Baby Boomlets and Baby Health:
Hospital Crowdedness, Treatment Intensity, and Infant Health

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To identify the causal relationship between health care spending and infant health, we introduce a new instrument: the number of infants born on a given day in a given hospital. The thought experiment is on a crowded day at-risk infants receive reduced care because resource constraints are binding. Using detailed information on every birth in California from 2002 to 2006, we find that hospital crowdedness impacts treatment intensity. We show that OLS estimates overestimate the benefits of medical care. Our results suggest that the mortality benefits from additional spending are negligible and that more intensive treatment increases hospital readmission rates. (JEL codes: I12, I18)

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I. Introduction

Childbirth is the most common medical procedure. According to Nationwide Inpatient Sample data (HCUP, 2005), almost 19 percent of all hospitalization were related to childbirth. Moreover, the second and third most expensive condition treated in US hospitals were "Mother's pregnancy and delivery" and "newborn infants", which accounted for 5.2 percent and 4.3 percent of the national hospital bill, respectively (Russo and Andrews, 2006). Despite escalating health care cost, little consensus has emerged about the value of additional hospital care for newborns.[1]

The key methodological challenge when identifying the causal relationship between health care spending and health is non-random selection of patients into treatment. To clearly identify the relationship between treatment intensity and health, an exogenous source of variation in treatment intensity is needed. This paper introduces a new identifying variable: hospital crowdedness measured in its simplest form by the number of infants born on a given day in a given hospital. We examine the effectiveness of additional treatment that stems from the non-uniform distribution of birth dates within a given hospital.[2] The thought experiment is that on a relatively uncrowded day an infant may receive more care either because the resource constraints are less binding or because health care providers respond to the temporary income shock by performing additional procedures.

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1 Evidence from studies investigating the effectiveness of new therapeutic improvements tends to suggest that additional spending generates declines in infant mortality (Richardson et. al., 1998; Cutler, 2005; Phibbs et. al., 2007; Almond et. al., 2010). However, other work suggests that neonatal resources have expanded to the point where additional benefits are negligible. (Goodman et. al., 2002). Using exogenous variation in access to treatment generated by Medicaid expansion, Currie and Gruber (1996) show that additional treatments to pregnant women and children lowered infant mortality, while Haas et. al. (1993) and Piper et. al. (1990) find no impact of Medicaid expansion on infant health.

2 This approach is similar in spirit to Hoxby (2000) who looks at the effect of class size on student achievement using exogenous variation in class size that stems from idiosyncratic variation in the population.

We compare infant health within the same hospital using variation in hospital charges per birth that arises from short-term hospital crowding after netting out time period (day of the week, month of the year, and calendar year) effects. By exploiting the variation in crowdedness within hospital and time period, our estimates are free of bias due to heterogeneity in health outcomes associated with resource availability, resource quality, or patient mix among hospitals and across time periods. After netting out time period effects, the number of infants born on a given day in a given hospital is a plausible instrument because the number of other infants who share the target infant's birthday should not have any independent impact on the target infant's health other than through the intensity of hospital care reflected in hospital charges. Moreover, the number of other infants born on a given day in a given hospital is highly correlated with health care spending for the target infant and other measures of treatment intensity.

Previous studies have used legislative mandates, specifically mandatory minimum length of stay coverage for hospitalization after birth to obtain estimates of the causal impact of additional treatment on infant health as measured by infant mortality and/or hospital readmission. These studies find no benefit of longer postpartum hospital stay on infant mortality and mixed results for readmission.[3] However, these studies are biased towards finding a null effect of treatment on infant health because their identification stems from treatment that is altered

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[3] Madden et. al. (2002) find little or no relationship between postpartum hospital stays and hospital readmission rates. Meara et. al. (2004) find rates of all-cause rehospitalization did not change in the year after legislation was introduced. Datar and Sood (2006) find that longer hospital stays for newborns are associated with lower probabilities of hospital readmissions but no impact on infant mortality. Using a similar methodology as Datar and Sood but a richer restricted use dataset, Evans et. al. (2008) find no effect of mandated extended hospital stays on infant mortality rate and mixed results for readmission rates depending on the medical risk of the subgroup. Almond and Doyle (2011) find that infants born shortly after midnight have longer hospital stays than infants born shortly before midnight due to hospital billing practice. They show that remaining in the hospital longer has no effect on readmission or mortality. Evans and Garthwaite (2012) find that for average newborn impacted by the law, longer stays have a statistically insignificant impact on hospital readmission rates. However, they find impacts for those infants with a high likelihood of readmission.

because of legislative mandates not the decision of medical personnel. If health care providers were practicing effectively, it is not surprising that laws that require longer hospitalization stays for infants (who would have been voluntarily discharged by medical personnel before the insurance mandate) do not produce any health gains. A related literature uses state-level variation in malpractice reform. Currie and Macleod (2008) find no impact of a reduction in procedures such as Caesarean section and inducement on newborn health following tort reform. A null effect of treatment intensity on newborn health might be expected, if the procedures eliminated are procedures of marginal medical value, procedures which the medical community was only performing out of fear of lawsuits.

Unlike these studies, our empirical approach exploits variation in treatment that stems from decisions made by medical personnel when responding to hospital capacity constraints. In other words, we are identifying the impact of care medical personnel chose to add when unconstrained, which is likely to be of more value than care added by legislative mandate or fear of malpractice. In addition, we estimate a policy relevant Local Average Treatment Effect. The infants who receive less care when the hospital is crowded are precisely the infants who would be the first ones to receive less care if hospitals became more resource constrained due to hospital closures or an increasing birth rate.

We focus on the causal relationship between hospital spending and infant health measured by mortality and hospital readmission.[4] As the best available summary measure of health inputs, hospital charges reflects length of stay, number of procedures, and kinds of procedures performed during the

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[4] There is a large related literature on hospital treatment intensity and adult health. Recent studies which use variation in treatment from exogenous changes in insurance coverage (Card et. al., 2009) or from automobile accident (Doyle, 2005) suggest that additional hospital care improves adult mortality. Picone et. al. (2003) find that higher hospitalization costs improve patient survival. However, studies that use geographic variation in hospital spending tend to find little to no benefit from additional spending (Skinner et. al., 2005; Landrum et. al., 2008).

hospitalization. Health care spending is also at the center of the policy debates. We further focus on at-risk infants. At-risk infants are those who are either born prematurely or with low birth weight. Our analysis suggests that differences in hospital charges between infants born on crowded days and uncrowded days are especially pronounced among at-risk infants. Additionally, mortality and hospital readmission are uncommon for non-at-risk infants. For example, the average one-year mortality rate for at-risk infants is 2.7 percent compare to 0.2 percent for those infants who are not at-risk. The very low mortality and readmission rates for non-at-risk infants could mask the beneficial effects of higher hospital spending.

Our dataset captures every birth that occurred in a California hospital between 2002 and 2006. After conditioning on a rich set of control variables, OLS estimates suggest that higher health care spending is associated with improved infant health. Our main finding is that at-risk infants who had more intensive hospital stays because they were born on uncrowded days fared no better than their busy day counterparts, and may fare worse. Additional health care spending does not improve infant health status as measured by neonatal (28 day) mortality or one-year mortality rates. When infant health is measured by 28-day readmission rate, we find evidence that additional spending is harmful. These finding are robust to alternative measures of crowdedness that account for the duration of the hospital stay. When we look at alternative measures of treatment intensity, similar findings are obtained.

Two related works document that hospitals respond to short-term variation in hospital crowdedness by reducing care. Evans and Kim (2006) find some evidence that “high-risk” adults who are admitted to a California hospital on a Thursday have slightly shorter lengths of stays if the following Friday and Saturday have above average admissions. Freedman (2012) documents that the probability of admissions to a Neonatal Intensive Care Unit (NICU) increases when there are more vacant NICU beds in an infant’s delivery hospital the day prior to birth. The effect is particularly large for low birth weight infants for whom there is discretion over the appropriate amount of treatment. These results align with our findings that hospitals change their treatment decisions in response to short-term variations in capacity constraints. In reduced form regressions, Evans and Kim (2006) find no impact of busy Friday/Saturdays on adult mortality, and small and normally insignificant impacts on readmission probabilities. They do not look at hospital charges or directly investigate the effect of length of stay on health outcomes. While interested in the causal impact of NICU admission on infant health, Freedman (2012) argues that it is inappropriate to use empty NICU beds as an instrument to estimate the effect of NICU admissions on hospital charges and health outcomes. First, NICU capacity may impact costs and outcomes through avenues other than NICU admission, say by substitution of resources between NICU and non-NICU patients. Since we focus on the impact of changes in hospital spending and not changes in NICU admissions in response to crowdedness, our estimates do not suffer from this concern. Second, Freedman states that infants who are placed into a NICU because there are empty beds are likely to be unobservably healthier violating the exclusion restriction.[5]

This paper is organized as follows: Section II describes the dataset and provides support for our empirical strategy. Section III discusses the relationship between hospital crowdedness and hospital spending. Section IV presents our main findings. Section V presents and discusses results from various robustness checks. Section VI discusses alternative explanations for our findings, and Section VII concludes the paper.

[5] Reduced formed estimates in Freedman (2012) suggest that low and very low birth weight infants born on days with vacant NICU beds have lower mortality rates, thus suggesting that additional capacity in the form of NICU beds would increase infant health. He notes that estimates on mortality due to empty NICU beds may overstate the true health effects if unobservably healthier infants are placed in the NICU. If we estimate a reduced form equation similar to the one estimated by Freedman, we find that infant born on days with relatively few deliveries have *higher* mortality rates. Freedman finds no impact of empty NICU beds on 28-day readmission.

II. Data and Descriptive Statistics

A. Data

The data used in this study are confidential data provided by the California Office of Statewide Health Planning and Development (OSHPD). The OSHPD data link infant hospital discharge records for all hospital stays during the first year of life with birth and death certificate data. The OSHPD data provide birth date and birth hospital for every hospital birth in California between 2002 and 2006, which are used to generate our identifying variable, the hospital-level crowdedness on delivery day. While lacking complete health procedure information, the data contain hospital charges for both infant and mother and length of hospital stay. It also provides detailed information on prenatal care, parental demographic information, newborn characteristics. We can exploit the linked nature of our data and construct our measures of newborn health: whether the newborn died within 28 days, died within a year, or was readmitted to any California hospital within 28 days[6].

For the purposes of this study, we measure hospital charges/spending[7] as the sum of hospital charges from all consecutive hospital stays after birth. If a newborn was transferred from the birth hospital, we track the infant charges for all transferred hospitals stays until the newborn was discharged and included

6 We cannot identify readmission if it occurs out of the state of California or if the infant is readmitted to a Federal hospital. Similarly we cannot identify an infant death if it occurs outside the state of California.

7 Throughout the paper we will use the terms hospital charges and spending interchangeably. Hospital charges include all charges for services rendered during the infant's stay at the facility, based on the hospital fully established rates. Hospital-based physician fees are excluded. We assume that hospital charges and physician fees are positively correlated. Additionally, hospital charges could be thought of as list prices, the actual prices are often much lower and vary by hospital and insurance companies. Thus we control for hospital fixed effects and type of insurance coverage in all regressions. We have no reason to believe that the negotiated price varies by the daily hospital crowdedness.

these charges in our measure of infant hospital charges. Similarly, hospital stay is the length of consecutive hospital stays after the birth including transfers.

Initial data from OSHPD contained 2,675,954 birth records with birth date and birth hospital. We exclude all Kaiser Foundation Hospitals because Kaiser Hospitals are exempt from reporting hospital charges. Instead of charging specifically for an inpatient stay, Kaiser Hospitals receive a constant monthly (capitated) payment from each member, whether or not that member is hospitalized. Additionally, a small fraction of our sample (less than three percent) is born in a hospital where very few deliveries occur. Because there is limited variation in crowdedness, we exclude all hospitals where the average daily number of births is less than two. This leaves us 2,329,810 births at 226 California hospitals. We construct our daily crowdedness measures from this sample.

Our analysis sample is further restricted to at-risk infants.[8] We classify an infant as at-risk if she is low birth weight or premature. Following Centers for Disease Control and Prevention definitions, we limit the sample to those births where birth weight was less than 2500 grams or gestation length was shorter than 37 weeks. We drop the handful of records with missing hospital charge information.[9] We also drop infants born during the first and the last weeks of our sample period. When we extend our measure of crowdedness, we cannot obtain the number of infants born before or after the delivery day for those infants born on the first or last week of the sample period, respectively. Among the 2.3 million infants that are born in one of 226 California hospitals between the years 2002 and 2006, there are 302,649 at-risk infants.

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8 If we conduct the analysis for all California births, we obtain qualitatively similar findings. If we focus on the non-at-risk infants, we find a small negative relationship between hospital crowdedness and hospital charges which can be entirely explained by longer hospital stays.

9 There were 391 observations with missing information on hospital charges. While the mortality rate for those with missing charge is very high, it is higher on *slow* day deliveries (49/79 or 62%), than *busy* day deliveries (58/102 or 57%).

Table 1 reports the summary statistics for our analysis sample of at-risk infants as well as the births which are used to construct the daily hospital crowdedness measures. The average infant in the analysis sample is born to a 28 year-old woman who completed high school. Over half of all births are to Hispanic women, and more than half of the births are covered under Medicaid. The average hospital charge for at-risk infants is around $64,000. Charges increase to $81,326 if we include hospital charges billed to the mother for the delivery. The average length of stay in hospital is 3.38 days, which rises to 10 days for at-risk infants. While one-year mortality of all infants is 0.5 percent that of at-risk infants is more than five times higher at 2.7 percent. The 28 day readmission rate for at-risk infants is 11.6 percent, four times higher than the rate for all infants. About 16 percent of at-risk births are multiple births compared to only 3 percent of all births.

B. Variation in Daily Hospital Crowdedness

Our identification strategy requires sufficient within hospital variation in the daily number of births. Panel A of Table 2 reports descriptive statistics for selected representative hospitals. Based on the average daily number of births, the smallest, every 10th percentile, and the largest hospitals are shown. The columns show the mean, the standard deviation, the minimum, the quartiles, and the maximum number of infants born a day for each representative hospital during our sample period. Table 2 shows there is sizable variation in the number of births within a given hospital. For example, in the 50th percentile hospital, 20 infants were born on the busiest day and only one infant was born on the slowest day. The interquartile ranges are also sizable.

While the numbers in Panel A give a sense of the underlying variation in crowdedness, the identification strategy is based on exogenous variation in hospital crowdedness. To isolate the plausibly exogenous variation in daily hospital crowdedness, we construct a residual crowdedness measure which captures the variation in crowdedness after netting out parity, hospital, and timing effects. Specifically, the residual crowdedness measure is the e_{ijt}'s from equation (1).

(1) $\text{Number of Births}_{ijt} = \text{Parity}_{ijt} + \text{Hospital}_{ij} + \text{Day of the Week}_{it} + \text{Year}_{it} + e_{ijt}$

Number of Births_{ijt} is the number of births in hospital j, on infant i's birth date t. Parity_{ijt} is a set of indicators for multiple births (twins, triplets, and quadruplets or more). We include parity, because multiple births are mathematically correlated with daily hospital crowdedness, and multiple births are associated worse health outcomes. Thus the variation in crowdedness that stems from multiple births violates the exclusion restriction.

The hospitals in our sample vary in capacity and resources, both of which are associated with better health outcomes (Gaynor et. al., 2005; Phibbs et al., 2007; Wehby et. al., 2012 Evans and Kim 2006), thus we include hospital fixed effects in equation (1). We condition on day of the week, because there are significantly more births on weekdays than weekends, and numerous authors have documented an association between weekend births and higher infant mortality rates (MacFarlane, 1978; Rindfuss et. al., 1979; Mangold, 1981). We further net out year fixed effects to avoid the bias from the correlation between increase in the number of births over time and the increase in hospital charges and infant health over time.

The first row of Panel B in Table 2 summarizes the variation in the residuals for our main crowdedness measure: daily number of births. In essence, the residual crowdedness measure captures the variation in hospital crowdedness after netting out parity, hospital, and time effects. Even after netting out variables that are associated with crowdedness, there is sizable variation in daily crowdedness. The residuals range from -19.83 to 19.63. At the 25th percentile of the residual distribution, there are 2 fewer daily births than the hospital-year-day of the week average, at the 75th percentile there are 1.9 more births than the hospital-year-day of the week average. Throughout the paper we refer to *slow* days as days with residuals in the bottom quartile of the residual distribution and *busy* days as days with residuals in the top quartile. The standard deviation of the residual crowdedness measure is 3.21 (by construction the mean is zero). We will discuss the magnitude of the first stage results assuming a change in the number of daily births of one standard deviation of the residual crowdedness measure.

C. Observable Characteristics by Hospital Crowdedness

The underlying assumption for our identification strategy is that hospital crowdedness should be uncorrelated with any infant level observable (and unobservable) trait that may impact infant health. We present evidence on the validity of our approach by reporting summary statistics separately for at-risk infants born on *busy* and *slow* days. Recall that for a given hospital, a delivery day is defined as *busy/slow* if the day is in the top/bottom quartile in the residual crowdedness distribution. In essence we ask, given a hospital, year, day of week, singleton or multiple births, whether infants born on *slow* days are different for their counterparts born on *busy* days in terms of observable characteristics. If there are differences in observable traits, it calls into question the identification strategy as it suggests that *slow* day births may differ in their health outcomes for reasons other than hospital crowdedness.

While conditioning on parity, hospital, year, and day of the week rules out many obvious threats to identification, other possible threats remain. One possible threat would be hospitals rerouting mothers in labor. If crowded hospitals direct mothers to less crowded hospitals based on potential delivery complications, our estimates would be biased because the underlying health status of infants born would be correlated with hospital crowdedness. Alternatively, say that extremely warm weather creates pregnancy complications that induce early labor. If so warm days would be more crowded, and those infants born on warm days would need more treatments due to shorter gestation lengths, thus threatening our identification strategy.

Table 3 presents evidence on validity of our instrument. The average *busy* day has about twice as many births as the average *slow* day. Hospitals do not appear to be using transfers to alleviate crowdedness. The underlying transfer rate is small. The likelihood of transfer at some point during the initial hospitalization is 0.1 percentage point higher on *busy* days. However the likelihood of transfer to another hospital on the infant's birth date is 0.1 percentage point lower on *busy* days.[10]

The rest of the table demonstrates that for many important infant background variables *slow* day births mirror their *busy* day counterparts. *Slow* day infants and *busy* day infants received similar levels of prenatal care. Parents of *slow* day infants tend to be younger and less educated. However the size of the difference is very small. For example, mothers of *slow* day infants are younger than mothers of *busy* day infants by 0.11 year, which is less than 40 days. The difference in maternal education is only 0.09 years. Differences in paternal education and age are also small. The gender and racial distribution of infant born on *slow* and *busy* days are almost identical. Importantly, insurance status also shows identical coverage across *slow* and *busy* day births. Generally, the differences in observable traits between infants born on *slow* and *busy* days are negligible.

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[10] To further investigate if hospitals are transferring newborns to relieve crowdedness, we estimated a regression of the likelihood of transfer on the infant's birth date on hospital crowdedness and a complete set of control variables. We find a small negative and statistically insignificant coefficient on the hospital crowdedness variable.

Given that we find that the reduction in spending associated with crowded days does not harm infant health, we would be concerned if *busy* day infants are healthier than their *slow* day counterparts. *Slow* and *busy* day infants have identical likelihood of reporting pregnancy complications. Additionally Caesarean section rates are also identical between *slow* day infants and *busy* day infants. Birth weight and gestation length are the most commonly used indicators of newborn health. The length of gestation for *slow* day infants and *busy* day infants are almost identical while at-risk infants born on *slow* days are only 8 grams lighter than their *busy* day counterparts.

Overall, Table 3 shows that, once we condition on hospital, year, day of the week, and parity, there are no apparent differences in observable family background and health indicators across infants born on crowded and uncrowded days. This suggests that the level of crowdedness is orthogonal to underlying infant characteristics that may impact spending and health. Hospital crowdedness thus mimics an experiment in which nature assigns, to each infant, a level of treatment intensity, independent of her background.

The bottom panels of Table 3 presents preliminary evidence of our findings. Despite the very similar pregnancy, newborn, parental, and insurance characteristics, infants born on *slow* days are treated more intensively than their *busy* day counterparts. Specifically, *slow* day infants report an additional $3,592 in hospital charges and an additional half day of hospitalization. Despite receiving more intensive medical care, *slow* day infants report slightly higher neonatal mortality, one-year mortality, and 28-day readmission rates which suggest the additional spending does not improve, and may harm, infant health.

III. The Impact of Daily Hospital Crowdedness on Hospital Spending

Figure 1 shows a visual representation of the relationship between hospital crowdedness and hospital charges. Specifically, Figure 1 plots the difference in average infant hospital charges between *busy* and *slow* day births grouped by 300 gram birth weight bins plotted at the midpoint. For instance, the data point which corresponds to 1350g shows the difference in average hospital charge for at-risk infants with birth weights between 1200g and 1500g. Infants in this birth weight category born on slow days have an additional $4,022 in hospital spending. As is clear from the figure, throughout the birth weight distribution, there are sizable differences in spending in favor of *slow* day infants.

Equation (2) formalizes the relationship between hospital crowdedness and health spending utilizing the difference in health care spending that arises from variation in the number of infants born on a given day in a given hospital. The first-stage equation for the IV estimate is:

$$(2) \qquad \text{Spending}_{ijt} = \theta_1 \text{Number of Births}_{ijt} + X'_{ijt}\theta_2 + \varphi_t + \lambda_j + \omega_{ijt}$$

where Spending_{ijt} is log of hospital charge for infant *i* born in hospital *j* at time *t*. If infant *i* was transferred to another California hospital after delivery, we tracked all transferred hospital stays and added all listed hospital charges to construct the infant spending measure. The key variable $\text{Number of Births}_{ijt}$ captures the number of infants born on infant *i*'s date of birth in hospital *j* on day *t*.

X'_{ijt} is a vector of control variables. Included in X'_{ijt} are pregnancy characteristics (the number of prenatal care visits, month the prenatal care began, and an indicator for pregnancy complications), parental characteristics (categorical dummy variables for age and education of mother and father, and insurance type), and newborn characteristics (gender, race, parity (twins, triplets, quadruples or more), and an indicator for whether the infant was the first born).

Flexible measures of birth weight and length of gestation are also included in X'_{ijt}. Because of the nonlinear impact on infant health, birth weight is categorized at 500g intervals and length of gestation is categorized at two week intervals.

φ_t is a set of time indicators for the day of the week, month of the year, major holiday (New Year's Day, Independence Day, Thanksgiving Day, or Christmas), and year. As mentioned earlier, there is an established association between weekend births and higher infant mortality rates. If weekends have lower quality staff and/or unobservably sicker infants, then weekend births will have poor health outcomes, and failure to control for the day of the week would bias our result.[11] We include indicator variable for each month of the year to account for the fact that infant and maternal characteristics are not uniformly distributed throughout the year (Dehejia and Muney, 2004; Buckles and Hungerman, 2013; Currie and Schwandt, 2013). There is seasonal variation in crowdedness as more children are born in summer months than in other months of the year. We also control for four major holidays when we observe a significant drop in the number of infants born. We suspect that the level and quality of hospital staffs and hospital care might be different on holidays. The year fixed effects control for the possible differences in health care price, resource capacity, and medical technology and knowledge over time which may be correlated with crowdedness. λ_j is a set of hospital fixed effects. Hospital fixed effects control for any hospital specific characteristic that may impact infant health. This includes variation in level of technology, training of staff, resource capacity and patient mix. Robust standard errors are clustered at the hospital level.

Table 4 reports the results of equation (2). When the number of infants born on a given day in a given hospital increases by one, hospital spending per at-

[11] However, more recent works (Dowding et. al., 1987; Gould et. al, 2003; Hamilton and Restrepo, 2003) show that difference in underlying infants' health and family background across weekend and weekday birth can account for the difference in mortality rates.

risk birth decreases by 0.41 percent. Since the average hospital charge for an at-risk birth is $64,126, a one standard deviation increase in residual crowdedness is associated with a decrease in hospital spending of $846. The first stage F-statistics is 17.58, thus we do not have a weak instrument problem.

We note that the reduction in spending does not simply reflect the fact that infants born on relatively uncrowded days have higher Caesarean section rates or longer hospital stays. To investigate the sensitivity of our results, we have estimated regressions including Caesarean section as an additional control in equation (2). The coefficient on Number of Births$_{ijt}$ is unchanged, suggesting that increased use of Caesarean section is not driving the observed increase in spending.[12] If we include length of hospital stay in equation (2), the coefficient on Number of Births$_{ijt}$ decreases by 25 percent but remains statistically significant at the one-percent level. While a quarter of the decrease in hospital charge can be attributed to longer hospital stays, the remaining 75 percent of the decrease in hospital charge is associated with more intense treatment. The first stage result provides strong evidence that health care providers change the intensity of treatment based on the short-term fluctuations in crowdedness in the hospital.

IV. The Effectiveness of the Health Care Spending on Newborn Health

Having established that hospital crowdedness is associated with lower spending, in this section, we investigate if this decrease in spending translates into worse infant health. Economic theory suggests, at the margin, the benefit of treatment will be smaller than the cost of providing care especially in the infant health setting. The infant health setting faces a combination of high prices for inputs, poorly restrained incentives for overutilization, and high incidence for malpractice

[12] We have also directly investigated if hospital crowdedness induces more Caesarean sections. The regression result (available upon request) reports statistically insignificant relationship between crowdedness and Caesarean section. Finally, our null relationship between spending and infant health persists in the sample of vaginal delivery infants.

lawsuits (Anupam, 2011). Additionally, the vast majority of births are covered by insurance which leads to inefficient overutilization due to moral hazard (Pauly, 1968).

Figure 2 provides graphical evidence that the additional spending that occurs on *slow* days does not translate into better health outcomes. Figure 2 reports the differences in one-year infant mortality rate between *busy* and *slow* day infants by the same 300 gram birth weight bins used in Figure 1. Despite the higher spending documented in Figure 1, *slow* day infants seem to, if anything, have higher mortality rates than their *busy* day counterparts. As is clear in Figure 2, across the birth weight distribution, the additional spending that *slow* day infants receive does not appear to translate into lower mortality.

To formally measure the causal effect of health care spending on infant health, we use equation (3) for the second stage estimation in our two-stage least square (2SLS) model:

$$(3) \qquad Y_{ijt} = \beta_1 \widehat{Spending}_{ijt} + X'_{ijt}\beta_2 + \lambda_j + \varphi_t + \varepsilon_{ijt}$$

where the dependent variable Y_{ijt} is an indicator of the health status of infant *i* who was born in hospital *j* at time *t*. We employ neonatal mortality, one-year mortality, and 28-day readmission rates as measures of infant health.[13] $\widehat{Spending}_{ijt}$ is predicted log of hospital charge for infant *i* from equation (2).[14] The rest of the control variables are the same as in equation (2).

OLS estimates of β_1 are likely to be biased due to unobserved variables in ε_{ijt}. The direction of bias is given by two elements: the relationship between

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[13] As a robustness check, we tried restricting the outcome variable to mortality rate from non-accidental causes only (excluding ICD-10 code 295-350). The results (available upon request) are similar to those when mortality from all causes is the outcome variable.

[14] Equation (3) is shown for conceptual purposes only. When performing our analysis, we use the ivreg2 commend in Stata.

the omitted variable and the outcome variable (Infant Health), and the relationship between the omitted variable and the variable of interest (Hospital Spending). Consider the case where unobservable parental characteristics such as cautiousness/responsibility influence infant health. If parental cautiousness correlates negatively with infant mortality or hospital readmission and positively with spending at birth (say, because cautious parents tend to have better insurance and/or demand additional or more expensive treatments at birth for their newborns), not including this variable in the equation (3) biases the OLS estimates downwards, since part of the estimated beneficial effect of spending on infant mortality or readmission can be attributed to parental cautiousness. On the other hand, less healthy infants may receive more intensive hospital treatment and have higher mortality and readmission rates biasing estimates of the returns to such care upward in situations where the medical practitioners can observe more health information than the econometrician. Since we have rich initial health measures, we expect the bias due to omitting parental characteristics will dominate and that OLS estimates will overstate the positive relationship between treatment intensity and infant health. We use hospital crowdedness to instrument for $Spending_{ijt}$ in equation (2) to address the endogeneity of health care spending.

Table 5 reports the estimated effects of hospital spending on infant health from a linear probability model where we first ignore and then address the endogeneity of health care spending. Column (i) in Table 5 reports the OLS estimates of the relationship between health care spending and the three infant health outcomes. The OLS results suggest that additional hospital spending leads to a statistically significant decrease in neonatal mortality, one-year mortality, and 28-day readmission rates. A ten percent increase in spending on at-risk infant is associated with 0.21 percentage point decrease in one-year mortality from a base of 2.7 percent, and 0.08 percentage point decrease in readmission rate from a base of 11.6 percent.

Column (ii) contains the corresponding 2SLS estimates of the impact of hospital charges on infant health. The Wu-Hausman F-tests indicate that the OLS estimates are biased towards finding a beneficial impact of hospital spending. When we use the exogenous variation in spending that arises from the relaxation of a capacity constraint, the marginal benefit of hospital spending on infant health is, at best, negligible. This is a key result of the paper. The additional spending on the infants who were born on slower days does not improve infant health as measured by neonatal mortality, one-year mortality, or 28-day readmission rates. Although statistically insignificant, the coefficients on the infant mortality measures are positive. Note that these outcomes are uncommon, making power a challenge even for at-risk infants (Evans, Garthwaite, and Wei, 2008). The result on readmission is statistically significant and implies that additional hospital spending increases the likelihood of hospital readmission. Specifically, a ten percent increase in spending on at-risk infants leads to 0.56 percentage point increase in the 28-day readmission rate. Together these results imply that, if anything, the additional health care spending harms the infants: a phenomenon known as iatrogenic harm. Numerous studies (Black, 1998; Fisher et. al., 2003; Landrum et. al., 2008; Grady and Redberg, 2010) find evidence consistent with iatrogenic harm. These studies argue that additional medical care might be harmful to patients because all treatments entail some risk. Additionally, greater use of diagnostic tests may find abnormalities which would not have caused harm and longer hospital stays increase the risk of infections.

Our IV estimates are identified off of infants who have additional charges billed solely because they were born on slow days. Given that the Table 6 reports *increased* infant mortality and hospital readmission associated with additional spending, we conclude that, on the margin, additional spending does not reduce infant mortality. In the next section we investigate the robustness of this finding.

V. Robustness Tests

a. Alternative Measures of Hospital Crowdedness

Thus far, hospital crowdedness is measured by the number of infants born on a given day in a given hospital. However, a newborn might also compete with infants born before and/or after her birth date for medical resources such as hospital beds, access to medical procedures, and hospital staff. Hence we create two alternative measures of crowdedness: Number of Infants on Delivery Date and Number of Infants during Stay. Equations (4) and (5) show the first step of how we construct these measures.

(4) Number of Infants Born Before$_{ij}$ =

$$\sum_{s=-1}^{-7} \omega 1_s \times \text{number of non\_at\_risk infants}_{ijs}$$

$$+ \sum_{r=-1}^{-14} \omega 2_r \times \text{number of at\_risk infants}_{ijr}$$

(5) Number of Infants Born After$_{ij}$ =

$$\sum_{s=1}^{7} \omega 1_s \times \text{number of non\_at\_risk infants}_{ijs}$$

$$+ \sum_{r=1}^{14} \omega 2_r \times \text{number of at\_risk infants}_{ijr}$$

Number of Infants Born Before$_{ij}$ is a weighted sum of the number of infants born in hospital *j* on the days prior to infant *i*'s birthday where the weights (ω1, ω2) correspond to fraction of non-at-risk and at-risk infant, respectively who remain in the hospital on the target infant's date of birth. Because at-risk infants have longer hospital stays, we use different lengths (s, r) and weights for at-risk infants and non-at-risk infants. For example, when s is -1, we determine the number of non-at-risk infants born one day prior to the target infant's birth date in hospital *j* and set $\omega 1_{-1}$ to 0.74, because 74 percent of non-at-risk infants remain in the hospital one day after they are born. We account for the non-at-risk births that happened up to seven days before. By the seventh day, more than 98 percent of non-at-risk infants are discharged from hospitals. We include at-risk infants born up to 14 days before the target infant's birth date.[15] We employ a similar procedure to compute Number of Infants Born After using the same lengths and weights.

Using equations (6) and (7), we then construct two alternative instruments: expected Number of Infants on Delivery Date and expected Number of Infants during Stay

$$\text{(6)} \quad \text{Number of Infants on Delivery Date}_{ijt} = \text{Number of Births}_{ij} + \text{Number of Infants Born Before}_{ij}$$

$$\text{(7)} \quad \text{Number of Infants during Stay}_{ij} = \text{Number of Births}_{ij} + \text{Number of Infants Born Before}_{ij} + \text{Number of Infants Born After}_{ij}$$

The average number of infants in the hospital on the target infant's delivery date is 23.1 and the average number of infants during the hospital stay is 35.8. We replace Number of Births_{ijt} in equation (1) with Number of Infants on Delivery Date_{ijt} and Number of Infants during Stay_{ijt}, respectively to construct two additional residual crowdedness measures. Panel B of Table 2 summarizes the additional measures of residual crowdedness from equation (1). These new residual crowdedness measures report smaller minimums, larger maximums, and wider interquartile ranges than the measure using only the crowdedness on the

[15] Sixteen percent of at-risk infants remain in the hospital for longer than 14 days. Given the small number of at-risk infants born a given day in a given hospital, extending beyond 14 days does not make a meaningful difference in the calculations.

target infant's birthday. The respective standard deviations of the new residuals are 4.85 and 6.67. When using the alternative measures of crowdedness, we will discuss the magnitude of the first stage results in the context of one standard deviation change in the new residual crowdedness measures.

Panel A in Table 6 reports the first stage regression results of equation (2) using three measures of hospital crowdedness. Column (ii) replicates the findings from Table 4 for comparison purposes. Columns (iii) and (iv) shows the results where $Crowded_{ijt}$ is measured by the expected number of infants on the target infant's delivery date and the expected number of infants during hospital stay, respectively. If newborns compete with infants born a few days before and/or after their birth date for medical resources, then these alternative measures of crowdedness should have stronger first stage results. Both additional measures of hospital crowdedness reveal a large, negative, and statistically significant relationship between hospital crowdedness and infant hospital charges. A one standard deviation decrease in either of the alternative residual crowdedness measures corresponds to an increase in spending around $1,300. Both the predicted change in hospital spending and the F-statistics are larger for the alternative measures of crowdedness suggesting that hospital treatment intensity is also influenced by crowdedness of the days surrounding the target infant's birth date.[16] Overall, the first stage results provide additional evidence that health care providers change the intensity of treatment based on short-term fluctuations in crowdedness in the hospital.

Panel B of Table 6 reports the second stage regression results using the alternative crowdedness measures. Each cell in Table 6 corresponds to a separate regression. Column (iii) shows the effect of additional healthcare spending that

[16] If we include the Number of Births, Number of Infants Born Before, and Number of Infants Born After as three separate variables in the first stage equation, we find each is statistically significant, but the coefficient on the Number of Infants Born After is the smallest.

occurs because the birth hospital was uncrowded on the day of and days prior to the delivery date. For both measures of mortality and the readmission measure, the results suggest that the additional spending slightly *worsens* infant health with the result being statistically significant at the 5% level when the outcome is one-year mortality, and at the 10% level for the other two outcomes. Specifically, a 10 percent increase in health care spending caused by a decrease in the number of infants in the hospital on the delivery date increases the one-year mortality rate by 0.31 percentage point, suggesting iatrogenic harm. Column (iv) reports the effectiveness of additional spending that stems from the variation in crowdedness over the infant's entire hospital stay. For all measures of infant health, we again find a positive relationship between additional spending and infant mortality/readmission. The coefficient estimates on one-year (consistency) mortality and 28-day hospital readmission are statistically significant at 10% level. All six Hausman tests for the alternative crowdedness measures reject the equivalence of OLS and 2SLS estimates. Overall the results using the more comprehensive measures of hospital crowdedness strengthen the main finding that additional hospital spending does not improve, and may harm, infant health.

b. Including Maternal Hospital Charges

The OSHPD records charges to the infant and charges to the mother as separate entries. Maternal hospital charges include some charges that are directly related to the labor and delivery (such as Caesarean sections). Thus infants may also receive health benefit from the spending assigned to their mothers. As a robustness check, we add to infant charges to the charges billed to the mother to examine the impact of all spending related to the delivery (delivery spending).

Panel A of Table 7 presents the impact of hospital crowdedness on delivery spending. While the first stage F-statistics are sufficiently large for all measures of hospital crowdedness, if one compares the estimated effects of a standard deviation increase in residual crowdedness on spending from Table 6 and Table 7, it is apparent that spending on the birth mother is less responsive to variation in hospital crowdedness than spending on the infant.[17] For instance, when the number of births on the target infant's birthdate is used as the crowdedness measure, the first stage results imply that a one standard deviation increase in residual crowdedness is associated with a decrease in delivery spending of a \$643. In contrast, there is an \$846 reduction in infant charges when we exclude maternal spending.

Panel B of Table 7 reports the impact of additional spending measured as sum of mother and infant charges using the three measures of crowdedness as instruments for delivery spending. Column (i) reports OLS results, which show a negative correlation between delivery spending and infant mortality/readmission. The Wu-Hausman F tests suggest that the OLS results are biased. Columns (ii)-(iv) confirm the main result that additional spending on mother and infant does not reduce neonatal mortality, one-year mortality, or readmission rates. Regardless of the measure of hospital crowdedness or the measure of infant health, we find a positive relationship between delivery spending and infant mortality/readmission.

c. The Impact of Length of Hospital Stay

Although we believe hospital spending is the best available comprehensive measure of hospital care, as an alternative measure of treatment intensity, we consider length of the hospital stay which is commonly used in the literate. As we did with hospital charges, if an infant is transferred to another California hospital, we add those days to the days spent at the hospital of birth. We examine if

[17] We also investigate if hospital crowdedness has an impact on maternal charges only. All three crowdedness measures have a statistically significant impact on maternal charges. Not surprisingly, maternal charges are most responsive to the crowdedness measure which captures only the number of births in the hospital on the target infant's birthday.

hospital crowdedness has any impact on the length of hospital stay and if longer hospital stays have a beneficial impact on infant health.

Panel A in Table 8 presents first stage results of hospital crowdedness on length of hospital stay using the three different measures of crowdedness. The first stage F-statistic is weak when we measure crowdedness using only the births that occur on the target infant's birth date. This is likely because the discharge decision for at-risk infants is made well past the birth date. Thus the length of hospital stay is not very responsive to the crowdedness on the birth date. However, the F-statistics and magnitude of the effect of a one standard increase in hospital crowdedness increase as we move to more comprehensive measures of hospital crowdedness. For instance, column (iv) reports that a one standard deviation increase in residual crowdedness over the entire hospital stay leads to a 1.32 day shorter hospital stay. Overall Panel A suggests that health care providers make discharge decisions based on the short-term fluctuations in crowdedness in the hospital.

Panel B presents OLS and 2SLS estimates of length of hospital stay (measured in days) on infant health. The OLS results, located in column (i), again suggest that additional treatment intensity, measured by length of hospital stay, is associated with a small but statistically significant reduction in infant mortality and readmission. However, the Wu-Hausman F tests suggest that the OLS results are biased towards finding a beneficial impact. Columns (ii)-(iv) report the second stage regression results on the impact of hospital stay on infant health. They are consistent with our main findings. Shorter stays that arise because of hospital crowdedness do not adversely impact either neonatal or one-year mortality. For all measures of crowdedness, we observe a positive relationship between length of stay and infant mortality. The effect is significant at 5 percent level when the predicted number of infant in the hospital on the delivery date is the instrument and one-year mortality is the outcome. An additional day in the hospital is associated with 0.61 percentage point increase in the one-year mortality rate. We further observe that longer hospital stays are associated with a marginally statistically significant increased likelihood of being readmitted to the hospital. Regardless of the measure of hospital crowdedness, infants that had longer hospital stays because they were born during less crowded periods are more likely to have subsequent hospital readmission.

VI. Discussion and Alternative Explanations

A possible explanation for the lessened treatment intensity associated with being born on a crowded days is recording bias. Specifically, hospitals may fail to record and bill all the procedures performed because the hospital personals are swarmed on busy days. If the recording bias is strong enough, infants born on slow days and busy days might receive the similar level of hospital care (although they report different levels) which would lead to a null findings of treatment on infant health in the second stage. We find no evidence of hospitals' failure to record procedures. Procedure data is available for 27.81 percent of infants born on busiest quartile days vs. 27.09 percent of infants born on slowest quartile days. We identify the four most commonly performed procedures - Circumcision (ICP9: 640), Vaccination NEC (ICP9: 9955), Insertion of Endotracheal Tube (ICP9: 9604), and Other Phototherapy (other than ultraviolet, ICP9: 9983). The difference in percentage of infant receiving each procedure across *slow* and *busy* days is smaller than 1 percentage point for all four procedures. Moreover, except Circumcision, which is more physician-intensive, the percentages of newborns reporting the other three procedures are higher among infants born on *busy* days. This suggests that hospitals are consistently recording the procedures regardless of hospital crowdedness. Additionally, as shown previously, length of the hospital stay also responds to hospital crowdedness but is unlikely to be subject to recording bias.

An explanation for the finding of iatrogenic harm is that the first stage results are being driving by teaching hospitals allowing low-skilled residents to perform extra procedures on slow days. We split the sample into teaching and non-teaching hospitals and find that if anything, the second stage coefficients of interest are larger in the non-teaching subsample.

Finally, the health effects documented above may simply be an artifact of supplier-induced demand.[18] If the additional treatments that occur on slow days are only occurring to pad the medical bills and not because they are deemed medically necessary, then one would expect little to no benefit, and possible harm. We conduct the following two exercises which suggest that the relationship between crowdedness and treatment intensity does not simply reflect supplier-induced demand and instead is consistent with the relaxation of binding capacity constraints.

First, we split the sample into deliveries that were covered by a managed care plan and those which were not. If supplier-induced demand is driving the negative relationship between crowdedness and spending, then we should expect a smaller or non-existent first-stage for those births covered by managed care. As is shown in Table 9, there is no difference in the response of hospital charges to crowdedness on the day of birth by managed care status.[19] While both samples have a sufficiently strong first stage, in neither sample do we observe positive health benefits from additional charges.

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[18] One of the features of medical market is an agency relationship between doctors and patients. Because doctors have asymmetrically more knowledge about medical care than their patients do, doctors are expected to behave as patients' agents when making treatment decisions. However, studies find that when doctors face negative income shocks, doctors may exploit the agency relationship and provide more care in order to maintain their income. See Chapter 9 of the Handbook of Health Economics (McGuire, 2000) and Gruber and Owings (1996) for reviews of the supplier induced demand literature.

[19] In results not shown that use the alternative measures of crowdedness, the range of the estimates is fairly tight. We also split the sample by not-for-profit status with the hypothesis that supplier induced demand might be larger at for-profit hospitals. We find no difference in the response of hospital charges to crowdedness by not-for-profit status.

Second, under the capacity constraint hypothesis, at low levels of crowdedness, additional births should not impact the amount of care (since the hospital still has ample capacity). At some point the hospital will exceed its normal capacity after which additional births will reduce spending per birth. Under a supplier-induced demand model, on the slowest days the hospital personnel face the largest reduction in income, as such spending should be greater at very low levels of crowdedness than at low-to-moderate levels of crowdedness.

To investigate if our results are being driving by capacity constraints, we construct crowdedness quintiles using the residual crowdedness measures from equation (1). Then we estimate equation (2) but replace the single daily crowdedness measure with five crowdedness quintiles. So the 80th decile would turn on if an infant was born on one of the ten busiest Mondays (or any other day of the week) in a given hospital in a given year. Figure 3 shows the results of this exercise graphically where we omit the slowest quintile. As shown in Figure 3, infants born on days which are below the median level of crowdedness for a given day of the week, hospital, year, and parity experience similar level of spending. However, once the hospital exceeds the median level of crowdedness, then as the hospital become more crowded, spending declines, especially for days in the busiest quintile. At-risk infants born when the hospital is most crowded experience 4 percent lower spending than infants born on the least crowded days. These findings are inconsistent with most supplier-induced demand models and suggest instead that the hospital face binding capacity constraints. In other words, the care that is cut is care that the hospital provides whenever it has the capacity to do so.

VII. Conclusions

Using the hospital level of crowdedness on an infant's birth date to generate exogenous variation in treatment intensity, we estimate the impact of additional health care measured by hospital spending on infants, delivery spending, and length of hospital stay on infant health using the universe of birth in California hospitals between the years 2002 and 2006. Our paper documents that on days when more deliveries occur, hospital respond to the short-term crowdedness by treating at-risk infant less intensively.

We identify the health consequences of hospital care on infants who received additional treatments solely because they were born on slow days when the hospital had excess capacity. We find no evidence that the additional hospital spending translates in to better infant health as measured by neonatal mortality, one-year mortality, and 28-day readmission rates, and some evidence that additional spending harms at-risk infants. These results suggest, at best, we are at the so-called "flat part of the curve" of the health production function. This is in contrast to the OLS estimates which consistently suggest that additional treatments reduce mortality and readmission. These findings are robust to alternative measures of hospital crowdedness and alternative measures of hospital treatment.

Unlike other studies that utilize variation in treatment intensity from legislative changes, the variation in treatment identified in our study stems from the medical personnel's decisions. We identify health consequences of reduction in care for infants born on busier days in the same hospital with identical health and family background characteristics. This is precisely the information from which the policy debates about staffing ratio and the number of newborn intensive care units benefit. Our findings suggest that new hospital construction, changes in staffing requirements, or reduction in birth rates that reduce crowdedness in maternity wards will lead to additional per infant hospital charges without increasing infant health status and possibly harming infant health.

Our result should be interpreted cautiously. While our measures of infant health are common in the literature and good proxies for infant health, they are not perfect measures of health status. It is possible that additional spending has benefits such as decreased levels of discomfort for infants or improved parental satisfaction that we are unable to measure. Additionally, because resources and commitment to prenatal health may differ across states and countries, our results might not be generalized beyond California. In particular, our results may not generalize to states that have certificate of need laws, which require state approval for hospital construction or expansion, in place.

Finally we note that the instrument which we introduce in this paper, hospital crowdedness, could be applied to other settings where there is little possibility of timing the onset of medical need. For example, the benefit of emergency room care for heart attack patients could be estimated by employing a hospital crowdedness measure to generate exogenous variation in access to treatment.

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Table 1. Summary Statistics

Variable	At-risk Infants Mean	At-risk Infants S.D.	All Births Mean	All Births S.D.
Pregnancy Characteristics				
Month prenatal care began	2.26	(1.49)	2.26	(1.41)
Number of prenatal visits	11.87	(5.29)	12.39	(4.05)
No pregnancy complication	0.31	(0.46)	0.34	(0.47)
Parental Characteristics				
Mother's age	28.16	(6.83)	27.92	(6.37)
Mother's education (years)	12.05	(3.41)	12.22	(3.43)
Father's age	31.02	(7.66)	30.85	(7.21)
Father's education (years)	12.01	(3.59)	12.19	(3.59)
Newborn Characteristics				
Boy	0.52	(0.50)	0.51	(0.50)
White	0.67	(0.47)	0.71	(0.45)
Black	0.10	(0.30)	0.07	(0.25)
Asian	0.08	(0.27)	0.08	(0.28)
Hispanic	0.51	(0.50)	0.52	(0.50)
First born	0.37	(0.48)	0.38	(0.49)
Single birth	0.84	(0.37)	0.97	(0.17)
Birth Characteristics				
Birth weight (g)	2561	(759)	3318	(574)
Gestation (days)	246	(31)	275	(24)
Caesarean section	0.44	(0.50)	0.31	(0.46)
Primary Payer				
Medicaid	0.53	(0.50)	0.51	(0.50)
Private insurance	0.41	(0.49)	0.44	(0.50)
Self-pay	0.02	(0.15)	0.02	(0.15)
Variables of Interest				
Infant hospital charge ($)	64,126	(193,941)	13,126	(78,972)
Mother and infant hospital charge ($)	81,326	(200,968)	26,624	(82,156)
Hospital stay (days)	10.01	(19.68)	3.38	(8.12)
Number of procedures[1]	1.08	(2.23)	0.43	(1.07)
Probability of transfer from birth hospital	0.067	(0.250)	0.017	(0.130)
Probability of the same day transfer	0.039	(0.194)	0.009	(0.094)
Outcome Variables				
Neonatal mortality	0.021	(0.145)	0.003	(0.058)
One-year mortality	0.027	(0.162)	0.005	(0.071)
28-day Readmission	0.116	(0.32)	0.031	(0.173)
Observations	302,649		2,329,810	

1. Data on procedures is available for only 28 percent of the observations.

Table 2. Descriptive Statistics of Number of Births for Selected Hospitals

	Mean (Daily Births)	Standard Deviation	Min	25th Percentile	50th Percentile	75th Percentile	Max
Panel A:							
Smallest Hospital	2.00	0.98	1	1	2	3	5
10 Percentile Hospital	4.35	1.86	1	3	4	6	10
20 Percentile Hospital	5.79	2.31	1	4	6	7	13
30 Percentile Hospital	6.94	2.58	1	5	7	9	16
40 Percentile Hospital	8.01	2.89	1	6	8	10	17
50 Percentile Hospital	8.76	3.25	1	6	9	11	20
60 Percentile Hospital	9.94	2.99	1	8	10	12	20
70 Percentile Hospital	11.57	3.66	1	9	12	13	25
80 Percentile Hospital	13.80	3.67	3	11	14	16	26
90 Percentile Hospital	17.43	4.40	3	14	17	20	31
Biggest Hospital	22.37	5.79	4	15	23	26	42
Panel B: Residual Crowdedness:							
Number of Births	0	3.21	-19.83	-2.00	-0.13	1.90	19.63
Number of Infants on Delivery Date	0	4.85	-48.25	-2.88	-0.09	2.81	42.75
Number of Infants during Stay	0	6.67	-73.2	-3.83	-0.10	3.78	45.72
Number of Hospitals	226						

Note:
The residual crowdedness measure is the respective residuals from a regression of a measure of hospital crowdedness on the complete set of hospital, year, day of the week, and parity fixed effects. We construct separate residuals for each of the three hospital crowdedness measure.

Table 3. Summary Statistics by Hospital Crowdedness

Variable	Slow Days Mean	Slow Days S.D.	Busy Days Mean	Busy Days S.D.	Difference
Number of infants born	7.89	(4.63)	15.63	(6.66)	-7.74
Transfer from birth hospital	0.064	(0.245)	0.065	(0.246)	-0.001
Same day transfer	0.037	(0.189)	0.036	(0.187)	0.001
Prenatal Care					
Month prenatal care began	2.23	(1.47)	2.24	(1.47)	-0.01
Number of prenatal visits	12.00	(5.26)	12.06	(5.32)	-0.06
Parental Characteristics					
Mother's age	28.16	(6.83)	28.26	(6.86)	-0.11
Mother's education (years)	12.03	(3.40)	12.12	(3.41)	-0.09
Father's age	31.03	(7.66)	31.09	(7.64)	-0.06
Father's education (years)	11.99	(3.58)	12.09	(3.61)	-0.11
Newborn Characteristics					
Boy	0.52	(0.50)	0.52	(0.50)	0.00
White	0.66	(0.47)	0.67	(0.47)	0.00
Black	0.10	(0.30)	0.10	(0.30)	0.00
Asian	0.08	(0.27)	0.08	(0.27)	0.00
Hispanic	0.52	(0.50)	0.52	(0.50)	0.00
First born	0.37	(0.48)	0.37	(0.48)	0.00
Primary Payer					
Medicaid	0.53	(0.50)	0.53	(0.50)	0.00
Private insurance	0.40	(0.49)	0.41	(0.49)	0.00
Self-pay	0.02	(0.15)	0.02	(0.15)	0.00
Birth Characteristics					
Caesarean section	0.44	(0.50)	0.44	(0.50)	0.00
Pregnancy complication	0.67	(0.47)	0.67	(0.47)	0.00
Birth weight (g)	2546	(768)	2553	(756)	-8
Gestation (days)	245.6	(30.0)	246.2	(31.3)	-0.6
Variables of Interest					
Infant hospital charge ($)	67,917	(201,518)	64,325	(191,285)	3,592
Mother hospital charge ($)	18,051	(23,139)	17,971	(23,252)	80
Hospital stay (days)	10.54	(20.45)	10.07	(19.57)	0.48
Number of procedures	1.15	(2.30)	1.10	(2.26)	0.05
Outcome Variables					
Neonatal mortality	0.023	(0.149)	0.020	(0.140)	0.003
One-year mortality	0.028	(0.166)	0.026	(0.158)	0.002
28-day Readmission	0.036	(0.185)	0.034	(0.182)	0.002
Observations	77,497		77,461		

Notes:
Slow/busy days are days with residual crowdedness in the bottom/top quartile of the residual distribution (see Equation (1)). Data on procedures is available for only 28 percent of the observations.

Table 4. Impact of Hospital Crowdedness on Hospital Spending for At-risk Infants

Number of Births	-0.0041
	(0.0010)***
	[$ 846]
F-Statistics	17.58
R-squared	0.5465
Observations	302,649

Notes:
The dependent variable is the log of hospital spending from all consecutive hospital stay after birth.
The regression includes indicators for: mother and father's age and education, infant's race, gender, parity, month prenatal care began, number of prenatal visits, insurance type, day of the week, month, year, holiday, birth weight categorized in 500 gram interval, and gestation in two weeks.
* denotes significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors corrected for clustering on hospital cells are reported in parentheses.
The number in bracket corresponds to the predicted change in average hospital charges from a one standard deviation change in the number of daily births after netting out parity, hospital, year, and day of the week effects (residual crowdedness).

Table 5. Impact of Hospital Spending on Infant Health

	OLS	2SLS
	(i)	(ii)
	Neonatal Mortality	
Log(Health Care Spending)	-0.0260	0.0276
	(0.0012)***	(0.0213)
Wu-Hausman F test		0.0013
	One-year Mortality	
Log(Health Care Spending)	-0.0214	0.0285
	(0.0012)***	(0.0263)
Wu-Hausman F test		0.0084
	28-day Readmission	
Log(Health Care Spending)	-0.0083	0.0557
	(0.0005)***	(0.0264)**
Wu-Hausman F test		0.0066
Observations	302,649	302,649

Notes:
Each cell represents a separate regression.
All models include the control variables listed in Table 4.
* denotes significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors corrected for clustering on hospital cells are reported in parentheses.

Table 6. Robustness of results to Alternative Measures of Hospital Crowdedness

	OLS	2SLS		
		Number of Births	Number of Infants on Delivery Date	Number of Infants during Stay
	(i)	(ii)	(iii)	(iv)
Panel A: 1st Stage Results				
Coefficient		-0.0041	-0.0042	-0.0032
Robust Standard Error		(0.0010)***	(0.0008)***	(0.0006)***
F-Statistics		17.58	25.99	25.27
R-squared		0.5465	0.5466	0.5466
		[$ 846]	[$1,306]	[$1,369]
Panel B: 2nd Stage Results				
		Neonatal Mortality		
Log(Health Care Spending)	-0.0260	0.0276	0.0242	0.0190
	(0.0012)***	(0.0213)	(0.0127)*	(0.0126)
Wu-Hausman F test		0.0013	0.0000	0.0002
		One-year Mortality		
Log(Health Care Spending)	-0.0214	0.0285	0.0306	0.0251
	(0.0012)***	(0.0263)	(0.0154)**	(0.0140)*
Wu-Hausman F test		0.0084	0.0000	0.0003
		28-day Readmission		
Log(Health Care Spending)	-0.0083	0.0557	0.0344	0.0326
	(0.0005)***	(0.0264)**	(0.0190)*	(0.0198)*
Wu-Hausman F test		0.0066	0.0156	0.0174
Observations	302,649	302,649	302,649	302,649

Notes:

Number of Births, Number of Infants on Delivery Date, and Number of Infants during Stay are the number of deliveries on target infant's birthdate, the expected number of infants in the hospital on the delivery date, and the expected number of infants during the hospitalization stay, respectively.

All models include the control variables listed in Table 4.

* denotes significant at 10%; ** significant at 5%; *** significant at 1%.

Robust standard errors corrected for clustering on hospital cells are reported in parentheses.

The number in bracket corresponds to the predicted change in average hospital charges from a one standard deviation change in the respective crowdedness measure after netting out parity, hospital, year, and day of the week effects (residual crowdedness).

Each cell in panel B represents a separate regression.

Table 7. Impact of Delivery Spending on Infant Health

	OLS	2SLS		
		Number of Births	Number of Infants on Delivery Date	Number of Infants during Stay
	(i)	(ii)	(iii)	(iv)
Panel A: 1st Stage Results				
Coefficient		-0.0025	-0.0023	-0.0018
Robust Standard Error		(0.0007)***	(0.0006)***	(0.0005)***
F-Statistics		12.98	16.82	14.93
R-squared		0.5649	0.5649	0.5649
		[$ 643]	[$ 907]	[$ 976]
Panel B: 2nd Stage Results				
		Neonatal Mortality		
Log(Health Care Spending)	-0.0411	0.0460	0.0444	0.0338
	(0.0015)***	(0.0375)	(0.0263)*	(0.0249)
Wu-Hausman F test		0.0016	0.0001	0.0004
		One-year Mortality		
Log(Health Care Spending)	-0.0333	0.0475	0.0562	0.0447
	(0.0015)***	(0.0457)	(0.0325)*	(0.0293)
Wu-Hausman F test		0.0104	0.0001	0.0005
		28-day Readmission		
Log(Health Care Spending)	-0.0100	0.0932	0.0625	0.0574
	(0.0005)***	(0.0430)**	(0.0375)*	(0.0379)
Wu-Hausman F test		0.0075	0.0199	0.0215
Observations	302,649	302,649	302,649	302,649

Notes:

Delivery spending is the sum of infant and maternal hospital charges.

Number of Births, Number of Infants on Delivery Date, and Number of Infants during Stay are the number of deliveries on target infant's birthdate, the expected number of infants in the hospital on the delivery date, and the expected number of infants during the hospitalization stay, respectively.

All models include the control variables listed in Table 4.

* denotes significant at 10%; ** significant at 5%; *** significant at 1%.

Robust standard errors corrected for clustering on hospital cells are reported in parentheses.

The number in bracket corresponds to the predicted change in average hospital charges from a one standard deviation change in the respective crowdedness measure after netting out parity, hospital, year, and day of the week effects (residual crowdedness).

Each cell in panel B represents a separate regression.

Table 8. Impact of Length of Hospital Stay on Infant Health

	OLS	2SLS		
		Number of Births	Number of Infants on Delivery Date	Number of Infants during Stay
	(i)	(ii)	(iii)	(iv)
Panel A: 1st Stage Results				
Coefficient		-0.0196	-0.0215	-0.0197
Robust S.E.		(0.0078)**	(0.0063)***	(0.0047)***
F-Statistics		6.54	11.03	17.29
R-squared		0.4736	0.4742	0.4742
		[-0.63 day]	[-1.04 day]	[-1.32 days]
Panel B: 2nd Stage Results				
			Neonatal Mortality	
Hospital Stay (1Day)	-0.0041	0.0057	0.0047	0.0031
	(0.0001)***	(0.0049)	(0.0027)*	(0.0021)
Wu-Hausman F test		0.0019	0.0000	0.0001
			One-year Mortality	
Hospital Stay (1Day)	-0.0035	0.0059	0.0061	0.0042
	(0.0001)***	(0.0059)	(0.0031)**	(0.0022)*
Wu-Hausman F test		0.0121	0.0001	0.0002
			28-day Readmission	
Hospital Stay (1Day)	-0.0007	0.0092	0.0058	0.0052
	(0.0000)***	(0.0051)*	(0.0034)*	(0.0031)*
Wu-Hausman F test		0.0109	0.0287	0.0309
Observations	302,649	302,649	302,649	302,649

Notes:
The dependent variable in Panel A is the length of the infant's first hospital stay (in days) including all transfers.
Number of Births, Number of Infants on Delivery Date, and Number of Infants during Stay are the number of deliveries on target infant's birthdate, the expected number of infants in the hospital on the delivery date, and the expected number of infants during the hospitalization stay, respectively.

All models include the control variables listed in Table 4.

* denotes significant at 10%; ** significant at 5%; *** significant at 1%.

Robust standard errors corrected for clustering on hospital cells are reported in parentheses.
The number in bracket corresponds to the predicted change in average hospital charges from a one standard deviation change in the respective crowdedness measure after netting out parity, hospital, year, and day of the week effects (residual crowdedness).

Each cell in panel B represents a separate regression.

Table 9. Managed Care vs. Traditional Insurance

	Managed Care (i)	Traditional Insurance (ii)
Panel A: 1st Stage Results		
Coefficient	-0.0040	-0.0038
Robust Standard Error	(0.0012)***	(0.0012)***
F-Statistics	10.35	10.10
R-squared	0.5463	0.5484
	[$ 818]	[$ 773]
Panel B: 2nd Stage Results		
	Neonatal Mortality	
Log(Health Care Spending)	0.0164	0.0565
	(0.0243)	(0.0420)
	One-year Mortality	
Log(Health Care Spending)	0.0062	0.0702
	(0.0281)	(0.0458)
	28-day Readmission	
Log(Health Care Spending)	0.0582	0.0650
	(0.0343)*	(0.0414)
Panel C: Summary Statistics		
Ave. Number of Births	10.50	10.51
Ave. Birth Weight	2565	2566
Ave. Gestation	246	246
Ave. Infant Charges	$ 63,553	$ 63,342
Ave. Mother and Infant Charges	$ 81,392	$ 81,210
Ave. Hospital Stay (Days)	9.95	9.92
Observations	177,045	122,074

Notes:

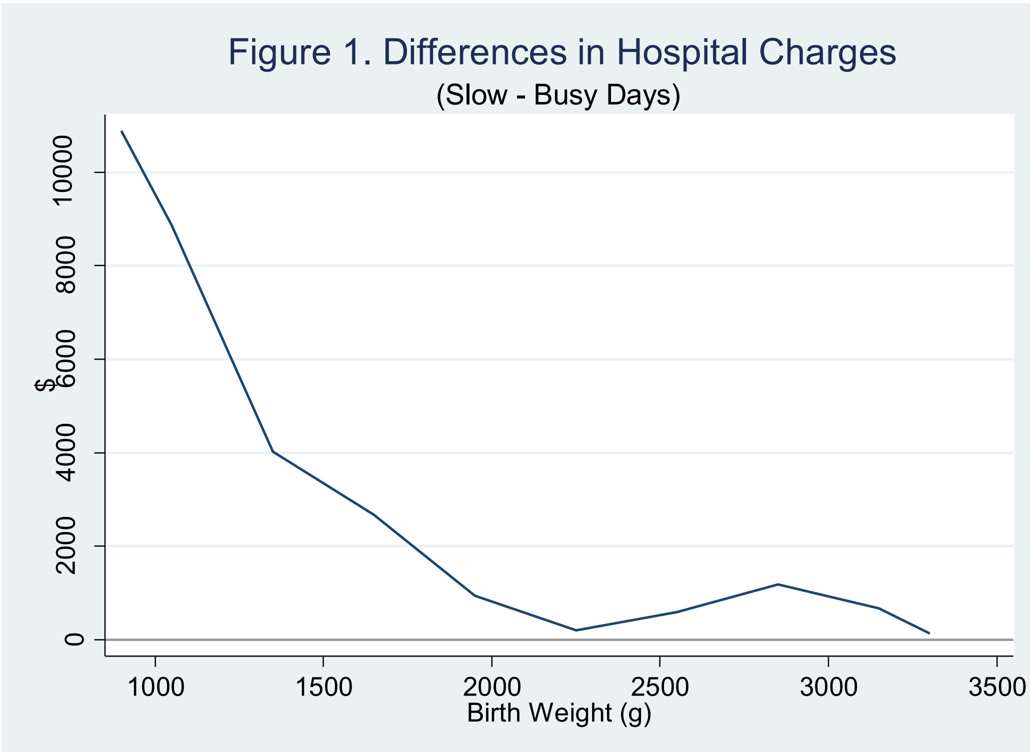
All models include the control variables listed in Table 4.

* denotes significant at 10%; ** significant at 5%; *** significant at 1%.

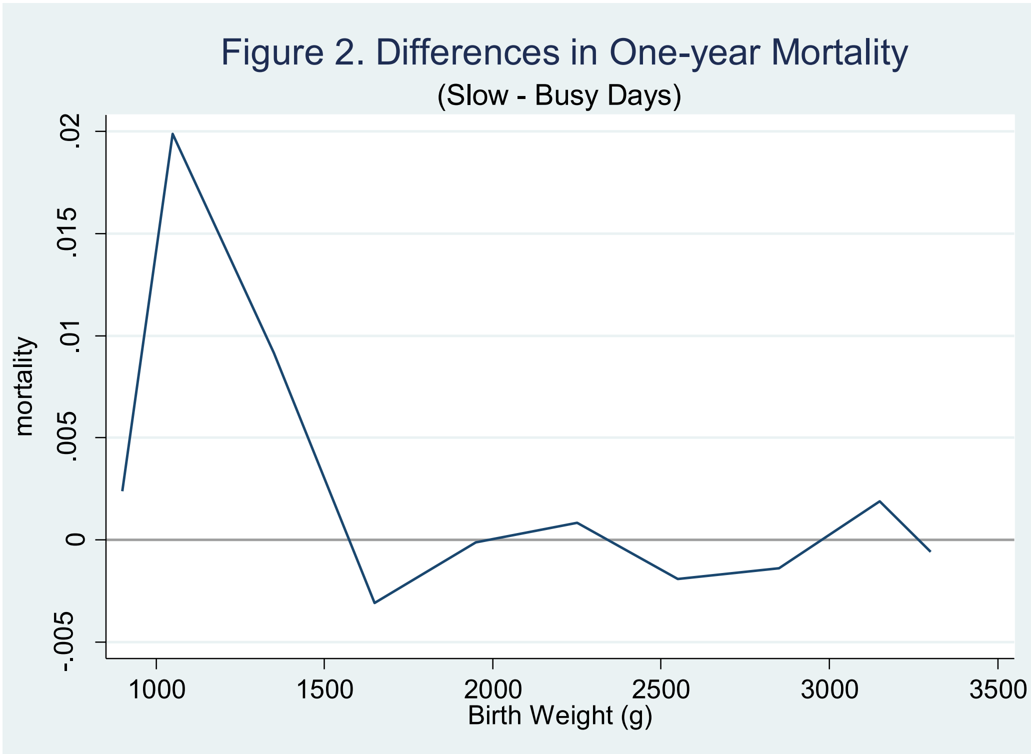
Robust standard errors corrected for clustering on hospital cells are reported in parentheses. The number in bracket corresponds to the predicted change in average hospital charges from a one standard deviation change in the number of daily births after netting out parity, hospital, year, and day of the week effects (residual crowdedness).

Each cell in panel B represents a separate regression.

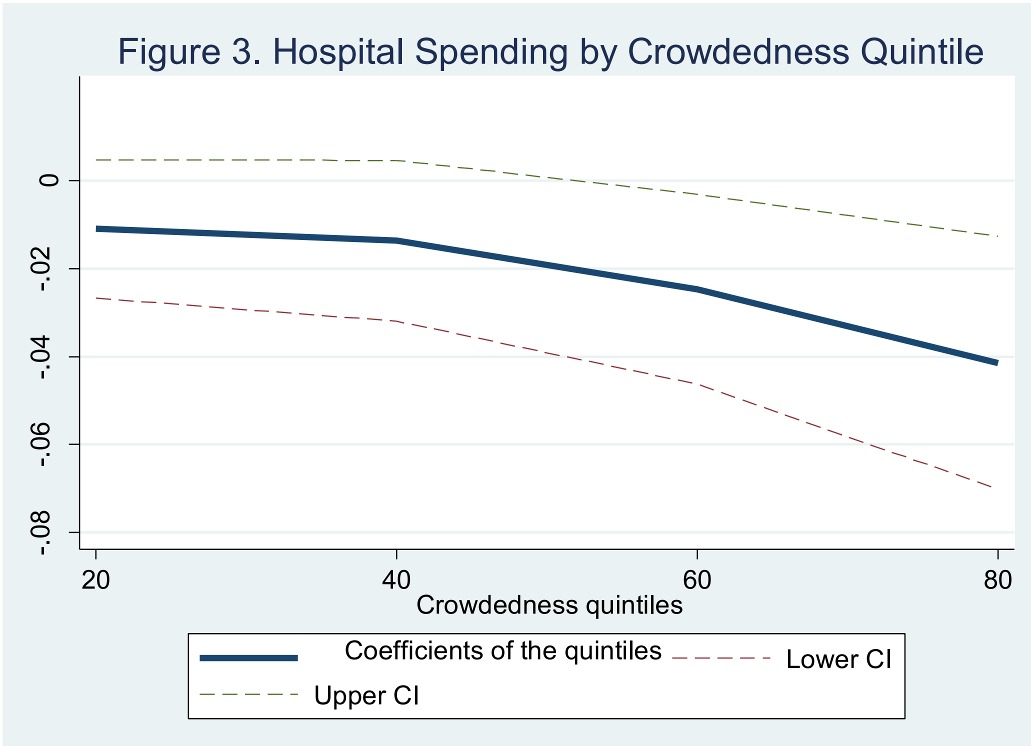
Insurance information is missing for 3,530 observations.



Note:
Each point corresponds to the difference in average hospital charges for *slow* and *busy* day births within a 300 gram birth weight bin. Births less than 900g and more than 3300g infants are grouped together. *Slow* days are days with crowdedness residuals in the bottom quartile as defined by equation (1). *Busy* days are days with crowdedness residuals in top quartile.



Note:
Each point corresponds to the difference in one-year mortality for *slow* and *busy* day births within a 300 gram birth weight bin. Births less than 900g and more than 3300g infants are grouped together.
Slow days are days with crowdedness residuals in the bottom quartile as defined by equation (1). *Busy* days are days with crowdedness residuals in top quartile.



Note: To construct Figure 3, we took the residuals from equation (1) and sorted them into five quintile crowdedness indicators from slowest to busiest. Each point corresponds to the coefficient estimate of the corresponding quintile crowdedness indicator in a regression with log hospital charges as the independent variable and full set of control variables listed in Table 4. The coefficients are relative to the bottom quintile (the slowest days according to the residual crowdedness), which is omitted. The dotted lines are 95 percentile confidence intervals.