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# The impact of early discharge laws on the health of newborns

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**Abstract**

Using an interrupted time series design and a census of births in California over a 6-year period, we show that state and federal laws passed in the late 1990s designed to increase the length of postpartum hospital stays reduced considerably the fraction of newborns that were discharged early. The law had little impact on re-admission rates for privately insured, vaginally delivered newborns, but reduced re-admission rates for privately insured c-section-delivered and Medicaid-insured vaginally delivered newborns by statistically significant amounts. Our calculations suggest the program was not cost saving.



*JEL classification:* I10; I12; I18

*Keywords:* Postpartum care; Early discharge; Newborn health

## 1. Introduction

Between 1970 and 1992, the average postpartum length of stay for mothers who delivered vaginally declined by 46%, 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% (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 (Declercq, 1999; Eaton, 2001). 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 1 January 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.[3] There is,

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[3] For example, see Udom and Betley (1998), Dato et al. (1995), Liu et al. (2004), and Madlou-Kay and DeFor (2005).

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however, limited and conflicting evidence about the impact of these laws on the health of the mothers and their newborns.[4]

In this paper, 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 26 August 1997, mandated that insurance carriers provide coverage for at least 48-h hospital stays for normal vaginal deliveries and at least 96-h 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 coverage for follow-up visits. Both the California and federal law exempted Medicaid births from coverage. However, as we demonstrate below, the unique structure of Medicaid managed care in California meant that the passage of the federal and state laws provided coverage to some Medicaid patients and an extension of the state law a year later further impacted Medicaid patients.

Using an interrupted time series model, we find that the California and federal law generated large and abrupt drops in the early discharge rate among newborns for vaginal and cesarean deliveries for births insured by private carriers. The impact on Medicaid births was not as large but substantial nonetheless. We find that the law had no statistically significant impact on the 28-day re-admission rates for privately insured newborns from vaginal deliveries. There is however evidence that births with higher rates of complications, such as Medicaid vaginal deliveries and privately insured c-sections, experienced statistically significant reductions in re-admission rates. There is scattered evidence that the laws reduced 28-day mortality rates for some newborns but the results are inconsistent and in most cases, not statistically significant.

Because early discharge laws exogenously increased the postpartum length of stay, we use their passage as instrumental variables for length of stay in a two-stage least squares (2SLS) model to obtain consistent estimates of the impact of early discharges on medical outcomes. In the samples of vaginal deliveries with complications and c-sections, being discharged early produces large increases in the probability of re-admission. Among newborns from uncomplicated vaginal deliveries, there is no evidence that an early discharge negatively impacts health for those with private insurance but there are large and statistically precise benefits for Medicaid patients in this subsample.

Our methods are similar to Datar and Sood (2006) who examined the same question using a public use version of the data we discuss below. Their paper reported estimates of the impact of the federal law on early discharge and re-admission rates in California. Because the public use data only identified the year of birth, the authors are unable to examine the impact of the State law and they did not consider the impact of expanding the state law to all Medicaid patients. Similarly, they did not combine the first-stage and reduced-form estimates into a 2SLS estimate as we do below. Our results are similar along some dimensions to theirs but as we show below, their use of public-use data generates estimates of the law's impact on re-admission rates that are too large.

## 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' (Braveman et al., 1995; Eaton, 2001). As an increasing number of new mothers were discharged prior to the medically recommended length of stay, the press took notice and began using terms such as "drive through deliveries" or "drive by deliveries" to describe early discharges.[5]

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

[4] Madden et al. (2004) found little evidence that early discharge placed infants at risk for readamission. In contrast, Meara et al. (2004) found that the early discharge law in Ohio reduced readmission rates but the estimates were statistically insignificant. Finally Datar and Sood (2006) found that the federal law generated large and statistically significant reductions in readmissions for newborns in California.

[5] 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*."

early[6] 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. Since then, early discharge laws have been adopted by 42 other states (Eaton, 2001).

Because of federal statute (the Employees Retirement Income Security Act), a large fraction of those with insurance were exempt from these state law, such as those with employer-provided insurance that were self-insured or written in another state. State lawmakers began to lobby the federal government to help close these gaps in coverage and these efforts resulted in the passage of the Newborns' and Mothers' Health Protection Act of 1996 (NMHPA), which was signed into law by President Clinton on 26 September 1996 and became effective on 1 January 1998. The federal law mandated insurance carriers provide for both the mother and newborn coverage for at least a 48-h hospital stay after a vaginal delivery and at least a 96-h hospital stays after a cesarean delivery. 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, 26 August 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.[7] However, the unique structure of California's Medicaid managed care plans meant that some Medicaid recipients were in fact covered by these statutes.

According to state policy adopted in 1993, enrolment in Medicaid managed care plans would be done at the county level and each county would be part of one of four types of structures[8]: a County Organized Health System (COHS), a two-plan model, a geographic managed care model (GMC), or no managed care. As their name suggests, COHS plans are managed care plans run by the county. In the Two-Plan model, one of the options would be a county plan, similar in structure to what is offered in COHS counties, and the other would be a private insurance plan that has a contractual obligation to insure county residents in their plan. In GMC counties, Medicaid recipients can enroll in privately managed care plans that provide contracts to enrollees in many counties. Duggan (2004) notes that the share of California Medicaid recipients enrolled in a managed care plan increased from 10% in 1991 to 51% in 1999.

Given the structure of Medicaid managed care in California, recipients enrolled in a privately run managed care plan were subject to the federal and state early discharge laws but enrollees in county-run Medicaid managed care plans and fee-for-service Medicaid were explicitly exempted from both the federal and state statutes. This distinction based on the source of insurance was confirmed in a memo written by the Department of Health Services on 16 January 1998.[9]

To provide equity in coverage for all Medicaid recipients, a bill was introduced in the state legislature in January of 1998 that would extend the California early discharge statute to all Medicaid patients in the state.[10] The bill was eventually passed on 26 August 1998, signed into law by the governor on September 20, 1998, and went into effect on 1 January 1999.[11]

## 3. Literature review

### *3.1. The health and medical consequences of short postpartum hospital stays*

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

[6] 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 h for uncomplicated vaginal births and a stay of less than 96 h for cesarean deliveries (American College of Obstetrics and Gynecology, 1992).

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

[8] Much of this text is a summary of the description of the California Medicaid managed care program found in Aizer, Currie and Moretti (forthcoming) and Duggan (2004).

[9] http://www.dhs.ca.gov/mcs/mcmcd/PDF/PolicyLetters1998_2003/Policy%2098-01.pdf.

[10] http://info.sen.ca.gov/pub/97-98/bill/asm/ab_1351-1400/ab_1397_bill_19980813_amended_sen.html.

[11] http://www.leginfo.ca.gov/pub/97-98/bill/asm/ab_1351-1400/ab_1397_bill_19980921_history.html.

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., 2000a,b). The magnitudes of these effects are sometimes quite large.[12]

In contrast, Dalby et al. (1996), Kotagal 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 found 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 subsequent treatment for jaundice. Although many of these studies have small samples, not all of the results are simply Type II errors in that some studies use very large samples.[13]

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 re-admissions and those with the greatest chance of a re-admission also have longer stays, then the expected negative relationship between length of stay and hospital re-admission rates estimated in OLS models should be biased upwards (closer to zero).

This type of selection 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) measured in days 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 re-admitted within 28 days after birth. 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 1. We report coefficients from the length of stay regression and marginal effects from the 28-day re-admission logit.

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 re-admission 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 shorter length of stays and lower odds of being re-admitted 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 re-admission logit is statistically insignificant. Likewise, the coefficients on white and black children are of opposite signs in the two models.

### 3.2. *Analyses of early discharge laws*

Udom and Betley (1998), Dato et al. (1995), Liu et al. (2004), and Madlou-Kay and DeFor (2005) examined the impact of early discharge laws on the postpartum length of stay and costs. All studies show the laws increased the postpartum length of stay and increased costs, while some studies demonstrate the laws increased length of stays for those not impacted by the law such as Medicaid recipients and the self-insured.

[12] For example, using Washington State linked birth certificate and hospital discharge abstracts covering 310,578 live births from 1991 to 1994, Liu et al. (1997) used logistic regressions to assess the impact of an early discharge (a discharge less than 30 h after birth) on the risk of rehospitalization within 1 month of birth. They found that newborns discharged early had a 28% higher 7-day re-admission rate and a 12% 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% higher for infants discharged early compared to those with longer hospital stays.

[13] Using Ohio Medicaid Claims data linked to vital statistics files for 102,678 full-term births from 1 July 1991 to 15 June 1995, Kotagal et al. (1999) found that the fraction of newborns discharged early increased 185% over the period (from 21 to almost 60%). 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, Danielsen 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.

Table 1
OLS estimates of length of stay and logit estimates of 28-day re-admission equations, Newborns in California private/Medicaid sample, 1995–1996

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

OLS parameter estimate or marginal effect. *Note*: Standard errors in parentheses. 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.

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.[14]

Meara et al. (2004) examined the impact of the Ohio early discharge law on the health of newborns in Medicaid. 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 re-admission 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 found that a follow-up visit that occurred within 3 days of discharge generated statistically significant reductions in hospital re-admission probabilities.

Our study is methodologically similar to Datar and Sood (2006) who utilized public-use versions of the data we use in this project to examine the impact of the federal early discharge law. Using data on all births in California over the 1995–2000 period, the authors found that the federal law reduced the odds of a 28-day re-admission rates among newborns by a 9.3% in the first year and by nearly 20% in the third year. Both of these results are statistically significant at conventional levels. However, as we demonstrate below, these results dramatically overstate the effectiveness of these laws. Because Datar and Sood only have public use data, they are unable to effectively control for month-to-month variation in re-admission rates. As we show below, moving to a restricted-use sample generates substantially smaller and statistically insignificant results.

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) concluded that "heterogeneity and limitations of methodology and study design substantially limit conclusions that may be drawn from published studies (p. 291)." Braveman et al. (1995) concluded 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) concluded 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) used large samples but had limited controls and no quasi-experimental variation. Studies such as Malkin et al. (2000a,b), Danielsen et al. (2000), Kotagal et al. (1999), and Liu et al. (1997), used large samples, excellent control variables, but no quasi experimental variation postpartum length of stay. Studies such as Meara et al. (2004), Madden et al. (2002) or Madden et al. (2004) exploited quasi-experimental variation and had excellent control variables but used samples that were too small to generate statistically precise results.

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 and that of Datar and Sood are therefore the largest sample ever used to analyze the impact of postpartum length of stay on health outcomes. Second, although random assignment clinical

[14] 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.

trials are the gold-standard for inferring causal relationships, the number of observations necessary to eliminate Type II error concerns for many low incidence outcomes makes clinical trials 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 most previous studies, we examine the impact of early discharge for infants whose mothers experienced complications either during pregnancy or labor. These results are instructive in that early discharge appears to have little clinical risk for newborns that *a priori* have a low risk of re-admission.

## 4. Constructing the analytic file

### 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 1 January 1995 to 31 December 2000. The data set is generated and maintained by the State of California Office of Statewide Health Planning and Development (OSHPD) and is 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, 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 and 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 1 year after discharge. 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 6 years in our data set, there are approximately 3 million births in total, with almost 1.7 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 re-admission and mortality rates are higher for infants than mothers and as we demonstrate below, our samples have just enough power to produce statistical significance in two-stage models for these adverse events.

### 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 newborns. The most obvious outcome is whether the newborn was discharged early from the hospital. The federal and state laws provide coverage for a 48-h hospital stay following a vaginal birth and a 96-h stay following a c-section. Although our data set has the hour of birth, it does not have the hour the mother and newborn were discharged, and as a result, we cannot measure whether the explicit hours restrictions defined in the laws were being followed. We can however approximate time spent in the hospital by measuring the length of stay in days, which is simply the difference in calendar days between admission and discharge. Subsequently, the key outcome in our analysis is whether the infant was *Discharged early* which, for mothers who delivered vaginally, is a dummy variable that equals 1 if the newborn spent less than two nights in the hospital. For newborns whose mother delivered by c-section, this outcome equals 1 if the newborn spent less than four nights in the hospital after a c-section.

This measure will, by construction, understate whether patients are in fact discharged prior to the 48 or 96 h limit. For example, a vaginally delivered baby born at 10:00 p.m. on a Monday and discharged at 4:00 p.m. on a Wednesday would be counted by our measure as NOT being discharged early when in fact they were discharged prior to the 48 h limit. However, the purpose of the law was to provide mothers and newborns with a second night in the hospital and since length of stay is measured by calendar days, we are measuring this intent.

As we noted in the literature review, one concern with early discharges is that health care providers may not have sufficient time to detect certain conditions, requiring that parents seek treatment for the infants soon after discharge. Although the costs of these additional treatments can take many forms, most previous authors have measured adverse outcomes as hospital re-admissions. Even though this is a coarse outcome, it is a useful metric for two reasons. First, the bulk of medical expenses after the initial discharge are for inpatient services. Lewit and Monheit (1992, Table 2) showed that 91% of medical expenses in an infant's first year of life are for hospital stays (including the initial charges for the infant's childbirth hospital stay) and in our data set, 37.5% of hospital stay costs in the first year are for hospital stays after the birth. These numbers suggest that 79% of all post-discharge expenditures in the first year are for hospital stays. Second, re-admissions measure an outcome that can be prevented with longer stays. Newborns are re-admitted to the hospital within 28 days for a variety of reasons but the vast majority of re-admissions occur quickly after discharge, possibly indicating that a longer stay may provide some diagnostic benefits. Among infants re-admitted to the hospital within 28 days, half of all re-admission happen within 3 days of discharge, 63% within the first week and 75% within the first 2 weeks. In many cases, the primary diagnoses for re-admission indicate conditions that are associated with birth that would benefit from longer stays. Of newborns admitted to the hospital within 28 days of birth, the 10 leading primary diagnoses are jaundice (18.8% of all cases), other infections specific to the perinatal period (6.6%), other respiratory problems after birth (5.4%), respiratory distress syndrome in newborn (5.2%), transitory tachypnea of newborn (2.6%), hemolytic disease of fetus or newborn (2.4%), meconium aspiration syndrome (1.8%), disorders related to preterm infants (1.7%) and acute bronciolitis (1.7%). These 10 conditions represent the primary diagnosis for almost half of all 28-day re-admissions and all but the last one are specific to perinatal conditions.

We can 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. Initially however, we will focus on the 28-day re-admission rates. 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 restricted-use 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 1 year of birth, plus the cause and place of death. Following previous literature, we will use 28-day mortality rates for newborns.

### 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 the early discharge laws. However, in the pre-law period, even those with complicated deliveries had high rates of early discharge. For example, in the July 1995 through August of 1997 period, 71.9% of privately insured deliveries with complications were discharged early. Therefore, we work with three distinct subsamples: complicated and uncomplicated vaginal deliveries 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.[15] 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[16] or 23 complications during labor.[17] Although many patients whose DRG codes indicated a complicated delivery also had birth records that identified complications, the overlap was not perfect. Subsequently, we define a birth as uncomplicated if neither the DRG code of the mother nor the birth record identified a complication.

[15] DRG 370 represents cesarean deliveries with complications, and DRG 372 represents vaginal deliveries with complications.

[16] 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.

[17] 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.

Table 2
Descriptive statistics, privately and Medicaid insured births in California, 1 July 1995–31 December 2001, means and (standard deviations)

| Variable | Uncomplicated vaginal deliveries | | Complicated vaginal deliveries | | C-section deliveries | |
|---|---|---|---|---|---|---|
| | Private | Medicaid | Private | Medicaid | Private | Medicaid |
| Total charges (mother + infant) | $9,225 ($38,353) | $9,049 ($38,952) | $14,462 ($47,239) | $14,971 ($47,280) | $25,984 ($70,766) | $26,613 ($70,434) |
| % Mother's age < 20 | 0.089 (0.285) | 0.295 (0.456) | 0.075 (0.263) | 0.232 (0.423) | 0.043 (0.203) | 0.187 (0.390) |
| % Mother's age 20–24 | 0.201 (0.401) | 0.338 (0.473) | 0.171 (0.376) | 0.290 (0.454) | 0.130 (0.336) | 0.286 (0.451) |
| % Mother's age 24–30 | 0.317 (0.465) | 0.215 (0.411) | 0.299 (0.458) | 0.237 (0.425) | 0.282 (0.450) | 0.253 (0.435) |
| % Black mothers | 0.052 (0.221) | 0.075 (0.264) | 0.065 (0.246) | 0.086 (0.281) | 0.067 (0.251) | 0.094 (0.292) |
| % Other race mothers | 0.168 (0.373) | 0.080 (0.271) | 0.159 (0.365) | 0.090 (0.286) | 0.160 (0.366) | 0.066 (0.248) |
| % Hispanic mothers | 0.281 (0.449) | 0.658 (0.474) | 0.278 (0.448) | 0.607 (0.489) | 0.258 (0.437) | 0.641 (0.480) |
| % Mothers < high school education | 0.140 (0.347) | 0.572 (0.495) | 0.143 (0.350) | 0.563 (0.496) | 0.115 (0.319) | 0.531 (0.499) |
| % Mothers w h.s. degree | 0.534 (0.499) | 0.403 (0.490) | 0.545 (0.498) | 0.411 (0.492) | 0.535 (0.499) | 0.435 (0.496) |
| %w previous birth | 0.607 (0.488) | 0.628 (0.483) | 0.610 (0.488) | 0.668 (0.471) | 0.574 (0.495) | 0.650 (0.477) |
| Observations | 773,078 | 663,066 | 346,510 | 253,553 | 323,302 | 247,509 |

Numbers in parentheses are standard deviations. Dollar costs are measured in real $2000 using the consumer price index-all products index as a deflator.

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% of all births in the state over the period of analysis. Table 2 reports basic demographic information of mothers in our three samples, vaginal deliveries without and with complication and c-section deliveries. We report sample characteristics for privately insured and Medicaid deliveries and numbers in parentheses are standard deviations. The inpatient costs (newborn plus mother) measured in real $2000 of complicated vaginal deliveries generate about 60% more costs than an uncomplicated birth and c-sections are 70% more expensive than complicated vaginal deliveries. There is little difference within each subsample in the costs of Medicaid and non-Medicaid births. 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. Across all three samples, Medicaid mothers were more likely to be younger, less educated, members of racial and ethnic minorities, and more likely to have had a previous birth.

## 5. Graphical analysis of California law and federal law

### 5.1. The change in postpartum length of stay

In Fig. 1, we plot the monthly fraction of newborns delivered vaginally without complications that were released early (less than 2 days) in each month. For reasons that become apparent later, we provide data from 1 July 1995 through 31 December 2000. The vertical line in September of 1997 indicates the first full month the California law was in effect, the line at January 1998 indicates when the federal law became effective, and the third line represents when the state law was expanded to include all Medicaid recipients.

Note that in Fig. 1, there was an 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% 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 6 months later, this number had fallen to 50%, a 32% point decline and a 39% reduction. Early on in our sample, most insurance carriers knew the federal law would take effect in a 1998, but from Fig. 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 Fig. 1 (and subsequent figures), that the adjustments to a longer length of stay occurred by the end of the first quarter of 1998.

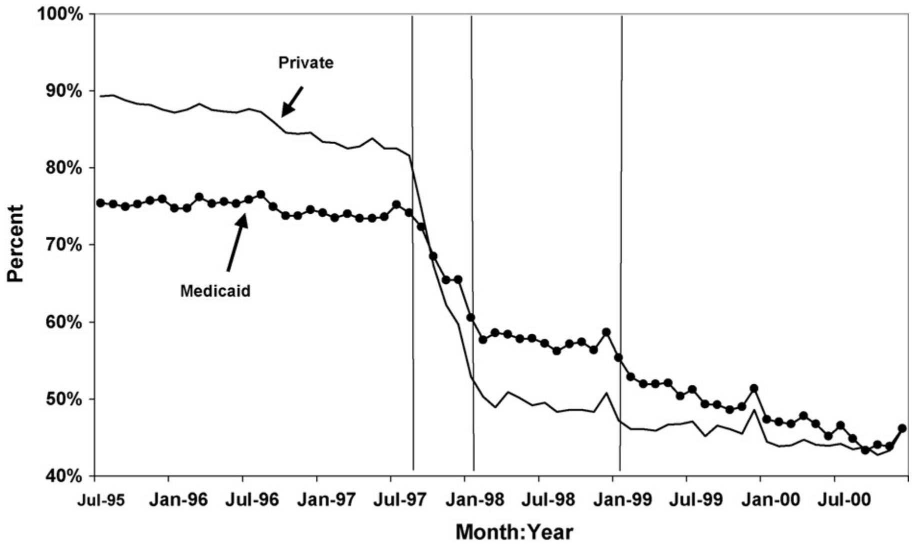


Fig. 1. Percentage newborn discharged early, vaginal deliveries without complications.

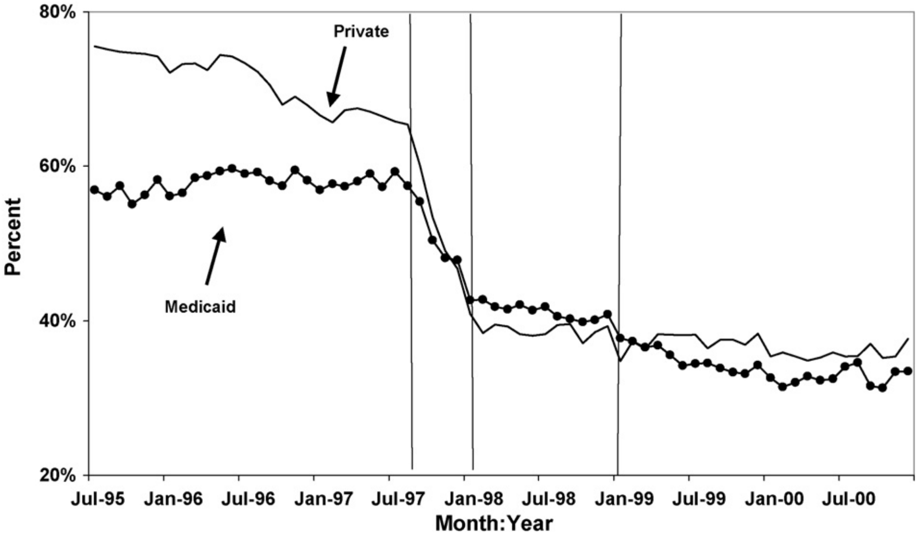


Fig. 2. Percentage newborn discharged early, vaginal deliveries with complications.

Notice also in Fig. 1 that although the California law only applied to a Medicaid recipients in particular managed care plans, there is also a noticeable but less dramatic drop in the fraction of newborns from uncomplicated vaginal births discharged early during the 6 months after passage of the California early discharge law. There is, however, another sharp decline in early discharges for this sample starting in January 1999 when the California law was extended to all Medicaid births.

Fig. 2 plots the early discharge rate for newborns after complicated vaginal deliveries. In this figure, we see that before the laws, there is a more pronounced downward trend in early discharges among those with privately insurance uncomplicated vaginal deliveries, with rates falling from 71 to 58% over the July 1995 through August 1997 period. Between August 1997 and February 1998 however, early discharge rates among the privately insured fell by an additional 27% points. Over the same period, the early discharge rate among Medicaid recipients in this subsample fell by almost 16% points. Although the rates of early discharge among uncomplicated and complicated vaginal deliveries were very different before the California law, the absolute change in rates was similar for both groups.

In Fig. 3 we graph the early discharge rate for newborns who were delivered via cesarean section. In this figure, an early discharge is defined as a postpartum length of stay that was less than 4 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% 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% points. Notice again the sharp decline in early discharge rates in the Medicaid subsample starting in January of 1999 when the state law was expanded to all Medicaid recipients. In contrast to the results for the two vaginal delivery samples, the numbers in this graph show continued declines in the early discharge rates after the effective date for the federal law. 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 estimate the magnitude of this effect precisely. Second, the extension of the state law to all Medicaid births appears to have been effective. Third, the timing of the change in time trends corresponded exactly with the effective dates of the state and federal law. Fourth, among vaginal deliveries, the federal law reduced early discharge rates for complicated deliveries by the same rate as for uncomplicated deliveries, even though the former group had substantially lower early discharge rates prior to passage of the federal law.

In Table 3, we provide numeric estimates that coincide with the graphical presentation in Figs. 1–3 and subsequent figures. These estimates are simple difference estimates that compare the post-federal law period (1 January 1998 and after) with the pre-state law period (1 July 1995–31 August 1997). The numbers in parentheses are standard errors that do not control for any within-group correlation. Notice the uniformly large declines in early discharge rates across all subsamples. The estimates do not control for any downward trend in early discharges prior to the law so

Table 3
Early discharge and re-admission rates for newborns, before and after federal law

| | | Uncomplicated vaginal deliveries | | Complicated vaginal deliveries | | C-section deliveries | |
|---|---|---|---|---|---|---|---|
| | | Private | Medicaid | Private | Medicaid | Private | Medicaid |
| Discharged early | | | | | | | |
| (1) | 7/1/95–8/31/97 | 0.859 (0.00064) | 0.749 (0.00082) | 0.707 (0.0013) | 0.578 (0.015) | 0.845 (0.0011) | 0.812 (0.0012) |
| (2) | 1/1/98–12/31/00 | 0.467 (0.00076) | 0.515 (0.00086) | 0.373 (0.0011) | 0.365 (0.0013) | 0.656 (0.0011) | 0.724 (0.0012) |
| | Difference (2) – (1) | −0.391 (0.0011) | −0.234 (0.0011) | −0.334 (0.0017) | −0.214 (0.0019) | −0.188 (0.0016) | −0.088 (0.0017) |
| Readmitted within 28 days | | | | | | | |
| (1) | 7/1/95–8/31/97 | 0.0320 (0.00033) | 0.0387 (0.00036) | 0.0359 (0.00052) | 0.0438 (0.00061) | 0.0231 (0.00044) | 0.0329 (0.00056) |
| (2) | 1/1/98-12/31/00 | 0.0308 (0.00026) | 0.0363 (0.00032) | 0.0318 (0.00032) | 0.0396 (0.00054) | 0.0210 (0.00032) | 0.0294 (0.00046) |
| | Difference (2) – (1) | −0.0012 (0.00042) | −0.0024 (0.00048) | −0.0040 (0.00064) | −0.0042 (0.00082) | −0.0021 (0.00053) | −0.0035 (0.0007) |
| Observations | | | | | | | |
| (1) | 7/1/95–8/31/97 | 292,222 | 283,181 | 126,545 | 110,891 | 117,281 | 110,891 |
| (2) | 1/1/98–12/31/00 | 433,404 | 339,622 | 198,786 | 129,014 | 196,520 | 129,014 |

Standard errors are reported in parentheses.

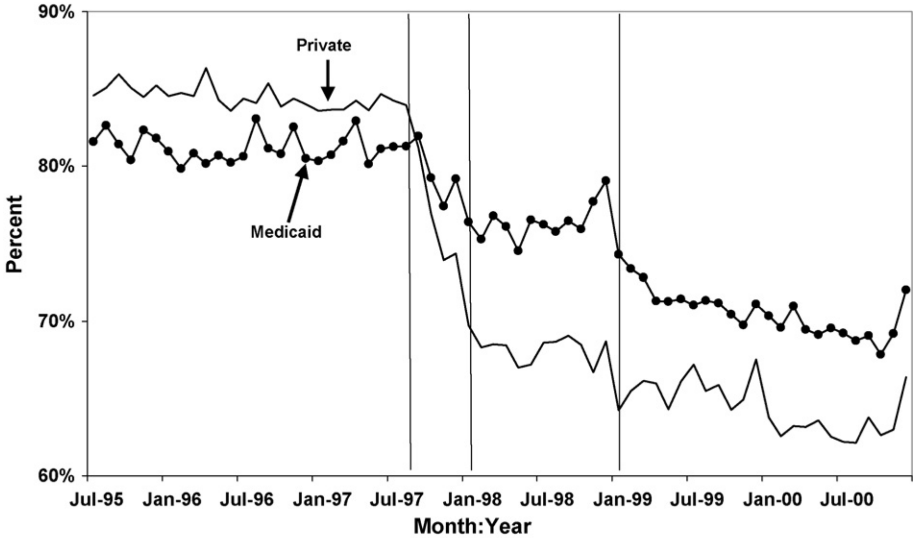


Fig. 3. Percentage newborn discharged early, c-section deliveries.

these are upper bounds on the likely treatment effect. In general, the early laws reduced early discharges of privately insured newborns born vaginally by over 30% points, of those with private insurance by almost 19% points, and corresponding estimates for Medicaid patients are 40–50% smaller in magnitude.

### 5.2. Changes in health outcomes

In Figs. 4–6, we plot the 28-day re-admission rate of newborns covered by private insurance for uncomplicated vaginal deliveries, complicated vaginal deliveries, and c-section deliveries, respectively. In each graph, we report the same vertical scale to make it easier to compare magnitudes across figures. In each of these figures, the month-to-month variation in 28-day re-admission rate dwarfs any systematic change in re-admission rates produced by the law, so it is difficult in these graphs to tell whether the law improved birth outcomes. Therefore, we also plot a dotted line representing the pre-state law and post-federal law means of the outcomes.

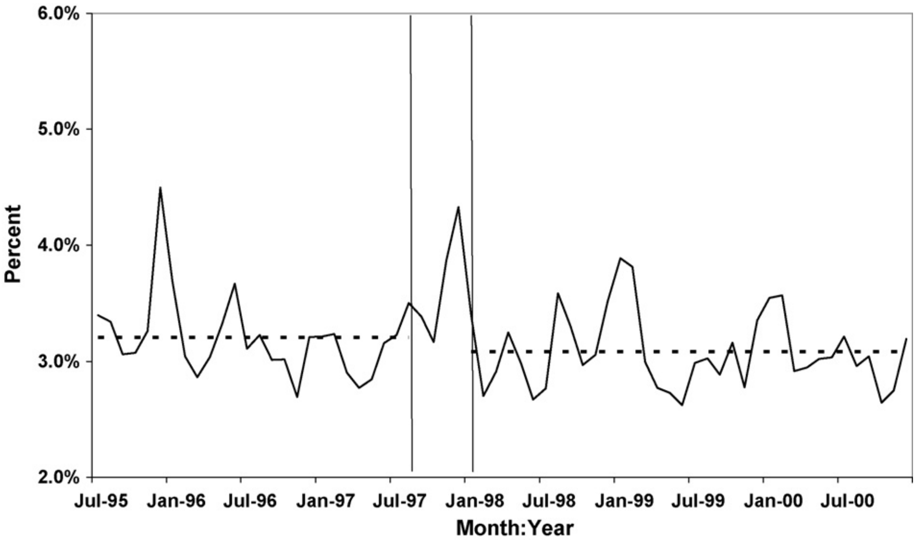


Fig. 4. Percentage newborns re-admitted within 28-days, vaginal deliveries without complications, private insurance.

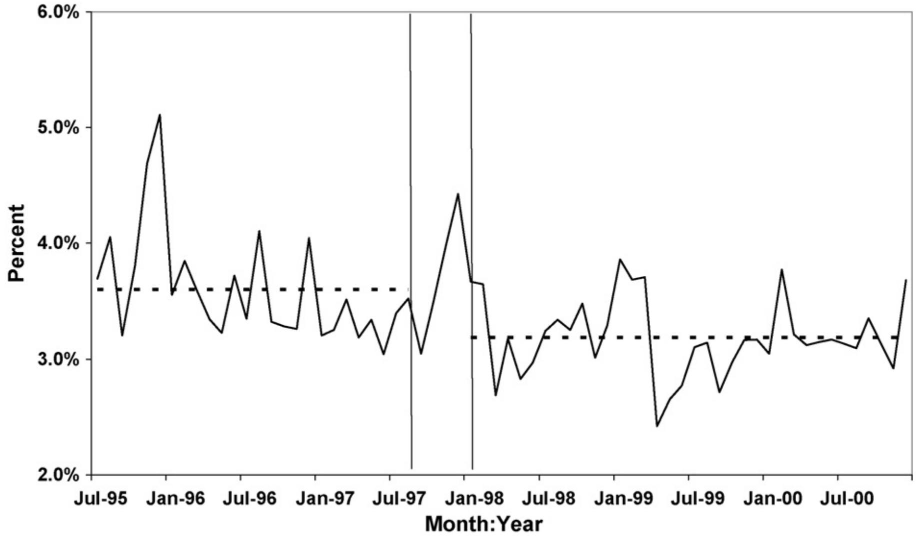


Fig. 5. Percentage newborns re-admitted within 28-days, vaginal deliveries with complications, private insurance.

Notice that in Fig. 4 for uncomplicated vaginal deliveries paid for by private insurance, there is virtually no time series trend in the re-admission rate and there is a small drop in mean rates after January 1998. As the numbers in Table 3 indicate, the difference in means between the two periods is only 0.12% points, indicating the federal early discharge law had little if any impact on the health of these infants. In contrast, we find more noticeable drops in newborn re-admission after the enactment of the federal law among privately financed complicated vaginal deliveries (Fig. 5) and c-sections (Fig. 6). As the numbers in Table 3 indicate, the simple differences in values over time suggest re-admission rates for these two groups fell by 0.4–0.2% points, respectively.

In Figs. 7–9, we report the times series for the 28-day re-admission rates among Medicaid newborns for vaginal deliveries without complications, vaginal with complications and c-section births, respectively. In contrast to Fig. 4, we see a more pronounced drop in newborn re-admission rates in Fig. 7 for uncomplicated vaginal deliveries paid for by Medicaid. Notice that in all three of these graphs, there also is a slight drop in the re-admission rate after the law

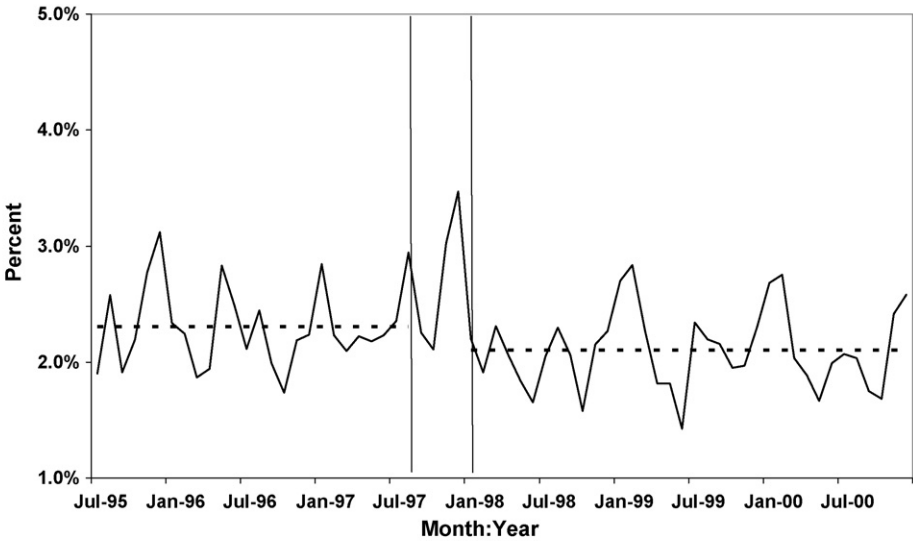


Fig. 6. Percentage newborns re-admitted within 28-days, c-section deliveries, private insurance.

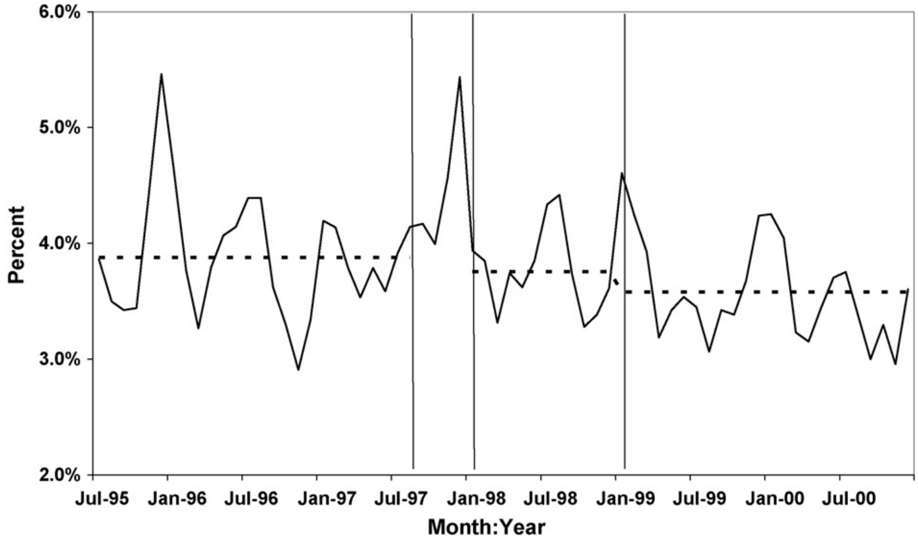


Fig. 7. Percentage newborns re-admitted within 28 days, vaginal deliveries without complications, Medicaid.

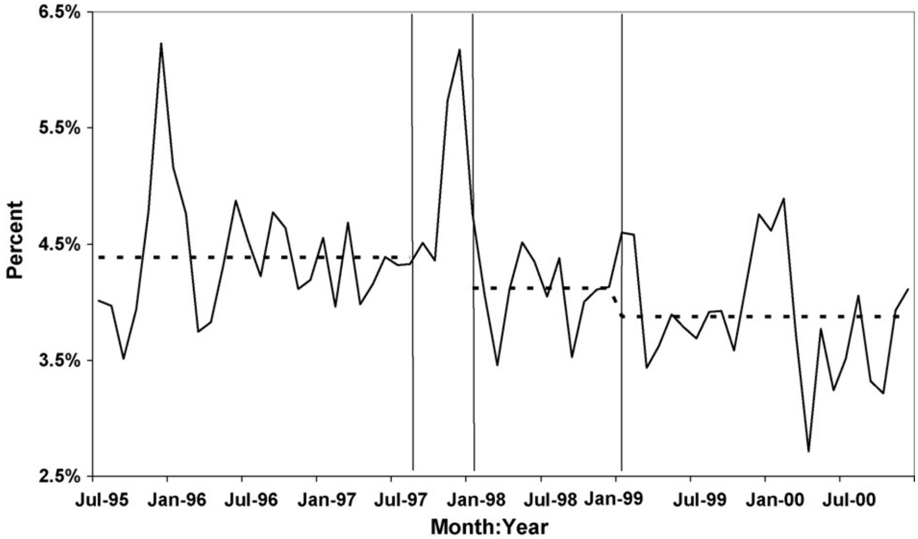


Fig. 8. Percentage newborns re-admitted within 28-days, complicated vaginal deliveries, Medicaid Insurance.

was extended to all Medicaid patients in January 1999. Among these three groups, re-admission rates were lower in the post-federal law period by 0.24, 0.42 and 0.35% points, respectively.

Figs. 4–9 point out that it will be difficult to determine whether the state law improved birth outcomes during the 4 months before the federal law became effective. Notice in Figs. 4 and 7 there is a noticeable increase in re-admission 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 re-admissions to the California law. But a closer inspection of the data suggests that something else was occurring. Notice in Fig. 4 that re-admission rates always spike in the late autumn and early winter months during the flu season. The winter of 1997/1998 was a particularly heavy flu season in California as was pointed out in a report released by OSHPD, and complications associated with the flu are a common reason for re-admission among infants.[18] This cyclic nature of newborn re-admissions suggests

[18] The title of this report is *California Health Care System: Overview Of The Hospital/Ems Crisis Winter Of* 1997-1998.

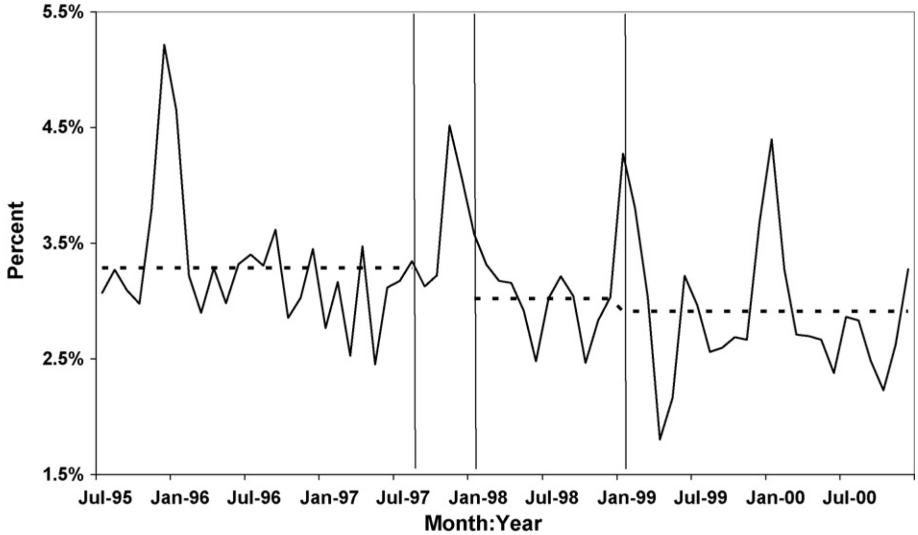


Fig. 9. Percentage newborns re-admitted within 28-days, c-section deliveries, Medicaid insurance.

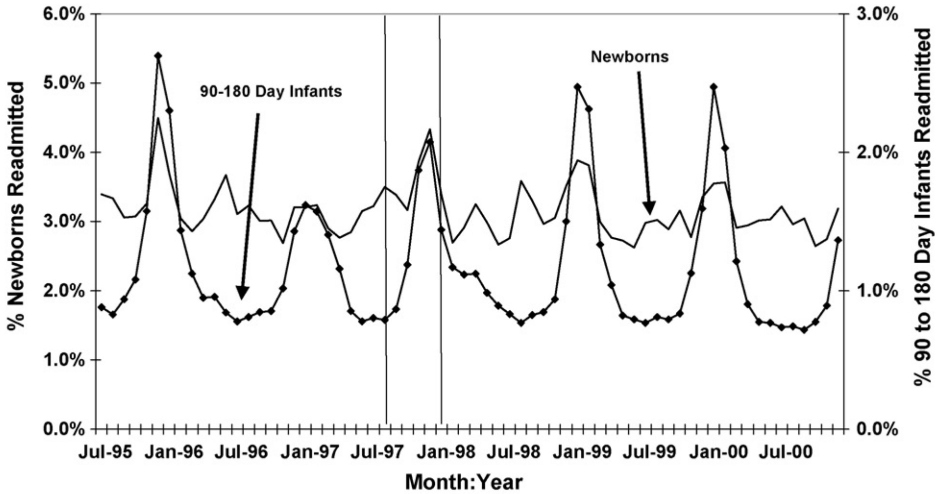


Fig. 10. Percentage newborns re-admitted in 28-days, private vaginal uncomplicated deliveries and 28-day hospital admission rate for 90–180 day old infants.

that we must control for month-to-month environmental conditions that may alter re-admission 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. Our use of children 90–180 days old does, however, mean that we lose the first 6 months of data in our analysis,[19] which is why our analysis samples begin on 1 July 1995. By including this variable, we are implicitly assuming that the early discharge laws do not change re-admissions 90–180 days after birth. This is verified later on in the paper when we demonstrate that most of the impact of the early discharge law is captured in the first 7 days after birth.

In Fig. 10, we average the 28-day re-admission rate of newborns and older infants up to the month level and plot the series over time for vaginal deliveries under private insurance. These series demonstrate that the admission rates of slightly older infants is highly correlated with the 28 day re-admission rate of newborns, and both indexes demonstrates

[19] 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.

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 re-admission rates.

## 6. Econometric model

### 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 at the same time by the federal law. Within California, the largest potential control group, Medicaid patients, were indirectly impacted by the law and eventually included in the coverage. Datar and Sood (2006) also found that the federal law increased the length of postpartum stays for the self-insured as well. Given the lack of any control group, we therefore use an interrupted time series model instead. Interrupted time series models are sometimes difficult to implement for a variety of reasons. It is often not clear 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 changes are 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

$$\begin{aligned} Y_{ikt} = {} & X_{ikt}\beta + \mathrm{HSA}(m)_{ikt}\mathrm{PAYER}(j)_{ikt}\,\mathrm{TREND}_t\,\delta(m,j) + \mathrm{STATELAW}_t\,\mathrm{PAYER}(j)_{ijt}\alpha_1(j) \\ & + \mathrm{FEDLAW}_t\,\mathrm{PAYER}(j)_{ikt}\alpha_2(j) + \mathrm{PAYER}(2)_{ikt}\,\mathrm{EXPANDED}_t\alpha_3 + \lambda(m,j) + \varepsilon_{ikt} \end{aligned} \tag{1}$$

where *X* is a vector of characteristics of the newborn (insurance PAYER, sex, race, ethnicity, hour, day and month of birth), the mother (such as age, education, and the number of previous births) and the hospital (hospital size,[20] ownership status,[21] and hospital service area (HSA)[22]) and the admission index for children 90–180 days old, which controls for the day-to-day conditions that may generate re-admissions. The variable $\varepsilon_{ikt}$ 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 that the pre-law and post-law trends are very similar as Figs. 1–9 demonstrate.[23] To eliminate any secular trend in the data, we include a monthly trend $\mathrm{MTREND}_t$ that equals 1 if the birthday of the newborn is in July of 1995, 2 in August of 1995, etc. We insert a full set of dummy variables that vary by PAYER (where $j=1$ for Private and $j=2$ for Medicaid) and HSA ($m=1-14$) plus we allow the trend terms to vary by these effects as well.

The key variables in the model are the vectors $\alpha_1$ and $\alpha_2$ and the parameter $\alpha_3$ that measure the impact of the state and federal, respectively. The variable STATELAW equals 1 from 27 August 1997 through 31 December 1997, while FEDLAW equals 1 from 1 January 1998 until the end of the sample. These variables vary by payer status (Private or Medicaid). The variable EXPANDED equals 1 from 1 January 1999 until the end of the sample, and it captures the expansion of the California law to all Medicaid patients and given the nature of the expansion in 1999, this variable is only interacted with Medicaid.

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[20] We break hospitals up into six groups based on average monthly number of deliveries. The groups are $<20$, $>20$ and $\le 50$, $>50$ and $\le 100$, $>100$ and $\le 150$, $>150$ and $\le 300$, and $>300$.

[21] 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.

[22] 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 or Orange County) but in many cases, areas include multiple counties.

[23] Taking the aggregate data for Fig. 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 4 months of the state law, we obtain an $R^2$ of 0.98.

There are two key outcomes: whether the newborn was discharged early (<2 days for vaginal births and <4 days for c-sections). Although these outcomes are discrete, we estimate linear probability models. We relax this assumption in later sections and demonstrate that even for low incidence events, linear probability estimates are very similar to estimates from logit models. In all models, we control for possible autocorrelation in errors by allowing for arbitrary correlation in errors within a hospital over time. This procedure also allows for arbitrary forms of heteroskedasticity which is present, by construction, in our linear probability models.

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 captures both of these changes. This distinction is potentially important. Suppose that early discharge increases the chance of a hospital re-admission, early follow-ups visits by patients released early eliminate this risk, and everyone released early has a follow-up visit. In this case, the coefficients on the $\alpha$'s in the re-admission equation 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. Galbraith et al. (2003) surveyed 2828 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 discharged early and those discharged later. Although in principal the $\alpha$'s 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.

### 6.2. *Two-stage least-squares estimates*

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 early discharge and adverse outcomes by using the information from the reduced-form models in a more structured way. Specifically, we use the adoptions of the federal and state laws as instrumental variables for early discharge in a two-stage least squares (2SLS) model to obtain consistent estimates of the impact of length of stay on medical outcomes.

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

$$28\text{DAY}_{ikt} = X_{1ikt}\beta_1 + \text{discharged early}_{ikt}\delta_1 + \varepsilon_{1ikt} \tag{2}$$

where for simplicity, we include all the covariates from Eq. (1) into one vector $X_1$. The key covariate in this regression is *discharged early*. As we noted above, equations such as (2) have been estimated by a number of authors but we suspect that cov(*discharged early*$_{ikt}$, $\varepsilon_{1ikt}$) < 0 so OLS estimates of $\delta_1$ will be biased down.

Two-stage least squares estimation requires that a researcher identify a variable that exogenously changes the endogenous covariate of interest but has no direct impact on health. In this case, the instruments are the enforcement dates for the state and federal law. As Figs. 1–3 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.[24]

[24] 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 discharged early 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 et al., 1992, 1999). 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.

## 7. Results

### 7.1. Reduced-form model regression results

Table 4 presents the OLS estimates results of Eq. (1) using three subsamples: vaginal deliveries without and with complications and cesarean births. We initially report linear probability results for two dichotomous outcomes: whether the newborn was discharged early and whether they were re-admitted within 28 days. We only report the coefficients on the legal variables and for space considerations, suppress the coefficients on the other key covariates.

For privately insured vaginal deliveries without complications, we find that the California and federal law reduced early discharge rates of newborns by 16 and 30% points, respectively, with the later result being 35% of the pre-law rates. The standard errors on these estimates are small and the results are statistically significant at conventional levels.[25] Interestingly, the federal law had only a slightly smaller impact on vaginal deliveries with complications although the mean discharged early is 15% points lower in this later group. For c-section deliveries, the federal law is estimated to reduce early discharge rates by 13.2% points. In general, the effect of the state law is about half the impact of the federal law for those with private insurance. The federal law is estimated to have reduced early discharge rates for Medicaid births by 12.4 and 14.2% points for vaginal deliveries without and with complications, respectively, and both estimates are precisely estimated. Expansion of the state law to cover all Medicaid births increased this effect by 5.2 and 5.8% points in the uncomplicated and complicated vaginal samples, respectively. By the time the state law was expanded, the early discharge statutes are estimated to have reduced early discharge rates for Medicaid-covered c-section newborns by 5.2% points.

In the next column of results for each subsample, we report the impact of the laws on 28-day re-admission rates. If longer stays reduce re-admission 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 increase in re-admissions rates of 0.11% point after the passage of the federal law for privately insured. Among uncomplicated vaginal deliveries paid for by Medicaid, the federal law is estimated to reduce re-admissions by 0.01% points, a result that is statistically insignificant at conventional levels. We do however see a statistically significant reduction in re-admission rates for Medicaid patients after the state law was expanded to include all Medicaid patients. In the same subsample, we observe a statistically significant *increase* in re-admissions after passage of the state law for both insurance types. This could be attributed to the law but more likely, this is 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.[26]

For vaginal deliveries with complications, we observe reductions in re-admission rates after the federal law and for both insurance types, but the results are statistically insignificant. The effect on re-admissions of the expansion of the state law of −0.41% point is statistically significant. Among those with private insurance delivered by c-section, there was a statistically significant drop in re-admission rates of a little more 0.3% points after the passage of the federal law.

Given the concerns that the spike in re-admissions during the December 1997 period is due to some other cyclic shock not related to the passage of the state statute, we also report in Table 3 estimates where we delete data from 1 September 1997 through 1 December 1997. Estimates from these models are reported in the lower half of the table. This model specification only allows us to identify the estimate on the federal law coefficients and the impact of the expansion of the state law to all Medicaid births. These estimates are nearly identical to the results found in the top half of the table.

Given differences in model specification, these results are not directly comparable to those in Datar and Sood (2006) who use public use versions of the data here to examine the same question. In their data set, the authors pooled all births from January 1995 through December 2000 in one model (vaginal and c-section, for privately insured, Medicaid and uninsured births) and estimated the reduced-form relationship between re-admission rates and the passage of the federal law.[27] The public use version of the data set does not identify month of birth and as a result the authors do not

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

[26] The coefficient (standard error) on the hospital admission index for 90–180 day old infants in this sample is 0.65 (0.059) but the correlation between this and 28-day readmission rates in this sample cannot capture the huge spike in newborn readmissions during December of 1997.

[27] Datar and Sood also delete many pregnancies with complications such as low birth weight infants and multiple births. However, these births are also potentially impacted by the law as well. In the pre-law period, we find that among births between 2000 and 2500 g in weight, roughly 55% were discharged early.

Table 4
OLS estimates, impact of early discharge laws on early discharge and 28-day re-admission rates for newborns, California births, July 1995–December 2000

| | Uncomplicated vaginal deliveries (1,435,917 observations) | | Complicated vaginal deliveries (601,781 observations) | | C-section deliveries (580,215 observations) | |
|---|---|---|---|---|---|---|
| | Discharged early | Readmitted w/in 28 days | Discharged early | Readmitted w/in 28 days | Discharged early | Readmitted w/in 28 days |
| All data, 1 July 1995–31 December 2000 | | | | | | |
| Federal law × private insurance | −0.3049 (0.0171) | 0.0011 (0.0011) | −0.2688 (0.0181) | −0.0007 (0.0014) | −0.132 (0.0122) | −0.0031 (0.0012) |
| Federal law × Medicaid | −0.1242 (0.0118) | −0.0001 (0.0012) | −0.1428 (0.014) | −0.0028 (0.0019) | −0.0299 (0.0076) | −0.0003 (0.002) |
| Expanded state law × Medicaid | −0.0562 (0.0078) | −0.0028 (0.0011) | −0.0577 (0.0069) | −0.0041 (0.0017) | −0.0434 (0.009) | −0.0009 (0.0015) |
| State law × private insurance | −0.1608 (0.0132) | 0.0047 (0.001) | −0.1573 (0.0114) | 0.0003 (0.0016) | −0.0582 (0.0086) | 0.0014 (0.0015) |
| State law × Medicaid | −0.0387 (0.0078) | 0.0043 (0.0015) | −0.0608 (0.01) | 0.0039 (0.0024) | −0.0087 (0.006) | 0.0016 (0.0022) |
| $R^2$ | 0.2469 | 0.0025 | 0.2091 | 0.0031 | 0.0949 | 0.0028 |
| Deleted data from September–December 1997 | | | | | | |
| | (1,348,203 observations) | | (564,954 observations) | | (546,409 observations) | |
| Federal law × private insurance | −0.3051 (0.0170) | 0.0011 (0.0010) | −0.2688 (0.0181) | −0.0008 (0.0014) | −0.1322 (0.0122) | −0.0031 (0.0012) |
| Federal law × Medicaid | −0.1248 (0.0119) | −0.0001 (0.0012) | −0.1438 (0.014) | −0.0026 (0.0019) | −0.0303 (0.0077) | −0.00027 (0.002) |
| Expanded state law × Medicaid | −0.0573 (0.0079) | −0.0028 (0.0011) | −0.0597 (0.0069) | −0.0039 (0.0017) | −0.0442 (0.0094) | −0.0008 (0.0014) |
| $R^2$ | 0.2526 | 0.0025 | 0.2139 | 0.0029 | 0.0972 | 0.0027 |

Standard errors are reported in parentheses. Standard errors are calculated allowing for arbitrary correlation in errors within a hospital. Other covariates include the hospital admission index for 90–80 day infants, mother's education, race, ethnicity, age, and previous births, the size and type of hospital, the hour, day and month of birth, plus insurance times HSA region effects and time trends that vary by insurance/HSA status.

examine either the impact of the passage of the state law or the extension of the state law to all Medicaid births. The only trend included in the model is a linear trend in years and the trend is common to all groups. The authors estimate separate dummy variables that indicate the impact of the first 3 years and the logit coefficients on these variables are −0.093, −0.118 and −0.197, respectively. Using our restricted-use sample, data for Medicaid and privately insured only, pooling data for both delivery types, including covariates similar to their and using a time trend based on years rather than months, we replicate results similar to theirs in that we find that the logit coefficients (standard errors allowing for within-hospital correlation in errors) on dummies for the first 3 years of the federal law are −0.1036 (0.022), −0.1446 (0.0283), and −0.1784 (0.0375). Given that our samples are slightly different (theirs includes the uninsured and excludes a large set of complications), we consider our results fairly close to theirs.

To compare their results to our basic model, we initially drop the first 6 months of the sample in order to merge in our admission index, we control for month of birth effects and we use a trend based on months rather than years. In the 28-day re-admission equation, we estimate logit coefficients for dummies on the first 3 years after the passage of the federal law of −0.0823 (0.0215), −0.1530 (0.0281) and −0.1653 (0.0370). Thus, the estimates do not change much when we make these alterations to the model. However, when we add a dummy for passage of the state law in 1997, much like the results in Table 4, we find a large positive coefficient on the state law dummy 0.0825 with a standard error of 0.0198. However, the estimated impacts of the first 3 years of the federal law drop considerably to −0.034 (0.020), 0.081 (0.028) and −0.073 (0.036). We cannot reject the null hypothesis that these coefficients are all equal. If we delete the last four months of 1997 when only the state law was in effect, these three coefficients fall to −0.028 (0.027), −0.071 (0.037), and −0.060 (0.048), and none are statistically significant at the 95% critical value. When we estimate a model that has only one coefficient for the federal law period, the parameter estimate is −0.023 (0.017). In short, lack of restricted-use data in Datar and Sood greatly overstates the impact of the laws.

As we note in Table 2, reimbursement rates for complicated deliveries are greater than for uncomplicated ones. The increase in the average length of stay that we document below may have altered the incentive to identify patients as being from complicated deliveries. If this is the case, then there may have been a change in the sample generated by the laws. Likewise, since the change in the length of stay was proportionally greater for vaginal deliveries than c-sections, there may have been an incentive to alter the mix of c-section deliveries. This does not appear to be a concern. We pooled all the observations from the three groups in Table 4 and ran a model similar to that in Eq. (1) where we use as the dependent variable either a dummy for whether the patient is identified as having a complication or a dummy that indicates the baby was delivered by a c-section. We delete the data for the August through December 1997 period, use the same controls as in Table 4, and test the joint hypothesis that the three law coefficients (Private × federal law, Medicaid × federal law, Medicaid × expansion of the state law) are jointly zero. In models with complications as the outcome of interest, with these 2.4 million observations, none of the law coefficients are statistically significant at conventional levels. There does however appear to be a slight tick up in c-section rates after passage of the federal law. The coefficient (standard error allowing for within hospital correlation in errors) on the Medicaid × federal and Medicaid × state expansion variables are 0.0059 (0.00223) and 0.0071 (0.0027), respectively. These are, however, very small changes in a sample with 25% c-section rates so our results are most likely not driven by changes in sample composition.

Much of the literature in this area examines 28-day re-admission rates as a key outcome but we can calculate 7- and 14-day re-admission rates as well. However, as we shorten the follow up after birth, the incidence rates fall considerably. Linear probability models tend to generate marginal effects similar to estimates from probit or logit models but only when the mean outcome is some distance from zero or one. Therefore, one must be concerned whether a linear probability model is appropriate for these low-incidence outcomes. In Table 5, we report the reduced-form logit regression models where the outcomes of interest are initially the 7-, 14-, and 28-day re-admission 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. In the fourth column of results, we also report the estimates from the linear probability model for the 28-day re-admission rate from Table 4 for comparison. In this table, we use the model that excludes the last 4 months of 1997.

There are a number of key results in Table 5. First, in general, the marginal effects from the 28-day logit and the liner probability estimates of the same equation produce similar results in all subsamples. The linear probability model appears to do an adequate job in this case. Second, marginal effects in the 7-day re-admission equation are, in general, a high fraction of the value of the same coefficient in the 28-day equation, indicating that the bulk of the re-admissions

Table 5
Reduced-form regressions logistic regressions of infant re-admission and mortality models, California privately insured and Medicaid patients, July 1995 through December 2000

| | Logit 7-day re-admission | Logit 14-day re-admission | Logit 28-day re-admission | OLS 28-day re-admission | Logit 28-day mortality |
|---|---|---|---|---|---|
| Vaginal deliveries without complications (1,348,203 observations) | | | | | |
| Federal law × private insurance | 0.0009 (0.0007) | 0.0012 (0.0009) | 0.0012 (0.0011) | 0.0011 (0.0010) | −0.000072 (0.00006) |
| Federal law × Medicaid | 0.0015 (0.0009) | 0.0007 (0.0009) | −0.0001 (0.0011) | −0.0001 (0.0012) | −0.000037 (0.00004) |
| Expanded state law × Medicaid | −0.0011 (0.0007) | −0.0018 (0.0008) | −0.0027 (0.0009) | −0.0028 (0.0011) | −0.000047 (0.00005) |
| Mean of dependent var. | 0.0178 | 0.0238 | 0.0341 | 0.0341 | 0.00024 |
| Complicated vaginal deliveries (564,954 observations) | | | | | |
| Federal law × private insurance | −0.0003 (0.0010) | −0.0011 (0.0011) | −0.0008 (0.0014) | −0.0008 (0.0014) | −0.000092 (0.0001) |
| Federal law × Medicaid | 0.0002 (0.0013) | −0.0007 (0.0014) | −0.0026 (0.0019) | −0.0026 (0.0019) | 0.000018 (0.0001) |
| Expanded state law × Medicaid | −0.0028 (0.0011) | −0.0037 (0.0011) | −0.0039 (0.0017) | −0.0039 (0.0017) | 0.000065 (0.0001) |
| Mean of dependent var. | 0.020 | 0.0261 | 0.0369 | 0.0369 | 0.00042 |
| C-section deliveries without complications (observations) | | | | | |
| Federal law × private insurance | −0.0014 (0.0009) | −0.0022 (0.0011) | −0.0.0034 (0.0012) | −0.0031 (0.0012) | 0.00016 (0.00014) |
| Federal law × Medicaid | 0.0003 (0.0011) | 0.0003 (0.0013) | −0.0002 (0.0016) | −0.0003 (0.002) | −0.00015 (0.00008) |
| Expanded state law × Medicaid | −0.0019 (0.0007) | −0.0028 (0.0008) | −0.0010 (0.0012) | −0.0008 (0.0014) | 0.00018 (0.00014) |
| Mean of dependant var. | 0.0095 | 0.0149 | 0.0257 | 0.0257 | 0.00038 |

Deleted data from September–December 1997. Standard errors are reported in parentheses. Standard errors are calculated allowing for arbitrary correlation in errors within a hospital. Other covariates include the hospital admission index for 90–180 day infants, mother's education, race, ethnicity, age, and previous births, the size and type of hospital, the hour, day and month of birth, plus insurance times HSA region effects and time trends that vary by insurance/HSA status.

Table 6
2SLS Estimates of Newborn 28-day re-admission equation, Privately insured and Medicaid deliveries in California, July 1995 through December 2000

| | Uncomplicated vaginal deliveries | Complicated vaginal deliveries | C-section deliveries |
|---|---|---|---|
| OLS estimates, newborn 28-day re-admission equation | | | |
| Discharged early | −0.0021 (0.0005) | 0.0002 (0.0006) | 0.0024 (0.0006) |
| Mean of dep. variable, 7/1/95–8/31/97 | 0.0354 | 0.0396 | 0.0276 |
| Observations | 1,435,917 | 601,781 | 580,215 |
| 2SLS estimates, newborn 28-day re-admission equation[a] | | | |
| Discharged early | −0.0019 (0.0031) | 0.0068 (0.0050) | 0.0243 (0.0087) |
| $F$-test ($p$-value) for 1st stage | 0.0000 | 0.0000 | 0.0000 |
| $p$-Value, Test of over-identifying restrictions | 0.0000 | 0.0002 | 0.1954 |
| 2SLS estimates, newborn 28-day re-admission equation[b] | | | |
| Discharged early | −0.0025 (0.0032) | 0.0059 (0.0050) | 0.0229 (0.0086) |
| $F$-test ($p$-value) for 1st stage | 0.0000 | 0.0000 | 0.0000 |
| $p$-Value, test of over-identifying restrictions | 0.0066 | 0.0594 | 0.9543 |
| Observations | 1,348,203 | 601,781 | 546,409 |

Standard errors are reported in parentheses. Standard errors are calculated allowing for arbitrary correlation in errors within a hospital. Other covariates include the hospital admission index for 90–180 day infants, mother's education, race, ethnicity, age, and previous births, the size and type of hospital, the hour, day and month of birth, plus insurance times HSA region effects and time trends that vary by insurance/HSA status.

[a] Using federal law × private, federal law × Medicaid, expanded state law × Medicaid, state law × private, state law × Medicaid as instruments.

[b] Using federal law × private, federal law × Medicaid, and expanded state law × Medicaid as instruments.

prevented by the law would have happened within the first week. The fact that much of the reductions in re-admissions is occurring within the first 7 days of birth is not a surprise; as we noted above, most re-admission occur within a few days of discharge.

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 three samples, neonatal mortality is anywhere from 6 per 10,000 to 32 per 10,000 so this adverse outcome is very rare. Unfortunately, the results from this analysis are not clear. Among privately insured newborns from uncomplicated vaginal deliveries, the marginal effect for the federal law is −0.000072 which is about one-sixth the sample mean with a standard error dramatically larger than the parameter size. Other results show similar magnitude but there is little consistent pattern in the sign and almost all results are smaller in magnitude than the standard errors. Given the estimated standard errors, the law change does not provide enough information to determine anything about the impact of an early discharge on infant mortality.

### 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 Eq. (2). As we noted above in Table 1, those most likely to have longer hospital stays are also those most likely to experience a 28-day re-admission. If the unobserved characteristics of mothers and their infants that predict 28-day re-admission rates are negatively correlated with the probability of an early discharge, then OLS estimates of Eq. (2) are biased towards zero.

In the first third of Table 6, we report OLS estimates of Eq. (2) to form a baseline to which we can compare 2SLS results. In this model, the 28-day re-admission dummy variable is the outcome of interest and key covariate is whether the newborn was discharged early. The estimated impact of being discharged early among uncomplicated vaginal deliveries is small at a statistically significant two tenths of a percentage point decrease in the re-admission rate. For complicated vaginal deliveries the estimated coefficient suggests a statistically insignificant effect on 28-day re-admission rate of three hundredths of a percentage point. In contrast, for the c-section samples, being released early is associated with a statistically significant 0.24% point increase in the 28-day re-admission rate.

In the next third of the table, we report 2SLS estimates that use the five instruments for postpartum length of stay: the federal and state laws interacted with Medicaid and private insurance, thus the extension of the state law to Medicaid patients in 1999. In the c-section and complicated vaginal deliveries samples, 2SLS estimates of the discharged early

Table 7
2SLS estimates of newborn 28-day re-admission equation, privately insured and Medicaid deliveries in California, July 1995 through December 2000

| | Vaginal deliveries | | C-section deliveries | |
|---|---|---|---|---|
| | Private | Medicaid | Private | Medicaid |
| 1st stage estimates: discharged early equation | | | | |
| Federal law × private insurance | −0.2934 (0.0163) | | −0.1324 (0.0122) | |
| Federal law × Medicaid | | −0.1346 (0.0108) | | −0.0287 (0.0074) |
| Expanded state law × Medicaid | | −0.0619 (0.0069) | | −0.0442 (0.0092) |
| Mean of dependent variable | 0.808 | 0.700 | 0.843 | 0.812 |
| Number of observations | 1,050,696 | 862,466 | 313,449 | 232,960 |
| Reduced-form estimates: 28-day re-admission equation | | | | |
| Federal law × private insurance | 0.00062 (0.00093) | | −0.0031 (0.0012) | |
| Federal law × Medicaid | | −0.00056 (0.0011) | | −0.00057 (0.0020) |
| Expanded state law × Medicaid | | −0.0031 (0.0010) | | −0.0013 (0.0015) |
| OLS estimates: 28-day re-admission equation | | | | |
| Discharged early | −0.0019 (0.0005) | −0.0013 (0.0008) | 0.0032 (0.0007) | 0.0282 (0.0343) |
| Mean of dependent variable | 0.033 | 0.040 | 0.023 | 0.033 |
| 2SLS Estimates: 28-day re-admission equation | | | | |
| Discharged early | −0.0021 (0.0032) | 0.0107 (0.0081) | 0.0233 (0.0090) | 0.0367 (0.0343) |

Standard errors are reported in parentheses. Standard errors are calculated allowing for arbitrary correlation in errors within a hospital. Other covariates include the hospital admission index for 90–180 day infants, mother's education, race, ethnicity, age, and previous births, the size and type of hospital, the hour, day and month of birth, plus insurance times HSA region effects and time trends that vary by insurance/HSA status. The *F*-test for the 1st stage is the test of the null hypothesis that all the instruments are zero.

variable are substantially larger than the OLS estimate. The estimate in the uncomplicated vaginal delivery sample is still negative and a statistically insignificant −0.19% points. In the complicated vaginal delivery sample, the 2SLS coefficient on the discharged early variable is 0.68% points, an estimate much larger than the OLS value, but with a *p*-value for the test that the coefficient is zero of 0.18. Finally, the same estimate in the c-section sample is 2.43% points, which is slightly smaller than the pre-treatment sample mean, substantially larger than the OLS estimate, and statistically significant at conventional levels. The *F*-tests that the instruments can be excluded from the first-stage equation of interest are all large, indicating that finite sample bias is not a concern here. Finally, we do reject the test of over-identifying restrictions at the 0.05 level in both vaginal delivery samples. In the one case where we find statistically significant 2SLS estimates, the *p*-value on this test approaches 0.20.

Given the imperfect controls for cyclic variation in the re-admission rates, we find in reduced-form models that the state law actually increased re-admission 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 this test since the federal instrument is predicting a positive benefit of an additional day of stay whereas the other instruments predict a negative effect.

The reduced-form estimates in Table 4 suggest that the early discharge laws improved outcomes for vaginally delivered newborns insured by Medicaid and privately insured newborn delivered by c-section. There is some evidence of a health benefit for Medicaid patients from c-section deliveries but no evidence of a benefit for privately insured vaginally delivered newborns. In Table 7, we consider separate samples based only on the method of delivery (vaginal versus c-section) and insurance (private versus Medicaid). In the table, we report the first-stages, the reduced-forms, plus the OLS and 2SLS estimates of Eq. (2).

The first-stage estimates for these samples are no surprise. The largest change in early discharge rates are for privately insured vaginal deliveries (a 29% point drop), the effect for Medicaid patients after the state law was expanded is about 33% smaller (a 20% point drop), and the same estimates for c-section deliveries are 50–60% smaller than the corresponding vaginal delivery results. The reduced-forms show no impact of the laws on re-admission rates for privately insured vaginal deliveries, but by the time the state law was expanded to all Medicaid patients, there is a statistically significant drop in re-admission rates for vaginally delivered newborns of 3 tenths of a percentage point. We find statistically significant drops in re-admission rates for privately insured c-section newborns of also three tenths

Table 8
Reduced from estimates of newborn 28-day re-admission equation, privately insured and Medicaid deliveries in California, July 1995 through December 2000

| | Uncomplicated vaginal deliveries | | | Complicated vaginal deliveries | | | C-section deliveries | | |
|---|---|---|---|---|---|---|---|---|---|
| | Federal law × private insurance | Federal law × Medicaid | Expanded state law × Medicaid | Federal law × private insurance | Federal law × Medicaid | Expanded state law × Medicaid | Federal law × private insurance | Federal law × Medicaid | Expanded state law × Medicaid |
| (1) Baseline model, Table 4 | 0.0011 (0.0010) | −0.00012 (0.0012) | −0.0028 (0.0011) | −0.00076 (0.0014) | −0.0026 (0.0019) | −0.0039 (0.0017) | −0.0031 (0.0011) | −0.00026 (0.0020) | −0.0008 (0.0014) |
| (2) Allow trends to vary by health service area, insurance status and Hispanic origin | 0.00088 (0.0010) | −0.00010 (0.0012) | −0.0027 (0.0011) | −0.00095 (0.0014) | −0.0027 (0.0019) | −0.0039 (0.0017) | −0.0030 (0.0012) | −0.00016 (0.0020) | −0.0008 (0.0015) |
| (3) Allow trends to vary by health service area, insurance status, Hispanic origin, and race | 0.00091 (0.0010) | −0.00014 (0.0012) | −0.0027 (0.0011) | −0.0010 (0.0015) | −0.0028 (0.0019) | −0.0039 (0.0017) | −0.0030 (0.0012) | −0.00009 (0.0020) | −0.0008 (0.0015) |
| (5) Add to model (1) quadratic time trends that vary with health service are and insurance status [$p$-value on $F$-test that the additional variables are zero] | 0.0016 (0.0011) [0.114] | −0.00047 (0.0011) | −0.0019 (0.0011) | 0.0009 (0.0016) [0.005] | −0.0034 (0.0019) | −0.0023 (0.0020) | −0.0033 (0.0014) [0.800] | 0.00011 (0.0021) | −0.0015 (0.0016) |
| (6) Add to model (5) cubic time trends that vary with health service area and insurance status [$p$-value on $F$-test that the additional variables are zero] | 0.0025 (0.0015) [0.027] | −0.00091 (0.0015) | −0.0009 (0.0015) | 0.0032 (0.0020) [0.001] | 0.0013 (0.0031) | 0.0005 (0.0027) | −0.0015 (0.0018) [0.567] | 0.0037 (0.0028) | 0.0007 (0.0022) |

Standard errors are reported in parentheses. Standard errors are calculated allowing for arbitrary correlation in errors within a hospital. Other covariates include the hospital admission index for 90–180 day infants, controls for payer status, mother's education, race, ethnicity, age, and previous births, the size, location and type of hospital, the hour, day and month of birth, plus time trends that vary by insurance status.

of a percentage point. There is some evidence of a decline in re-admissions for the same type of children but ones insured by Medicaid, but these results are statistically insignificant.

In the final two sets of results in Table 7, we report OLS and 2SLS estimates of the structural equation of interest. In the final three columns, we find large estimated impacts of being discharged early on re-admission rates but the only result that is statistically significant is the privately insured c-section births. The results for Medicaid c-section births are similar in magnitude but with standard errors similar in size to the parameter estimates. For vaginally delivered insured by Medicaid, the 2SLS estimate suggests early discharges increase the chance of a re-admission by 1% but the t-statistic on this estimate is only 1.32.

The basic reduced-form results from Table 4 are robust to a number of alterations to the model. These results are presented in Table 8 with the first row of the table reproducing the results from Table 4. In preliminary analysis with the data, we noted a pronounced cyclic time series pattern in the number of Hispanic births over the year, with peaks the late fall and troughs during the spring. For this reason, we allow the time trends and the group dummy variables to vary by health service area, insurance status and Hispanic origin (row 2), then in row (3), we also allow heterogeneity in these effects to vary by race as well. These additions do not change the estimated effects much.

In rows 4 and 5 we alter the basic estimates in row 1 by allowing for quadratic and cubic time trends. There is mixed evidence of the robustness of the model. Adding a quadratic trend does not change the qualitative nature of the results but adding the cubic term wipes out the statistically significant reduced form results from Table 4. In some cases, there is concern however that adding these additional terms is ‘over-fitting’ the model. In brackets in these two rows, we report the *p*-value on the joint test that these additional trends all have zero coefficients. In both cases, the restricted models are those from Table 4. For c-section deliveries, the *p*-value on these additional time trends is well above the 0.05 critical value in both cases.

## 8. Conclusion

In this paper, we use large and sudden changes 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. State and federal laws worked as intended in that the fraction of newborns discharged early fell dramatically for both privately insured and Medicaid newborns. However, changes in re-admission rates generated by the passage of the laws were not nearly as uniform. We estimate that the law had small if any benefits for the group with the lowest 28-day re-admission rate, privately insured newborns from uncomplicated vaginal deliveries, a group that represents 30% of births in our time period of analysis. However, for all other subsamples, there is some evidence that early discharges decreased re-admission rates. Among c-sectional deliveries, complicated vaginal births and Medicaid patients with complicated vaginal deliveries, we estimate early discharges decreased re-admission rates by a substantial amount, with many of these results being statistically significant at conventional levels. These results suggest that for routine pregnancies, early discharge of newborns pose little health concern, yet those with the highest risk of re-admission benefited enormously from passage of the early discharge laws.

We can use the reduced-form estimates above and other ancillary parameters to estimate the cost of the California and Federal laws for a representative year. In this case, we consider the final year in our sample which is 2000, a year in which there were 483,480 privately insured and Medicaid births in California. First, for the three subsamples we have been working with, we estimate regressions similar to those in Table 4 where the dependent variables are the length of stay for the mothers and the newborns. We then multiply these estimates by the median cost per day for 2000 for the particular subsample and the number of births in 2000.[28] Summing these calculations over all subsamples, insurers, and for mothers and newborns, we estimate that the law increased costs by $414 million.

There are a number of benefits of the law. First, we consider the benefits of reduced re-admissions generated by the statutes. Using a technique similar to the one outlined in the previous paragraph, we calculate that the law reduced re-admission by 912 in 2000. We find in our sample, among those re-admitted within 28 days of birth, the median cost per re-admission was $6737. This however is only one aspect of the cost. Parents are willing to pay to reduce the risk

[28] For example, in 2000, our sample contains 144,878 uncomplicated privately insured vaginal deliveries, the law is estimated to increase the average length of stay for newborns by 0.346 days and the average cost per day was roughly $1132 so the law is estimated to have increased costs for this group by $56.7 million.

of an adverse health event. Unfortunately, there are no academic studies that have measured parent's willingness to pay to reduce a newborn re-admission. Alberini and Krupnick (2000) note that in direct comparisons, willingness to pay values are larger than cost of illness calculations by a factor of 1.6–4. Taking the upper bound on this, the monetary benefit of 921 fewer re-admissions is 921(4)(6737) = $24.8 million. This number does not include any other health care costs such as fewer postpartum doctor visits but that number should be small since we note above that re-admissions are the major health care cost for infants in the first year of life.

We estimate that about 60,000 women stayed an extra night in the hospital, and revealed preference indicates that these women received positive value from this extra night of peace and quiet. However, there are not studies to indicate what women are willing to pay for an additional night of stay. Prior to the law, since most mothers chose not to spend the roughly $2200 it would have cost to keep her and her infant in the hospital for an extra day, this would appear to be the upper bound on this value for the mothers whose behavior was changed as a result of the law. If the benefits of reduced hospitalizations are $25 million and the costs of the laws are $414 million, then the law would only be cost effective if moms valued the extra night in the hospital at (414 million–25 million)/60,000 or $6483 which is roughly three times the median cost of an extra day in the hospital for both the mother and her newborn. Overall, the early discharge laws do not appear to be a cost effective policy.

These results suggest that altering the law so that only complicated deliveries would be given extra postpartum stays would save resources with little cost to health. This could however increase the incentive to classify patients as having a complication when they do not in order to allow mothers and their newborns more time in the hospital. Some criteria such as using the presence of specific conditions or requiring the presence of more easily verifiable markers such as low birth weight would generate fewer opportunities for strategic behavior. We do note, however, that hospitals already receive higher reimbursements for patients identified as having complications so whether a complication-based early discharge law would alter these existing incentives remains to be seen.

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