation time, sick leave, holidays, and other paid time off.

The next outcome measures are the utilization of productive hours and the fraction of hours for different types of nursing personnel: RNs, LPNs and aides. RNs are more highly trained than LPNs, and LPNs are more skilled than aides. Productive hours are actual working hours and include regular working hours and overtime hours. The fraction of hours worked by each type of nurse is calculated as the hours of nursing for each type divided by the sum of productive hours for all nurses. One concern in recent years is that hospitals may have replaced higher-skilled nursing personnel like RNs with lower-skilled nursing personnel like LPNs and aides.[22] Needleman *et al*. (2002) found shorter length of stays and lower rates of urinary tract infections when care was provided by RNs instead of LPNs or aides. Unruh (2003a) found a greater incidence of nearly all adverse events occurred in hospitals with fewer licensed nurses, so the welfare of patients might be jeopardized by the utilization of lower-skilled nursing personnel. If hospital unions raise the cost of some nurses more than others, then hospitals may respond by hiring a different mix of nurses.

Labor costs are a large fraction of total cost for hospitals.[23] Previous work by Salkever (1982) has argued that that hospital labor cost increases after hospital become unionized. Therefore, labor cost on RNs, LPNs, and aides and relative expenditure on different types of nursing personnel are also outcome variables in this study. Hospital expenditures on RNs, LPNs and aides are calculated as hours multiplied by average hourly earnings.

Another outcome variable is the utilization of RN, LPNs and aides per discharge or per bed days. Unionization may encourage hospitals to change the absolute level of nursing resources used. However, since there is some evidence that relative inputs in hospitals impact outcomes, we also divide the inputs by outputs such as bed days.

The last outcome variable we examine is the utilization of contract nursing personnel. The California Hospital Financial data set starts reporting the average

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[22] Unruh (2001) found that during period 1991 to 1997, the utilization of licensed nursing staff declined while nursing assistants increased in Pennsylvania hospitals. Unruh (2003b) concluded that during period 1991 to 2000, there were a slightly decrease in the utilization of RN relative to LPN in Pennsylvania hospitals.

[23] Based on *Underlying causes of rising health care cost: hearing before the Committee on Finance*, United States Senate, One Hundred Third Congress, first session, October 6, 1993, on average, labor cost accounts for 46% of hospital total cost.

hourly earnings and hours of contract nursing personnel in year 1993. However, the data only reports the average wage and hours of contract RN, LVN and aides together. Since contract nursing personnel are usually not union members, they could be substitutes for more expensive unionized nursing personnel. Therefore, the utilization of contract nursing personnel is an important measure of effect of unionization on employment of nursing personnel. Since the contract nursing personnel information is only available year 1992 and on, we restrict all regressions to the 1992 to 2000.

3.3.4 Estimation Models

To measure the impact of union election result on labor outcomes of hospital employees, we use a basic difference-in-difference estimator. We would like to compare the outcomes of nursing personnel before and after the union certification elections. A simple comparison of outcomes for these workers before and after the treatment could produce a biased estimate of the impact of unions if factors that occur at the same time as the treatment are not adequately captured by covariates. For example, inflation-adjusted wages of nurses were falling in the 1990s so a simple difference estimator may understate the true impact of unionization on wages if these factors are not considered. To estimate the change in outcomes of nursing personnel that would have occurred in treated hospitals in the absence of intervention, we include hospitals that did not change their union status in the comparison group. This is a standard difference-in-difference model that compares the change in outcomes of nursing personnel before and after the treatment to the estimated change in outcomes of nursing personnel of control hospitals. The specific model we estimate is outlined in equation (1):

$$(1)\ Y_{it} = \theta_1 WON_{it} + \theta_2 LOST_{it} + \mu_i + YEAR_t + \varepsilon_{it}$$

The dependent variable Y_{it} is a measure of average hourly earnings or employment hours of different types of nursing personnel. The subscript i represents each hospital, and t is time. The dummy variable WON_{it} is the treatment variables of interest that equals 1 in hospitals after a successful union certification election and 0 otherwise. A comparison variable LOST_{it} equals 1 in hospitals after a failed certification election. The parameters θ_1 and θ_2 are therefore the key outcomes of interest. The variable μ_i is a hospital-specific fixed effect that controls for permanent differences across hospitals of the outcomes of their nursing personnel and $YEAR_t$ measures year fixed effect. The remaining variable is ε_{iht} which is an idiosyncratic error. We control for possible autocorrelation in errors by allowing for arbitrary correlations in errors within a hospital level over time. There are no other independent variables to control for hospital characteristics in the model, since all the other hospital level variables in the data set such as size, location and ownership do not vary over time and therefore are captured by hospital fixed effects.

Union certification election may not permanently change hospitals or they may take some time to tale effect. Therefore, we compare the outcomes of nursing personnel one or two years after the union certification election and three plus years after the union elections. $WON1to2_{it}$ is set to 1 one and two year after the union certification elections and $WON3PLUS_{it}$ equals 1 after that period. $LOST1to2_{it}$ and $LOST3PLUS_{it}$ are similarly defined. These variables are incorporated into Model (2):

(2)

$$Y_{it} = \theta_1 WON1to2_{it} + \theta_2 WON3PLUS_{it} + \theta_3 LOST1to2_{it} + \theta_4 LOST3PLUS_{it} + \mu_i + YEAR_t + \varepsilon_{it}$$

3.3.5 Potential Problem of Using the Fixed Effect Model

The difference in difference model outlined above includes a complete set of hospital fixed effect μ_i to controls for permanent differences across hospitals that may be correlated with both the propensity to become unionized and the outcomes of interest such as earnings. By construction, the model captures those unmeasured hospital characteristics that do not vary over time. If the reason that hospitals become unionized is because of these unmeasured but fixed characteristics, then the fixed effects will capture these differences and produce consistent estimates of the unionization effect. If however hospitals become unionized because of an unmeasured time-varying characteristic that is also correlated with the outcomes of interest, than the model may produce inconsistent estimates.

These time varying shocks are a source of concern for this analysis. During the past decade, hospital operations have been impacted in important ways by trends such as the emergence of managed health care, technological advancement, the growth of the Medicaid and Medicare programs, and hospital consolidation. Hospital responded to these structural changes by attempting to become more efficient in the production of healthcare services. Pierson and Williams (1994) note that 63 percent of all hospitals engaged in some reengineering initiatives based on a survey of 1000 hospitals in 1994. These same changes in market structure and organization of healthcare may also provide an incentive for nursing personnel to become unionized. If the same factor that altered the incentive to unionize also altered labor market outcomes for nurses, then the model will generate biased estimates.

For example, suppose that a hospital facing stiff competition from a nearby hospital reduces the size of the hospital staff and trims back the annual raises for nurses. In response, nurses successfully attempt to unionize. In this case, the same factors that lead to lower growth in wages also lead to unionization so the model may attribute a lower wage to the union.

However, as we illustrate below, we examine a number of outcomes that signal something about the underlying characteristics of the hospital such as the total number of hospital discharges and average length of patient hospital stays. As we note below, it appears that union status has no impact on these outcomes. The results provide some evidence that there were no structural change of hospital operations after nursing personnel got unionized, and our fixed effect assumption about hospitals are supported by those results.

3.4 Results

Table 3-3 presents regression results for the difference-in-difference models outlined in equations (1) and (2) with average hourly earnings of RNs, LPNs and aides, total hospital discharge and average length of stay (LOS) of patients as outcome variables. Each row represents one regression, and the first column lists the dependent variable for each regression. In contrast to the vast literature on union relative wage effects, these results indicate that for RNs and LPNs, average hourly earnings decreased no matter the hospital won or lost a unionization election although for LPNs, the effect of unionization on log average hourly earnings are not statistically significant. For RNs, average hourly earnings decreased by 5.66 percent after hospitals won unionization elections, and this result is statistically significant at 10% level. Based on model (2), for RNs, the average hourly earnings reduction happened within two years after hospital won the unionization elections. For aides and managers, the unionization election did not statistically significantly affect their average wage rate.

The results from Table 3-3 are contrary to previous literature which has established that union members realize wage premiums, not only among nurses in particular but for most workers in general. The differing results could be explained by some important differences between this study and previous analyses of hospital unions. First, we use a hospital-level data set instead of one with individual-level data. With individual level data, workers who migrate to unions may earn more than people who remain non-unionized, but, this does not identify what wages would have been in the establishment if it were not unionized. Second and more importantly, most previous studies use cross-sectional regressions. In our panel data set, we use hospital fixed effects to control for the possibility that certain types of hospitals are more likely to gain union representation with hospital fixed effects.

To compare the regression results for a panel data with difference in difference regression model to previous results from a cross sectional data set, in Table 3-4, we estimate a model where we ignore the longitudinal nature of the data[24] and estimate cross-sectional regressions. Instead of adding hospital fixed effects, we add variables that describe the characteristics including measure of hospital size[25], hospital service area and ownership of hospitals. In Table 3-4, for all kinds of nursing personnel, the average earned wage increased by a statistically significant amount after hospitals won unionization elections. For RNs, the average wage increased by 4.31 percent, for LPNs, it was 2.09 percent and for aides, the average wage increased by 2.36 percent. Based on the regression results of model (2), the average wage increase occurred three or more years after hospitals won unionization elections. These results indicate that the positive union wage premium found in most cross-sectional studies may be due to the fact that unions are more likely to appear in higher-wage hospitals. Losing unionization elections did not affect the average wage of nursing personnel in a statistically significant or qualitatively important manner.

So the cross sectional regression results are consistent with previous literature that unionized hospitals realized union wage premium. Why are there differences between regression results using panel data and difference in difference model, and cross sectional regression results? First, the cross sectional regressions are estimating the difference between unionized and non-unionized hospitals. So the cross sectional

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[24] We use the same data set with each observation in the panel hospital level data as one cross sectional observation.

[25] We break hospitals up into 6 groups based on average monthly number of total cases. HOSP_SIZE=1 if average monthly case<=400; HOSP_SIZE=2 if 400 <average monthly case<= 1000; HOSP_SIZE=3 if 1000 <average monthly case<= 3000; HOSP_SIZE= 4 if 3000 <average monthly case<= 7500; HOSP_SIZE=5 if 7500 <average monthly case<= 13000; HOSP_SIZE=6 if average monthly case>13000.

regression results are the average effects of unions of previous periods and the effects of unionization that occurs today. Second, as previous stated, bigger hospitals are more likely to be unionized; hospitals owned by church or other non-profit organization were less likely to be unionized. The fixed effect model purges the difference between unionized and non-unionized hospitals, but the cross sectional regression could not. One last reason is that the wage measurement here is not actual wage of each individual nursing personnel, and they are average hourly wage rate measured by total salaries and wages paid (include overtime payment) divided by total productive hours, so either the change of numerator or denominator would affect the average hourly wage. For now, we could not get the conclusion that unionization decreased wage of nursing personnel and do not know why average hourly wage decreased after unionization elections.

Table 3-3 also report results of regressions with log total number of hospital discharges and average length of stay (LOS) of patients as dependent variables. The objective is to check whether there were significant changes in hospital operations after hospital had unionization elections. Based on the results in Table 3-3, unionization increased the number of discharges and decreased the average LOS of patients, but these results are not statistically significant.

Table 3-5 reports results where the outcome variables measure productive hours and relative utilization of RNs, LPNs and aides. After hospitals won unionization elections, hospitals used more hours of RNs, LPNs and aides. However, utilization RNs and LPNs increased relative to aids, although these results are not statistically significant. It is a different story after hospital employees lost a certification election. After hospital employees lost unionization elections, 23.96 percent more aides' hours were utilized and relative to productive hours of RNs and LPNs, 4.06 percentage points more aides' hours were utilized. Those results are statistically significant. So hospitals used more low-skilled nursing personnel after collective bargaining unit was not successfully organized within hospitals.

The effect of unionization on average expenditures on RNs, LPNs, and aides are presented in Table 3-6. Labor costs are measured as hours multiplied by the average earned wage. Since hospitals utilized more hours of aides after losing unionization elections, their aid costs were 50.53 percent higher. For cost on different types of nursing personnel relative to the total labor cost on nursing personnel, after nursing personnel lost the unionization elections, hospitals spent 2.64 percentage points less on labor cost of RNs relative to the total labor cost on nursing personnel. Hospital labor costs on nursing personnel were not affected in a statistically significant manner by winning certification elections.

Table 3-7 reports the usage of RNs, LPNs and aides per discharge or per bed days. From the regression results, the usage of RNs, LPNs and aides per discharge or per bed days were not significantly affected by winning or losing hospital unionization elections.

The regression results of contract nursing personnel are presented in Table 3-8. The California Financial data set reports contract RNs, LPNs, and aides together as contract nursing personnel. Contract nursing personnel is complement to nursing personnel on payroll, and they usually do not involve in the unionization elections and are not union members. After hospitals had unionization elections, hospitals used

89.45 percent more contract nursing personnel than before. The increase of utilization of hours of contract personnel was huge, but given the fact that the utilization of contract nursing personnel was only a very small percent (4.19%) of total hours of nursing personnel for a hospital, the increase was not that big in the term of absolute values. The average hourly earnings of contract nursing personnel decreased although the result of average hourly earnings is not statistically significant. Relative to hours of nursing personnel on payroll, less contract nursing personnel were utilized in three or more years after hospital became unionized and the result was not statistically significant.

To summarize, after hospital had unionization elections, the operation of hospitals are as the following: number of discharges increased and average LOS of patients decreased and those results were not statistically significant. More hours of aides who are lower-skilled were utilized after hospitals lost unionization elections. After hospitals won unionization elections, more hours of contract nursing personnel who were not union members were utilized.

As results from Table 3-4 indicate, the total hours of RNs, LPNs and aides increased after hospital became unionized although these results were not statistically significant. There are two ways to increase the total hours of nursing personnel on payroll; one is to extract more hours from present nursing personnel (and maybe increasing overtime hours), and the second is to hire more nursing personnel. If the hospital used the first strategy, then the average hourly earnings calculated by dividing earnings by hours should have increased since at the margin, overtime hours are more expensive than normal hours. However, the average hourly earnings of nursing personnel decreased making it unlikely that overtime hours increased. This suggests that hospitals hired more nursing personnel and unionized nursing personnel worked less overtime hours after hospitals won the unionization elections.

Average hourly earnings is calculated by using normal salaries plus overtime wage divided by the sum of normal working hours and overtime hours. Nursing personnel were paid at a higher rate for overtime hours, so with the reduction of overtime hours, even if the normal salaries increased, salaries earned from overtime hours decreased, and for average hourly earnings, the numerator decreased at a bigger proportion compared to the denominator, so with the reduction of expensive overtime hours, the average hourly earnings decreased after hospital became unionized. There are evidences from news and articles that nursing personnel worked less overtime hours after they were unionized. However, since California Financial data set does not have information of number of nursing personnel, this hypothesis could not be tested in this paper.

What happened in hospitals after hospitals had unionization elections? If the hospital wins the unionization elections, a bargaining unit is formed, and the union bargains with hospitals on wage, hours, benefits, staffing ratio and etc. For nursing personnel, less overtime hours are benefits from bargaining with hospitals and more contract nursing personnel are utilized as substitute of previous overtime hours of nursing personnel on payroll. What really happened to the hourly earnings of nursing personnel? The effect on hourly earnings could not be directly examined by using California Financial data set since only average hourly earnings are reported. But one thing could be sure that with a bargaining unit, it is rare to see the real hourly earnings of nursing personnel decreases compared to before. The next part reports direct evidence of effect of unionization on hourly earnings by using Current Population Survey (CPS) outgoing rotation group from year 1983-2004.

3.5 Supplemental Evidence from the CPS-ORG Data Set

The Current Population Survey (CPS) is a monthly survey of about 60,000 households. Each household entering the CPS is administered 4 monthly interviews, then ignored for 8 months, then interviewed again for 4 more months. Month 4 and 8 are called the outgoing rotation groups (ORG) because respondents leave the sample either temporarily or permanently. In these months, respondents are asked questions of their union status, usual weekly earning, and usual weekly hours. The lag between the month 4 and month 8 interviews is one year. Responses from these months can be merged by household ID, age, occupation and other criteria to form a one-year panel data set.

The CPS is a household-based survey and although an observation may have the same ID in a one-year period, these two observations may not represent the same person. Based on the design of the data set, in order to match observations over time to form a panel, observations of the same person should meet the following criteria: share the same household id and same household number; have less than a two year age difference, same interview month and one year interview time difference. The matching rate for years 1984, 1985, 1994, 1995 and 2004 are very poor due to the different sample frame used in those years. The matching rates for the other years and for the whole sample are 74% percent, and this result is close to the 75% matching rate found by Hirsch & Schumacher (1998) for all workers.

After merging the CPS-ORG data, we can use the occupation codes to restrict the sample to RNs, LPNs and aides, and use the industry codes to restrict these workers to hospital employees. For the sub sample of RNs, LPNs and aides, the matching rate is 59% which is much lower than the rate for the general workforce. The low matching rate of nurses should be caused by the high turnover rate of nurses. According to the report *Acute Care Hospital Survey of RN Vacancies and Turnover Rates in 2000* released in January 2002 by the American Organization of Nurse Executives, the average annual RN turnover rate in acute care hospitals was 21.3%. If job turnover is accompanied by a household move, then the respondent would not be interviewed in the sample twice.

With this sample, we can estimate models similar to the ones in the previous section but with individual-level data. In these models, we compare the wages of nurses who move from union to nonunion status over the year and vice versa with nurses who do not change union status. Again, this is a simple difference-in-difference model that controls for the fact that different types of nurses are more or less likely to end up in unions. This same type of methodology has been used by other authors to examine such questions as inter-industry wage differentials (Krueger and Summers 1988) and the effect of degree of generosity of workers' compensation on injury duration (Meyer *et al.* 1995).

Table 3-9 reports change of union status for interviewees for RN, LPN and aides in the merged CPS-ORG sample. Almost 15 % of the interviewees stayed as union members in both month 4 and month 8, and 73.10% were not union member in either period. Slightly more than 6% of interviewees joined a labor union and almost an equal number separated from a union.

The following equation describes empirically the difference in difference model we estimate:

$$(3)\ Y_{it} = \theta_j UNION_{it} * OCCUPATION_{ji} + u_i + YEAR_t + \varepsilon_{it}$$

Where Y_{it} measures the labor market outcomes available in the CPS-ORG. The observations vary across time and interviewees which are indexed by t and i respectively. The variable u_i in model (3) is an individual fixed effect, $YEAR_t$ is a year fixed effect and ε_{it} is an error term. UNION is a dummy variable that measures union status in year t for person i. Since there are 3 occupations (RN, LPNs and aids), we allow the union effect to vary by occupation (j=1, 2, 3).

To examine whether certain types of workers selected into union jobs, we estimate a model similar to (3) that ignores the longitudinal nature of data. This model is outlined in equation (4)

$$(4)\ Y_{it} = \theta_j UNION_{it} * OCCUPATION_{ji} + \alpha_1 X_i + YEAR_t + \varepsilon_{it}$$

The vector X_i in model (4) includes a set of control variables that describe characteristics of the interviewee such as sex, race, ethnicity, education, marital status, state, age and age squared. In this model, we have exchanged the fixed individual effects for individual characteristics.

The key outcomes are natural logs of the reported hourly wage and weekly earning. Hourly wage variable is only available for those who get paid by hour and weekly earning include tips and overtime pays. Table 3-10 reports the sample descriptive statistics for each sub sample and total RN, LPN and aides sample. The samples are for full time workers with usual hours of working per week higher than 20 hours and hourly wage higher than $4. Compared with samples of LPN and aides, there are higher percent of college graduate, higher percent of white, lower percent of black, lower percent of Hispanic in the RN sample. On average RNs earned the highest hourly wage and worked fewer hours per week than LPN and aides.

To be comparable with results from California Financial data set, only data from year 1992 to 2000 are used in these regressions. Regression results of difference in difference model (3) are presented in Table 3-11. Weekly earning of RNs, LPNs and aids were 3.55 percent, 1.45 percent and 10.6 percent higher, respectively, after they became union members. The regression results with hourly wage, and weekly earning divided by usual hours tell the similar story. Aides realized the highest union wage premium compared to RNs and LPNs.

Regression results of cross sectional model (4) are reported in Table 3-12. The results from regarding the data as cross sectional are very similar to results from estimating the difference in difference model by using panel data. RNs, LPNs and aides earned a higher wage after they became union members. For weekly earning, weekly earning of RNs was 4.96 percent higher after being a union member and for

LPNs and aides, their weekly earnings were 2.14 percent and 12.33 percent higher. Lower skilled nursing personnel like aides realized a higher percent of union wage premium than higher skilled nursing personnel like LPNs and RNs.

We can see that regression results with hourly wage or weekly earnings as outcome variables by using difference in difference model (3) are very close to results using cross sectional regression model (4). The fixed effect model purges the differences between unionized and non-unionized nurses. From both regressions, nursing personnel who are union members earned a higher wage. The consistency between the difference in difference model with fixed effect and cross sectional regressions means that at the individual level, workers with above average earnings potential were not more likely to select into unions.

Based on Table 3-12, for RNs, LPNs and aides, their usual hours of working per week were not statistically significantly affected by whether they were union member or not. For another measure of hours, hours worked last week, RNs worked 1.0852 more hours after they became a union member.

The results from CPS data show that effect of unionization on individual level measure of wage, hourly wage and weekly earning. However, for hours of working, CPS data does not have a good measure of overtime hours of RNs, LPNs and aides either. We could not see direct evidence about how unionization affects overtime hours of RNs, LPNs and aides.

3.6 Conclusion

In this chapter, we use hospital level panel data set merged with unionization election results and collective bargaining contract information to measure the direct effect of unionization election, that is, effect of collective bargaining on wage and employment of nursing personnel. Compared to previous cross sectional studies, we estimate what happens in hospitals after they become unionized. This contrasts with most previous estimates that examine cross sectional differences in earnings for union and non unionized hospitals.

We find that after the failure of unionization elections, hospitals used more aides who are lower-skilled than RNs and LPNs who are higher-skilled. This could jeopardize the health of patients. After hospitals won unionization elections, more contract nursing personnel hours were utilized. After unionization, the average hourly wage of RNs and LPNs decreased by a statistically significant amount. These results are consistent with a management policy of using more nurses but less overtime hours after union certification. Although contract nurses may be less skilled than staff nurses, it is not clear what effect this would have on patient welfare since these workers may be substituting nurses who otherwise would have been on overtime and therefore, maybe more fatigued and less alert.

There are several limitations to this study. First, the hospital level financial data is not detailed enough to measure the effect of union on wages and overtime hours of particular individuals. It would be useful to have detailed data of employees in hospitals. Second, given the fact that wage is measured in the terms of average hourly earnings in California Hospital Financial data set, the effect of unionization on wages instead of average hourly earnings could not be examined. Using merged CPS dataset, we find nursing personnel realized union wage premium as a union member. However, the CPS data set did not have a good measure of overtime hours either. We do not have evidence to measure the effect of union on overtime hours of nursing personnel. Last, the unionization information is not detailed enough to measure the specific characteristics of contract bargaining unit and number of bargaining unit in a hospital. We may also be understating the extent of unionization if hospitals failed to submit bargaining contract expiration notices.

Chapter 4: Concluding Remarks

For effect of postpartum length of stay, 2SLS estimates suggest that for children whose mothers had cesarean deliveries or had complications during pregnancy or labor, an extra day in the hospital reduced readmission rates. In contrast, we do not find any statistically significant medical benefit for infants whose mothers had vaginal deliveries without complications. So requiring all deliveries to stay certain time in hospital is "a simple solution to a problem whose health consequences are unclear" as Declercq (1999). The existence of high percent of early discharge even after the effectiveness of early discharge laws means the underlying effect of law is to ensure the coverage of insurance is not a factor to discharge mothers and newborns early from hospitals, and the time of discharge is a decision of the physicians in consultation with the mother.

For effect of hospital unionization, in the dissertation, the direct effect on nursing personnel were examined. Effect on patient outcomes is the ultimate goal to study effect of unionization. This could be done by using hospital patient level dataset such as state inpatient dataset. If the individual level hospital nursing personnel is available, the mechanism how union affects patient outcomes could also be examined in more detail.

Table 2-1 OLS Estimates of Length of Stay and Logit Estimates of 28-Day Readmission Equations, Newborns in California Private/Medicaid Sample, 1995 – 1996

OLS Parameter Estimate or Marginal Effect

	Vaginal Deliveries		C-sections			Vaginal Deliveries		C-Sections	
		28-day		28-day			28-day		28-day
	LOS	Readmit.	LOS	Readmit.		LOS	Readmit.	LOS	Readmit.
Covariates	(1)	(2)	(1)	(2)		(1)	(2)	(1)	(2)
Private	-0.3079	-0.0055	-0.3353	-0.00641	Married	-0.1525	0.0002	-0.2247	0.00003
Insurance	(0.0129)	(0.0003)	(0.0514)	(0.0007)		(0.0119)	(0.0003)	(0.0472)	(0.0006)
Non-profit hospital	-0.3609	-0.0002	-0.1389	0.0006	Mother ≤ 20 years	-0.4392	-0.00005	0.2129	0.0103
	(0.0157)	(0.0004)	(0.0640)	(0.0007)	of age	(0.0242)	(0.0006)	(0.0898)	(0.0014)
For-profit hospital	-0.4290	-0.0019	-0.6868	-0.0016	Mother 21-25 years	-0.3836	-0.0006	-0.2005	0.00003
	(0.0184)	(0.0005)	(0.0723)	(0.0009)	of age	(0.0211)	(0.0005)	(0.0730)	(0.0010)
Boy	0.1191	0.0056	0.0562	0.0045	Mother 26-30 years	-0.2630	-0.0007	-0.2979	-0.0023
	(0.0100)	(0.0002)	(0.0395)	(0.0005)	of age	(0.0197)	(0.0005)	(0.0651)	(0.0009)
0 complications	-3.5362	-0.0151	-7.7511	-0.0255	Mother 31-35 years	-0.1497	-0.00006	-0.1765	-0.0045
during pregnancy	(0.0814)	(0.0009)	(0.2162)	(0.0013)	of age	(0.0201)	(0.0005)	(0.0642)	(0.0010)
1 complication	-2.2407	0.0008	-4.7291	-0.0004	White, non-Hispanic	-0.1172	0.0010	-0.2165	0.0026
during pregnancy	(0.0829)	(0.0010)	(0.2213)	(0.0014)		(0.0130)	(0.0004)	(0.0513)	(0.0009)
2 complications	-1.9378	0.0026	-3.0720	0.0057	Black, non-Hispanic	0.4360	-0.0025	1.1868	-0.0026
during pregnancy	(0.0901)	(0.0014)	(0.2438)	(0.0020)		(0.0222)	(0.0008)	(0.0819)	(0.0015)
0 complications	-1.7962	-0.0087	-1.4601	-0.0089	Other race, non-	0.0068	0.0024	0.2498	-0.0003
during delivery	(0.0399)	(0.0006)	(0.0704)	(0.0009)	Hispanic	(0.0169)	(0.0006)	(0.0689)	(0.0013)
1 complication	-1.7728	-0.0037	-1.8587	-0.0053	Mother < HS	0.1261	-0.0017	-0.0899	0.0002
during delivery	(0.0496)	(0.0011)	(0.1065)	(0.0017)	education	(0.0198)	(0.0005)	(0.0759)	(0.0010)
2 complications	-1.2634	0.0009	-1.2082	0.0006	Mother HS education	0.0660	-0.0010	-0.2032	-0.0006
during delivery	(0.0428)	(0.0007)	(0.0731)	(0.0009)		(0.0175)	(0.0004)	(0.0651)	(0.0009)
No previous births	0.2152	0.0053	-0.9158	-0.0070	Mother some college	0.0448	0.0001	-0.1615	-0.0024
	(0.0185)	(0.0004)	(0.0720)	(0.0009)	education	(0.0175)	(0.0005)	(0.0643)	(0.0010)
1 previous birth	-0.0222	-0.0017	-0.8449	-0.0027	Observations	754,107	754,107	205,625	205,625
	(0.0176)	(0.0004)	(0.0700)	(0.0009)	Mean of outcome	1.6950	0.0450	4.4162	0.0535
2 previous births	-0.0757	-0.0027	0.6587	0.0007	R^2/-2 Log like.	0.0222	0.0086	0.0344	0.0169
	(0.0187)	(0.0005)	(0.0746)	(0.0010)					

Note: Standard errors in parenthesis. Other covariates include month and year dummy variables. The reference categories are Medicaid insurance, government hospital, girls, 3 or more complications during pregnancy or delivery, unmarried mothers, and mothers aged 36 or more who are Hispanic and with a college education.

Table 2-2 Sample Descriptive Statistics of Mothers,
July 1995 – Dec 2000

	Deliveries without complications		Deliveries with complications	
	Vaginal	C-sections	Vaginal	C-sections
Mothers				
Mean age	27.72	29.25	27.60	29.03
% Less than High School	32.44%	29.65%	29.08%	26.72%
% White	81.13%	81.57%	79.34%	79.33%
% Black	6.10%	7.18%	7.24%	8.18%
% Hispanic	45.50%	45.52%	39.46%	39.54%
Mean Previous # of Births	1.05	1.21	1.05	0.96
Observations	1,359,308	238,553	504,029	326,173

Table 2-3 Distribution of Postpartum Length of Stay,
July 1995 through August 1997

	Vaginal Deliveries	
	Uncomplicated	Complicated
0 days	26,042 (4.77%)	8,867 (4.47%)
1 days	416,116 (76.17%)	123,321 (62.12%)
2 days	78,827 (14.43%)	40,588 (20.45%)
3 days	9,279 (1.70%)	8,054 (4.06%)
> 3 days	16,056 (2.93%)	17,685 (8.90%)
Total Observations	546,320 (100%)	198,515 (100%)
	Cesarean Deliveries	
	Uncomplicated	Complicated
0 days	868 (1.03%)	3,209 (2.51%)
1 days	1,740 (2.06%)	2,629 (2.06%)
2 days	35,001 (41.44%)	42,328 (33.11%)
3 days	38,263 (45.30%)	53,515 (41.87%)
4 days	3,112 (3.68%)	7,959 (6.23%)
5 days	1,112 (1.32%)	3,164 (2.48%)
> 5 days	4,366 (5.17%)	15,019 (11.74%)
Total Observations	84,462 (100%)	127,823 (100%)

Note: Frequencies of days of postpartum length of stay of newborns in pre law periods reported and percent to total sample reported in parentheses.

Table 2-4 Reduced-Form Regressions, Private Insurance and Medicaid Deliveries in California, July 1995 through December 2000

	LOS < Mandated Time	LOS in Days	28-Day Readmit.	Log Total Charges	LOS < Mandated Time	LOS in Days	28-Day Readmit.	Log Total Charges
	Vaginal deliveries without complications (1,359,308 Observations)				Vaginal deliveries with complications (504,029 Observations)			
Federal law x	-0.3088	0.3609	-0.0006	0.0875	-0.2715	0.4371	-0.0037	0.1034
Private insurance	(0.0176)	(0.0202)	(0.0012)	(0.0155)	(0.0201)	(0.0686)	(0.0033)	(0.0203)
Federal law x	-0.1079	0.1488	-0.0003	0.0219	-0.1253	0.2138	-0.0056	0.0228
Medicaid	(0.0120)	(0.0200)	(0.0013)	(0.0143)	(0.0150)	(0.0812)	(0.0026)	(0.0183)
State Law x	-0.1629	0.2164	0.0036	0.0349	-0.1602	0.2216	-0.0024	0.0289
Private Insurance	(0.0135)	(0.0180)	(0.0012)	(0.0098)	(0.0132)	(0.0596)	(0.0029)	(0.0161)
State law x	-0.0142	0.0375	0.0044	-0.0091	-0.0359	0.0084	0.0001	-0.0174
Medicaid	(0.0084)	(0.0156)	(0.0015)	(0.0121)	(0.0110)	(0.0717)	(0.0032)	(0.0175)
Mean of Y	0.8093	1.3196	0.0406	8.6168	0.6659	2.2555	0.0571	8.9355
R-squared	0.2571	0.0466	0.0035	0.3309	0.2184	0.0209	0.0089	0.2298
	C-section deliveries without complications (238,553 Observations)				C-section deliveries with complications (326,173 Observations)			
Federal law x	-0.1189	0.4305	-0.0055	0.0476	-0.1400	0.7289	-0.0061	0.0612
Private insurance	(0.0144)	(0.0755)	(0.0028)	(0.0149)	(0.0144)	(0.1311)	(0.0043)	(0.0242)
Federal law x	-0.0049	0.1356	-0.0014	-0.0118	-0.0193	0.3531	-0.0044	-0.0168
Medicaid	(0.0093)	(0.0608)	(0.0029)	(0.0153)	(0.0092)	(0.1345)	(0.0043)	(0.0165)
State Law x	-0.0492	0.2669	-0.0001	0.0237	-0.0656	0.5700	0.0014	0.0205
Private Insurance	(0.0107)	(0.0787)	(0.0029)	(0.0117)	(0.0098)	(0.1287)	(0.0037)	(0.0185)
State law x	0.0149	0.0142	0.0027	-0.0240	0.0084	0.1453	0.0024	-0.0348
Medicaid	(0.0080)	(0.0866)	(0.0031)	(0.0155)	(0.0086)	(0.1218)	(0.0040)	(0.0156)
Mean of Y	0.8983	3.2112	0.0408	9.4327	0.7955	4.6239	0.0597	9.6565
R-squared	0.1090	0.0299	0.0074	0.2785	0.1073	0.0361	0.0159	0.2064

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, plus time trends that vary by health service area, payer, and hospitals size, and corresponding lower order interaction terms.

Table 2-5 Quantile Regression Results, Log (Total Charges) Equation, Private Insurance and Medicaid Deliveries in California, July 1995 through December 2000

	Q=0.25	Q=0.50	Q=0.75	Q=0.25	Q=0.50	Q=0.75
	Vaginal Deliveries without Complications (1,215,666 observations)			Vaginal Deliveries with Complications (410,824 observations)		
Federal Law*	0.0957	0.0921	0.0816	0.1134	0.1073	0.0821
Private Insurance	(0.0017)	(0.0017)	(0.0022)	(0.0036)	(0.0039)	(0.0056)
Federal Law*	0.0425	0.0461	0.0508	0.0556	0.0515	0.0476
Medicaid	(0.0017)	(0.0017)	(0.0022)	(0.0037)	(0.0040)	(0.0056)
State Law*	0.0218	0.0291	0.0377	0.0345	0.0410	0.0221
Private Insurance	(0.0023)	(0.0023)	(0.0029)	(0.0048)	(0.0052)	(0.0074)
State Law*	-0.0118	-0.0012	0.0077	-0.0165	-0.0080	0.0068
Medicaid	(0.0022)	(0.0022)	(0.0029)	(0.0047)	(0.0052)	(0.0073)
Pseudo R^2	0.2361	0.2395	0.2308	0.1977	0.1803	0.1457
	Cesarean Deliveries without Complications (223,098 observations)			Cesarean Deliveries with Complications (285,001 observations)		
Federal Law*	0.0499	0.0420	0.0212	0.0524	0.0426	0.0371
Private Insurance	(0.0036)	(0.0033)	(0.0049)	(0.0034)	(0.0041)	(0.0074)
Federal Law*	0.0140	0.0149	0.0108	0.0095	0.0140	0.0044
Medicaid	(0.0036)	(0.0034)	(0.0050)	(0.0035)	(0.0042)	(0.0076)
State Law*	0.0083	0.0080	0.0046	0.0045	0.00001	-0.0015
Private Insurance	(0.0047)	(0.0044)	(0.0066)	(0.0044)	(0.0053)	(0.0096)
State Law*	-0.0309	-0.0178	-0.0122	-0.0205	-0.0219	-0.0417
Medicaid	(0.0048)	(0.0045)	(0.0068)	(0.0047)	(0.0057)	(0.0103)
Pseudo R^2	0.2165	0.2288	0.2171	0.1783	0.1618	0.1327

Note: Standard errors are reported in parentheses. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, plus time trends that vary by health service area, payer, and hospitals size.

Table 2-6 Reduced-Form Regressions Logistic Regressions of Infant Readmission and Mortality Models, California Privately Insured and Medicaid Patients, July 1995 through December 2000

	7-day readmission	14-day readmission	28-day readmission	28-day mortality	7-day readmission	14-day readmission	28-day readmission	28-day mortality
	Vaginal deliveries without complications (1,359,311 Observations)				Vaginal deliveries with complications (504,029 Observations)			
Federal law x	-0.0006	-0.0003	-0.0006	0.00007	-0.0026	-0.0029	-0.0037	-0.0007
Private insurance	(0.0010)	(0.0011)	(0.0013)	(0.0001)	(0.0030)	(0.0031)	(0.0034)	(0.0004)
Federal law x	0.0010	0.0005	-0.00005	-0.000006	-0.0030	-0.0032	-0.0047	0.0004
Medicaid	(0.0011)	(0.0011)	(0.0012)	(0.0001)	(0.0024)	(0.0023)	(0.0023)	(0.0004)
State Law x	0.0018	0.0028	0.0042	0.0002	-0.0031	-0.0024	-0.0012	0.0001
Private Insurance	(0.0009)	(0.0010)	(0.0011)	(0.0001)	(0.0024)	(0.0026)	(0.0028)	(0.0004)
State law x	0.0022	0.0026	0.0041	0.0002	-0.0036	-0.0017	0.0007	0.0003
Medicaid	(0.0011)	(0.0012)	(0.0013)	(0.0001)	(0.0024)	(0.0024)	(0.0026)	(0.0004)
Mean of Y	0.0237	0.0299	0.0406	0.0006	0.0385	0.0452	0.0571	0.0020
	C-section deliveries without complications (238,554 Observations)				C-section deliveries with complications (326,175 Observations)			
Federal law x	-0.0045	-0.0052	-0.0057	-0.0003	-0.0039	-0.0044	-0.0060	0.0003
Private insurance	(0.0026)	(0.0029)	(0.0032)	(0.0005)	(0.0042)	(0.0043)	(0.0044)	(0.0007)
Federal law x	-0.0009	-0.0008	-0.0009	-0.0004	-0.0008	-0.0006	-0.0032	0.0003
Medicaid	(0.0024)	(0.0025)	(0.0026)	(0.0006)	(0.0032)	(0.0034)	(0.0037)	(0.0008)
State Law x	-0.0016	-0.0015	0.0012	-0.0008	0.0011	0.0028	0.0032	0.0003
Private Insurance	(0.0027)	(0.0030)	(0.0031)	(0.0008)	(0.0028)	(0.0030)	(0.0034)	(0.0008)
State law x	0.0022	0.0045	0.0030	0.0004	0.0028	0.0033	0.0033	-0.0004
Medicaid	(0.0021)	(0.0023)	(0.0025)	(0.0006)	(0.0030)	(0.0031)	(0.0032)	(0.0008)
Mean of Y	0.0235	0.0295	0.0408	0.0019	0.0415	0.0477	0.0597	0.0044

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time.. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, plus time trends that vary by health service area, payer, and hospitals size, and corresponding lower order interaction terms.

Table 2-7 2SLS Estimates of 28-Day Readmission Equation,
Private Insurance and Medicaid Deliveries in California,
July 1995 through December 2000

	Deliveries without complications		Deliveries with complications	
Covariate	Vaginal	C-sections	Vaginal	C-sections
	OLS estimates, 28-Day Readmission as dependent variable			
LOS	-0.0013	-0.0022	-0.0012	-0.0017
	(0.0002)	(0.0003)	(0.0001)	(0.0002)
Observations	1,359,150	238,437	503,795	325,811
	2SLS Estimates, 28-Day Readmission as dependent variable (Using Federal Law*Private Insurance, Federal Law*Medicaid, State Law*Private Insurance and State Law*Medicaid as instruments)			
LOS	-0.0009	-0.0121	-0.0114	-0.0071
	(0.0031)	(0.0063)	(0.0068)	(0.0050)
	[0.764]	[0.054]	[0.093]	[0.158]
Observations	1,359,150	238,437	503,795	325,811
P-value on test of Overid. restrictions	0.0000	0.6686	0.5320	0.1038
	2SLS Estimates, 28-Day Readmission as dependent variable (Deleting data for 9/1997 through 12/1997 and using Federal Law*Private Insurance, Federal Law*Medicaid as instruments)			
LOS	-0.0015	-0.0126	-0.0108	-0.0087
	(0.0033)	(0.0064)	(0.0069)	(0.0051)
	[0.647]	[0.05]	[0.115]	[0.086]
Observations	1,275,833	224,769	472,971	306,444
P-value on test of Overid. restrictions	1	0.9995	0.3633	0.9509

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, hospitals size, plus time trends that vary by health service area and payer, and the corresponding lower order interaction terms.

Table 2-8 Reduced-Form Regressions, Medicaid Fee-For-Service Counties and Counties with percent of Medicaid managed care < 10% in California, July 1995 through December 2000

	LOS < Mandated Time	LOS in Days	LOS < Mandated Time	LOS in Days
	Vaginal Deliveries without Complications (81,864 observations)		Vaginal Deliveries with Complications (35,016 observations)	
Federal law x Private insurance	-0.1581	0.1704	-0.2036	0.4943
	(0.0095)	(0.0283)	(0.0164)	(0.1627)
Federal law x Medicaid	-0.0709	0.0774	-0.0644	-0.1584
	(0.0082)	(0.0244)	(0.0141)	(0.1398)
State Law x Private Insurance	-0.0726	0.0496	-0.1390	0.4017
	(0.0101)	(0.0302)	(0.0177)	(0.1763)
State law x Medicaid	-0.0262	0.0276	-0.0101	-0.2350
	(0.0085)	(0.0255)	(0.0155)	(0.1539)
Mean of Y	0.8344	1.2337	0.6590	2.0661
R^2	0.2277	0.1215	0.2229	0.2183
	Cesarean Deliveries without Complications (14,910 observations)		Cesarean Deliveries with Complications (21,244 observations)	
Federal law x Private insurance	-0.0788	0.3060	-0.1116	0.6839
	(0.0151)	(0.1305)	(0.0173)	(0.2496)
Federal law x Medicaid	0.0045	-0.1320	0.0030	0.1285
	(0.0141)	(0.1214)	(0.0158)	(0.2273)
State Law x Private Insurance	-0.0172	0.0609	-0.0302	0.0137
	(0.0156)	(0.1342)	(0.0186)	(0.2675)
State law x Medicaid	0.0154	-0.1748	0.0151	-0.1973
	(0.0152)	(0.1307)	(0.0168)	(0.2418)
Mean of Y	0.9301	2.7225	0.8412	3.6570
R^2	0.1258	0.2828	0.1764	0.2301

Note: Standard errors reported in parentheses. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, plus time trends that vary by health service area, payer, and hospitals size, and corresponding lower order interaction terms.

Table 2-9 Reduced-Form Regressions, Medicaid COHS counties (Percent of Medicaid managed care >10%) in California, July 1995 through December 2000

	LOS < Mandated Time	LOS in Days	LOS < Mandated Time	LOS in Days
	Vaginal Deliveries without Complications (184,362 observations)		Vaginal Deliveries with Complications (76,758 observations)	
Federal law x	-0.3297	0.3492	-0.3173	0.6601
Private insurance	(0.0054)	(0.0169)	(0.0085)	(0.0835)
Federal law x	-0.2006	0.3063	-0.2432	0.4766
Medicaid	(0.0072)	(0.0226)	(0.0118)	(0.1158)
State Law x	-0.2115	0.2596	-0.2028	0.3358
Private Insurance	(0.0058)	(0.0181)	(0.0093)	(0.0909)
State law x	-0.0921	0.1691	-0.1195	0.3604
Medicaid	(0.0075)	(0.0235)	(0.0123)	(0.1210)
Mean of Y	0.8479	1.2328	0.7239	1.9141
R^2	0.2756	0.0564	0.2481	0.028
	Cesarean Deliveries without Complications (29,508 observations)		Cesarean Deliveries with Complications (43,971 observations)	
Federal law x	-0.1688	0.4221	-0.1810	1.0953
Private insurance	(0.0123)	(0.0989)	(0.0113)	(0.2024)
Federal law x	-0.0453	0.2504	-0.1026	0.8418
Medicaid	(0.0176)	(0.1411)	(0.0161)	(0.2877)
State Law x	-0.0731	0.2660	-0.1142	1.1790
Private Insurance	(0.0136)	(0.1096)	(0.0124)	(0.2206)
State law x	0.0083	0.1515	-0.0646	1.0722
Medicaid	(0.0191)	(0.1535)	(0.0173)	(0.3088)
Mean of Y	0.9044	3.0692	0.8067	4.2628
R^2	0.1527	0.054	0.1321	0.0533

Note: Standard errors reported in parentheses. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, plus time trends that vary by health service area, payer, and hospitals size, and corresponding lower order interaction terms.

Table 2-10 Reduced-Form Regressions, Medicaid Two-Plan and GMC counties (Percent of Medicaid managed care >10%) in California, July 1995 through December 2000

	LOS < Mandated Time	LOS in Days	LOS < Mandated Time	LOS in Days
	Vaginal Deliveries without Complications (1,005,098 observations)		Vaginal Deliveries with Complications (351,223 observations)	
Federal law x	-0.3147	0.3757	-0.2676	0.4149
Private insurance	(0.0025)	(0.011)	(0.0043)	(0.0541)
Federal law x	-0.1026	0.1415	-0.1140	0.2165
Medicaid	(0.0027)	(0.0119)	(0.0052)	(0.0662)
State Law x	-0.1615	0.2227	-0.1599	0.2397
Private Insurance	(0.0027)	(0.0118)	(0.0047)	(0.0592)
State law x	-0.0061	0.0248	-0.0249	0.0055
Medicaid	(0.0029)	(0.0126)	(0.0055)	(0.0693)
Mean of Y	0.7992	1.3468	0.6495	2.3645
R^2	0.2658	0.0462	0.2255	0.0181
	Cesarean Deliveries without Complications (180,781 observations)		Cesarean Deliveries with Complications (235,540 observations)	
Federal law x	-0.1140	0.4435	-0.1343	0.7129
Private insurance	(0.0054)	(0.0621)	(0.0052)	(0.1059)
Federal law x	0.0011	0.1279	-0.0107	0.3194
Medicaid	(0.0059)	(0.0673)	(0.0063)	(0.1285)
State Law x	-0.0484	0.2674	-0.0565	0.4758
Private Insurance	(0.0059)	(0.0679)	(0.0057)	(0.1156)
State law x	0.0164	0.0089	0.0147	0.1049
Medicaid	(0.0063)	(0.0720)	(0.0067)	(0.1368)
Mean of Y	0.8937	3.2956	0.7896	4.7904
R^2	0.1042	0.0277	0.1052	0.0323

Note: Standard errors reported in parentheses. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, plus time trends that vary by health service area, payer, and hospitals size, and corresponding lower order interaction terms.

Table 2-11 Reduced-Form Regressions, Medicaid Deliveries in California, July 1995 through December 2000

	LOS < Mandated Time	LOS in Days	LOS < Mandated Time	LOS in Days
	Vaginal Deliveries without Complications and Medicaid (625,023 observations)		Vaginal Deliveries with Complications and Medicaid (206,567 observations)	
Federal Law* Medicaid1	-0.1517	0.1379	-0.1497	0.1144
	(0.0037)	(0.0163)	(0.0067)	(0.0861)
Federal Law* Medicaid2	-0.1235	0.1785	-0.1461	0.1832
	(0.0030)	(0.0130)	(0.0054)	(0.0687)
Federal Law* Medicaid3	-0.0808	0.1308	-0.1121	0.2452
	(0.0028)	(0.0123)	(0.0054)	(0.0696)
Federal Law* Medicaid4	-0.1074	0.1342	-0.1073	0.1579
	(0.0027)	(0.0116)	(0.0050)	(0.0637)
State Law*Medicaid1	-0.0535	0.0685	-0.0527	0.0715
	(0.0078)	(0.0339)	(0.0135)	(0.1731)
State Law*Medicaid2	0.0020	0.0370	-0.0174	-0.1498
	(0.0049)	(0.0214)	(0.0083)	(0.1066)
State Law*Medicaid3	0.0234	-0.0059	-0.0257	0.1540
	(0.0044)	(0.0191)	(0.0087)	(0.1107)
State Law*Medicaid4	-0.0321	0.0382	-0.0355	-0.0069
	(0.0036)	(0.0158)	(0.0068)	(0.0864)
Mean of Y	0.7537	1.4261	0.6022	2.5332
R^2	0.2453	0.0489	0.2175	0.0256
	Cesarean Deliveries without Complications and Medicaid (107,201 observations)		Cesarean Deliveries with Complications and Medicaid (133,223 observations)	
Federal Law* Medicaid1	-0.0637	0.3301	-0.0407	0.3558
	(0.0076)	(0.0889)	(0.0083)	(0.1663)
Federal Law* Medicaid2	-0.0542	0.3682	-0.0419	0.6313
	(0.0060)	(0.0705)	(0.0065)	(0.1307)
Federal Law* Medicaid3	-0.0038	0.1980	-0.0147	0.5108
	(0.0055)	(0.0648)	(0.0064)	(0.1288)
Federal Law* Medicaid4	0.0231	-0.0324	0.0060	-0.0680
	(0.0052)	(0.0612)	(0.0059)	(0.1186)
State Law*Medicaid1	-0.0025	0.0259	0.0125	0.1613
	(0.0177)	(0.2072)	(0.0176)	(0.3529)
State Law*Medicaid2	-0.0041	-0.0477	-0.0012	0.1992
	(0.0106)	(0.1244)	(0.0105)	(0.2103)
State Law*Medicaid3	0.0144	0.1214	0.0055	0.5157
	(0.0089)	(0.1040)	(0.0103)	(0.2072)
State Law*Medicaid4	0.0302	-0.0449	0.0225	-0.1861
	(0.0072)	(0.0840)	(0.0082)	(0.1644)
Mean of Y	0.8916	3.3182	0.7698	4.8914
R^2	0.0752	0.0371	0.0927	0.0452

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, 90-180 days old 28 days admission index, day of the week, month, hospital ownership, plus time trends that vary by health service area, payer, and hospitals size, and corresponding lower order interaction terms.

Table 3-1 Descriptive Statistics of California Hospitals,
California Hospital Financial dataset 1998

	Unionized Hospitals	Non-Unionized Hospitals	T Value (H_0: Mean $_{unionized}$ = Mean $_{non\text{-}unionized}$)
No of hospitals	123	293	552
Average yearly discharge	8,195	5,772	3.36
	(6,893)	(6,624)	
Average licensed beds	248	181	3.12
	(191)	(205)	
%Church owned	7.85%	17.07%	-2.81
%Non-Profit Corporation	35.15%	42.28%	-1.37
%Non-Profit Other	3.07%	3.25%	-0.10
%For-Profit Corporation	30.38%	27.64%	0.56
%County owned	7.85%	0	3.23
%District owned	11.95%	8.94%	0.89

Note: Standard deviations are reported in parentheses.

Table 3-2 Descriptive Statistics of California Hospitals that had Unionization Elections in period 1992-2000, at the end of year 1998

	Hospitals won unionization election	Hospitals lost unionization elections	T Value (H_0: Mean $_{lost}$ = Mean $_{won}$)
No of hospitals	20	11	
Average yearly discharge	7,722	10,058	1.14
	(5,008)	(6,212)	
Average licensed beds	230	247	0.27
	(147)	(182)	
%Church owned	10%	9.09%	0.08
%Non-Profit Corporation	45.00%	45.45%	-0.02
%Non-Profit Other	10.00%	9.09%	0.08
%For-Profit Corporation	35.00%	36.36%	-0.07

Note: Standard deviations are reported in parentheses.

Table 3-3 Regressions of Average Hourly Earnings of Nursing Personnel on Payroll, Difference in Difference Regression

Variables	Won	Lost	Won1to2	Won3plus	Lost1to2	Lost3plus	Mean of Y	R Squared	Observations
Log (RN wage)	-0.0566 (0.0268)	-0.0080 (0.0353)					3.1845	0.7193	3609
Log (RN wage)			-0.0764 (0.0293)	-0.0314 (0.0385)	-0.0250 (0.0397)	-0.0173 (0.0398)	3.1845	0.7198	3609
Log (LPN wage)	-0.0315 (0.0293)	-0.0203 (0.0367)					2.7174	0.6811	3478
Log (LPN wage)			-0.0319 (0.0272)	-0.0357 (0.0312)	-0.0355 (0.0441)	-0.0437 (0.0490)	2.7174	0.6813	3478
Log (aides wage)	0.0231 (0.0200)	0.0014 (0.0279)					2.3309	0.8295	3427
Log (aides wage)			0.0213 (0.0213)	0.0299 (0.0231)	-0.0204 (0.0323)	-0.0258 (0.0361)	2.3309	0.8295	3427
Log (manager wage)	0.0186 (0.0250)	0.0223 (0.0188)					3.3125	0.6692	3340
Log (manager wage)			0.0287 (0.0302)	0.0028 (0.0283)	-0.0240 (0.0391)	-0.0175 (0.0432)	3.3125	0.6693	3340
Log (total discharge)	0.0995 (0.0938)	0.0116 (0.0506)					8.1314	0.9787	3574
Log (total discharge)			0.0848 (0.0837)	0.0787 (0.0939)	-0.0045 (0.0382)	-0.0277 (0.0714)	8.1314	0.9787	3574
Average (LOS)	-0.7334 (0.6416)	-0.0014 (0.3708)					9.5281	0.9236	3519
Average (LOS)			-0.6172 (0.5331)	-0.4554 (0.6663)	0.1051 (0.3181)	0.3721 (0.5822)	9.5281	0.9235	3519

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Unreported covariates include fixed effects for month, year and hospitals.

Table 3-4 Regressions of Average Hourly Earnings of Nursing Personnel on Payroll, Cross Sectional Regressions

Variables	Won	Lost	Won1to2	Won3plus	Lost1to2	Lost3plus	Mean of Y	R Squared	Observations
Log	0.0431	-0.0028					3.1845	0.4341	3422
(RN wage)	(0.0107)	(0.0268)							
Log			-0.0230	0.0430	0.0004	-0.0121	3.1845	0.4343	3422
(RN wage)			(0.024)	(0.0109)	(0.0207)	(0.0328)			
Log	0.0209	0.0016					2.7174	0.3841	3297
(LPN wage)	(0.0102)	(0.0242)							
Log			-0.0329	0.0217	0.0011	-0.0006	2.7174	0.3848	3297
(LPN wage)			(0.0242)	(0.0104)	(0.0259)	(0.0287)			
Log	0.0236	0.0078					2.3309	0.498	3252
(aides wage)	(0.0131)	(0.0246)							
Log			-0.0092	0.0264	0.0252	-0.0050	2.3309	0.4986	3252
(aides wage)			(0.0230)	(0.0134)	(0.0251)	(0.0276)			
Log	0.0218	0.0048					3.3125	0.3028	3172
(manager wage)	(0.0127)	(0.0182)							
Log			0.0479	0.0231	-0.0077	0.0039	3.3125	0.3034	3172
(manager wage)			(0.0364)	(0.0127)	(0.0181)	(0.0215)			
Log (total	0.0577	-0.0068					8.1314	0.8642	3385
discharge)	(0.0584)	(0.0880)							
Log (total			0.0533	0.0586	-0.0635	0.0236	8.1314	0.8642	3385
discharge)			(0.0621)	(0.0584)	(0.0774)	(0.0982)			
Average (LOS)	2.4016	1.7383					9.5281	0.375	3337
	(1.3837)	(1.0998)							
Average (LOS)			2.3590	2.3568	1.7692	1.4075	9.5281	0.3748	3337
			(2.1523)	(1.3951)	(1.0375)	(1.1252)			

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Unreported covariates include hospital size, hospital service area, hospital ownership, year and month fixed effect.

Table 3-5 Regressions of Utilization of Hours of Nursing Personnel, Difference in Difference Regression

Variables	Won	Lost	Won1to2	Won3plus	Lost1to2	Lost3plus	Mean of Y	R Squared	Observations
Log(RN hours)	0.0768	0.023					11.4389	0.9709	3609
	(0.0854)	(0.0674)							
Log(RN hours)			0.0765	0.0568	0.0383	-0.0080	11.4389	0.9709	3609
			(0.0785)	(0.0865)	(0.0492)	(0.0952)			
Log(LPN hours)	0.0937	0.0905					9.9511	0.8559	3464
	(0.1442)	(0.1374)							
Log(LPN hours)			0.0894	0.0777	0.1888	-0.0099	9.9511	0.856	3464
			(0.1291)	(0.1486)	(0.1357)	(0.2343)			
Log(aides hours)	0.0485	0.2396					10.5487	0.8317	3425
	(0.2273)	(0.1385)							
Log(aides hours)			0.1724	0.0318	0.2938	0.1086	10.5487	0.8319	3425
			(0.2281)	(0.2121)	(0.1828)	(0.1650)			
RN hours/	0.0115	-0.0456					0.5927	0.8851	3337
(RN+LPN+aides hours)	(0.0214)	(0.0213)							
RN hours/			0.0049	-0.0021	-0.0406	-0.0548	0.5927	0.8851	3337
(RN+LPN+aides hours)			(0.0206)	(0.0250)	(0.0182)	(0.0337)			
LPN hours/	0.0108	0.0041					0.1441	0.8138	3330
(RN+LPN+aides hours)	(0.0122)	(0.0168)							
LPN hours/			0.0103	0.0112	0.0050	0.0114	0.1441	0.8139	3330
(RN+LPN+aides hours)			(0.0113)	(0.0139)	(0.0136)	(0.0263)			
Aides hours/	-0.0239	0.0406					0.2609	0.8538	3337
(RN+LPN+aides hours)	(0.0254)	(0.0235)							
Aides hours/			-0.0164	-0.0111	0.0350	0.0390	0.2609	0.8536	3337
(RN+LPN+aides hours)			(0.0272)	(0.0230)	(0.0219)	(0.0344)			

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Unreported covariates include fixed effects for month, year and hospitals.

Table 3-6 Regressions of Total Cost of Nursing Personnel, Difference in Difference Regression

Variables	Won	Lost	Won1to2	Won3plus	Lost1to2	Lost3plus	Mean of Y	R Squared	Observations
Log(RN cost)	0.0206	0.0156					14.6245	0.9716	3609
	(0.0885)	(0.0679)							
Log(RN cost)			0.00008	0.0262	0.0145	-0.0249	14.6245	0.9716	3609
			(0.0826)	(0.0948)	(0.0627)	(0.0889)			
Log(LPN cost)	0.0806	0.0804					12.6438	0.8442	3479
	(0.1438)	(0.1397)							
Log(LPN cost)			0.0690	0.0664	0.1599	-0.0358	12.6438	0.8443	3479
			(0.1296)	(0.1530)	(0.1369)	(0.2308)			
Log(aides cost)	-0.1211	0.5053					12.8407	0.8205	3441
	(0.3130)	(0.2674)							
Log(aides cost)			-0.0384	-0.0655	0.5838	0.2800	12.8407	0.8205	3441
			(0.3374)	(0.2486)	(0.3048)	(0.2486)			
Log(RN+LPN+aides	0.0366	0.0244					15.0640	0.9740	3393
costs)	(0.0961)	(0.0540)							
Log(RN+LPN+aides			0.0225	0.0430	0.0217	-0.0154	15.0640	0.9740	3393
costs)			(0.0878)	(0.0950)	(0.0503)	(0.0731)			
RN cost/	0.0006	-0.0264					0.7238	0.8903	3393
(RN+LPN+aides cost)	(0.0144)	(0.0142)							
RN cost/			-0.0066	-0.0020	-0.0227	-0.0316	0.7238	0.8903	3393
(RN+LPN+aides cost)			(0.0160)	(0.0206)	(0.0120)	(0.0234)			
LPN cost/	0.0091	0.0066					0.1231	0.8157	3393
(RN+LPN+aides cost)	(0.0094)	(0.0122)							
LPN cost/			0.0092	0.0093	0.0066	0.0133	0.1231	0.8157	3393
(RN+LPN+aides cost)			(0.0081)	(0.0116)	(0.0101)	(0.0194)			
Aides cost/	-0.0097	0.0198					0.1531	0.8854	3393
(RN+LPN+aides cost)	(0.0170)	(0.0135)							
Aides cost/			-0.0025	-0.0073	0.0161	0.0183	0.1531	0.8753	3393
(RN+LPN+aides cost)			(0.0194)	(0.0183)	(0.0124)	(0.0206)			

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Unreported covariates include fixed effects for month, year and hospitals.

Table 3-7 Regressions of Staffing Ratio of Payroll Nursing Personnel, Difference in Difference Regression

Variables	Won	Lost	Won1to2	Won3plus	Lost1to2	Lost3plus	Mean of Y	R Squared	Observations
RN hours/Number of Discharges	-0.5712 (2.8450)	0.2139 (1.3898)					32.7710	0.8347	3556
RN hours/Number of Discharges			-1.0404 (3.7677)	0.5162 (1.6879)	1.1622 (1.6344)	1.3056 (2.4258)	32.7710	0.8347	3556
RN hours/(Beds*Days)	0.2353 (0.2389)	0.0969 (0.1966)					2.1946	0.8975	3542
RN hours/(Beds*Days)			0.2218 (0.2203)	0.2141 (0.2244)	0.1192 (0.1449)	0.0613 (0.2530)	2.1946	0.8975	3542
LPN hours/Number of Discharges	-0.4922 (1.3401)	0.6760 (0.8012)					10.7409	0.8645	3431
LPN hours/Number of Discharges			0.1420 (0.8145)	-0.4664 (1.7752)	0.9933 (0.6908)	1.7517 (1.5919)	10.7409	0.8645	3431
LPN hours/(Beds*Days)	0.0583 (0.0600)	0.0559 (0.0719)					0.5108	0.7979	3432
LPN hours/(Beds*Days)			0.0545 (0.0545)	0.0582 (0.0612)	0.0681 (0.0683)	0.0566 (0.0913)	0.5108	0.7979	3432
Aides hours/Number of Discharges	-1.3564 (1.7101)	0.8457 (1.0059)					21.9083	0.8948	3398
Aides hours/Number of Discharges			-1.7208 (2.1248)	-0.1170 (2.0714)	1.4264 (0.9219)	0.8575 (1.6514)	21.9083	0.8948	3398
Aides hours/(Beds*Days)	0.0357 (0.1510)	0.2712 (0.1340)					0.9031	0.7989	3399
Aides hours/(Beds*Days)			0.0486 (0.1263)	0.0221 (0.1654)	0.2829 (0.1383)	0.2145 (0.1563)	0.9031	0.7989	3399

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Unreported covariates include fixed effects for month, year and hospitals.

Table 3-8 Regressions of Relative Hours and Utilization of Contract Nursing Personnel, Difference in Difference Regression

Variables	Won	Lost	Won1to 2	Won3plus	Lost1to2	Lost3plus	Mean of Y	R Squared	Observations
Log (Contract Nursing Personnel wage)	-0.0273 (0.0609)	-0.0441 (0.0534)					3.5278	0.5568	1952
Log (Contract Nursing Personnel wage)			-0.0191 (0.052)	-0.0170 (0.0805)	-0.0799 (0.0539)	-0.0751 (0.0741)	3.5278	0.557	1952
Log (Contract Nursing Personnel hours)	0.8945 (0.4100)	0.2285 (0.5166)					8.4116	0.6894	1956
Log (Contract Nursing Personnel hours)			0.6244 (0.3434)	0.2137 (0.7192)	0.0897 (0.5411)	0.6199 (0.5978)	8.4116	0.6891	1956
Log(payroll+ contract nursing personnel hours)	0.3339 (0.1218)	0.0678 (0.0771)					12.3350	0.966	1866
Log(payroll+ contract nursing personnel hours)			0.2670 (0.1040)	0.1861 (0.1281)	0.0220 (0.0986)	0.0082 (0.1055)	12.3350	0.9657	1866
Contract/total nursing personnel hours	-0.0043 (0.0166)	-0.0035 (0.0091)					0.0395	0.6049	1867
Contract/total nursing personnel hours			0.0027 (0.0102)	-0.0297 (0.0251)	0.0022 (0.0113)	0.0066 (0.0126)	0.0395	0.6083	1867

Note: Standard errors reported in parentheses allow for arbitrary correlation in errors within a hospital over time. Unreported covariates include fixed effects for month, year and hospitals.

Table 3-9 CPS merged Outgoing Rotation Group Union Status Change, 1984-2004

Interviewees	Month8 - nonunion member	Month8 – union member
Month4 - nonunion member	12,786(73.10%)	1,086(6.21%)
Month4 – union member	1,062(6.07%)	2,558(14.62)

Note: For all RN, LPN and aides interviewees in CPS merged Outgoing Rotation Group

Table 3-10 CPS merged Outgoing Rotation Group Sample Descriptive Statistics, 1984-2004

Variable	RN Sample	LPN Sample	Aides sample	Total Sample
Mean age	40.4	41.6	42.4	40.9
	(9.8))	(10.3)	(11.9)	(10.3)
% College Graduate	74.08%	23.91%	8.99%	57.84%
% White	87.31%	80.51%	65.39%	82.93%
% Black	6.34%	16.53%	29.50%	11.29%
% Hispanic	6.95%	7.57%	9.09%	7.37%
Mean hourly wage	23.06	14.95	12.18	20.29
	(6.31)	(3.81)	(4.93)	(7.36)
Mean (weekly earning/usual hours)	23.61	15.27	12.63	20.88
	(6.86)	(4.30)	(5.32)	(7.81)
Mean usual hours of working	36.69	36.72	37.62	36.85
	(7.40)	(6.59)	(5.94)	(7.10)
Mean Hours working last week	37.21	36.81	37.59	37.23
	(9.96)	(9.39)	(9.06)	(9.75)
Observations	21,720	3,237	4,973	29,930

Note: All samples are for full time workers with usual hours of working per week>=20 and hourly wage > $4. Standard deviations are reported in parentheses.

Table 3-11 Regression Results from Merged CPS samples, Difference in Difference Regressions, 1992-2000

Variables	Log (hourly wage)	Log(weekly earning/usual hours)	Log(weekly earning)	Usual hours of working per week (1)	Hours working last week (2)	(2)-(1)
	RN, LPN and aides samples (12349 observations)					
Union*RN	0.0184	0.0229	0.0355	0.4306	1.3029	0.9856
	(0.0192)	(0.0166)	(0.0175)	(0.2775)	(0.5298)	(0.4889)
Union*LPN	-0.0045	0.0177	0.0145	-0.3426	-0.1562	-0.1799
	(0.0490)	(0.0447)	(0.0473)	(0.7481)	(1.4124)	(1.3034)
Union*Aides	0.1058	0.0991	0.1060	0.2325	1.9131	1.7382
	(0.0359)	(0.0315)	(0.0333)	(0.5265)	(1.0030)	(0.9256)
R Squared	0.8907	0.8604	0.8662	0.8682	0.7903	0.6968
	For RN sample only (9,400 observations)					
Union	0.0112	0.0308	0.0430	0.4014	1.4135	1.0578
	(0.02)	(0.0173)	(0.0182)	(0.2980)	(0.5645)	(0.5159)
R Squared	0.8188	0.7735	0.8137	0.8775	0.8005	0.7051

Note: Standard errors are reported in parentheses. Unreported covariates include fixed effects for month, year and individual fixed effect.

Table 3-12 Regression Results from Merged CPS samples,
Cross Sectional Regressions, 1992-2000

Variables	Log (hourly wage)	Log(weekly earning/usual hours)	Log(weekly earning)	Usual hours of working per week (1)	Hours working last week (2)	(2)-(1)
	RN, LPN and aides samples (12349 observations)					
Union*RN	0.0382	0.0414	0.0496	0.3111	1.0852	0.6990
	(0.0153)	(0.0134)	(0.0147)	(0.2562)	(0.4280)	(0.3725)
Union*LPN	0.0113	0.0033	0.0214	0.2991	1.4140	0.8894
	(0.0418)	(0.0377)	(0.0413)	(0.7219)	(1.2011)	(1.0453)
Union*Aides	0.0810	0.1125	0.1233	0.3458	1.2356	0.8478
	(0.0307)	(0.0268)	(0.0294)	(0.5128)	(0.8567)	(0.7456)
R Squared	0.8315	0.7985	0.7922	0.7505	0.6856	0.5957
	For RN sample only (9,400 observations)					
Union	0.0210	0.0312	0.0389	0.3199	1.0535	0.6679
	(0.0158)	(0.0140)	(0.0153)	(0.2807)	(0.4588)	(0.3937)
R Squared	0.7311	0.6801	0.7131	0.7648	0.7055	0.6161

Note: Standard errors are reported in parentheses. Unreported covariates include age, age squared, fixed effects for race, marital status, education, ethnicity, sex, year, and state.

Figure 2-1 % Postpartum Length of Stay of Newborns Less than 2 days, Vaginal Deliveries without Complications

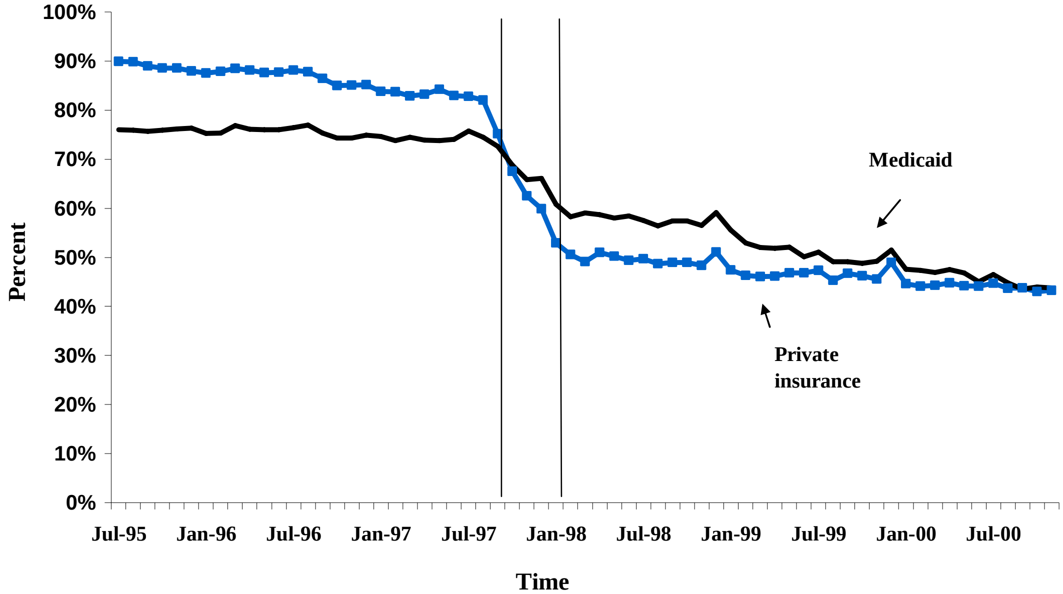


Figure 2-2 % Postpartum Length of Stay of Newborns Less than 4 days, C-section Deliveries without Complications

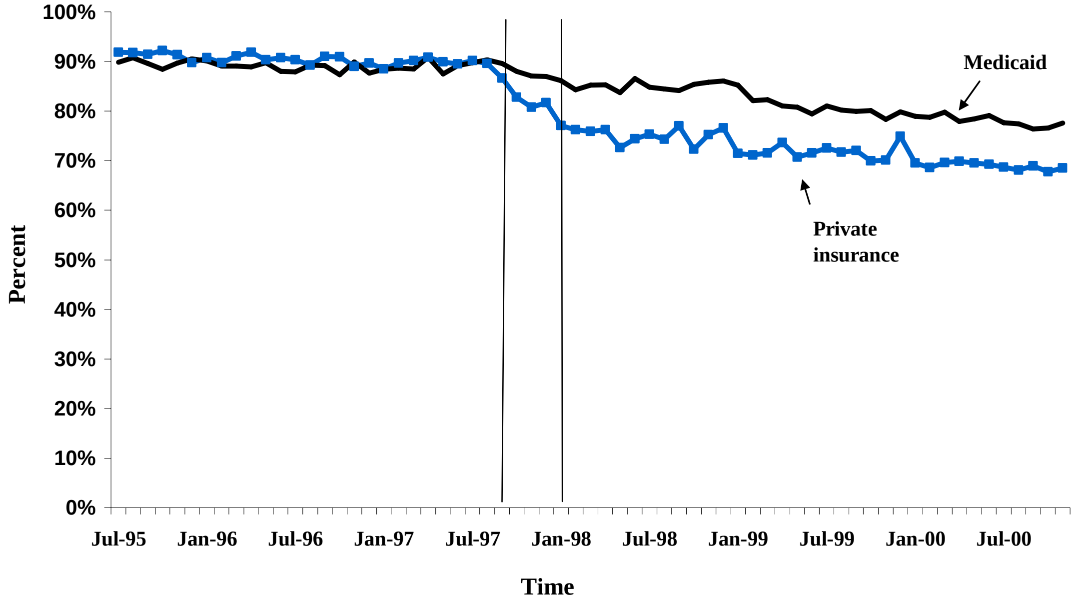


Figure 2-3   % Postpartum Length of Stay of Newborns Less than 2 days, Vaginal Deliveries with Complications

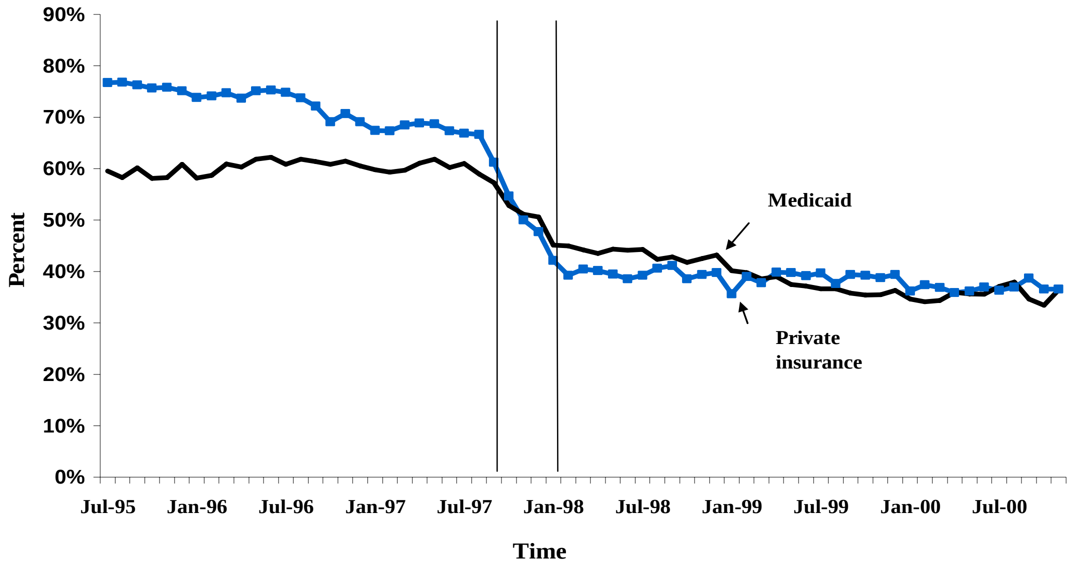


Figure 2-4 % Postpartum Length of Stay of Newborns Less than 4 days,
C-section Deliveries with Complications

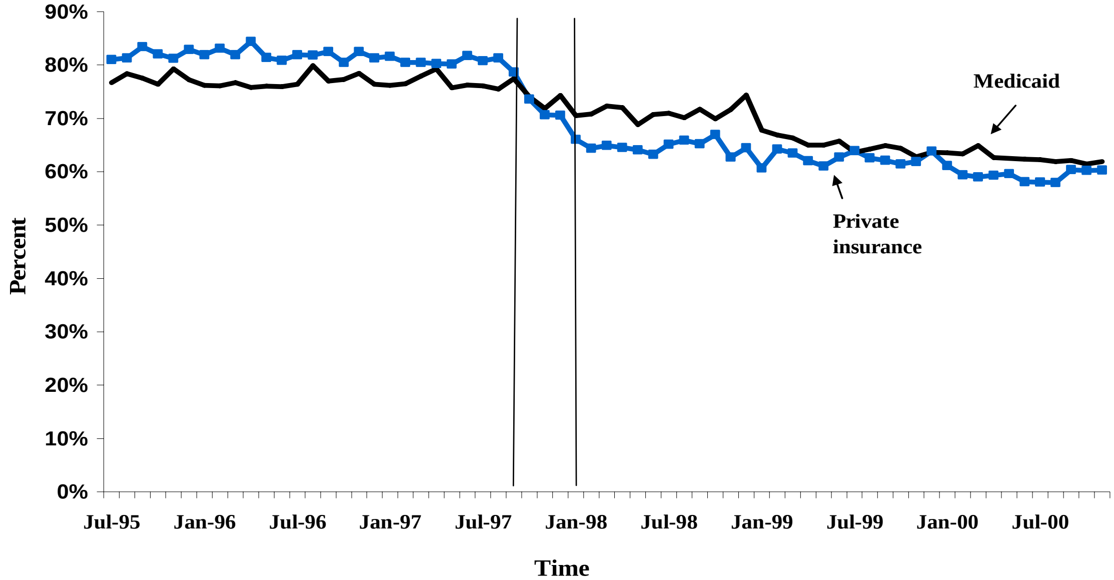


Figure 2-5 % with Complications

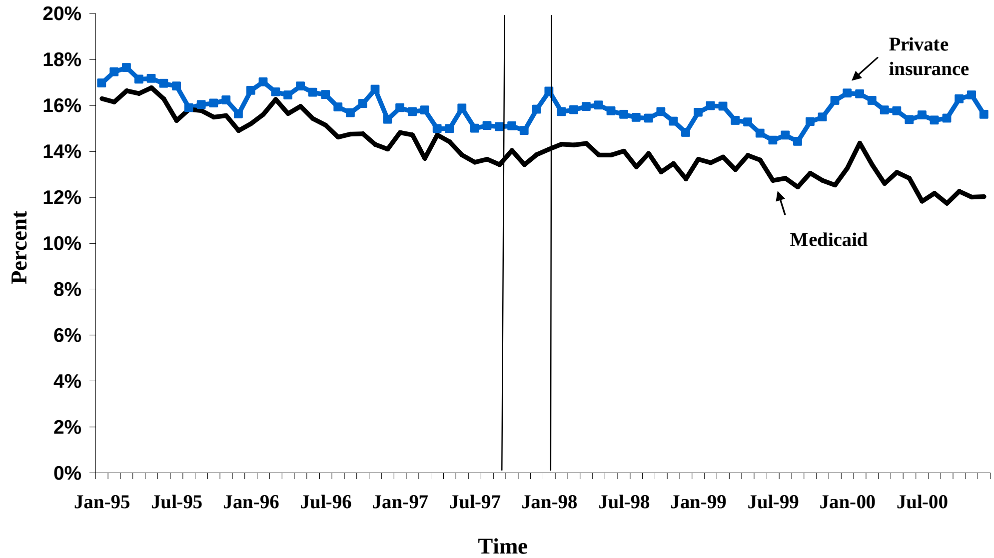


Figure 2-6 % C-section Delivery to Total Deliveries

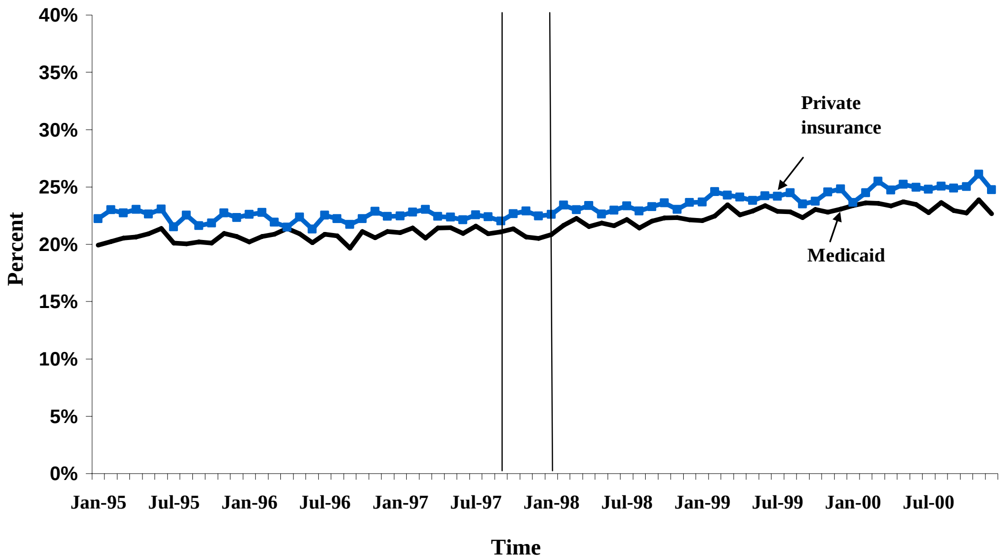


Figure 2-7 % Different Insurance Type

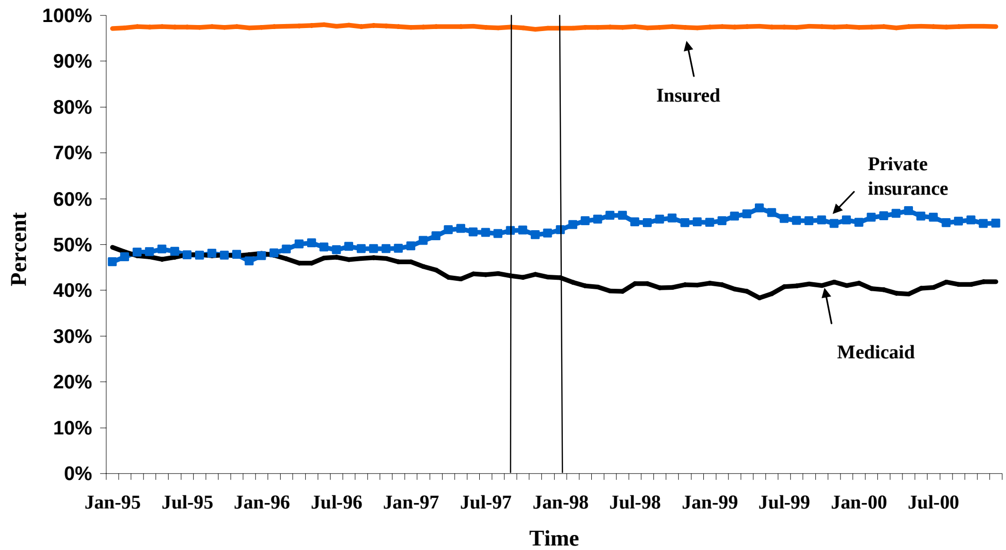


Figure 2-8 28 Day Readmission Rate of Newborns,
Vaginal Deliveries without Complications

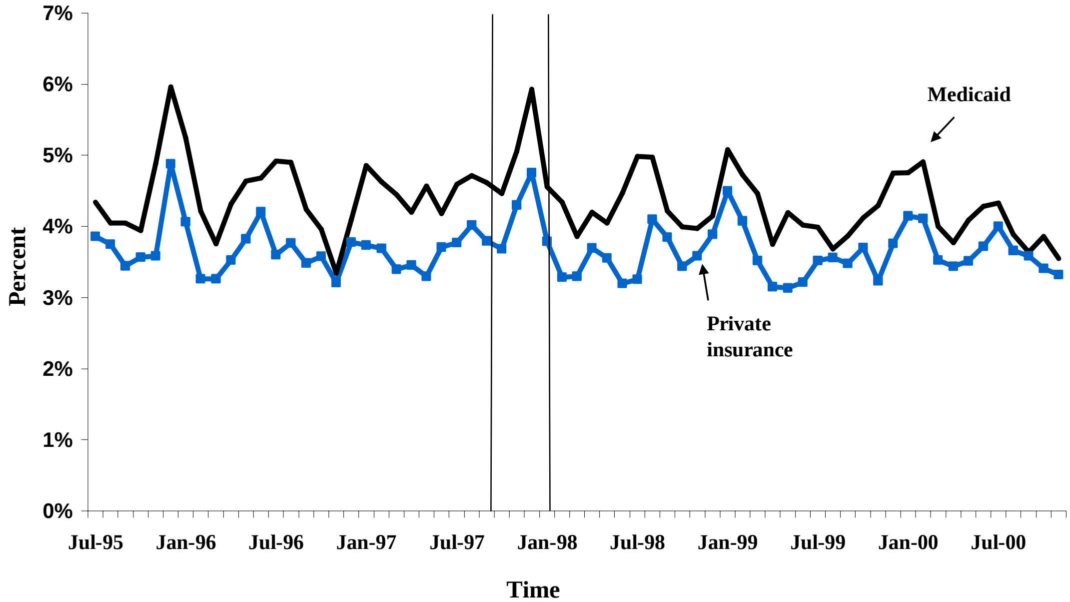


Figure 2-9 28 Day Readmission Rate of Newborns,
C-section Deliveries without Complications

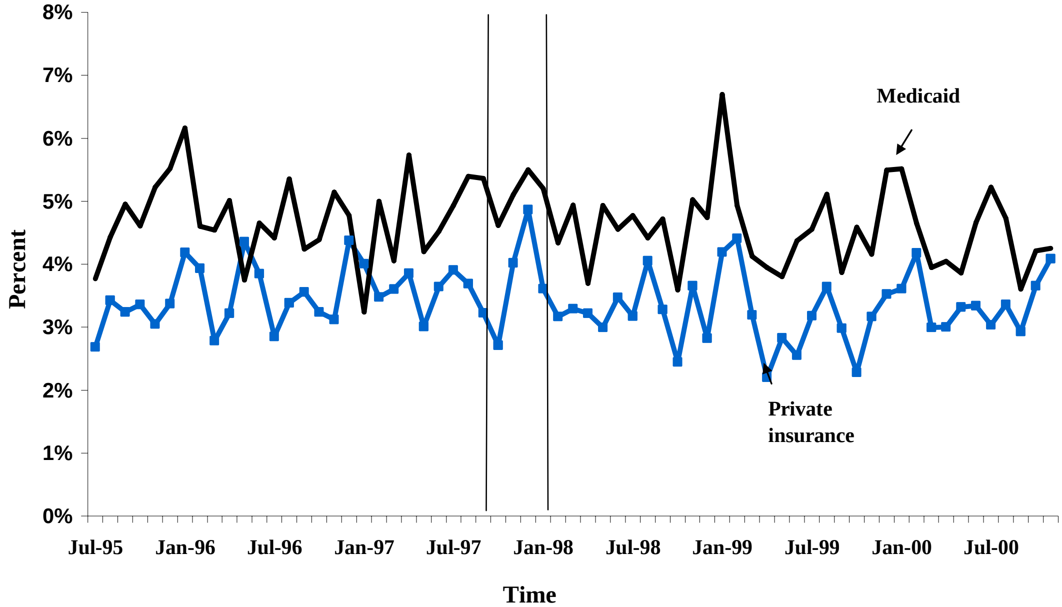


Figure 2-10 28 Day Readmission Rate of Newborns,
Vaginal Deliveries with Complications

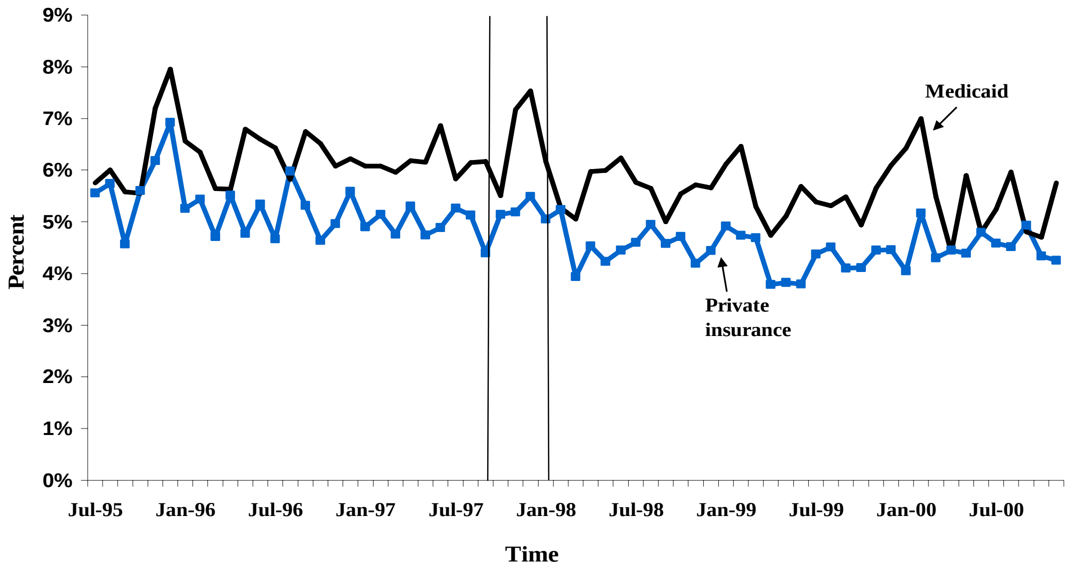


Figure 2-11 28 Day Readmission Rate of Newborns,
C-section Deliveries with Complications

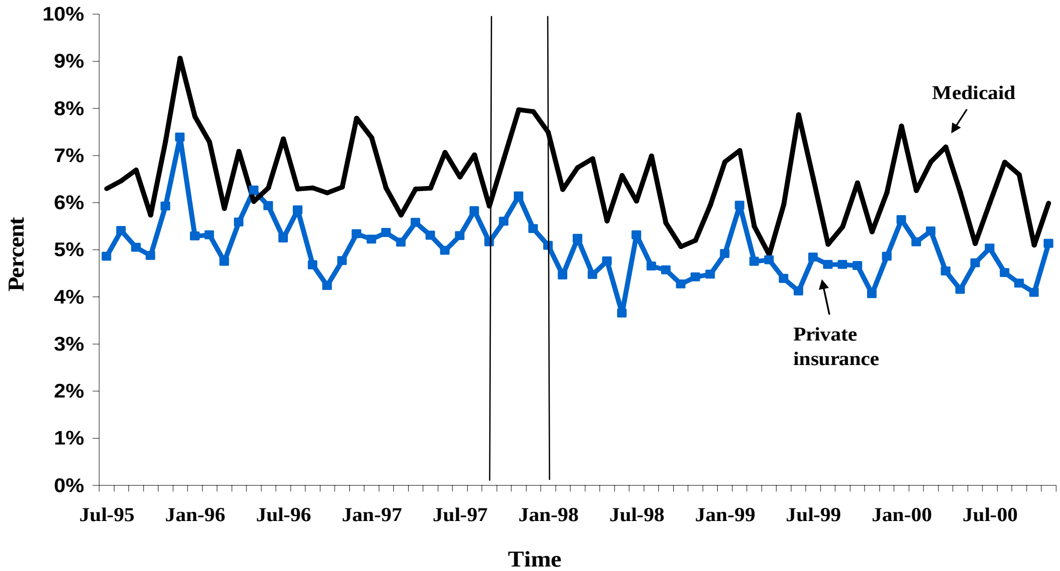


Figure 2-12 Newborn's 28 day Readmission Rate and
90-180 Year Old 28 days Hospital Admission Rate,
Normal Vaginal Deliveries and Private Insurance

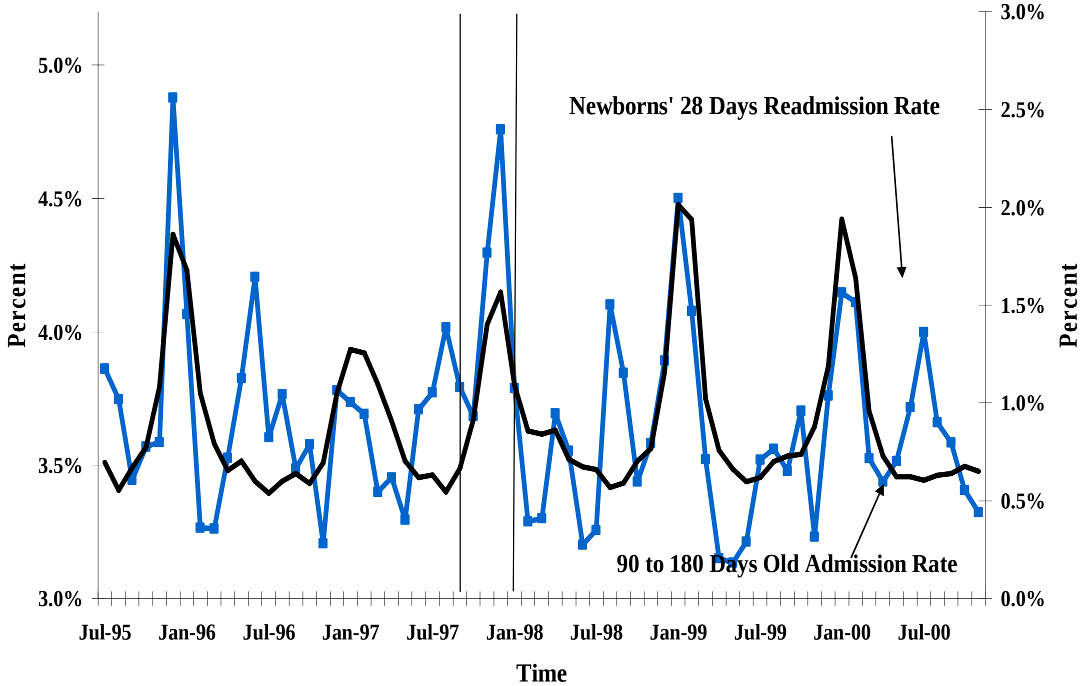


Figure 2-13 Newborn's 28 day Readmission Rate and
90-180 Year Old 28 days Hospital Admission Rate,
Normal Vaginal Deliveries and Medicaid

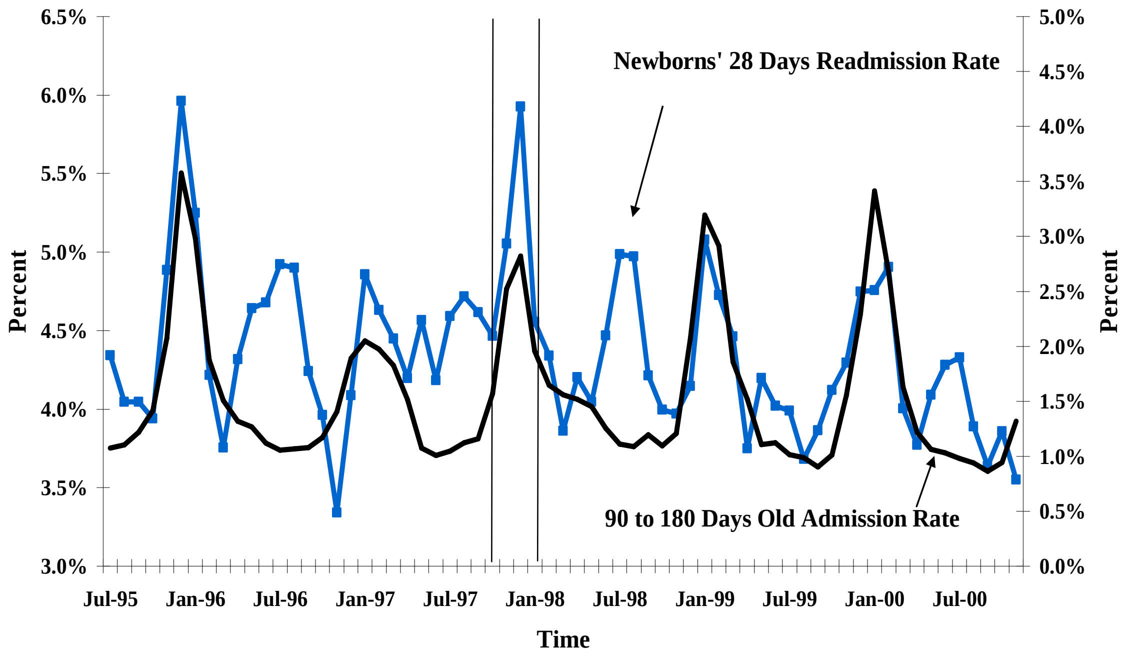
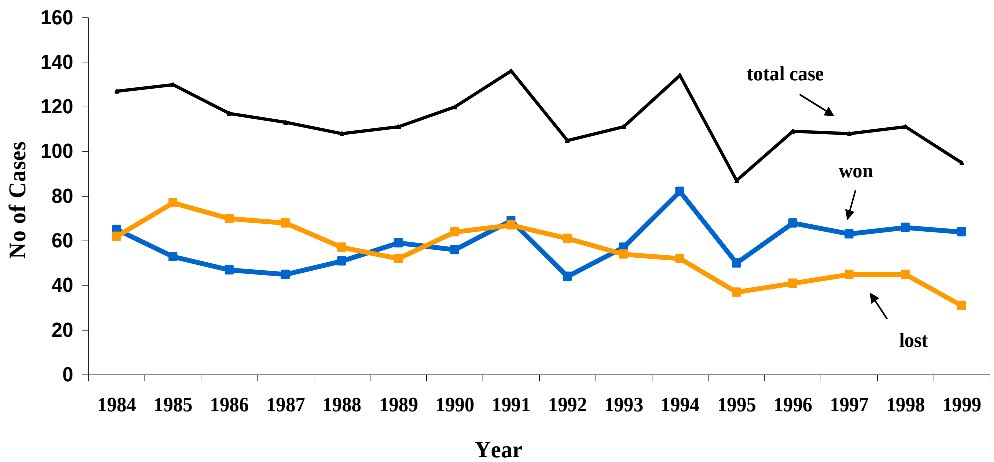


Figure 3-1 Hospital Unionization Election Cases from National Labor Relation Board Union Election Report



Appendix A

Datar and Sood (2006) used public-use versions of the linked California hospital discharge data for the years 1991 – 2000 to study the effect of the Federal early discharge law. The authors use three different outcomes: hours in the hospital, 28 day readmission rate and 1 year mortality rate of newborns. In their data set, they do not have the birth date of newborns and could only identify the year of birth.

Datar and Sood used an interrupted time series design to examine the effect of federal early discharge law. Instead of estimating models for sub samples of deliveries, they pooled all births together in one model: vaginal and cesarean births, private insured, Medicaid insured and uninsured births, and uncomplicated and complicated births. Although they used their entire sample (1991-2000) for the hours of admission equation, they used data for a smaller subset of years, 1995 – 2000, for the 28-day readmission models. Since we are using July 1995 to December 2000 data and also using the interrupted time series design strategy to examine the effect of early discharge law on California newborns, our results should be very close to results of Datar and Sood (2006). However, Datar and Sood (2006) found a large impact of Federal law on 28 days readmission rates. Specifically, they estimate the Federal law produced a 10 percent reduction the first year the law was in effect and almost a 30 percent reduction of 28 day readmission rates by year 2000. The increasing impact of the law over time is surprising since they do not find the impact of the law on hours in the hospital increases over time. Their results are different from ours that in our largest sample, uncomplicated vaginal deliveries, the federal law had a small and statistically insignificant effect on 28 days readmission rates

Datar and Sood excluded all multiple-birth, premature and low birth weight newborns for their sample, and we only delete newborns with extreme low or high birth weight or extreme short gestation period. So on average, our sample has a higher 28 days readmission rate and we work with four distinct subsamples in our study: complicated and uncomplicated vaginal and cesarean deliveries. Given the small number of births this impacts and since these births are unlikely to be impacted by the law anyway, the difference of sample selection should not cause the large differences in the results.

Datar and Sood examined the effect of federal law by estimating a model with a year time trend and 3 terms estimating changes in readmission rate 1,2, and 3 years after the passage of the federal law. Sine they do not have information of birth month of newborns and the California early discharge law took into effect in the middle of year 1997, they could not examine the effect of California law.

Based on figure 2-8 and Figure 2-9 which depict the change of 28 day readmission rates over time, there was a noticeable increase in readmission rates in December of 1997, the fourth full month that the California law was in effect and the last month before the federal law took effect. As we demonstrate in chapter 2, this was caused by the particularly heavy flu season in California that winter. Datar and Sood do not control for this type of month-to-month variation in readmission rates because they do not have month of birth in the public use version of their data set. Since this large increase in re-admissions occurs right before the Federal law goes into effect, using an interrupted time series design might make it look like the law is effective. Their model also suggests that the pre-treatment trend in re-admission rates are positive so if readmission rates after the law stay constant, their model will also estimate an increasing impact of the law over time.

To test this conjecture, we estimate versions of the models in Datar and Sood (2006) that control to a greater degree for the month to month variation in readmission rates. First, we use our sample to replicate the basic results in their paper. We construct a sample similar to theirs by pooling all deliveries into one model, we use our set of demographic characteristics, but, we initially ignore the fact we have the exact date of birth and add to the Datar and Sood (2006) model an annual time trend. In the first row of Table A-1, we reports the logit regression results from this replication exercise. Datar and Sood report the parameters from the logit model which is by definition the change in the log odds of a 28-day readmission generated by the law. Since readmission rates are so low, this ends up to be very close to a percentage change in the risk of an admission, so we only report logit coefficients in Table A-1.

We can replicate the statistically significant reduction in re-admissions as in Datar and Sood although we do not generate the large increase in the effectiveness of the law over time. Overall however, as a first pass, our model is strikingly similar to theirs, especially in the 1st year of the law. Their logit coefficient estimate was -0.093 and we estimate a value of -0.083.

In the next row, we estimate a model where we keep the annual time trend but now use the month of birth information in our restricted use sample and add a dummy for the four months when the state law was only in effect, from September through December of 1997. These results are reported in the second row of the table. The result from this model suggest that the state law had a large positive effect on 28 days readmission rate which, as we argue in Chapter 2, is simply a result of the fact the law went into effect in a quarter when there were high re-admissions due to the cold and flu season. However, by simply adding this dummy variable, the large impacts of the federal law during its first three years are almost completely eliminated. Now, we estimate that the federal law has a small and statistically insignificant effect on readmission rates.

Since the spike in September 1997 through December 1997 made the federal law look effective in the Datar and Sood (2006) model, we estimate some addition models where we successively eliminated months December through September. These results are reported in rows 3, 4, 5, and 6 of Table A-1. The results change a lot by deleting these few months. These results provide additional evidence that the large impact of federal law estimated in the Datar and Sood paper was caused by their lack of data on the month of birth which prevented them from capturing the large spike in readmission rates for children born right before the federal law went into effect.

In Table A-2, we do the similar test on four sub samples used in our work: uncomplicated and complicated vaginal and cesarean delivery samples. First we report regression results from the Datar and Sood model, and then estimate models that add in a dummy to capture the state law effect. As in the results from Table A-1, we see that in the regression results for uncomplicated deliveries, not controlling for the state law generates a large and statistically significant reduction in readmission rates but once we control for the state law, these results vanish.

In summary, due to the lack of the month of birth of newborns in their pubic use data set, Datar and Sood could not control the spike of readmission rates during the flu season just prior to implementation of the Federal law. Their results are therefore contaminated by the extraordinarily high readmission rates during period September 1997 through December 1997. Since we can identify month/day of birth, using 90 to 180 days old babies' 28 day readmission index as a control variable and dummies for the state law, we could control for this one time shock in readmissions and our results indicate little if any benefit of loner hospital stays for uncomplicated vaginal deliveries.

Table A-1 Logit Regression Results,
Total Samples with Whether Readmitted within 28 Days as the Dependent Variable

	1 Year after Federal Law	2 Year after Federal Law	3 Year after Federal Law	State Law Effect	Mean of Y	Observations
Datar and Sood Model	-0.0832 (0.0141)	-0.1037 (0.0194)	-0.0875 (0.0250)		4.64%	2,404,829
With State law effect	-0.0183 (0.0166)	-0.0111 (0.0230)	0.0326 (0.0298)	0.1221 (0.0163)	4.64%	2,404,829
Without Dec1997 data	-0.0658 (0.0147)	-0.0788 (0.0202)	-0.0555 (0.0260)		4.64%	2,367,814
Without Nov1997-Dec1997 data	-0.0402 (0.0152)	-0.0425 (0.0210)	-0.0081 (0.0271)		4.64%	2,333,165
Without Oct1997-Dec1997 data	-0.0292 (0.0159)	-0.0267 (0.0219)	0.0124 (0.0283)		4.64%	2,296,441
Without Sep1997-Dec1997 data	-0.0183 (0.0166)	-0.0111 (0.0230)	0.0327 (0.0298)		4.64%	2,258,761

Note: Standard errors reported in parentheses. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, year time trend, payer of deliveries and complicated or not.

Table A-2 Logit Regression Results,
Uncomplicated Vaginal and Cesarean Delivery Samples

	Datar and Sood Model	With state law effect	Datar and Sood Model	With state law effect
	28-Day Readmit. (Uncomplicated Vaginal Deliveries, 1,353,623 observations)		28-Day Readmit. (Complicated Vaginal Deliveries, 499,422 observations)	
1 Year after	-0.0778	0.0135	-0.0623	-0.0400
Federal Law	(0.0198)	(0.0234)	(0.0284)	(0.0333)
2 Year after	-0.1069	0.0231	-0.0862	-0.0544
Federal Law	(0.0271)	(0.0324)	(0.0391)	(0.0463)
3 Year after	-0.1258	0.0430	-0.0217	0.0195
Federal Law	(0.0350)	(0.0419)	(0.0503)	(0.0597)
State law		0.1696		0.0425
		(0.0227)		(0.0332)
Mean of Y	0.0406	0.0406	0.0571	0.0571
	28-Day Readmit. (Uncomplicated Cesarean Deliveries, 233,969 observations)		28-Day Readmit. (Complicated Cesarean Deliveries, 317,815 observations)	
1 Year after	-0.1238	-0.0903	-0.1089	-0.0386
Federal Law	(0.0495)	(0.0583)	(0.0351)	(0.0412)
2 Year after	-0.1619	-0.1143	-0.0925	0.0074
Federal Law	(0.0678)	(0.0807)	(0.0480)	(0.0570)
2 Year after	-0.1729	-0.1112	-0.0311	0.0988
Federal Law	(0.0878)	(0.1045)	(0.0619)	(0.0736)
State law		0.0618		0.1345
		(0.0565)		(0.0407)
Mean of Y	0.0408	0.0408	0.0597	0.0597

Note: Standard errors reported in parentheses. Sample means are for the pre-law period. Unreported covariates include fixed effects for age group, race, education and ethnicity of mother; race and education of father, sex and birth hour of newborns, year time trend, and payer of deliveries.

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