Capacity and Utilization in Health Care:
The Effect of Empty Beds on Neonatal Intensive Care Admission*

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Abstract

The supply of neonatal intensive care units (NICUs) greatly increased over the 1980s and 1990s. This paper examines how additional availability of NICU beds impacts their utilization using hospital discharge data from California and New York. Because overall trends in utilization are jointly determined by both supply and demand, I propose a measure of short term variation in supply that is unlikely to be correlated with unobserved demand determinants. I exploit within hospital-month variation in the number of vacant NICU beds in an infant's delivery hospital the day prior to birth. In hospital specific-month fixed effect regressions, I find that on average the number of available NICU beds increases the likelihood of NICU admission. Disaggregating this effect by birth weight categories reveals that the effect is very small for very low birth weight infants (less than 1,500 grams). However, above the very low birth weight threshold, the effect of empty beds on NICU admission jumps discretely and is large for low birth weight infants (1,500 to 2,500 grams). Thus, empty beds have little effect for the sickest infants who necessitate intensive care regardless of external factors such as supply, but do change treatment patterns for infants likely to be on the margin of needing intensive care. I also provide evidence that these results are not driven by the refusal of necessary care when units are capacity constrained, but instead are likely to reflect excess utilization when excess supply is available.

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1 Introduction

Amid rising health care costs and the political debate over health reform, excessive utilization of health care is an important topic. The large amount of regional variation in health expenditures, which appears not to be correlated with health outcomes (Fisher et al., 2003*a*; Fisher et al., 2003*b*), provides some evidence that health care is consumed to the point where the marginal benefit is below the marginal cost. One concern is that the availability of supply itself directly leads to excess utilization of health resources. Theoretically, physicians and hospitals face financial incentives to provide additional care on the margin when MRI machines, catheterization labs, or hospital beds are available. Additionally, moral hazard in insurance can lead to over utilization when facilities are available and patients are insulated from the full cost of their care. However, as Fuchs (2004) points out, empirically testing the hypothesis that simply the availability of medical resources leads to excessive utilization is difficult and requires variation in supply that is uncorrelated with demand determinants.

In this paper, I examine the effect of supply on utilization in the context of neonatal intensive care units (NICUs) using hospital discharge data from California and New York. I overcome the endogeneity between supply and utilization by using short run variation in available NICU beds. I estimate the effect of the number of empty beds available in the NICU the day prior to birth on the probability that an infant is admitted to the NICU including hospital-specific month fixed effects. These fixed effects flexibly control for many unobserved factors that might be correlated with NICU utilization and allow the estimates to exploit within hospital-month variation in the availability of NICU beds. These within hospital-month shocks are unlikely to be correlated with the health of infants born the following day, and I provide empirical evidence to support this identifying assumption.

Neonatal intensive care is an important and interesting health care market to examine the effect of availability on utilization. It has been claimed that the increase in the supply of NICUs has outpaced demand, and in particular, the growth in the number of small NICUs in community hospitals has been unnecessary (e.g. Howell et al., 2002; Schwartz, 1996; Schwartz, Kellogg and Muri, 2000; Baker and Phibbs, 2002).[1] Entry into the NICU market entails high fixed costs. In a market like neonatal intensive care in which the marginal costs are low relative to the fixed costs and potentially relative to insurance reimbursements, hospitals have incentives to increase utilization to recoup these fixed costs. Beyond recouping fixed costs, in order for a NICU to directly provide revenue to the hospital and income to the physician, the beds must be utilized; therefore, in the context of this trend, there may be particularly large scope for available supply to increase utilization. Furthermore, almost all births in the United States are covered by insurance, so risk-averse parents, insulated from the full cost of their infant's care, may prefer additional care for their infant if it is available.

If the availability of neonatal intensive care directly leads to additional utilization of neonatal intensive care, there are a variety of important costs that could be incurred. First, there is the economic cost associated with using care beyond the point where the marginal benefit outweighs the marginal cost. There are also psychic costs associated with an infant being cared for in a NICU. The birth of a child is a stressful time for parents, and seeing an infant in intensive care and thinking he or she may have health problems provides additional stress and worry. Additionally, there are potentially *negative* health effects of unnecessary care in the NICU. For example, epidemiologists have documented an increasing prevalence of nosociomal, or hospital borne, infections that can lead to mortality, morbidity, and longer lengths of stay and are difficult to predict and diagnose (e.g. Clark et al., 2004; Benjamin et al., 2000; Kossoff, Buescher and Karlowicz, 1998).[2] Increased exposure to such infections could be one potential cost of spending unnecessary time in the NICU.

I find that on average, more empty beds on the day prior to an infant's birth does increase the probability of NICU admission. Disaggregating the effects by birth weight categories reveals that the effects are small for very low birth weight infants (those weighing

[1] The number of NICUs more than doubled over the 1980s and 1990s with 89% of new units in smaller, community hospitals (Baker and Phibbs, 2002). By 1995, the number of available bed-days exceded the number of medically necessary bed-days by a factor of 2.4 (Howell et al., 2002).

[2] For example, Kossoff, Buescher and Karlowicz (1998) find that the prevalence of these infections increased from 2.5 cases per 1000 admissions in 1981 to 1985 to 28.5 per 1000 in 1991 to 1995 in one particular NICU.

less than 1,500 grams). Above the very low birth weight threshold, the effect of empty beds on admission jumps discretely, and there is a large effect for low birth weight infants (those weighing between 1,500 and 2,500 grams), as high as 1.4% in CA and 1.8% in NY for each additional empty bed. While the effect size decreases for normal birth weight infants it is still large in magnitude. The effect size increases again among high birth weight infants in CA. These results suggest that empty beds have the smallest effect for the sickest infants who necessitate intensive care regardless of external factors such as supply and have the largest effect for low birth weight and high birth weight infants, two groups likely to be on the margin of needing intensive care.

It is possible that the effect of availability on utilization is at least partially driven by NICUs that are capacity constrained and must turn away patients when crowded. I cannot completely rule out this possibility, but I argue that, while this mechanism may be present, it is unlikely to be driving the result. A hospital can transfer an infant to another hospitals when its NICU is crowded and has little incentive not to do so. When I allow for the fact that infants who are not admitted to the NICU at the birth hospital may be transferred to other hospitals, I find that the effect of empty beds on utilization becomes very small for VLBW infants but shows little change for infants above the VLBW threshold. This finding suggests that VLBW infants are transferred when the NICU is crowded, but higher birth weight infants admitted to the NICU when more beds are available are likely to represent over-utilization. I also show that the effect of empty beds on NICU admission is still present when I remove from my sample very crowded days or hospitals that are often facing capacity constraints.

Finally, I show that hospital resources utilization as measured by charges and length of stay also increase with the number of empty beds available. While available capacity may affect overall resource utilization through channels other than NICU admission, this finding suggests that capacity induced admission does reflect increased costs. I do find that empty beds lead to lower mortality rates for very low and low birth weight infants, although these estimates are more tentative than the other findings of the paper since infants born on days with more available capacity may, if anything, have better unobserved health. If this were the case, it would imply that the estimates of the effect of empty beds on NICU admission and utilization are underestimates, but would lead me to overestimate the mortality benefit of available capacity.

2 Previous Literature

There are two mechanisms that may lead capacity to increase utilization. Moral hazard in insurance describes the tendency for patients to spend more on medical care when these expenditures are partially or fully paid by the insurer than they would if they were paying the full cost (Arrow, 1963; Pauly, 1968). Moral hazard may be particularly important in the case of infant care. While NICU stays are expensive, almost all child births in the United States are covered by public or private insurance.[3] In addition, parents are likely to be very risk averse with regard to their infants' health, leading them to demand even more care when the price is low. Moral hazard is a mechanism that leads individuals to consume more than the optimal amount of health care, though it does not directly mean that available supply will lead to additional utilization. However, Glazer and Rothenberg (1999) point out that it is difficult to deny care when capacity is available. Also, in the context of the utilization of neonatal intensive care, moral hazard can only occur if a bed is available for the infant. Two identical sets of parents may choose to consume additional neonatal intensive care resources because insurance provides a low price, but the behavior can only be realized when beds are available.

The second mechanism is supplier (or physician) induced demand, which occurs when the provider exploits his information advantage over the patient and provides excess treatment to increase revenue (Evans, 1974; Fuchs, 1978; Pauly, 1981). McGuire and Pauly (1991), Gruber

[3]In the dataset analyzed below, 96% of deliveries in California and 95% in New York are paid for by some form of insurance. Russell et al. (2007) report a similar percentage for infants in the 2001 National Inpatient Sample.

and Owings (1996), and McGuire (2000) formalize the idea by modeling the physician's utility function as increasing in income (which increases in the amount of care provided) and decreasing in inducement. The physician will induce demand to the point where the marginal return to inducement is equal to the marginal utility cost of inducement.[4]

Empirical studies of demand inducement have examined both income effects and substitution effects. The early literature on income effects was problematic as it looked for cross sectional relationships between the number of physicians (or physician-to-population ratios) and utilization, the idea being that when there are additional providers in a market, each individual provider's income decreases (McGuire, 2000). The most convincing study of income effects is Gruber and Owings (1996), who look at the decrease in demand associated with decreasing fertility during the 1970s. They find that a 10% decrease in fertility leads to a 0.97 percentage point increase in the rate of cesarean sections, which are more generously reimbursed than normal deliveries. Their result implies that doctors respond to the negative income shock associated with a decrease in demand by altering treatment practices to maintain income. Studies of substitution effects examine physician responses to fee differentials between complementary treatments. For example, Gruber, Kim and Mayzlin (1999) show that increases in Medicaid fee differentials between cesarean and vaginal deliveries increase the cesarean delivery rate.

Similarly to induced demand, when reimbursements are determined by groups of diagnoses, it may be the case that physicians diagnose patients with more generously reimbursed conditions. Dafny (2005) examines the effect of a policy change that leads to large changes in reimbursement rates for Medicare patients. Medicare reimbursements are based on Diagnosis Related Groups (DRGs), and she finds that in response to changes in DRG specific reimbursement rates, hospitals tend to "upcode" patients to the diagnosis codes with the

[4]Physicians may also induce if they practice defensive medicine in fear of malpractice litigation (McGuire, 2000). The empirical evidence on the importance of this concern is mixed. Kessler and McClellan (1996) find malpractice reform intended to reduce liability caused a reduction in expenditures on heart disease treatment. In contrast Baicker and Chandra (2004) find little evidence of increased utilization for states with increased malpractice costs across a variety of treatments. Kim (2006) finds that malpractice risk does not change the probability of cesarean delivery or other OB/GYN treatment decisions.

largest price increases.[5] As discussed above, Gray et al. (1996) find that many normal birth weight infants admitted to the NICU require only monitoring and no intensive treatments. Admitting these marginal infants to the NICU may provide an opportunity for hospitals to "upcode" the infants in order to receive a higher reimbursement.

Baras and Baker (2009) is the study most directly related to this paper and attempts to identify the direct causal effect of supply on utilization. They examine the effect of the availability of MRI scanners on MRI use for lower back pain, a condition for which the use of MRIs is controversial.[6] They include geographic market fixed effects to control for cross sectional differences in unobserved preferences and health and find that increases in MRI availability lead to increases in MRI usage and surgery rates.

I exploit a different sort of time-series variation in availability in the context of neonatal intensive care by utilizing hospital-specific month fixed effects to identify the effect of the number of empty NICU beds on the probability of an infant being admitted to the NICU. There are two major differences between this strategy and a strategy using geographic fixed effects to look at the effect of aggregate supply on utilization. First, I exploit variation in availability within a hospital-month pair, allowing me to control for unobserved patient preferences at a fine level. It is unlikely that changes in patient preferences within a hospital and within a month are correlated with within hospital-month changes in NICU availability. Second, the variation in availability that I exploit is not driven by the hospital's decision to offer neonatal intensive care. Instead, it is driven by the availability of NICU beds *conditional* on the hospital offering a NICU and, furthermore, the size of the NICU. As such, the variation is only driven by the health of infants born prior to a given infant.

In the context of neonatal intensive care, Profit et al. (2007) find that the probability of discharge is correlated with the NICU census (the number of patients being treated in

[5]More specifically Dafny (2005) finds that patients are more likely to be diagnosed as a case "with complications" instead of "without complications" when the reimbursement differential between the two increases.

[6]Using MRIs to diagnose lower back pain cases often detects and leads to surgery for lower back abnormalities that are not necessarily the cause of the pain (Baras and Baker, 2009).

the NICU) at the time of discharge. My paper differs by examining the decision to admit an infant to the NICU. Both margins are likely important drivers of expenditures and have different implications. If capacity affects the intensive margin through the timing of discharge and therefore length of stay, it may be the case that infants who need care are receiving more care than necessary. However, if capacity affects the extensive margin by changing who is admitted to the NICU it could impact infants who are not in need of intensive care.

3 Data

3.1 Data Sources

This paper requires data documenting individual infant hospitalizations and hospitals' NICU capacities. From California, I utilize the Office of Statewide Health Planning and Development's (OSHPD) Linked Patient Discharge Data/Birth Cohort File and State Utilization Data File of Hospitals from 1991 to 2001. The Linked Discharge Data File provides records of all California births in non-Federal hospitals in a given year. The data set links patient discharge data to vital statistics on births and infant deaths. It includes all of an infant's hospitalizations within the first year of life and links an infant's delivery, transfer, and readmission records. For each hospitalization, the data set includes information on an infant's health at birth such as gestation and birth weight; demographics such as education and race of the mother and father; and detailed information about diagnoses, treatment, charges, length of stay, and discharge status. The Utilization Data File contains annual hospital level data on capacity and utilization and includes variables indicating a hospital's annual number of NICU beds and NICU discharges.

The New York data sets are provided by the New York State Department of Health and include the Statewide Planning and Research Cooperative System (SPARCS) inpatient discharge data and Institutional Cost Reports from 1994 – 2003. I have obtained all SPARCS inpatient discharge observations for infants within their first year of life and mothers entering the hospital to give birth and can link the mother and infant observations. This data set provides similar information on hospital care to the California Linked Discharge Data File, however, it does not link to vital statistics and does not link infants over time. As such, I cannot follow an infant's transfer or readmission path and cannot identify some demographics that come from the birth certificate. I will discuss these difference in detail in Section 5 when describing the regression variables. The Institutional Cost Reports contain annual hospital level capacity and utilization data including for NICUs.

3.2 Classifying NICU Admission and Counting Empty Beds

The New York SPARCS inpatient discharge data lists each of a patients accommodations during their hospital stay, the order in which they occurred, and the length of each. Each accommodation is identified by a UB-92 Accommodation Code, and there are 6 codes for newborns that include Nursery, Nursery - Level I, Nursery - Level II, Nursery - Level III, Nursery - Level IV, and Other Nursery. Level III is labeled by the accommodation definitions as "Intermediate Care" and Level IV is labeled as "Intensive Care."[7] For ease of presentation, I consider only accommodation in Level IV as NICU admission.[8] Using each infant's admission date and accomodation information, I derive each hospital's daily NICU census by counting how many patients have a Level IV accomodation code on a given day. Empty beds are calculated for each hospital-day as the number of Neonatal Intensive Care Unit beds reported in that hospital-year's Institutional Cost Report minus the daily number of NICU occupants.

Unfortunately, the California Linked Discharge Data File does not include accommodation codes and does not otherwise identify if an infant is admitted to the NICU. For California, I thus impute whether an infant is admitted to the NICU based on a set of ob-

[7] See `http://www.health.state.ny.us/statistics/sparcs/sysdoc/appi.htm` for details.

[8] In the sample of infants born in hospitals with at least Level III accommodations, only about 1.5% of infants ever have a Level III accomodation whereas about 13% ever have a Level IV accomodation. Results of all analysis are very similar when counting Level III or IV as NICU admission and are available from the author upon request.

servables in each record. Phibbs et al. (1996) use earlier years of this same data set and a criteria based on Diagnosis Related Group (DRG) codes, birth weight, length of stay, and diagnoses to identify the population of infants most likely to have been cared for in the NICU.[9] I take guidance from their procedure along with input from a neonatologist that I interviewed in creating my NICU admission selection algorithm.

A key difference between my approach and Phibbs et al. (1996) is that, while these authors study the population of those most likely to be admitted to the NICU, I am using NICU admission itself as an outcome and using it to build my key explanatory variable. Therefore it is important for me to be more precise in assigning NICU admission. To maximize the accuracy with which I assign admission, I calibrate my approach to match the number of NICU admissions reported in the Utilization Data File for each hospital-year pair. This target number of admissions is equal to the sum of the number of NICU discharges and the number of infants transferred from the NICU to another ward within the hospital.[10] Also, because I am imputing both the dependent variable and the independent variable of interest, I must calculate them in a way that does not "assume" induced demand. Therefore, unlike Phibbs et al. (1996), I do not use variables such as diagnoses in my imputation because induced demand or upcoding motives may lead to inaccurate recording of such variables. I predominantly use measures of hospital resource utilization to impute NICU admission.

First, I divide observations into three types of records: births, transfers, and readmissions.[11] Second, I prevent NICU admission for three types of records: (1) readmission records more than 8 weeks after birth if the DRG at birth had indicated a normal newborn, (2) readmission records more than 8 weeks after birth if the most recent hospitalization was greater than 4 weeks prior to the readmission; and (3) all subsequent transfer and readmission records following these two types of records. According to the neonatologist that I

[9] The authors do not explicitly describe their criteria for choosing diagnoses that lead to NICU admission.

[10] Discharges include those who died, were transferred to another hospital, or were discharged to home.

[11] Transfers are identified as any record in which the admission source is from another acute care hospital and follows a record for the same infant in which the discharge status is to another acute care hospital. All other records that are not birth records are identified as readmission records.

interviewed, readmitted infants can be cared for in the NICU, but not if they are readmitted long after birth, particularly if they were healthy at birth. Healthy infants at birth will likely be too large for the NICU bassinets if readmitted long after birth.

All other birth, transfer, or readmission observations not described above are considered candidates for NICU admission. Phibbs et al. (1996) impute likely admission for infants with a length of stay greater than five days. I find in my data set that a threshold of 5 days is too inclusive and in many hospitals would impute admissions for more infants than my target allows. Therefore, the third step of my procedure assigns NICU admission to all infants with a length of stay greater than 10 days. This threshold still overshoots the target in some hospitals, but by far less than when using a five-day threshold. Fourth, I impute the rest of the admissions necessary to meet the target number in each hospital-year by selecting infants with the highest charges per day. NICU stays are extremely expensive, so it is very likely that the most expensive babies have accumulated their charges in the NICU.[12] 26.59% of admissions are imputed based on stays longer than 10 days. The remaining 73.41% are chosen based on charges per day.

Once admission has been imputed within my sample, I derive the daily census for each NICU by counting how many patients are present based on their hospital admission date and length of stay. It is important to note that, unlike the New York data in which I observe the exact days an infant has each accommodation code, in California I must assume an infant admitted to the NICU spends its entire hospital stay in the NICU, so I may be overestimating the number of patients in the unit on a given day. In Section 4 I discuss the ramifications of this inherent measurement error.

3.3 Analysis Samples

Appendix Table A.1 describes the construction of the analysis sample for each state. I first restrict the sample to hospital-year pairs that report a positive number of neonatal intensive

[12]Even if the infant does not receive a large amount of intensive treatment in the NICU, the per diem charge would be higher than the normal newborn nursery.

care beds and patients in the hospital level data sets. In New York I also exclude hospital-years with no infant records reporting a Level IV accommodation code.[13] Second, I eliminate hospital-years that either report zero births in the hospital level data or have no birth records present in the inpatient data.[14] I also eliminate a small number of hospital-years for which the number of births reported by the hospital level data and the number of births in the inpatient data differ by more than 10% in California and 25% in New York. Third, I eliminate hospital-years for which all patients are missing charge data in Califonra. Without data on hospital charges, I cannot assign NICU admission for infants in these hospitals.[15]

In California the sample that remains contains an average of 121.91 hospitals per year and 4,028,735 infant records of which 3,566,527 are birth records. In New York the remaining sample includes 29.8 hospitals per year, 1,015,366 total infant records, and 863,246 birth records. At this point I classify NICU admissions for all remaining observations and construct the daily empty beds measure. In California, there are some cases where many infants had a length of stay greater than 10 days and the admission imputation algorithm leads to too many admissions as compared to the target number of discharges. I drop all observations for a hospital-year in which the target number of discharges differs from the number of imputed admissions by more than 10%. This restriction only removes 1.27 hospitals per year and 1.9% of the birth observations.

Finally, I construct the analysis sample from the remaining birth records. I drop a very small number of observations in California for which the admission date or birth weight is missing. Finally, I exclude observations from 1991 in California and 1994 in New York, because I do not observe the stock of infants in a NICU at the beginning of the sample.[16] I

[13]These restrictions eliminate birth records from non-NICU hospitals, but they do not eliminate subsequent records for patients transferred to or readmitted to a NICU hospital if they were born in a non-NICU hospital.

[14]This restriction in effect eliminates children's hospitals from the sample. I am focusing on the NICU admission decision at the hospital of birth, so I do not consider children's hospitals that do not provide delivery services and only receive neonatal intensive care patients via transfer or readmission.

[15]This restriction excludes Kaiser owned hospitals because they do not report hospital charges in the data. All hospitals excluded by this restriction are in fact Kaiser hospitals. No other hospitals are missing charges for all patients. In my final sample only 1,208 or 0.04% of individual infants are missing charge data. Therefore, the results of this paper are not relevant to Kaiser owned hospitals.

[16]The 99th percentile of length of stay for NICU admitted patients is 91 days, so excluding one year of

also exclude observations from 2003 in New York because the data does not include observations on infants admitted in 2003 but discharged in 2004. The final analysis sample includes 3,131,948 birth observations from an average of 121.1 hospitals per year in California and 687,086 birth observations from an average of 29.38 hospitals per year in New York.

4 Empirical Framework

To identify the effect of NICU availability on utilization, I estimate a linear probability model where the probability of NICU admission is a function of the number of empty NICU beds the day prior to birth, observed characteristics of the infant, and fixed effects controlling for unobserved hospital differences, health trends and seasonality, and differential trends and seasonality across hospitals. I estimate the following regression equation for infant i, born at time t, in hospital h separately for each state:

$$admit_{ith} = \alpha + EmptyBeds_{t-1,h}\beta + \mathbf{X}_{ith}\mathbf{\Gamma} + \delta_{th} + \varepsilon_{ith} \tag{1}$$

$admit_{ith}$ is an indicator equal to 1 for being admitted to the NICU. In New York, this indicator is equal to one if an infant ever has a Level IV accommodation during its birth hospitalization.[17] $EmptyBeds_{t-1,h}$ is the measure of how many empty beds are available in the birth hospital's NICU the day prior to the infant's birth. I use the number of empty beds on the day prior to birth because the contemporaneous value of this variable is correlated with NICU admission by construction, as an admitted infant would be counted against the number of empty beds on its birth date. $\mathbf{X}_{ith}$ is a vector of characteristics specific to the infant which I describe in more detail in Section 5. δ_{th} are hospital-specific month fixed effects and ε_{ith} is a random error term. All standard errors are clustered at the hospital level to allow unobserved determinants of NICU admission to be correlated within hospitals but

data should be sufficient to allow the stock of patients to be accurate after one year.

[17] Results using a dependent variable of having a Level IV accommodation as the first accommodation code or within the first two accommodation codes are very similar and available from the author upon request.

maintain the assumption that they are independent across hospitals.

The hospital-specific month effects, δ_{th}, allow the unobserved probability of admission to vary for each hospital within each month. Clearly, it is desirable to control for differences across hospitals in the types of patients they attract and their treatment practices. Hospitals vary greatly in their use of neonatal intensive care. For example, in California the mean hospital has a NICU admission rate of 12.97% with a standard deviation of 9.50%. Furthermore, differences in hospital treatment styles will directly affect the dependent and independent variables. For example, a high intensity hospital will likely have a higher NICU admission rate and may operate closer to capacity. In fact, at the hospital level the correlation coefficient of the NICU admission rate and the number of empty beds faced by the average infant is -0.24 in California and -0.02 in New York. Taking scale into account, the correlation between the NICU admission rate and the percent of empty beds faced by the average infant is -0.37 and -0.29, respectively.

In addition, it is important to control for the fact that characteristics of mothers giving birth and the health of their infants are quite cyclical (Buckles and Hungerman, 2008). Figure 1 shows this seasonal relationship by plotting the fraction of births that are very low birth weight and the NICU admission rate by quarter for my two samples. There is a large amount of quarter to quarter variation in both rates, and utilization closely tracks health trends.

However, including only time dummies in the empirical model is potentially insufficient if these cycles are heterogeneous and vary across hospitals. Serial correlation in infant health within a hospital would lead to downward biased estimates of β because periods with few empty beds would also be periods with few subsequent NICU admissions. To the extent that hospital-specific month effects flexibly control for these cycles separately for each hospital, I am able to purge this unobserved correlation from the regression and exploit within hospital-month deviations from the hospital-month average number of empty beds. These short run deviations are more likely to be uncorrelated with a particular infant's unobserved health.

While this assumption is untestable in practice, I provide supportive evidence of its validity in Section 5.

While these fixed effects are necessary to identify the effect of empty beds on NICU admission, they lead to identifying the effect from a very specific source of variation – unexpected shocks to the number of empty NICU beds. For example, if hospitals decrease their overall threshold for the type of infant they admit to the NICU because they are often under capacity and, therefore, over the course of a longer period of time admit more infants due to available supply, this effect would be absorbed by the hospital-month fixed effects. However, if patients and hospitals respond to short term deviations in available capacity, they likely respond to broader variation in available capacity as well. Short term effects of capacity on utilization imply additional economic, psychic, and health costs themselves, but any potential broader effects would greatly magnify these costs.

It is also important to note that imputing NICU admission in California introduces measurement error into both the dependent and independent variables. Furthermore, the measurement error in the two variables will be correlated, but the direction of the correlation is ambiguous. On one hand, suppose over a certain period of time in a given hospital, the actual NICU patients are less sick than usual and therefore accumulate fewer charges. If my algorithm fails to assign NICU admission to some of these newborns, I would both overestimate the number of empty beds available the day before infant i's birth and underestimate NICU admission for infant i. These errors would bias the estimates of β downward. On the other hand, it may be the case that when my algorithm assigns NICU admission to too many infants on the day prior to infant i's birth date, infant i himself will be less likely to be assigned admission because there are fewer slots available for imputed admission in that hospital-year's quota. In this case, estimates of β would be biased upward. Unfortunately, there is no way of telling to what extent and in which direction measurement error occurs. To the extent that these errors are constant within a hospital-month, they would only shift the mean number of empty beds and mean admission probability in a hospital-month and be absorbed by the hospital-specific month fixed effects. These concerns are also minimized by the fact that the results presented in the next section are quite similar in California and New York where admission is observed directly.

If the number of empty NICU beds affects the NICU admission decision, the effect is likely to vary by characteristics of the infant. Presumably the care decisions of the sickest infants will be independent of excess capacity in the NICU. Infants around the margin of needing NICU care are the most likely to be admitted as a result of available beds. For this reason, I allow the effect of empty beds to differ by the baseline health of the infant. In addition to estimating Equation (1) for the full sample, I estimate it for five subsamples stratified by birth weight: very low birth weight (VLBW) infants weighing less than 1,500 grams (3.33 pounds), low birth weight (LBW) infants between 1,500 and 2,500 grams (3.33 to 5.5 pounds), two groups of normal birth weight (NBW) infants, one ranging from 2,500 to 3,250 grams (5.5 to 7.15 pounds) and the other from 3,250 to 4,000 grams (7.15 to 8.81 pounds), and high birth weight (HBW) infants above 4,000 grams (8.81 pounds). I also present results that trace out the effect more flexibly by estimating Equation (1) for subsamples stratified at 250-gram increments.[18]

After presenting estimates of the effect of empty beds on NICU admission, I also assess how empty beds impact broader measures of resource utilization and health outcomes. Ideally, it would be informative to use instrumental variables to estimate the effect of empty bed induced NICU admissions on health care costs and health outcomes. Unfortunately, this instrumental variables approach is inappropriate because empty beds may affect costs and outcomes through avenues other than NICU admission. For example, when fewer infants are in the NICU, physicians and nurses may have additional time to treat non-NICU patients, changing their costs and outcomes as well. Therefore, I estimate reduced form estimates similar to Equation (1) with other measures of hospital resource use and mortality as depen-

[18]Birth weight is the best measure of an infant's health stock at birth (Almond, Chay and Lee, 2005; Cutler and Meara, 2000) and is measured more accurately than gestation. In Section 8 I examine the robustness of my results to stratifying by gestation instead of birth weight in California.

dent variables. Though these estimates may reflect other channels besides NICU admission, they provide suggestive evidence of the effect of capacity on overall resource utilization and health outcomes. These estimates also allow me to compare the costs and benefits of being born when additional capacity is available by calculating the marginal return to capacity induced spending.

5 Results

5.1 Summary Statistics

Before presenting the estimation results, this section discusses summary statistics of the analysis sample and provides some supportive evidence for the identifying assumption. Table 1 lists sample means for the six analysis samples in each state. The differences in mean NICU admission rates by birth weight further motivate providing estimates separately for each subsample. In California (New York), while 13.5% (13.2%) of newborns are admitted to the NICU, 76.9% (85.8%) of VLBW newborns are admitted and 52.3% (53.2) of LBW newborns are admitted. This number falls to 11.4% (9.7%) for the first NBW group and 9.2% (7.2%)for the second NBW group, before rising slightly to 12.3% (11.2%) for the HBW group. The similarites in these birth weight specific admission rates across states also provides some support for the California admission imputation.

There are some differences in demographic characteristics across the birth weight samples, but for the most part these differences are not very large. The three lightest groups are more likely to be covered by Medicaid and have lower education than the two heaviest groups (note, mother's education is not available in New York). There are large differences in the fraction of infants whose mothers are black with VLBW and LBW infants having a much higher fraction than the heavier groups. On the other hand, the heavier groups have higher proportions of Hispanic mothers.[19] Demographics are different across states: California has a

[19]This is consistent with the well documented "Hispanic paradox" that Hispanics typically have lower

much higher concentration of Hispanic births and New York has a much higher concentration of black births.

There are more noticeable differences across birth weight samples in health related characteristics. Not surprisingly, infants born at lower birth weights are more likely to be multiple births. Information on prenatal care is not available in the New York data, but California mothers of lighter infants have received slightly less prenatal care; although, this difference is likely mechanical, as shorter gestational age limits the possible number of visits. This is confirmed by the fact that there are are very small differences in the month prenatal care began across birth weight samples. Heavier infants are less likely to have congenital anomalies, less likely to be diagnosed with a clinical condition[20] (except for HBW infants) and have longer gestation. Overall, these health characteristics are similar across California and New York.

Table 2 provides summary statistics of the NICU environment on the day prior to birth for the full sample of newborns in each state. On average, newborns in California (New York) are born in a hospital with 21.469 (26.953) NICU beds, though this varies widely, as the standard deviation is 17.278 (13.668) beds. On average, there are 1.895 (9.889) empty beds available in the NICU, and the standard deviation is 9.049 (9.889). While these numbers give a sense of the baseline NICU environment, the identification strategy is based on variation after partialing out the hospital-specific month fixed effects. For each state, the third row of Table 2 summarizes the variation in the residuals from a regression of the number of empty beds on these fixed effects. In other words, it summarizes how the number of empty beds deviates from the within hospital-month mean number of empty beds. By construction the mean of this variable is zero. The standard deviation is 3.105 in California and 2.757 in New York. At the 25th percentile California newborns face 1.681 less empty beds than the hospital-month average, and at the 75th percentile they face 1.696 more beds than the

socioeconomic status but better health outcomes.

[20]Clinical conditions include hydrops due to isoimmunization, hemolytic disorders, fetal distress, fetus affected by maternal condition, oligohydramnios, other high-risk maternal conditions, placenta hemorrhage, premature rupture of membrane, and prolapsed cord as defined in Phibbs et al. (2007).

hospital-month average, while this range is -1.488 to 1.516 in New York. When discussing estimation results, I will refer to these measures in order to discuss the magnitudes.[21] I will discuss my results in the context of changing the number of empty beds by 3 since it is approximately equal to the standard deviation and the difference between the 75th and 25th percentiles of residual empty beds.

The identifying assumption for my framework to estimate the causal effect of the number of empty beds on NICU admission is that unobserved within hospital-month deviations in admission probability are uncorrelated with unobserved within hospital-month deviations in the number of empty beds. Table 3 provides supportive evidence of this claim by comparing observable characteristics by the number of empty beds available. For each state, this table divides observations by whether the residual number of empty beds the day prior to birth is above or below the median. For simplicity, I present means of the observable characteristics without partialing out the fixed effects. There is some evidence that infants born on days with above median residual empty beds are less healthy than those born on below median days, but these differences are quite small. For example, they are slightly more likely to be multiple births.[22] Otherwise, there are little to no differences in demographic, pregnancy, or infant characteristics on days with above or below median residual empty beds for both states, suggesting unobserved determinants of NICU admission are likely not associated with residual empty beds. The NICU admission probability is higher for infants born on days with higher residual empty beds, providing preliminary evidence on the effect of empty beds on admission.

5.2 The Effect of Empty Beds on NICU Admission

In this section, I discuss the regression estimates of the effect of empty beds on NICU admission controlling for various observed characteristics and hospital-specific month fixed

[21]While not shown here, these measures of variation are similar for each birth weight sample.

[22]If it is the case that, even conditional on hospital-month fixed effects, infants born on crowded days are slightly less healthy, this would work against finding a positive effect of empty beds on NICU admission.

effects as described by Equation (1).

The main regression results are presented in Table 4 where each row lists coefficient estimates for a different birth weight sample and Panels A and B report the results for California and New York, respectively. For reference, Column 1 of each panel repeats the mean NICU admission rate and the number of observations for each sample. I first discuss the results for the California sample and then discuss the minor differences between these results and those from the New York sample. Column 2 presents estimates with no controls included beyond the hospital-month fixed effects. For all six samples, the coefficient estimates are positive and precisely estimated. The only control variables that appreciably impact any of the coefficient estimates are the birth weight dummies added in Column 2. These dummies decrease the size of the coefficients for the full, VLBW, and LBW samples. However, after adding birth weight controls, the coefficient estimates are quite insensitive to the addition of day of week dummies, demographic characteristics, pregnancy characteristics, and infant characteristics in Column 3.[23] If there are any differences in health characteristics associated with empty beds, they appear to be fully accounted for by including birth weight controls, and the stability of these coefficient estimates to the addition of all other controls further supports the evidence presented above that empty beds are not correlated with observed characteristics after conditioning on hospital-specific time effects.[24]

Focusing on the main results with all controls included in Column 4, an additional empty bed leads to a 0.15 percentage point increase in the probability of NICU admission. Relative

[23]Demographic characteristics include mother's age, mother's age squared, education indicators in California only (some college, college degree, more than a college degree), insurance status indicators (Medicaid, managed care, and self pay in California; Medicaid, Medicaid HMO, other government, HMO, and Blue Cross in New York), and race and ethnicity indicators (black, other race, and Hispanic). Pregnancy characteristics include sex, parity (CA only), a multiple birth indicator, month prenatal care began (CA only), and number of prenatal care visits (CA only). Infant characteristics include indicators for having a congenital anomaly, a clinical condition, being small for gestational age, and being large for gestational age.

[24]Though not reported in the table, regression estimates that control for whether or not the infant is delivered by cesarean section are identical to those in Column 4. While a cesarean section is an important risk factor, I prefer not to include it in the regressions. Since it is a treatment decision, it is potentially endogenous to the number of empty beds, as the number of empty NICU beds may weigh into a physicians decision on if and when to schedule a cesarean delivery. Below I examine the robustness of my results to excluding infants delivered by cesarean section from the sample.

to the overall mean rate of admission, this represents an effect of 1.09% as reported in Column 5. However, there is important heterogeneity in this effect. The coefficient estimates are highest for the VLBW and LBW samples (0.31 and 0.49 percentage points, respectively) and lower for the two NBW samples, before increasing slightly for the HBW sample (0.14, 0.09, an 0.16 percentage points, respectively). These magnitudes are difficult to compare because of the large differences in admission rates by birth weight. Therefore, in Column 5 I compare the results relative to mean admission probabilities for each sample. Here the relative effect is actually smallest for VLBW infants at 0.40%. This effect increases to 0.94% for LBW infants, 1.19% and 0.97% for the two NBW groups, and 1.32% for the HBW group.

The results for New York presented in Panel B are quite similar to the results in California, with two important exceptions. Once controls are included, the New York VLBW coefficient is not statistically significant and about a third of the magnitude of the California VLBW coefficient. While there is a small effect of empty beds on admission for VLBW infants in California, this effect is not present in New York. Second, The coefficient for the second group of NBW infants is not statistically significant in New York, suggesting that this group of healthy infants does not receive additional care when the NICU has available capacity.

As expected, the smallest relative effects are among the VLBW infants. To get a better sense of the magnitude of these effects, it is useful to scale them by a measure of the actual variation in the number of empty beds, again focusing on the California results. As discussed above, the standard deviation of the residual number of empty beds and the difference between the 25th and 75th percentiles of these residuals are around 3. So, even for three bed change, the VLBW estimate implies an effect of only 1.2%. While it appears that the number of empty beds impacts the probability that VLBW infants are admitted to the NICU in California, the effect seems quite small for this group, which is the group one would expect the smallest impact of external factors on treatment choices. A three bed change in the number of empty beds leads to an increase in the NICU admission probability by 2.82%, 3.57%, 2.91% and 3.96% for LBW, NBW1, NBW2, and HBW infants respectively.

To further disaggregate the effect I estimate Equation (1) for subsamples at 250-gram birth weight increments. The coefficient estimates and 95% confidence intervals are plotted by birth weight in Figure 2a, and the percentage effects relative to each subsample's mean NICU admission probability are plotted in Figure 2b, with California in black and New York in gray. In these figures, the birth weight along the horizontal axis represents the upper bound of each subsample. For all subsamples in California, the coefficient estimates are positive, and except for the 750 to 1,000 and 1,250 to 1,500 subgroups, they are all statistically significant at the 5% level. The pattern is similar in New York; although, the estimates are less precise, likely due to the smaller sample size.

The relative and absolute effects are flat and small for infants below 1,500 grams. Interestingly, there is then a discrete increase moving from just below to just above 1,500 grams. While the absolute effect declines through the LBW range, the relative effect, which takes into account the declining mean admission rate through this range, rises to a peak at 2,500 grams in California and 2,250 grams in New York. The relative effect then declines slightly through the NBW range. Finally, the relative effect size begins to climb again through the HBW range – those above 4,000 grams – in California, but becomes very noisy in New York. These patterns are consistent with those discussed above in the more aggregated estimates, but plotting the effects for narrower birth weight groups makes clear that the effect of empty beds on NICU admission is quite small for the least healthy infants as measured by VLBW, discretely jumps above this threshold and climbs through the group of LBW infants who are likely on the margin of needing NICU care, decreases for the most healthy NBW infants, and, at least in California, increases again for another potentially marginal group of HBW infants.

The finding that the effect of empty beds on NICU admission probability increases from just below to just above the VLBW threshold is interesting in the context of Almond et al. (2010). These authors use the VLBW "rule of thumb" to identify the effect of additional treatment on mortality. They find discretely higher charges but lower mortality rates for infants just below this threshold. It is interesting that this rule of thumb also seems to affect how physicians respond to empty beds. Below the rule of thumb, there appears to be little room for judgment, and empty beds have little effect on NICU admission. Above the rule of thumb, there appears to be more room for external factors to impact admission decisions.

5.3 The Mitigating Effects of Inter-Hospital Transfer

A positive effect of empty NICU beds on NICU admission does not necessarily imply that excess capacity leads to excessive utilization of NICUs. It may be the case that this effect is at least partially driven by infants being denied NICU care when a NICU is full and therefore capacity constrained. However, infants who need neonatal intensive care are often transferred to other hospitals. 19% (7%) of California (New York) VLBW infants are transferred from their birth hospital to another hospital and 8% (1.7%) are transferred on the day of birth. If infants are more likely to be transferred when the NICU at the birth hospital is crowded, I will overestimate the effect of the number of empty beds on NICU admission as these transferred infants (and eventual NICU patients) will be considered to not be admitted. To understand the extent to which this occurs, Table 5 provides estimates of the effect of empty beds on an indicator for whether or not an infant is transferred to another hospital for both states.

In California the number of empty NICU beds has a negative and statistically significant impact on the probability of ever being transferred (Column 1) and being transferred on the first day (Column 2) for all subsamples except HBW infants. However, the effect size is extremely small in magnitude for infants above the low birth weight threshold.[25] In New York, empty beds only leads to a statistically significant decrease in transfer rates for VLBW and LBW infants.

For California VLBW infants, transfers mitigate a large portion of the effect of empty beds on NICU admissions. To show this directly, Columns 3 and 4 show estimates of the

[25]This is consistent with the fact that heavier infants have mean transfer rates under 1% and transfer rates on the first day of less than 0.3%.

effect of empty beds on an indicator variable that is equal to one if the infant is admitted to the NICU *or* transferred. Compared to the baseline estimates in the last column of table 4 the effect is cut by one third for VLBW infants from 0.31 percentage points to 0.20 percentage points. Transfers also have a small mitigating effect for LBW infants, decreasing the effect from 0.49 percentage points to 0.45 percentage points. Not surprisingly, transfers do not mitigate the effect of empty beds for heavier infants. In New York, where empty beds does not impact admission probabilities for VLBW infants, transfers do not change the results. These results suggest that the effect of empty beds on the ultimate treatment received by the sickest infants is negligible, while healthier infants who are more likely to be on the margin of needing intensive care are impacted more. Furthermore, it suggests that if hospitals transfer infants when it is medically necessary, much of the effect of empty beds on NICU admission for infants above the VLBW threshold is likely due to excessive utilization of services as opposed to binding capacity constraints. I will further explore capacity constraints in the Robustness section below.

5.4 Hospital Level Heterogeneity

To this point, I have examined the effect of the number of empty beds on NICU admission separately by birth weight. However, incentives to make treatment decisions based on available capacity may be heterogeneous along many other dimensions in addition to birth weight. In this section I explore how the effect of empty beds on NICU admission differs by hospital characteristics as well.

I first separate hospitals by NICU size. Examining effects by NICU size is interesting considering the fact that much of the recent NICU diffusion has been among smaller, less sophisticated NICUs. Additionally, while hospital-specific month fixed effects inherently control for the NICU size, they do not account for the fact that, for example, NICUs with 10 beds only have scope to deviate from their mean by a small number of beds, whereas NICUs with 50 beds can have much wider deviations from their means. Not surprisingly, the standard deviation of residual empty beds from a regression of empty beds on the hospital-specific month effects is 2.068 for the sample of California infants born in hospitals with less than 20 NICU beds and 3.744 in hospitals with more than 20 NICU beds. In Columns 1 and 2 of Table 6 I separate hospitals by NICU size and present estimates for infants born in hospitals with less than 20 NICU beds and greater than or equal to 20 NICU beds. Again, Panel A presents results from California and Panel B presents results from New York.

In California, the results indicate that empty beds have a higher effect on NICU admission in hospitals with smaller NICUs across all birth weights. In New York, however, smaller NICUs respond more strongly to empty beds for LBW infants only. In fact, for the two heaviest groups, larger NICUs respond more strongly to empty beds.

Table 6 also shows results by a hospital characteristic that may directly affect financial incentives: ownership status. Duggan (2000) shows that privately owned not-for-profit hospitals are similarly responsive to financial incentives compared to for-profit hospitals, while government owned hospitals are less responsive, and Duggan (2002) finds that not-for-profit hospitals behave more similarly to for-profit hospitals when they compete more directly with other for-profit hospitals. On the other hand Dafny (2005) finds that for-profit hospitals are more likely to upcode patients to "with complication" DRGs when price differences between DRGs increase.

Columns 3-5 show the effect of empty beds on NICU admission for government owned, privately owned not-for-profit, and for-profit hospitals, respectively. Note that in New York there are an insufficient number of for-profit hospitals in the sample to obtain estimates for this group. In California, the effects are positive and statistically significant for all three hospital types in all birth weight subsamples except for VLBW infants in for-profit hospitals, suggesting that hospitals with all three ownership structures respond to available capacity. The point estimates are generally largest for infants born in for-profit hospitals, although this is not true for VLBW infants where the effect is not statistically significant in for-profit hospitals and is largest in not-for-profit hospitals. Generally these results suggest that empty beds lead to additional NICU utilization for all three ownership types, but consistent with Dafny (2005) the effect is strongest in for-profit hospitals in California. In New York, the overall effect is actually slightly larger in Government owned hospitals, and this is being driven by NBW infants.

5.5 Individual Level Heterogeneity

I also examine the effect of empty beds on admission probability separately by individual characteristics. Table 7 presents results with the baseline results repeated in Column 1. Column 2 shows results for Hispanic infants. In California the estimates for infants of Hispanic mothers are very similar to the baseline samples except for a smaller and not statistically significant estimate for VLBW infants. In New York, some of the estimates for the Hispanic sub groups are quite noisy, but overall are similar to the baseline estimates.

Results for infants with black mothers are presented in Column 3. These point estimates reveal that the effect of empty beds on NICU admission is higher for VLBW black infants than the VLBW baseline, lower for LBW black infants relative to the LBW baseline, and very similar for black infants and the baseline at higher birth weights. One possible explanation for the very high point estimate for VLBW black infants is that these infants are more prone to face capacity constraints that prevent them from receiving treatment; however, the estimate (not shown here) for VLBW black infants when considering the outcome of NICU admission or transfer is still over twice the size of the baseline point estimate. In New York, the estimates are similar to the baseline but slightly stronger for black infants in the two heaviest groups.

Column 3 presents results for samples of infants whose mothers have no college education. Mother's education is not available for New York, so only California results are shown. If non-college educated individuals show a larger effect, it may be the case that induced demand is stronger for less well informed patients. On the other hand, if the effect is smaller, it could suggest higher educated individuals are more risk averse. The point estimate suggest a slightly higher effect of empty beds on NICU admission for VLBW infants whose mothers have no college education, but no differences at the other birth weights.

Columns 5 through 7 of Table 7 considers that there may be direct differences in incentives by insurance status. Among the privately insured, managed care organizations provide fewer financial incentives that are directly tied to the type of treatment administered and provide tighter monitoring of behaviors that might be considered moral hazard. In fact, managed care has been shown to slow the adoption of neonatal intensive care units (Baker and Phibbs, 2002). However, anecdotally, managed care has been hesitant to limit reimbursement of infant care (Horwitz, 2005, see online appendix), so it is an empirical question whether the effect of supply on utilization differs between these two groups of patients. Almost all births that are not covered by private insurance are covered by Medicaid. While Medicaid typically does not reimburse as generously as private insurance, there is still scope for induced demand and moral hazard in this population.

The results in Columns 5 through 7 for California reveal little difference in the point estimates between private non-managed care patients and managed care patients except for VLBW infants where there is a larger effect for managed care patients. This result is in line with the fact that managed care may be hesitant to limit infant health care. As a caveat, because they do not report hospital charges, my sample excludes all Kaiser hospitals in California. Kaiser likely provides the weakest scope for induced demand and moral hazard, but unfortunately, I cannot test the effect of empty beds on NICU admission for this strongest form of managed care. That said, the New York data defines HMO as an insurance status instead of managed care more broadly. Column 6 of Panel B shows that there is little to know response to empty beds among HMO covered infants in New York. This suggests that in New York HMO insurers provide stronger control over utilization.

The results for Medicaid patients are similar to privately insured patients except for a slightly higher point estimate among VLBW Medicaid patients relative to VLBW privately insured patients in California and a lower point estimate for LBW Medicaid infants in New

York. Even if Medicaid reimburses neonatal intensive care less generously than private insurance companies, the availability of empty beds still increases the probability of NICU admission for Medicaid patients.

5.6 Robustness

I now discuss the robustness of my results to various specification and sample considerations. First, I estimate my regressions by gestational age subgroups instead of birth weight in California[26] to ensure that measuring health by gestation leads to similar conclusions. The first panel of Figure A.1 plots coefficient estimates and 95% confidence intervals for subsamples at one-week of gestation intervals. The second panel plots these coefficient estimates relative to the mean NICU admission probability in each one-week subgroup. While not as distinct as with the birth weight specifications, a similar pattern occurs. The coefficient estimates are all positive and most are statistically significant at the 5% level. The relative effects are small for infants with low gestational ages. Gestation of less than 32 weeks is considered very preterm, 32-36 weeks moderately preterm, and 37 weeks or higher term. The effect size increases substantially between 33 and 34 weeks – in the middle of the moderately preterm range. After this threshold, the pattern is a bit noisy, but relatively flat until increasing for infants with long gestational ages.

Results of various other robustness checks are presented in Table 8, with the baseline results from Table 4 repeated in Column 1. As discussed above, adding birth weight dummies to the regression of NICU admission on the empty beds variable and the hospital-specific month fixed effects changed the empty beds coefficient. Here I examine whether including more specific birth weight controls impacts the results. Column 2 of each panel of Table 8 presents results including birth weight dummies in 50-gram increments instead of the relatively crude 250-gram increments. For all of the birth weight subgroups in both states, results are almost identical whether I include birth weight controls in 50 or 250-gram increments.

[26]Gestational age is reported in the birth certificate, so it is not available in the New York SPARCS hospital discharge data.

One potential behavior on the part of hospitals and physicians that could bias my results is the ability to time a delivery through either delaying or inducing labor or scheduling cesarean sections. If physicians attempt to deliver high risk infants when NICUs are less crowded, my estimates would be biased towards finding a positive effect of empty beds on NICU admission. To consider this, Column 3 presents results excluding cesarean deliveries, one group for which timing of birth may be endogenous. The results are quite similar to the baseline estimates for both states, implying potentially planned cesareans do not bias the results.

Finally, I restrict the sample to hospitals operating below their capacity constraints as proxied by their number of NICU beds. In Column 4 I drop all observations on days when the NICU is less than 5% empty. In Column 5 I drop all observations for hospital-year pairs that appear to frequently operate at or above capacity; I exclude hospital-year pairs in which the number of NICU occupants exceeded NICU beds for more than 60% of the year.[27] These restrictions have little effect on the point estimates in either state. This finding again suggests that the results are not being driven by the denial of neonatal intensive care when NICUs are crowded.

5.7 Additional Measures of Utilization

Results to this point suggest that additional availability of NICU beds increases the probability of being admitted to the NICU. Here I assess whether or not additional NICU stays reflect actual changes in medical utilization. As discussed in Section 4 it is likely inappropriate to estimate the effect of NICU admission on costs with empty beds as an instrument, because

[27]In California the assumption that infants are admitted to the NICU immediately when born and leave the NICU at discharge, can directly lead me to overestimate the number of infants in the NICU on a given day. However, this assumption is likely not driving this large number of days over capacity. The Utilization Data File also lists the number of total days infants spent in the NICU for a given year. When dividing this number by the number of bed-days available (the number of beds times the number of days they are available to be used), I find that the utilization data also indicate that NICUs are operating over capacity, with bed-days used exceeding bed-days available in 24% of hospital year observations. Also, the ratio of utilized bed-days to available bed-days is highly correlated with the percentage of days a hospital is over capacity under my admission algorithm. The correlation coefficient between these two measures is 0.69.

available capacity may affect costs through avenues other than NICU admission. Therefore, Table 9 presents reduced form estimates of the effect of empty beds on length of stay and hospital charges. Though these estimates may reflect other channels besides NICU admission, they provide suggestive evidence of the effect of capacity on overall resource utilization. The results for all three outcomes are very similar across states.

Overall, infants born on days with additional empty beds spend about 0.02 additional days in the hospital and incur about $75 of additional charges. The absolute effects are largest for VLBW and LBW infants who accumulate about $1,000 and between $225 and $320 in additional charges for each empty bed, respectively. Relative effects using the log of length of stay and charges are higher for LBW infants than for NBW infants, and the least for the heaviest group of NBW infants. VLBW infants born on days with more empty beds do see relative increases in length of stay and charges, suggesting that at least part of this effect may working through channels other than NICU admission. That said, these results provide suggestive evidence that, in addition to a higher likelihood of NICU admission, infants born on less crowded days have higher costs and longer hospital stays than equivalent infants born on more crowded days.

The results to this point suggest that empty beds lead to additional utilization of the NICU and hospital resources and these results are not driven by foregone care when hospitals are capacity constrained. The predicted effect of this additional care on health outcomes is ambiguous. It could be the case that additional NICU care is health improving, or it could be detrimental to health if it exposes infants to unnecessary treatment with potential side effects or hospital-born infections. If additional NICU admissions are used only to receive increased reimbursement and do not materially change the actual care provided, they may have no impact on health outcomes.

I provide suggestive evidence on the health effects of empty beds using the California data, which also links the patient discharge records to vital statistics data on infant deaths within a year of life. Using this linkage, I estimate reduced form regressions of empty beds on a one-year mortality indicator for each birth weight subsample. Mortality is a very low probability event for normal birth weight infants, and not surprisingly, I find no statistically significant effect of empty beds on mortality for any of the normal or high birth weight subsamples.[28] I do however find that each additional bed reduces mortality by 0.04 percentage points for LBW infants and by 0.16 percentage points for VLBW infants. These results should be interpreted with caution. If anything, infants born on days with more empty beds may be healthier than those born on crowded days. These unobserved differences would bias NICU admission and resource utilization coefficients downwards, suggesting these estimates may be lower bounds. However, these differences would also bias mortality estimates downwards, suggesting that mortality improvements due to empty beds may be overestimates of the true health effects.

With this caveat in mind, I also estimate the effect of charges on mortality, instrumenting for charges with empty beds. This allows me use variation in empty beds to calculate the cost of a life saved. This is similar to Almond et al. (2010), who use the discontinuity in costs and mortality just around the VLBW threshold to estimate the marginal return to spending and calculate the cost per life saved. For this calculation, I convert hospital charges to costs using cost to charge ratios constructed from California Annual Hospital Financial Disclosure Data.[29] I estimate the cost of a life saved for the VLBW sample as $295,672 and for the LBW sample as $406,948. These estimates are smaller than Almond et al. (2010)'s estimate of about $600,000. They are also much lower than the quality adjusted value of a life for low birth weight infants of approximately $2.7 million calculated by Cutler and Meara (2000). However, because capacity increases utilization for normal birth weight infants as well with no effect on mortality, the cost of a life saved over the entire sample is $906,300. This is still much lower than the value of a life, but would be even lower if capacity did not

[28] I also find no effect on hospital readmission, which occurs more frequently for these samples

[29] The information to calculate these ratios is only available beginning in 1995, so these calculations are restricted to this sample. Mortality and charges regressions for this sample with cost information are similar to the full sample. Also, while estimates of the effect of empty beds on costs are smaller than for charges due to this deflation, the relative effects are similar.

lead to additional utilization for higher birth weight infants. Again, these estimates must be interpreted with caution. If infants born on days with more empty beds are actually healthier than those born on more crowded days, the true increase in costs would be larger and the true decrease in mortality smaller, leading to a larger cost per life saved.

6 Conclusion

The effect of the availability of medical resources on their rate of utilization is a difficult to identify parameter. This paper examines this question in the context of neonatal intensive care, an important and interesting setting due to the increase in the number of hospitals offering NICUs. To identify the effect of availability on utilization, I estimate the effect of the number of empty beds available in the NICU the day prior to an infant's birth on the probability that the infant is admitted to the NICU using inpatient data from California and New York. Including hospital-specific monthly fixed effects in my regressions allows me to exploit within hospital-month variation in NICU availability. I therefore flexibly control for factors correlated with an individual's choice of hospital, hospital treatment style, cyclicality of infant health, and cyclicality of infant health specific to each hospital.

I find that, on average, an increase in the number of empty NICU beds of 3 (an approximately one standard deviation change) increases the probability of being admitted to the NICU by about 3.25% in both states. The magnitude of the effect is small for VLBW infants in California and not statistically signficiant in New York. Allowing for the fact that many VLBW infants are transferred on their first day in the hospital further mitigates the effect for this group in California. The effect size then jumps discretely after this VLBW threshold is crossed. The effect appears to be the largest for the heaviest of the LBW group in both states and for HBW infants in California, two groups likely on the margin of needing and not needing neonatal intensive care.

These estimates suggest that the availability of neonatal intensive care beds leads to additional neonatal intensive care utilization. In the context of the diffusion of neonatal intensive care, the finding of a measurable effect of hospital-month deviations in empty beds on NICU admission, suggests that there is likely scope for supply to lead to additional NICU utilization in general. I also provide evidence that the estimates are not being driven by crowded NICUs denying care. I provide suggestive evidence of increased length of stay and charges for infants born on days when the NICU is less crowded. While I do find that low and very low birth weight infants born on less crowded days have lower mortality rates, these estimates could be biased towards finding an effect if infants born on less crowded days have better unobserved health. Future research is warranted to better understand the economic costs and the health effects of being admitted to the NICU because empty beds were available and the relationship between capacity and utilization in other health care settings

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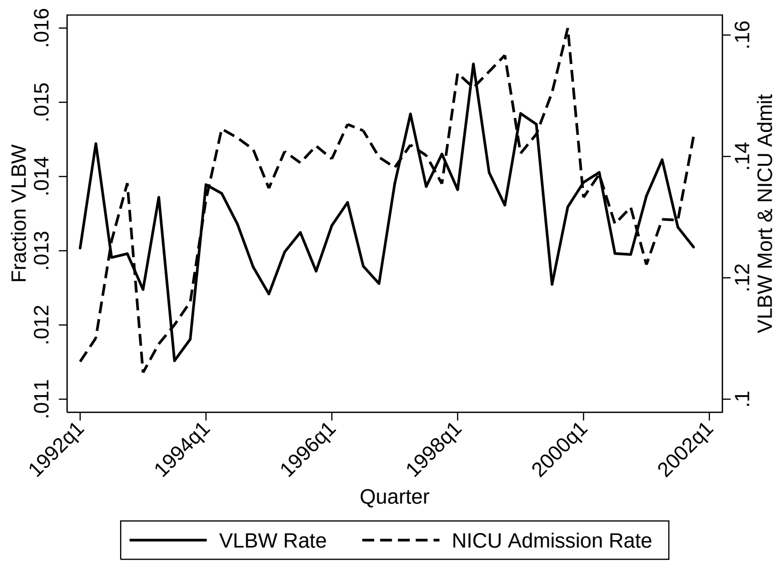
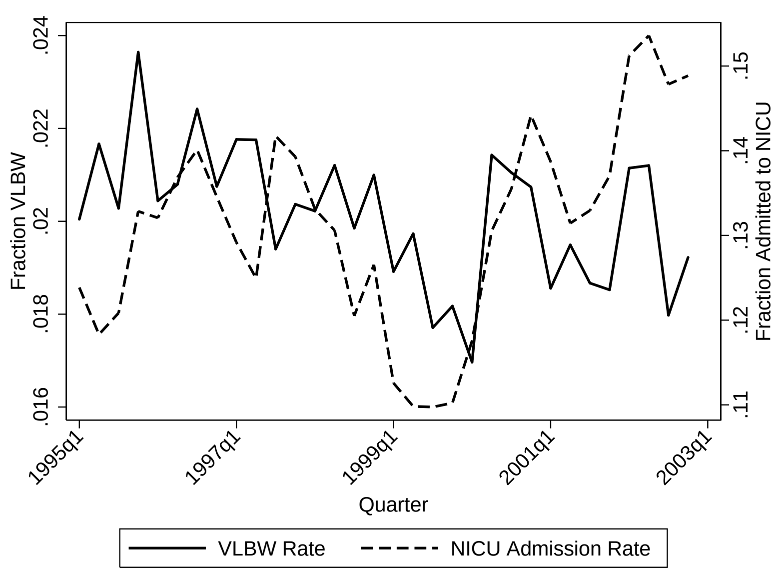
Figure 1: Very Low Birth Weight and NICU Admission Over Time

(a) California



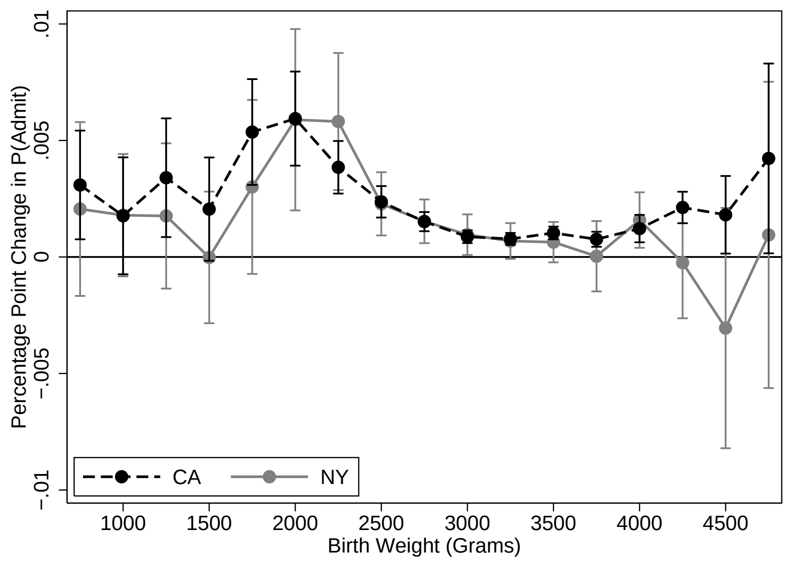
(b) New York



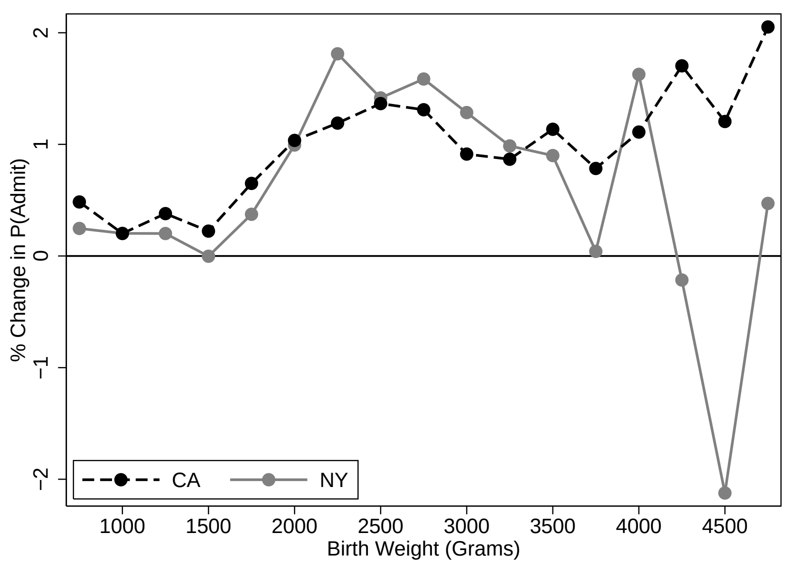
Notes: This figure plots trends in the fraction of VLBW infants and the fraction of infants admitted to the NICU by quarter from the analysis samples. VLBW infants are measured on the left hand side axis while NICU admission is measured on the right hand side axis.

Figure 2: Effect of Empty Beds on NICU Admission by Birth Weight

(a) Coefficients and 95% Confidence Intervals



(b) Coefficients Normalized by NICU Admission Rate



Notes: The top panel plots coefficient estimates and 95% confidence intervals from separate regressions of NICU admission on the number of empty beds the day before birth for samples stratified by birth weight in 250-gram increments. Specifications include all control variables described in the notes to Table 4, including hospital-specific month fixed effects. All standard errors are clusterd at the hospital level. The bottom panel plots these coefficient estimates divided by the NICU admission rate of each birth weight subgroup.

Table 1: Sample Means by Birth Weight Sample

	Panel A: California						Panel B: New York					
	All	VLBW	LBW	NBW1	NBW2	HBW	All	VLBW	LBW	NBW1	NBW2	HBW
NICU Admission	0.135	0.769	0.523	0.114	0.092	0.123	0.132	0.858	0.532	0.097	0.072	0.112
Mother's Demographics												
Age	27.482	27.931	27.690	26.967	27.552	28.623	22.256	13.930	15.669	21.649	23.542	24.616
Medicaid	0.475	0.495	0.512	0.502	0.461	0.432	0.378	0.421	0.412	0.413	0.359	0.312
Managed Care (HMO in NY)	0.385	0.338	0.349	0.364	0.397	0.419	0.244	0.232	0.223	0.228	0.254	0.273
Self Pay	0.037	0.029	0.035	0.040	0.037	0.032	0.052	0.044	0.052	0.057	0.050	0.046
Some College	0.183	0.189	0.180	0.176	0.185	0.197	–	–	–	–	–	–
College	0.110	0.089	0.098	0.104	0.114	0.119	–	–	–	–	–	–
College Plus	0.083	0.067	0.078	0.078	0.087	0.090	–	–	–	–	–	–
Black	0.074	0.168	0.136	0.092	0.059	0.044	0.197	0.319	0.258	0.225	0.174	0.146
Other Race	0.125	0.100	0.132	0.158	0.113	0.076	0.332	0.272	0.311	0.359	0.327	0.282
Hispanic	0.474	0.440	0.427	0.469	0.485	0.471	0.105	0.085	0.101	0.110	0.105	0.096
Pregnancy Characteristics												
Male	0.512	0.510	0.478	0.454	0.530	0.628	0.513	0.502	0.476	0.457	0.536	0.635
Multiple Birth	0.028	0.238	0.219	0.034	0.003	0.000	0.039	0.267	0.251	0.041	0.004	0.001
Parity	2.150	2.257	2.225	2.061	2.153	2.370	–	–	–	–	–	–
Mnth. Pren. Care Began	2.450	2.148	2.425	2.500	2.440	2.394	–	–	–	–	–	–
# of Pren. Visits	11.532	8.599	10.968	11.336	11.704	12.017	–	–	–	–	–	–
Infant Characteristics												
Congenital Anomaly	0.010	0.090	0.035	0.009	0.006	0.007	0.013	0.103	0.039	0.011	0.007	0.010
Clinical Condition	0.107	0.268	0.166	0.052	0.065	0.437	0.086	0.193	0.114	0.028	0.047	0.448
Small for Gest.	0.004	0.058	0.044	0.002	0.000	0.000	0.004	0.061	0.040	0.001	0.000	0.000
Large for Gest.	0.067	0.016	0.018	0.013	0.036	0.422	0.063	0.013	0.019	0.008	0.033	0.442
Birth Weight (Grams)	3337.54	993.00	2171.40	2973.22	3577.76	4273.25	3281.27	997.66	2151.76	2963.07	3571.82	4289.47
Gestation (Weeks)	39.464	29.381	36.341	39.211	40.033	40.426	–	–	–	–	–	–
N	3,131,948	42,040	173,895	1,032,399	1,560,110	323,504	687,086	13,764	47,515	236,578	323,340	65,889

Notes: This table presents sample means for the full sample and each of the five birth weight subsamples in each state. Dashes indicate variables not available in the New York SPARCS inpatient data set.

Table 2: Summary Statistics of Empty Beds

	Mean	St. Dev.	25th Pct.	50th Pct.	75th Pct.
California Full Sample (N=3,131,948)					
NICU Beds	21.469	17.278	8.000	16.000	28.000
Empty NICU Beds	1.895	9.050	-3.000	2.000	7.000
Residual Empty Beds	-0.000	3.105	-1.681	0.065	1.696
New York Full Sample (N=687,086)					
NICU Beds	26.953	13.668	14.000	27.000	36.000
Empty NICU Beds	9.979	9.889	3.000	9.000	15.000
Residual Empty Beds	-0.000	2.757	-1.488	0.000	1.516

Notes: This table provides summary statistics of the number of NICU beds, the number of empty NICU beds and the residual empty NICU beds for the full sample in each state. The residuals are from separate regressions of empty beds on hospital-specific month fixed effects for each state.

Table 3: Sample Means by Residual Empty Beds

	Panel A: California		Panel B: New York	
	Below Median	Above Median	Below Median	Above Median
NICU Admission	0.128	0.142	0.123	0.141
Mother's Demographics				
Age	27.469	27.495	22.402	22.110
Medicaid	0.475	0.474	0.381	0.375
Managed Care (HMO in NY)	0.385	0.385	0.241	0.247
Self Pay	0.037	0.037	0.053	0.051
Some College	0.183	0.183	–	–
College	0.109	0.111	–	–
College Plus	0.084	0.083	–	–
Black	0.074	0.074	0.198	0.197
Other Race	0.125	0.125	0.332	0.331
Hispanic	0.475	0.474	0.107	0.104
Pregnancy Characteristics				
Male	0.512	0.512	0.514	0.513
Multiple Birth	0.026	0.030	0.037	0.041
Parity	2.149	2.151	–	–
Mnth. Pren. Care Began	2.450	2.450	–	–
# of Pren. Visits	11.537	11.528	–	–
Infant Characteristics				
Congenital Anomaly	0.009	0.010	0.012	0.013
Clinical Condition	0.106	0.109	0.086	0.087
Small for Gest.	0.004	0.004	0.004	0.005
Large for Gest.	0.067	0.067	0.063	0.063
Birth Weight (Grams)	3344.30	3330.78	3289.51	3273.02
Gestation (Weeks)	39.493	39.436	–	–
N	1,565,998	1,565,950	343,595	343,491

Notes: This table presents sample means for the full sample and each of the five birth weight subsamples by residual empty beds in each state. The residuals are from separate regressions of empty beds on hospital-specific month fixed effects for each state. Dashes indicate variables not available in the New York SPARCS inpatient data set.

Table 4: Effect of Empty Beds on NICU Admission

	Panel A: California					Panel B: New York				
	P(Admit) and Sample Size	Regression Coefficients *Dep. Var.: NICU Admission*			Relative Effect	P(Admit) and Sample Size	Regression Coefficients *Dep. Var.: NICU Admission*			Relative Effect
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Full Sample	0.135	0.0024**	0.0015**	0.0015**	1.09%	0.132	0.0030**	0.0015**	0.0014**	1.07%
	3,131,948	(0.0002)	(0.0001)	(0.0001)		687,086	(0.0004)	(0.0003)	(0.0003)	
VLBW	0.769	0.0047**	0.0034**	0.0031**	0.40%	0.858	0.0019**	0.0013	0.0013	0.15%
(0 to 1,499 G)	42,040	(0.0008)	(0.0007)	(0.0007)		13,764	(0.0010)	(0.0009)	(0.0009)	
LBW	0.523	0.0075**	0.0052**	0.0049**	0.94%	0.532	0.0079**	0.0050**	0.0049**	0.93%
(1,500 to 2,499 G)	173,895	(0.0008)	(0.0006)	(0.0005)		47,515	(0.0013)	(0.0010)	(0.0010)	
NBW1	0.114	0.0014**	0.0014**	0.0014**	1.19%	0.097	0.0016**	0.0016**	0.0013**	1.39%
(2,500 to 3,249 G)	1,032,399	(0.0001)	(0.0001)	(0.0001)		236,578	(0.0003)	(0.0003)	(0.0003)	
NBW2	0.092	0.0009**	0.0009**	0.0009**	0.97%	0.072	0.0005	0.0005	0.0005	0.76%
(3,250 to 4,000 G)	1,560,110	(0.0001)	(0.0001)	(0.0001)		323,340	(0.0004)	(0.0004)	(0.0004)	
HBW	0.123	0.0017**	0.0017**	0.0016**	1.32%	0.112	0.0012**	0.0012**	0.0009*	0.84%
(4,000+ G)	323,504	(0.0002)	(0.0002)	(0.0002)		65,889	(0.0006)	(0.0006)	(0.0006)	
Hospital-Month FE		X	X	X			X	X	X	
Birth Weight FE			X	X				X	X	
Day of Week FE				X					X	
Demographics				X					X	
Pregnancy Char.				X					X	
Infant Char.				X					X	

Notes: Each row presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of NICU admission on the number of empty beds for the full sample and each of the five birth weight subsamples in each state. All specifications include hospital-specific month fixed effects. Birth weight fixed effects are in 250-gram increments. Day of week fixed effects are dummies for 6 of the 7 days of the week. Demographics include mother's age, mother's age squared, race, ethnicity, and insurance coverage. Pregnancy characteristics include number of prenatal care visits (CA only), month in which prenatal care began (CA only), parity (CA only), sex, and multiple birth status. Infant characteristics include an indicator for having a congenital anomaly, an indicator for having a clinical condition, and indicators for small and large for gestational age. * p<.10, ** p<.05

Table 5: Mitigating Effects of Inter-Hospital Transfers

	Panel A: California				**Panel B: New York**			
Dependent Var:	Transfer	Transfer Day 1	Admit or Transfer	Admit or Transfer Day 1	Transfer	Transfer Day 1	Admit or Transfer	Admit or Transfer Day 1
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Full Sample	-0.0001**	-0.0001**	0.0014**	0.0014**	-0.00003	-0.00003**	0.0014**	0.0014**
	(0.00003)	(0.00002)	(0.0001)	(0.0001)	(0.00002)	(0.00001)	(0.0003)	(0.0003)
VLBW	-0.0015**	-0.0011**	0.0020**	0.0020**	-0.0001	-0.0005*	0.0010	0.0011
(0 to 1,499 G)	(0.0006)	(0.0003)	(0.0006)	(0.0006)	(0.0009)	(0.0003)	(0.0009)	(0.0009)
LBW	-0.0009**	-0.0005**	0.0044**	0.0045**	-0.0002	-0.0002*	0.0049**	0.0049**
(1,500 to 2,499 G)	(0.0002)	(0.0001)	(0.0005)	(0.0005)	(0.0002)	(0.0001)	(0.0010)	(0.0010)
NBW1	-0.0001**	-0.0001**	0.0013**	0.0013**	-0.00003	-0.00001	0.0013**	0.0013**
(2,500 to 3,249 G)	(0.00003)	(0.00002)	(0.0001)	(0.0001)	(0.00003)	(0.00002)	(0.0003)	(0.0003)
NBW2	-0.00003*	-0.00002*	0.0009**	0.0009**	0.00001	0.000004	0.0005	0.0005
(3,250 to 4,000 G)	(0.00002)	(0.00001)	(0.0001)	(0.0001)	(0.00002)	(0.00001)	(0.0004)	(0.0004)
HBW	-0.00003	-0.00001	0.0016**	0.0016**	0.00001	0.00001	0.0010*	0.0010*
(4,000+ G)	(0.00004)	(0.00002)	(0.0002)	(0.0002)	(0.00004)	(0.00003)	(0.0006)	(0.0006)

Notes: Each row presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions for the full sample and each of the five birth weight subsamples in each state. In Column 1 the dependent variable is whether the infant is ever transfered, Column 2 whether the infant is transfered on the first day, Column 3 whether the infant is admitted to the NICU or ever transferred, and Column 4 whether the infant is admitted to the NICU or transferred on the first day. Specifications include all control variables described in the notes to Table 4, including hospital-specific month fixed effects. * $p<.10$, ** $p<.05$

Table 6: Heterogeneous Effects by Hospital Characteristics

	Panel A: California					Panel B: New York				
	NICU Beds		Hospital Ownership			NICU Beds		Hospital Ownership		
Dependent Var: NICU Admission	Less Than 20	Greater Than 20	Gov.	Non-Profit	For-Profit	Less Than 20	Greater Than 20	Gov.	Non-Profit	For-Profit
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Full Sample	0.0023**	0.0011**	0.0017**	0.0014**	0.0022**	0.0015	0.0014**	0.0021**	0.0012**	–
	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0004)	(0.0016)	(0.0002)	(0.0009)	(0.0004)	
N	1,785,118	1,346,830	546,232	2,219,680	371,924	230,313	456,773	130,073	555,555	
VLBW	0.0075**	0.0022**	0.0014	0.0036**	0.0010	0.0059	0.0006	0.0012	0.0016	–
(0 to 1,499 Grams)	(0.0018)	(0.0005)	(0.0011)	(0.0008)	(0.0016)	(0.0040)	(0.0009)	(0.0015)	(0.0011)	
N	17,463	24,577	7,425	30,221	4,426	3,435	10,329	3,104	10,623	
LBW	0.0091**	0.0034**	0.0055**	0.0047**	0.0065**	0.0102**	0.0038**	0.0033**	0.0053**	–
(1,500 to 2,499 Grams)	(0.0009)	(0.0005)	(0.0008)	(0.0006)	(0.0017)	(0.0017)	(0.0009)	(0.0016)	(0.0011)	
N	89,595	84,300	31,687	123,318	19,151	14,693	32,822	9,889	37,478	
NBW1	0.0022**	0.0010**	0.0013**	0.0013**	0.0020**	0.0014	0.0013**	0.0018**	0.0012**	–
(2,500 to 3,249 Grams)	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0004)	(0.0014)	(0.0003)	(0.0007)	(0.0004)	
N	586,234	446,165	181,525	728,132	124,497	78,729	157,849	47,920	188,226	
NBW2	0.0013**	0.0007**	0.0013**	0.0007**	0.0018**	-0.0006	0.0008**	0.0018**	0.0003	–
(3,250 to 4,000 Grams)	(0.0002)	(0.0001)	(0.0004)	(0.0001)	(0.0004)	(0.0019)	(0.0002)	(0.0008)	(0.0005)	
N	902,873	657,237	268,026	1,108,103	187,106	110,379	212,961	58,485	264,172	
HBW	0.0022**	0.0014**	0.0017**	0.0016**	0.0019**	0.0005	0.0010**	0.0014	0.0008	–
(4,000+ Grams)	(0.0003)	(0.0003)	(0.0005)	(0.0003)	(0.0007)	(0.0019)	(0.0005)	(0.0012)	(0.0007)	
N	188,953	134,551	57,569	229,906	36,744	23,077	42,812	10,675	55,056	

Notes: Each cell presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of NICU admission on the number of empty beds. Each row presents estimates from the full sample and each of the five birth weight subsamples. Columns 1-2 present estimates from samples defined by the number of NICU beds and Columns 3-5 by hospital ownership. There are insufficient observations from for-profit hospitals in New York to estimate this subsample. Specifications include all control variables described in the notes to Table 4, including hospital-specific month fixed effects. * p<.10, ** p<.05

Table 7: Heterogeneous Effects by Individual Characteristics

	Panel A: California							Panel B: New York						
		Demographics			Insurance Status				Demographics			Insurance Status		
Dependent Var: NICU Admission	Baseline	Hispanic	Black	No College	Private/Non-Managed Care	Managed Care	Medicaid	Baseline	Hispanic	Black	No College	Private/Non-HMO	HMO	Medicaid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Full Sample	0.0015**	0.0014**	0.0015**	0.0015**	0.0014**	0.0013**	0.0015**	0.0014**	0.0018**	0.0028**	–	0.0015**	0.0003	0.0021**
	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0003)	(0.0007)	(0.0004)		(0.0003)	(0.0006)	(0.0003)
N	3,131,948	1,486,098	231,056	1,952,067	323,065	1,206,000	1,486,377	687,086	72,326	135,667		213,604	167,798	259,646
VLBW	0.0031**	0.0022	0.0069**	0.0038**	0.0017	0.0028**	0.0036**	0.0013	0.0035	0.0019	–	0.0006	0.0015	0.0024
(0 to 1,499 Grams)	(0.0007)	(0.0014)	(0.0020)	(0.0009)	(0.0012)	(0.0014)	(0.0010)	(0.0009)	(0.0067)	(0.0025)		(0.0016)	(0.0022)	(0.0016)
N	42,040	18,491	7,080	27,573	5,763	14,228	20,820	13,764	1,176	4,394		3,983	3,187	5,794
LBW	0.0049**	0.0051**	0.0032**	0.0047**	0.0049**	0.0046**	0.0047**	0.0049**	0.0027	0.0044**	–	0.0072**	0.0028	0.0036**
(1,500 to 2,499 Grams)	(0.0005)	(0.0006)	(0.0008)	(0.0005)	(0.0016)	(0.0011)	(0.0005)	(0.0010)	(0.0028)	(0.0018)		(0.0016)	(0.0021)	(0.0012)
N	173,895	74,188	23,579	111,801	18,042	60,606	89,117	47,515	4,806	12,237		14,236	10,575	19,590
NBW1	0.0014**	0.0013**	0.0013**	0.0013**	0.0015**	0.0011**	0.0013**	0.0013**	0.0028**	0.0024**	–	0.0014**	0.0009*	0.0016**
(2,500 to 3,249 Grams)	(0.0001)	(0.0002)	(0.0004)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0008)	(0.0006)		(0.0005)	(0.0005)	(0.0004)
N	1,032,399	483,914	94,563	662,321	96,980	375,625	518,062	236,578	26,061	53,112		68,565	53,834	97,595
NBW2	0.0009**	0.0009**	0.0006**	0.0009**	0.0006**	0.0008**	0.0010**	0.0005	0.00002	0.0022**	–	0.0006	-0.0008	0.0018**
(3,250 to 4,000 Grams)	(0.0001)	(0.0002)	(0.0003)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0004)	(0.0008)	(0.0007)		(0.0004)	(0.0007)	(0.0004)
N	1,560,110	757,033	91,753	957,999	164,556	619,948	718,624	323,340	33,964	56,297		103,742	82,214	116,088
HBW	0.0016**	0.0017**	0.0006	0.0015**	0.0016**	0.0017**	0.0016**	0.0009*	0.0022	0.0030**	–	0.0012*	0.0007	0.0011
(4,000+ Grams)	(0.0002)	(0.0003)	(0.0019)	(0.0003)	(0.0007)	(0.0002)	(0.0003)	(0.0006)	(0.0022)	(0.0015)		(0.0007)	(0.0011)	(0.0009)
N	323,504	152,472	14,081	192,373	37,724	135,593	139,754	65,889	6,319	9,627		23,078	17,988	20,579

Notes: Each cell presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of NICU admission on the number of empty beds. Each row presents estimates from the full sample and each of the five birth weight subsamples. Column 1 repeats the baseline estimates from Table 4. Columns 2-4 present estimates from samples defined by demographic characteristics and Columns 5-7 by insurance status. The New York SPARCS inpatient data does not include education levels. Specifications include all control variables described in the notes to Table 4, including hospital-specific month fixed effects. * p<.10, ** p<.05

Table 8: Robustness Checks

Dependent Var: NICU Admission	Panel A: California					Panel B: New York				
	Baseline	50 G BW Dummies	Exclude C-Sections	> 5% Empty	< 60% of Days Over Cap.	Baseline	50 G BW Dummies	Exclude C-Sections	> 5% Empty	< 60% of Days Over Cap.
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Full Sample	0.0015**	0.0015**	0.0012**	0.0020**	0.0017**	0.0013**	0.0013**	0.0009**	0.0016**	0.0016**
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
N	3,131,948	3,131,948	2,428,260	1,800,078	2,269,669	702,924	702,924	527,946	603,988	674,252
VLBW	0.0031**	0.0030**	0.0035**	0.0045**	0.0042**	0.0018	0.0018	0.0003	0.0024*	0.0017
(0 to 1,499 Grams)	(0.0007)	(0.0007)	(0.0011)	(0.0013)	(0.0008)	(0.0011)	(0.0011)	(0.0022)	(0.0013)	(0.0011)
N	42,040	42,040	18,915	22,680	28,483	13,986	13,986	6,009	12,403	13,569
LBW	0.0049**	0.0049**	0.0046**	0.0059**	0.0060**	0.0056**	0.0056**	0.0053**	0.0056**	0.0055**
(1,500 to 2,499 Grams)	(0.0005)	(0.0005)	(0.0005)	(0.0011)	(0.0008)	(0.0009)	(0.0009)	(0.0009)	(0.0010)	(0.0009)
N	173,895	173,895	107,492	96,513	122,061	48,457	48,457	28,924	42,361	46,812
NBW1	0.0014**	0.0014**	0.0011**	0.0016**	0.0015**	0.0012**	0.0012**	0.0007**	0.0016**	0.0014**
(2,500 to 3,249 Grams)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
N	1,032,399	1,032,399	830,504	587,706	741,901	241,987	241,987	190,625	208,805	232,433
NBW2	0.0009**	0.0009**	0.0007**	0.0013**	0.0010**	0.0005	0.0005	0.0003	0.0007**	0.0009**
(3,250 to 4,000 Grams)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.0002)	(0.0002)
N	1,560,110	1,560,110	1,245,920	903,757	1,138,587	330,986	330,986	257,806	283,088	316,950
HBW	0.0016**	0.0016**	0.0014**	0.0027**	0.0019**	0.0005	0.0005	0.0011**	0.0008*	0.0008*
(4,000+ Grams)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0002)	(0.0005)	(0.0005)	(0.0005)	(0.0004)	(0.0004)
N	323,504	323,504	225,429	189,422	238,637	67,508	67,508	44,582	57,331	64,488

Notes: Each cell presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of NICU admission on the number of empty beds. Each row presents estimates from the full sample and each of the five birth weight subsamples. Column 1 repeats the baseline estimates from Table 4, Column 2 includes birth weight controls at 50-gram increments, Column 3 excludes infants delivered by cesarean section, Column 4 excludes observations from hospital-days in which the number of NICU beds exceeds the number of patients by less than 5%, and Column 5 excludes hospital-years in which the number of NICU patients exceeds the number of NICU beds for more than 60% of the year. Specifications include all control variables described in the notes to Table 4, including hospital-specific month fixed effects. * p<.10, ** p<.05

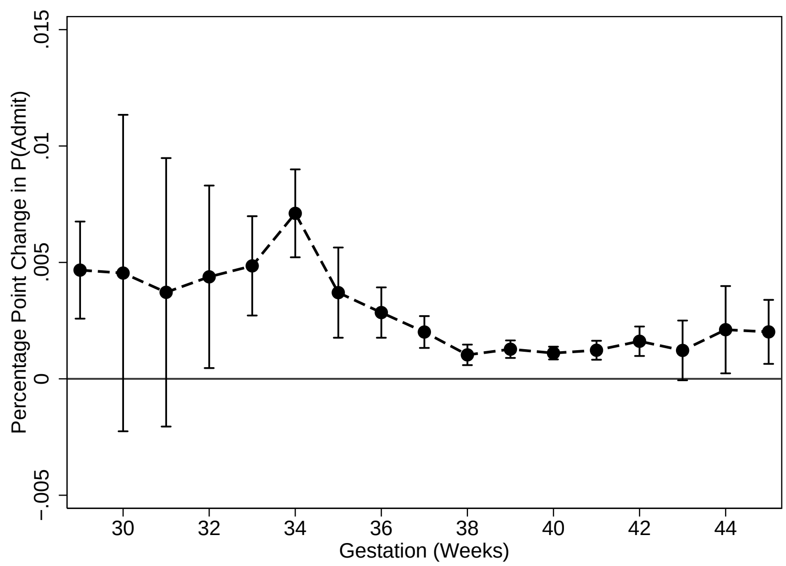
Table 9: Effect of Empty Beds on Length of Stay and Hospital Charges

	Panel A: California				**Panel B: New York**			
	LOS	Ln(LOS)	Charges	Ln(Charges)	LOS	Ln(LOS)	Charges	Ln(Charges)
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Full Sample	0.0192**	0.0021**	77.09**	0.0041**	0.0212**	0.0021**	75.45**	0.0040**
	(0.0018)	(0.0002)	(9.154)	(0.0003)	(0.0034)	(0.0003)	(11.61)	(0.0005)
N	3,131,948	3,131,948	3,130,740	3,126,453	687,086	687,086	687,086	687,086
VLBW	0.2380**	0.0136**	1,078**	0.0130**	0.3397**	0.0170**	1,159**	0.0175**
(0 to 1,499 Grams)	(0.0445)	(0.0022)	(258.5)	(0.0026)	(0.1215)	(0.0048)	(442.2)	(0.0055)
N	42,040	42,040	41,928	41,583	13,764	13,764	13,764	13,764
LBW	0.0971**	0.0103**	318.0**	0.0167**	0.0866**	0.0093**	226.1**	0.0146**
(1,500 to 2,499 Grams)	(0.0124)	(0.0011)	(59.73)	(0.0018)	(0.0170)	(0.0017)	(48.17)	(0.0023)
N	173,895	173,895	173,864	173,600	47,515	47,515	47,515	47,515
NBW1	0.0106**	0.0014**	44.28**	0.0035**	0.0114**	0.0017**	45.01**	0.0037**
(2,500 to 3,249 Grams)	(0.0014)	(0.0002)	(8.443)	(0.0003)	(0.0028)	(0.0004)	(7.952)	(0.0007)
N	1,032,399	1,032,399	1,031,997	1,030,762	236,578	236,578	236,578	236,578
NBW2	0.0049**	0.0009**	17.08**	0.0022**	0.0058**	0.0007**	22.86**	0.0018**
(3,250 to 4,000 Grams)	(0.0008)	(0.0001)	(4.988)	(0.0002)	(0.0014)	(0.0002)	(4.482)	(0.0004)
N	1,560,110	1,560,110	1,559,544	1,557,541	323,340	323,340	323,340	323,340
HBW	0.0070**	0.0011**	31.48**	0.0034**	0.0054	0.0015**	25.59	0.0025**
(4,000+ Grams)	(0.0018)	(0.0003)	(6.941)	(0.0006)	(0.0057)	(0.0007)	(18.30)	(0.0010)
N	323,504	323,504	323,407	322,967	65,889	65,889	65,889	65,889

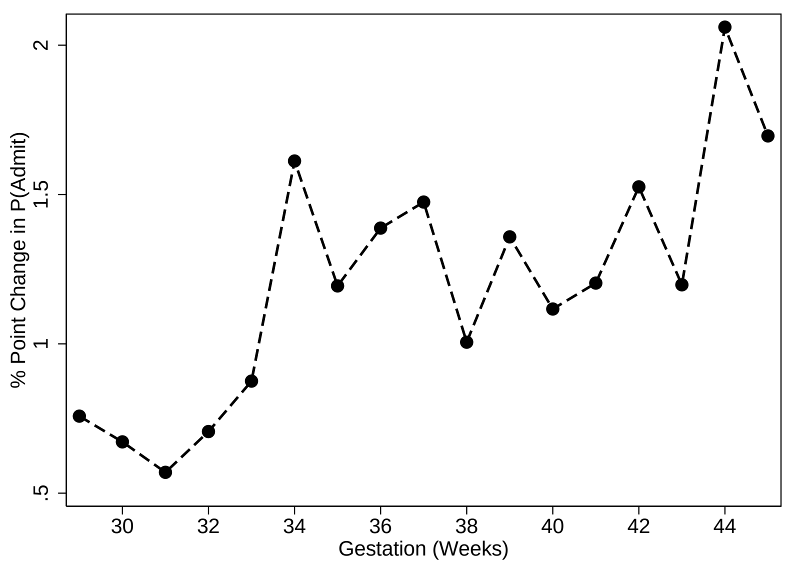
Notes: Each cell presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) of length of stay and hospital charges on the number of empty beds. Each row presents estimates from the full sample and each of the five birth weight subsamples. Specifications include all control variables described in the notes to Table 4, including hospital-specific month fixed effects. * p<.10, ** p<.05

Figure A.1: Effect of Empty Beds on NICU Admission by Gestation

(a) Coefficients and 95% Confidence Intervals



(b) Coefficients Normalized by NICU Admission Rate



Notes: The top panel plots coefficient estimates and 95% confidence intervals from separate regressions of NICU admission on the number of empty beds the day before birth for samples stratified by gestation in 1-week increments. Specifications include all control variables described in the notes to Table 4, including hospital-specific month fixed effects. All standard errors are clusterd at the hospital level. The bottom panel plots these coefficient estimates divided by the NICU admission rate of each gestation subgroup.

Table A.1: Constructing Analysis Sample

Panel A: California					
	Average # of	# of Observations			
	Hospitals per Year	Births	Transfers	Readmissions	Total
Initial	387.27	6,221,001	96,041	736,762	7,053,804
# of NICU Beds > 0	158.27	4,445,751	92,925	596,589	5,135,265
# of NICU Admissions > 0	139.55	4,269,275	91,600	581,939	4,942,814
# of Births > 0	144.27	4,264,353	67,554	469,546	4,801,453
Birth Diff < 10%	140.45	4,152,220	64,329	452,827	4,669,376
Non-Missing Charges	121.91	3,566,527	57,440	404,768	4,028,735
Algorithm Diff < 10%	119.64	3,495,411	55,926	393,283	3,944,620
Admission Date Present	119.64	3,477,195	55,926	393,283	3,926,404
Birth Weight Present	119.64	3,440,074	55,717	392,429	3,888,220
Year > 1991	121.10	3,131,948	50,492	369,092	3,551,532

Panel B: New York				
	Average # of	# of Observations		
	Hospitals per Year	Births	Transfers/Readmissions	Total
Initial	190.70	2,441,064	397,934	2,838,998
# of NICU Beds > 0	52.70	1,288,037	242,177	1,530,214
# of NICU Admissions > 0	39.90	1,049,778	198,910	1,248,688
# of Births > 0	37.80	1,004,792	187,304	1,192,096
Birth Diff < 25%	29.80	863,246	152,120	1,015,366
Non-Missing Charges	29.80	863,246	152,120	1,015,366
Algorithm Diff < 10%	29.80	863,246	152,120	1,015,366
Admission Date Present	29.80	863,246	152,120	1,015,366
Birth Weight Present	29.80	863,246	152,120	1,015,366
Year > 1994 or < 2003	29.38	687,086	125,909	812,995

Notes: This table lists the number of observations and average number of hospitals per year after the imposition of each sample restriction.