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

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Abstract

Because geographic variation in medical care utilization is jointly determined by both supply and demand, it is difficult to empirically estimate whether capacity itself has a causal impact on utilization in health care. In this paper, I exploit short-term variation in Neonatal Intensive Care Unit (NICU) capacity that is unlikely to be correlated with unobserved demand determinants. I find that available NICU beds have little to no effect on NICU utilization for the sickest infants, but do increase utilization for those in the range of birth weights where admission decisions are likely to be more discretionary.

Amid rising health care costs and the political debate over health reform, excessive utilization of health care is an important topic. One concern is that the availability of supply itself directly leads to additional utilization, or, as Roemer (1961) put it, that “A built bed is a filled bed.” Theoretically, physicians and hospitals face financial incentives to provide additional care on the margin when MRI machines, catheterization labs, or hospital beds are available. In addition to overutilization on the margin, these incentives can dynamically lead to further overinvestment in these high fixed cost facilities as providers compete for patients.

Regional medical spending and utilization has been found to be correlated with regional differences in physician supply and hospital bed capacity (e.g. Fisher et al., 2000, 2003, 2004). However, even controlling for observable demographic characteristics, cross sectional comparisons across regions are not likely to represent causal estimates. As Fuchs (2004) points out, empirically testing the hypothesis that simply the availability of medical resources leads to additional utilization is difficult; it requires variation in supply that is not driven by patient demand and unobserved health conditions.

In this paper, I overcome the endogeneity between capacity and utilization by exploiting short-run variation in the availability of neonatal intensive care unit (NICU) beds. Using hospital discharge data from California and New York, 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, conditional on 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. Unlike regional measures of capacity, within-hospital-month

shocks are unlikely to be correlated with patient demand and health characteristics, and I provide empirical evidence to support this identifying assumption.

Neonatal intensive care is an important and interesting health care market in which to examine the effect of availability on utilization. It has been claimed that the increase in the supply of NICUs has outpaced medical necessity (e.g. Schwartz, 1996; Schwartz, Kellogg and Muri, 2000; Baker and Phibbs, 2002; Howell et al., 2002).¹ Likewise, Harrison and Goodman (2015) find that, after adjusting for health risk factors, the fraction of infants admitted to the NICU has increased for newborns of all birth weights between 1997 and 2012. Figure 1 shows that the number of total NICU beds and the number of infants treated in NICUs both greatly increased over the period of my study in California and New York. Meanwhile, the number of births and the number of low birth weight births were both generally decreasing.

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. Given these trends and incentives, health policy researchers have suggested that there may be particularly large scope for available supply to increase utilization in NICUs (Carrol, 2015; Harrison and Goodman, 2015), and this paper seeks to identify a causal estimate of this effect.

If availability directly leads to additional utilization of neonatal intensive care, a variety of important costs could be incurred. First, there is the economic cost associated with using care beyond the point at which 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 may cause additional stress and worry. There are also potentially *negative* health effects of unnecessary care in the NICU. For example, epidemiologists have documented an increasing prevalence of hospital borne infections that can lead to mortality, morbidity, and longer lengths of stay and are difficult to predict and diagnose (e.g. Kossoff, Buescher and Karlowicz, 1998; Benjamin et al., 2000; Clark et al., 2004). Increased exposure to such infections could be one potential cost of spending unnecessary time in the NICU.

In addition to these costs, the relationship between capacity and utilization could lead to dynamic welfare losses. If hospitals invest in NICU capacity and can then utilize this capacity independent of patient demand, they face incentives to continue investing in more NICU capacity. This feedback can then further increase potential overinvestment. Figure 1 shows that capacity and utilization both do increase overtime, though quantifying these dynamic incentives are beyond the scope of this paper. Instead, I seek to establish a causal estimate of the static effect of capacity on utilization, providing motivation for further research regarding these dynamics.

¹The number of NICUs more than doubled over the 1980s and 1990s (Baker and Phibbs, 2002). By 1995, the number of available bed-days exceeded the number of medically necessary bed-days by a factor of 2.4 (Howell et al., 2002).

I find that exploiting exogenous variation in available capacity still leads to the conclusion that capacity increases utilization. On average, having 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 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 that they 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. Empty beds may also increase NICU admissions through non-financial incentives if capacity decreases the option value of holding a bed for a future infant with greater health benefits or improves the quality of care provided to other NICU patients. I cannot completely rule out these possibilities, but I provide evidence that these mechanisms are unlikely to be fully driving the results and that financial incentives are likely to play an important role. Finally, I show that hospital resource utilization as measured by charges and length of stay also increases with the number of empty NICU beds available at birth. I do find that empty beds are correlated with lower mortality rates for very low and low birth weight infants; however, these estimates are more tentative than the other findings of the paper, since available capacity may improve health outcomes through channels other than NICU admission.

I. Background and Conceptual Framework

A. Institutional Details

Before specifically modeling NICU admission decisions, I briefly describe how admission decisions are made in practice and how financial incentives are generally aligned. Most high-level NICUs are staffed by at least one neonatologist, a subspecialty within pediatrics, often accompanied by neonatal nurse practitioners. In many lower-level NICUs that predominantly provide monitoring, nutrition, and ventilation but few advanced procedures, the NICU may be staffed by pediatricians, pediatric hospitalists, or nurse practitioners.

Regardless of the providers staffing the NICU, these individuals are the primary decision makers when it comes to NICU admissions. As a result, the individuals making the admission decision are the individuals most directly facing any financial incentives, in terms of both physician reimbursement and responsibility for the cost center of the hospital generating the hospital reimbursements. However, there is important heterogeneity in when those staffing the NICU become involved in an infant's care. For example, in smaller, lower-level units, members of the pediatrics team may staff the NICU, attend all deliveries, and be involved in care decisions immediately. In higher-level units, the physicians making NICU

admission decisions may not be involved in an infant's care until they are actively consulted, prior to or after the delivery. I explore the implications of this heterogeneity in Section V.

The financial incentives facing a hospital and a physician can vary by payer type and by hospital. In general the financial incentives are aligned in such a way that NICU admission is profitable (Horwitz, 2005, see online appendix). From the hospital's perspective, payments can be based on a per diem, fee for service, or prospective based on the infant's diagnosis; however, during my study period, fee for service and per diem reimbursements dominated. During the 1990s managed care insurers were not aggressive in cost-containment efforts for NICU services (Schulman, 2003), and as Horwitz (2005, see online appendix) notes, discussions of NICU cost containment efforts did not begin appearing in the medical literature until the early 2000s (e.g. Richardson et al., 2001; Schulman, 2003).

Whether the physician faces a direct financial incentive is also likely to vary. Some neonatologists and pediatricians are private-practice physicians with admitting privileges at a hospital. These physicians directly bill insurers for the physician portion of reimbursement, and therefore face a direct financial incentive to admit the infant to the NICU, become the principal care provider, and receive physician reimbursement. Physician reimbursements are also likely to be generous. Neonatology practices are typically monopolists in a given hospital or geographic area and generally have strong bargaining power over insurers, particularly during my study period (Schulman, 2003). Other physicians, most notably residents in academic hospitals, are employed directly by the hospital (Richardson et al., 2001). In these settings, the direct financial incentive may not be as great for the physician; however, they still may be pressured by hospital management to improve profitability. In Section V I explore how patient and hospital characteristics impact the effect of capacity on utilization.

B. Conceptual Model

I now provide a simple framework for thinking about how available capacity might influence NICU admission decisions through financial incentives and other avenues. In this section I discuss the physician as the key decision maker, but the hospital itself faces similar incentives, as discussed above. Models of physician decision-making suggest that supplier-induced demand can occur 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 and Owings (1996), and McGuire (2000) formalize the idea by modeling the physician's utility function as increasing in income, which increases with inducement, and decreasing in inducement itself. Another related line of models assumes the physician maximizes a utility function that is a weighted function of his own well being and the patient's (McGuire, 2000). As a result, the physician may provide additional treatment when it improves his financial well-being, but is disciplined by the fact that harm to the patient decreases his own utility as well. For simplicity, I consider this second line of models.

In modeling NICU admission decisions, I assume the physician's action is a binary decision of whether to admit an infant to the NICU or not. The net payoff of NICU admission relative to the normal nursery varies with the health condition of the infant, which for simplicity is

expressed as a single index, b , and is referred to as birth weight below. Let $U(b)$ equal the physician's overall incremental payoff of admitting an infant to the NICU, which is a weighted function of the physician's internal incremental payoff $u^d(b)$ and the patient's incremental payoff $u^p(b)$:

$$U(b) = \omega u^d(b) + (1 - \omega) u^p(b) \quad \text{where} \quad \partial u^d(b) / \partial b \leq 0 \quad \text{and} \quad \partial u^p(b) / \partial b \leq 0 \quad (1)$$

The physician admits an infant to the NICU if this expression is greater than zero. Both the physician's and the patient's net payoffs are assumed to decrease with birth weight. From the patient's perspective, the health benefits of NICU admission are less likely to outweigh the costs (both financial and potentially emotional) for healthier infants. From the physician's perspective, NICU admission may be associated with a smaller financial incentive for healthier infants whose care in the NICU may involve less intensive interventions.

Additionally, if the physician receives a payoff from conforming to professional norms and following standard treatment protocols, he may face a smaller internal payoff in admitting a healthier infant to the NICU.² Finally, if physicians are risk averse and see NICU admission as a “safer” decision in an uncertain case, this contribution to the payoff is likely to decrease as the patient is observably healthier.

Suppose b^* is the birth weight at which $U(b)$ is equal to zero, and the physician is just indifferent between admitting the infant to the NICU and not. To discuss how available NICU capacity may impact admission, I consider financial and non-financial factors that might lead b^* to vary with capacity:

1. Income Effects—An implication of the McGuire and Pauly (1991), Gruber and Owings (1996), and McGuire (2000) induced demand models is that physicians may increase inducement in response to negative income shocks. The intuition is that physicians trade off the income gains from inducement with the utility cost of inducement. When facing a negative income shock, the marginal utility of income rises, leading physicians to be more willing to increase inducement in order to increase income. In my context, one can think of a decrease in NICU patients as a negative income shock.³ As empty beds increase, physicians see their income fall and become more willing to raise the threshold b^* at which they admit infants to the NICU, since the marginal private utility of admission has increased.

2. Option Value—Admitting an infant to the NICU forgoes the option value of using that bed for a different infant in the future. If the financial benefit and the health benefit of admitting an infant to the NICU decrease with b , physicians may prefer to forgo admitting a heavier infant if it precludes the ability to admit a lighter infant in the future.⁴ Similarly, a physician may prefer to forgo admitting an infant with less generous insurance in order to

²For example, Almond et al. (2010) find discrete changes in NICU admission probabilities around the 1500-gram birth weight threshold, likely as a result of rules of thumb and standard treatment protocols.

³This can be thought of as a short run version of the empirical context in Gruber and Owings (1996), who find that decreases in fertility rates increase cesarean section utilization.

⁴Neonatologists I have spoken too have suggested that when NICUs are full the attitude is generally to make room for a new, high-severity patient, typically by moving a long-staying patient to the pediatric floor. This behavior is likely not costless, but may decrease the salience of these health related incentives.

admit a subsequent infant with more generous insurance. As more beds are available, this form of opportunity cost is likely to decrease since admitting one infant is less likely to preclude admitting a future infant. Therefore, the value of b^* at which NICU admission provides a positive overall net benefit increases with capacity.

3. Congestion Externalities—The quality of care provided in the NICU could increase with capacity if physician and nurse time and resources are spread too thin when the NICU is congested. As more capacity becomes available, decreasing these spillover effects, physicians may be more likely to admit a marginal infant.

The main empirical test of this paper is to determine whether available capacity impacts treatment choices. While I am unable to fully disentangle these financial and non-financial mechanisms behind this relationship, I provide suggestive evidence in Section V on the role of financial incentives.

C. Previous Empirical Research

Two other studies have attempted to address some of the endogeneity inherent in cross-sectional comparisons of capacity and utilization. Baker et al. (2003) and Baras and Baker (2009) utilize regional fixed effects to exploit time-series variation in the availability of a variety of medical technologies. The former study finds a positive correlation between changes in the availability of a number of technologies and both spending on and utilization of those technologies. The latter finds an effect of the availability of MRI scanners on MRI use for lower back pain, a condition for which the use of MRIs is controversial.

I exploit a different sort of time-series variation 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 that uses 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 finer level. It is unlikely that changes in patient preferences within a hospital and within a month are correlated with changes in NICU availability within the hospital-month. 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 the size of the NICU. Therefore, 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 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 already admitted to the NICU receive 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 any intensive care.

II. Data

A. 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 the 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 differences in detail in Section IV in my description of the regression variables. The Institutional Cost Reports contain annual hospital-level NICU capacity and utilization data.

B. Measuring NICU Admission and Empty Beds

The New York SPARCS inpatient discharge data lists each of a patient's 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, including Nursery - Level III corresponding to "Intermediate Care" and Nursery - Level IV corresponding to "Intensive Care." For ease of presentation, I consider only accommodation in Level IV as NICU admission.⁵ Using each infant's admission date and accommodation information, I derive each hospital's daily NICU census by counting how many patients have a Level IV accommodation 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.

⁵Level III accommodations occur rarely in the data, and results available upon request are very similar when counting both Level III and IV as NICU admission.

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 measures of hospital resource utilization likely to correspond with NICU care. I also take into account guidance from Phibbs et al. (1996), who use earlier years of this same data set to identify a population of infants most likely to have been cared for in the NICU, input from neonatologists that I interviewed, and empirical patterns in the New York data. I calibrate my approach to match the number of NICU admissions reported in the Utilization Data File for each hospital-year pair.

I begin by considering all infant records except those transfer and readmission records occurring long after birth as potential NICU admission candidates. I then assign NICU admission to all infants with a length of stay greater than 10 days. Finally, 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.⁶ 24.98% of admissions are imputed based on length of stay and 75.02% are chosen based on charges per day. Online Appendix A provides additional details about this classification algorithm.

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, which allows me to observe the exact days on which infants have each accommodation code, in California I must assume an infant admitted to the NICU spends its entire hospital stay in the NICU, therefore I may be overestimating the number of patients in the unit on a given day. In Section III I discuss the ramifications of this inherent measurement error.

C. Analysis Samples

I now summarize the construction of the analysis samples, with more details provided in Online Appendix B and Online Appendix Table A.1. I begin with the universe of infant hospital admissions in each state. Because my research question and identification strategy are directly related to NICU capacity at the delivery hospital, I first restrict my sample to hospitals that operate NICUs and then eliminate childrens' hospitals that do not perform deliveries. I then make some minor restrictions based on data quality and to facilitate my NICU admission algorithm. I eliminate a small number of hospital-years for which the number of births reported by the hospital-level data and the number of birth observations in the inpatient data differ greatly and hospital-years for which all patients are missing charge data in California.⁷

At this point I classify NICU admissions for all remaining observations and construct the daily empty beds measure. After performing the NICU admission algorithm, I drop all observations for hospital-years in which the target number of discharges differs from the

⁶Even if the infant does not receive a large amount of intensive treatment in the NICU, the amount charged by the hospital would be higher than in the normal newborn nursery.

⁷This restriction excludes Kaiser-owned hospitals, which do not report hospital charges in the data; therefore, the results of this paper are not relevant these hospitals.

number of imputed admissions by more than 10%. I drop a very small number of individual observations in California for which the admission date or birth weight is missing. I then 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.⁸ I 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 samples include 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. For the years of data that remain in my final sample, in California my analysis includes 56% of all births and 81% of all births occurring in hospitals with NICUs. In New York, the corresponding numbers are 35% and 85%.⁹

III. 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, seasonality, and differential trends and seasonality across hospitals.¹⁰ I estimate the following regression equation for infant i , born at time t in hospital h separately for each state:

$$admit_{ith} = \alpha + EmptyBeds_{t-1,h}\beta + \mathbf{X}_{ith}\Gamma + \delta_{th} + \varepsilon_{ith} \quad (2)$$

$admit_{ith}$ is an indicator for being admitted to the NICU. $EmptyBeds_{t-1,h}$ is the measure of how many empty beds are available in the birth hospital's NICU the day prior to the infant's birth.¹¹ 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 that I describe in more detail in Section IV. δ_{th} are fixed effects for each hospital-month-year,¹² 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 while maintaining the assumption that they are independent across hospitals.

⁸The 99th percentile of length of stay for NICU-admitted patients is 91 days, so excluding one year of data should allow the stock of patients to be accurate after one year.

⁹There are some changes over time in the number of hospitals in the sample. Online Appendix B provides evidence that these composition changes do not impact my results.

¹⁰I have explored the robustness of my main results to a conditional logit model and present coefficient estimates in Online Appendix Table A.3. While I cannot calculate marginal effects that are comparable to linear probability model coefficient estimates, the pattern of coefficient estimates is similar to the main results presented below. Note, the conditional logit model does not converge for two subsamples.

¹¹In Online Appendix Table A.4, I explore robustness to this specification by presenting estimates in which the independent variable is measured as the number of empty beds per births (average number of daily births in a hospital-year) and the percent of empty beds. The pattern of effects is very similar to that in the main results.

¹²This amounts to about 14,000 fixed effects for 120 hospitals over 120 months in California and about 2,800 fixed effects for 29 hospitals over 96 months in New York.

As discussed above, capacity may impact the threshold b^* at which an infant is admitted to the NICU. However, this threshold may always remain within a certain range if positive patient health benefits always dominate physician incentives at the lowest levels of b , and if negative patient health effects or patient costs dominate physician incentives at high levels of b . In other words, the effect of capacity on NICU admission is likely to vary by infant characteristics, and 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 (2) 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 (NBW1 and NBW2) 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 for subsamples stratified at 250-gram increments.

The hospital-specific month effects, δ_{th} , allow the unobserved probability of admission to vary for each hospital flexibly over time at monthly intervals. 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 14.76% with a standard deviation of 10.59%. Hospital-specific month fixed effects control for these differences and isolate the response to changes in capacity, conditional on a hospital's overall characteristics.

In addition, it is important to control for the fact that infants health characteristics are quite cyclical (Buckles and Hungerman, 2008). I find similar patterns in my data with a large amount of quarter-to-quarter variation in both the fraction of births that are very low birth weight and the NICU admission rate. Not surprisingly, utilization trends over time closely track health trends. These patterns can be seen in Online Appendix Figure A.1.

However, including only time dummies in the empirical model is potentially insufficient if these cycles and general time trends are heterogeneous 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 and general time trends separately for each hospital, I am able to purge this unobserved correlation from the regression and exploit within-hospital-month deviations in the number of empty beds. These short run deviations are more likely to be uncorrelated with a particular infant's unobserved health.

Note that this identification strategy does 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 dynamic effects would greatly magnify these costs. Additionally, it is worth noting that I observe day-to-day capacity variation. If some hospitals are particularly adept at responding to bed availability within the same day, this would dampen the capacity variation I observe and lead me to underestimate the static effect of capacity on utilization.

The identifying assumption underlying my empirical framework is that unobserved within-hospital-month deviations in admission probability are uncorrelated with within-hospital-month deviations in the number of empty beds. In other words, if the types of infants born in a hospital systematically change when there are shocks to the number of available empty NICU beds, the empirical strategy could overestimate or underestimate the effect of NICU beds on NICU admissions. With this in mind, there seem to be three major potential threats to validity in this context; two of these are mechanical in nature, and the third is related to potential behavioral responses. I discuss these potential threats here and provide empirical evidence that they are unlikely to greatly impact the estimated coefficients in Section IV.

As discussed above, the hospital-month fixed effects are intended to control for the cyclical of infant health. However, months are a coarse and arbitrary unit of time, and there still may be within-month serial correlation in infant health. If this were the case, my estimates would *underestimate* the effect of availability on utilization. This type of serial correlation would imply that infants born on days in which the NICU has become more empty due to previously born infants being healthy would be less likely to need NICU admission, as they themselves are healthier than infants born in preceding days. Alternatively, the hospital-month fixed effects may appropriately control for cyclical in infant health, but mean reversion may create a spurious positive correlation between capacity and utilization. In other words, if infants born in recent days have been healthier than average, and therefore, there has been an increase in the number of available empty beds, infants born on subsequent days may simply be more in need of NICU admission due to mean reversion. This would lead me to *overestimate* the effect of capacity on utilization.

An additional potential threat to identification is more related to behavior: selective referral. It could be the case that infants born on days with more empty beds are different from infants born on days with less capacity, because physicians, hospitals, or mothers respond to NICU capacity in decisions about where a delivery will occur. Keeping in mind the hospital-month fixed effects, it would have to be the case that delivery location responds to within-hospital-month shocks to available beds.

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 these errors occur. 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. Additionally, the New York data, where NICU admission is observed directly, do not suffer from this form of measurement error. Therefore, with regard to accuracy of measuring NICU admissions and empty beds, estimates based on the New York data may be considered more reliable. That being said, these concerns are minimized by the fact that the results presented in the next section are quite similar in California and New York, and results for New York are similar when I instead impute NICU admission.

IV. Main Results

A. Summary Statistics

Table 1 lists sample means for a selection of observed characteristics from 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 15.5% (13.2%) of newborns are admitted to the NICU, 77.2% (85.8%) of VLBW newborns and 55.1% (53.2%) of LBW newborns are admitted. There are also noticeable differences across birth weight samples in health-related characteristics. For example, infants born at lower birth weights are more likely to be multiple births, have congenital anomalies, or be diagnosed with a clinical condition.¹³ Overall, these health characteristics are similar across California and New York.

There are some small differences in demographic characteristics across the birth weight samples. The three lightest groups are more likely to be covered by Medicaid, and VLBW and LBW infants have higher proportions of black mothers. On the other hand, the heavier groups have higher proportions of Hispanic mothers. Demographics are different across states: California has a much higher concentration of Hispanic births and New York has a much higher concentration of black births.

Table 2 provides summary statistics of the NICU environment 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, with a standard deviation of 17.278 (13.668). On average, there are 2.547 (9.979) empty beds available in

¹³Clinical 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).

the NICU, with a standard deviation of 8.887 (9.889).¹⁴ While these numbers describe the baseline NICU environment, the identification strategy is based on within hospital-month changes. The third row of Table 2 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 3.073 in California and 2.757 in New York. At the 25th percentile newborns in California face 1.667 fewer empty beds than the hospital-month average, and at the 75th percentile they face 1.696 more beds than the hospital-month average, while this range is -1.488 to 1.516 in New York.

B. 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 as described by Equation (2). The main regression results are presented in Table 3, 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. Throughout the paper all coefficient estimates from regressions with binary dependent variables are multiplied by one hundred and presented in percentage point terms. 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 any 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. Column 3 adds 250-gram birth weight dummies and Column 4 adds all other controls, including day of week dummies insurance status,¹⁵ race, ethnicity, mother's age and education, gender, multiple births, parity, prenatal care variables, the presence of congenital anomalies and other clinical conditions.¹⁶ 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.¹⁷ However, after adding birth weight controls, the coefficient estimates remain stable with the addition of all other observed characteristics. If there are any differences in health characteristics associated with empty beds, they appear to be fully accounted for by including birth weight controls, suggesting that empty beds are not correlated with observed characteristics after conditioning on hospital-specific time effects and birth weight.

¹⁴In California the 25th percentile of empty beds is -2 . This may be partially driven by my assumption in the admission algorithm that an infant spends its entire hospital stay in the NICU. That said, the California Utilization Data File also suggests that the number of bed-days used exceeds bed-days available in 24% of hospital-year observations. Table 6 shows that my main results are robust to excluding hospitals frequently near their capacity constraint from the analysis, which also removes most of the negative empty bed values.

¹⁵Online Appendix Table A.5 presents the fraction of births by insurance status and year for each state. Note that the large discrete change in managed care insurance in California in 1995 is a function of a change in how the primary payer is coded in the discharge data. Prior to 1995, I include all Blue Cross/Blue Shield (BC/BS) plans as private, non-managed care. Beginning in 1995, BC/BS managed care plans can be separately identified, and I code them as managed care plans in these later years. Online Appendix Table A.2 reports the main results are robust to estimation separately by time period.

¹⁶More specifically, these controls include mother's age and 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), race and ethnicity indicators (black, other race, and Hispanic), sex, parity (CA only), a multiple birth indicator, month prenatal care began (CA only), number of prenatal care visits (CA only), and indicators for having a congenital anomaly, a clinical condition, being small for gestational age, and being large for gestational age.

¹⁷In results available upon request from the author, the results are not further affected by including finer-grained birth weight dummies at 50-gram increments.

Focusing on the main results for California with all controls included in Column 4, an additional empty bed leads to a 0.16 percentage point increase in the probability of NICU admission. Relative to the overall mean rate of admission, this represents an effect of 1.01% 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 0.50 percentage points, respectively). They are lower for the two NBW samples and increase slightly for the HBW sample. 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 0.90% for LBW infants, 1.15% and 0.86% for the two NBW groups, and 1.13% for the HBW group.

The results for New York are presented in Panel B. While the New York estimates are less precise due to smaller sample sizes, the coefficient estimates are quite similar across states with two exceptions. The New York VLBW coefficient is not statistically significant and is about a third of the magnitude of the California VLBW coefficient. Second, the coefficient for the second group of NBW infants is not statistically significant in New York. This comparison across states suggests that the inherent measurement error resulting from NICU admission imputation in California is likely not driving the main findings, but does suggest the statistically significant effect for VLBW and heavier normal birth weight infants are less robust. Online Appendix Table A.6 also shows that results for New York are quite similar when I instead impute NICU admission following the same algorithm performed on the California data.¹⁸

To further disaggregate the effect, I estimate Equation (2) 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. 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 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

¹⁸The correlation coefficient between the imputed and actual admission indicators is 0.838, with 3% of infants being “misclassified” by the imputation algorithm.

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. The California data set also includes a measure of gestational age, another proxy for an infant's health at the time of birth. I find that the estimated patterns are similar when stratified by gestational age rather than birth weight, as shown in Online Appendix Figure A.2. 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 discretion and for external factors to impact admission decisions.

In order to get a better sense of the magnitude of these estimates, I perform the following thought exercise. My main estimates for the full sample imply that one empty bed increases NICU admission by about 1% in both states. Assuming that my point estimates represent the average treatment effect of capacity on NICU admission, I consider what my estimates imply about the fraction of total NICU admissions that occur because empty beds are available. As described in Table 2, the average infant in California is born with 2.5 empty beds available in the NICU and the average infant in New York is born with 10 empty beds available. To be conservative I present a range of calculations assuming infants are born on days with between 2.5 and 10 empty beds.¹⁹

Given my estimate that one empty bed increases NICU admission by 1% and the number of empty beds faced by the average infant, my estimates imply that between 2.5% and 10% of NICU patients are admitted to the NICU due to the number of beds available when they are born. This is equivalent to between 1,200 and 4,900 additional admissions per year in California and between 300 and 1,200 per year in New York.

C. Additional Identification Checks

As discussed in Section III the key identifying assumption of my estimates is that within-hospital-month variation in empty beds is uncorrelated with within-hospital-month variation in unobserved determinants of NICU admission. While it is impossible to truly test the validity of this assumption, I provide some suggestive evidence here. I discuss tests that generally speak to all three threats to validity discussed above, namely serial correlation, mean reversion, and selective referral, by examining the correlation between empty NICU beds and observed infant characteristics. I then briefly discuss some direct tests related to selective referral.

The fact that, once birth weight is controlled for, my estimates are virtually unchanged as observed characteristics are added in Table 3 suggests that further observed NICU

¹⁹The absolute number of available beds in California is likely to be an underestimate since, as I discussed in Section II, I do not observe exact discharge timings. Using these numbers to assess the effect of overall capacity on utilization may also produce an underestimate since they reflect infants admitted in response to capacity.

determinants are not meaningfully correlated with the identifying variation in empty beds. Here I more directly examine these correlations. I take a regression approach and present a series of estimates of Equation (2), where the dependent variables in each regression are one of the observed characteristics. These regressions control only for hospital-month fixed effects and for the 250 gram birth weight fixed effects, since the coefficients above became stable after adding birth weight controls.

While I run these regressions for all control variables included in the main specification along with birth weight and gestation (30 in California and 22 in New York), I present the results of a representative selection of these regression estimates in Table 4. While I do find some coefficients to be statistically significant at the 5% level, most of these effects are quite small. For example, an additional empty bed is associated with an increase in the likelihood of multiple births by 0.03 and 0.04 percentage points in California and New York, respectively, and an increase in the likelihood of a congenital anomaly by 0.01 percentage points in both states. When I take into account the multiple outcomes being tested in these regressions via a Bonferroni correction, only mother's education, multiple birth, and clinical condition (driven by large for gestational age) in CA and multiple births in NY remain statistically significant at the 5% level.

These estimates suggest that empty beds may have a small negative correlation with observed infant health, consistent with either mean reversion or selective referral. However, the stability of the main estimates to the addition of controls beyond birth weight suggests that any correlation between observed characteristics and empty beds is very small and does not impact the relationship between empty beds and NICU admission.

I also provide some evidence on selective referral more specifically. While I cannot observe women who are turned away from a hospital or referred to a different hospital in response to NICU capacity, I do observe women who are admitted at one hospital and transferred to a different hospital prior to delivery in California. Of the births in my sample, only 0.24% of mothers were transferred to the delivery hospital from a different hospital that also operated a NICU on the same day as the delivery; however, 3.67% of VLBW and 0.74% of LBW mothers were transferred.

In results presented in Online Appendix Table A.7 I test whether maternal transfer decisions are correlated with empty beds at the first hospital she is admitted to. I find a very small negative and statistically significant coefficient for mothers of VLBW infants and no statistically significant effect for LBW infants. Additionally, I find that estimates of my main specification are robust to excluding infants whose mothers are transferred prior to delivery. These results suggest that maternal transfers are unlikely to bias my results.

If it were the case that hospitals turn away deliveries prior to the mother being admitted, or if physicians direct women to hospitals as a function of NICU capacity, we might expect this to occur most often when it is more feasible for the woman to travel to a different hospital. I therefore estimate specifications in which I interact empty beds at the delivery hospital with the number of miles from that hospital to the nearest other hospital with a NICU. If this type of selection is influencing my estimates, and therefore deliveries of infants more likely to

need NICU care are diverted to other hospitals when capacity is low, we would expect larger positive effects of empty beds on NICU admission in hospitals with close neighbors. If anything, I find the opposite to be true in results presented in Online Appendix Table A.8. The coefficients on the interaction term between empty beds and distance are positive and statistically significant in California and not statistically significant in New York. These results suggest that the hospitals that could most feasibly turn mothers away in response to NICU capacity do not treat sicker patients on days in when beds are available.

V. Mechanisms

A positive effect of empty NICU beds on NICU admission does not necessarily imply that excess capacity leads to excessive utilization of NICUs due to financial incentives. As discussed in Section I, health related option values and congestion externalities could all contribute to this relationship. It may also be the case that the estimated effect is at least partially driven by infants being denied potentially necessary and valuable NICU care when a NICU is full and therefore capacity-constrained. In this section I provide a variety of evidence that, while part of the above estimates may be due to these other mechanisms and capacity constraints, financial incentives are an important contributor to the relationship between empty beds and NICU admissions.

A. Inter-Hospital Transfer

Infants who need neonatal intensive care not available at their birth hospital 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 birth hospital's NICU is crowded, I will overestimate the effect of empty beds on NICU admission, as these transferred infants (and eventual NICU patients) will be considered to not be admitted. If, however, I still find effects of capacity after accounting for inter-hospital transfers, it would suggest that capacity constraints are not the main drivers of the results.

This prediction fits with a slightly adapted version of the model described in Section I where the physician faces three choices for the infant's location of care: the normal nursery, the NICU in the current hospital, or a different hospital. The physician would again consider financial incentives, non-financial incentives, and the patient's utility and choose the option with the overall greatest utility. Such a model suggests two birth weight thresholds: one for admitting the patient to the current hospital's NICU and one for transferring to another hospital. When capacity is constrained, the health benefits of transferring to another hospital are likely to outweigh the financial benefits of keeping the infant at the original hospital.

Table 5 provides estimates of the effect of empty beds on whether or not an infant is transferred to another hospital. In California empty NICU beds has a negative and statistically significant impact on the probability of ever being transferred (Column 1) for all subsamples except HBW infants. However, the effect size is extremely small in magnitude for infants above the low birth weight threshold. In New York, empty beds only lead to a statistically significant decrease in transfer rates for VLBW and LBW infants.

For VLBW infants in California, transfers mitigate a large portion of the effect of empty beds on NICU admissions. Column 2 estimates the effect of empty beds on an indicator variable that is equal to one if the infant is admitted to the NICU *or* is transferred. Compared to the baseline estimates in the last column of Table 3 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 0.50 to 0.45 percentage points. Not surprisingly, transfers do not mitigate the effect of empty beds for heavier infants. These results suggest that if hospitals transfer infants when it is medically necessary, much of the estimated effect of empty beds on NICU admission for infants above the VLBW threshold is not likely to be driven by capacity constraints.

As an extension of this analysis, I also consider how available capacity at nearby NICUs impacts whether an infant ultimately receives neonatal intensive care. If the positive effect of capacity on NICU admission is driven by capacity constraints at the birth hospital, we might expect capacity at nearby hospitals to also impact NICU treatment decisions as physicians attempt to place the infant in a NICU with an available bed. Alternatively, if financial incentives drive the relationship between capacity and utilization, we might expect the birth hospital's capacity to have a much stronger effect on NICU admission than capacity at surrounding hospitals. In order to explore this, I regress the indicator for NICU admission or transfer on the number of available NICU beds at the delivery hospital and the three closest hospitals.²⁰ In results presented in Online Appendix Table A.8 I find the coefficients on nearby NICU capacity to be very close to zero and not statistically significant in California. Some of the coefficients are statistically significant in New York, but the point estimates are much smaller than the estimated effect of the birth hospital's capacity. Overall these results suggest that capacity at the focal hospital is by far the most dominant capacity-related driver of NICU admissions.

B. Excluding Hospitals Near Capacity Constraints

In order to directly test whether capacity constraints drive the results, I estimate regressions in which I exclude hospitals that frequently operate near their capacity constraints. These results also speak to the role of congestion externalities, which may be more salient when hospitals are near capacity constraints. In Table 6 I exclude hospital-year pairs in which the median daily fraction of empty beds is less than 20%. In both states I find patterns of results similar to the main estimates. This finding suggests that variation in empty beds away from NICU capacity constraints does lead to changes in NICU admission decisions. These results do not conclusively imply that capacity constraints and congestion externalities are not partially driving the main results, as I do find positive point estimates when estimating these regressions using just the sample excluded from Table 6; however, they do suggest that empty beds increase NICU admission when these mechanisms are least likely to be relevant and financial incentives are still salient.

²⁰In both states, the mean number of empty beds at nearby hospitals is a bit larger than the mean number of empty beds at the birth hospital. Capacity at nearby hospitals is also not highly correlated with capacity at the birth hospital, suggesting there is sufficient independent variation to identify the effect of each capacity measure. The correlation coefficients between residualized empty beds at the birth hospital and each of the three nearest hospitals are between -0.009 and 0.008 .

C. Incentives and Heterogeneity

As discussed above in Section I, there are a number of financial incentives that may cause the NICU admission birth weight threshold (b^*) to vary with available NICU capacity. In this section, I explore how NICU admission responds to empty beds differentially across both hospital and patient characteristics related to these mechanisms.

Hospital ownership may impact how NICU admission decisions respond to available capacity. We might expect that for-profit hospitals respond to financial incentives in their treatment decisions more than government hospitals. Predictions about privately owned non-profit hospitals are ambiguous, as some researchers have found them to be similar to for-profits in their responses to financial incentives and others have found them to be less responsive.²¹ In Column 1 of Table 7 I find that all three ownership types increase NICU admissions in response to empty beds; however, the effect is statistically significantly larger among for-profit hospitals.

Teaching status may also impact responses to financial incentives, since many physicians at teaching hospitals are typically salaried. Additionally, teaching hospitals' missions to serve the community and provide education may lead them to be less responsive to financial incentives. In line with these predictions, I find that teaching hospitals in California (where I can observe teaching status in my hospital-level data) are statistically significantly less responsive to available capacity in Column 2 of Table 7.²²

Finally, I explore differential effects by NICU size. Hospitals with smaller NICUs may respond more strongly to the number of empty beds, since one empty bed likely represents a larger share of revenue than it would in hospitals with larger NICUs. Additionally, smaller NICUs are likely to face more financial stress. As discussed in Section I, smaller NICUs may also be more responsive to capacity since the NICU team is more likely to be directly involved in earlier stages of the infant's care. In Column 3 of Table 7 I do in fact find the effect of empty beds on NICU admission to decrease with NICU size in California. However, this does not appear to be the case in New York, where the interaction effects are statistically insignificant and the largest effects appear to occur in medium-sized NICUs.

At the patient level, type of insurance may play an important role in determining the financial incentives of physicians and hospitals. Among the privately insured, managed care organizations provide fewer financial incentives that are directly tied to treatment and provide tighter utilization monitoring. Almost all births not covered by private insurance are covered by Medicaid, which typically reimburses less generously than private insurers.²³ In

²¹Duggan (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. On the other hand Dafny (2005) finds that for-profit hospitals are more responsive to upcoding incentives, and Horwitz (2005) finds that for-profit hospitals are more responsive to profitability in service line provision decisions.

²²Note, teaching hospitals are all either non-profit or government owned. I therefore exclude for-profit hospitals from this regression so as not to conflate the effect of teaching status with ownership status.

²³While not specific to NICU care, data used in Currie, Gruber and Fischer (1995) and Freedman, Lin and Simon (2014) reveal that in 1992 Medicaid physician payments for total obstetric care of a vaginal birth were 34% and 24% of private payments in California and New York, respectively. New York Medicaid reimburses hospitals prospectively based on the AP-DRG system, which is designed to better match reimbursements to hospital costs for younger patients. California Medicaid hospital payments during my study period were based on flat per diem rates negotiated with each hospital, with some hospitals negotiating higher rates for specific types of care, including NICUs.

Column 4 of Table 7 I find that the effect of empty beds on NICU admission is smaller for managed care patients in California. Note, because they do not report hospital charges, my sample excludes all Kaiser hospitals in California, so I cannot test the effect of empty beds on NICU admission for this strongest form of managed care. The New York data includes HMO as an insurance status rather than managed care more broadly; however, I find no statistically significant difference in the response for HMOs in New York.

The results for Medicaid patients are not statistically different from those for privately insured patients in both states. Even if Medicaid reimburses neonatal intensive care less generously than private insurance companies do, the availability of empty beds still increases the probability of NICU admission for Medicaid patients at the margin. This suggests that perhaps other mechanisms, such as congestion externalities and capacity constraints are contributing to these effects for Medicaid patients.

In addition to comparing Medicaid covered infants to privately covered infants, there are potentially important differences in financial incentives within Medicaid. As documented by Duggan (2004) and Aizer, Currie and Moretti (2007) Medicaid managed care coverage increased greatly during the time period covered by my data, leading to a shift in Medicaid reimbursements from fee-for-service payments to various agreements negotiated by private insurance plans. As I have found that hospitals respond less strongly to empty beds for private managed care patients, we may expect smaller responses for Medicaid managed care patients as compared to Medicaid fee-for-service patients.

In my hospital inpatient data, I can separately identify infants whose primary payer is a Medicaid managed care plan for all years in New York and between 1999 and 2001 in California.²⁴ Column 5 of Table 7 presents results in which I include an interaction between Medicaid managed care and empty beds, restricting the sample to Medicaid infants and years in which I can observe Medicaid managed care plans. While I find no statistically significant difference between Medicaid managed care and traditional Medicaid in New York, I do find smaller effects of empty beds on NICU admission for Medicaid managed care infants in California.

Overall, I find the effect of empty beds on NICU admission to be stronger in hospitals with stronger financial incentives. I also find the effect to be weaker for patients with managed care insurance in California, both among the Medicaid and the privately insured populations. These results again suggest that financial incentives are important determinants of the effects that I find; however, this conclusion is weaker in New York where I do not find differences by insurance status.

D. Utilization and Health Outcomes

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 and infant health outcomes. I estimate

²⁴In New York, the fraction of my analysis sample covered by Medicaid managed care increases from 2% to 13%, accounting for 29% of Medicaid covered observations by 2002. In CA, Medicaid managed care accounts for roughly 30% of Medicaid observations during the three years in which I am able to observe it.

regressions similar to those in Equation (2) with other measures of hospital resource use, mortality, and hospital readmission as dependent variables. 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 for noncompliers and 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 or infants who would have been admitted to the NICU regardless of capacity.

Table 8 presents reduced-form estimates of the effect of empty beds on length of stay and hospital charges. The results for both 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 in additional charges. The effects are largest for VLBW and LBW infants who accumulate about \$1,000 and between \$225 and \$300 in additional charges for each empty bed, respectively.²⁵ The increase in length of stay and charges for VLBW infants suggests that at least part of this effect may be working through channels other than NICU admission; however, these results provide suggestive evidence that, in addition to a higher likelihood of NICU admission, capacity leads to additional hospital resource utilization.

I also provide suggestive evidence on the health effects of empty beds, which could be ambiguous depending on the value of NICU care for the marginal infant and potential adverse effects of unnecessary admissions. The California data set links the patient discharge records to vital statistics data on infant deaths within a year of life and subsequent hospital readmissions. The mean one-year mortality rate is 24.26% for VLBW infants and drops to 1.8% for LBW infants and below 0.5% for all other groups. Because mortality is not likely to capture the health implications of a marginal NICU admission for higher birth weight infants, I also utilize 28-day hospital readmission rates as a health outcome. Approximately 3% of infants that are above the VLBW threshold experience a readmission to the hospital within 28 days of birth.²⁶

Not surprisingly, I find no statistically significant effect of empty beds on mortality for any of the normal or higher birth weight subsamples in Table 8. I also find no evidence that empty beds impact hospital readmission rates for any of the birth weight subgroups, but when the full sample is pooled I find a statistically significant positive effect. While empty beds lead to increased utilization for higher birth weight infants, this utilization may not be health improving and may have potential adverse effects.

I do, however, find that each additional bed reduces mortality by 0.039 percentage points for LBW infants (2.2% of the sample mean) and 0.166 percentage points for VLBW infants (0.68% of the sample mean). These magnitudes are large. Taken together with the NICU admission results, they would imply that an empty bed induced NICU admission could decrease mortality by 7.8 and 53.7 percentage points, respectively, effect sizes which are far

²⁵Results using log of length of stay and charges lead to similar conclusions.

²⁶Evans, Garthwaite and Wei (2008) point out that hospital readmission is an outcome that can often be prevented by appropriate treatment in an infant's initial hospital stay.

greater than sample mean mortality rates. However, these mortality effects are likely to reflect impacts for non-compliers and other channels beyond NICU admission, so they should likely not be interpreted as the treatment effect of NICU admission.

With these caveats in mind, I also compare the effect of empty beds on hospital charges to the impact on mortality to calculate the implied 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.²⁷ I estimate the cost of a life saved for the VLBW sample as \$237,579 (95% CI of \$33,328 to \$441,831) and for the LBW sample as \$370,467 (95% CI of \$34,356 to \$706,578). These estimates are smaller than Almond et al. (2010)'s estimate of about \$600,000 (95% CI of \$30,000 to \$1.2 million). Note that these estimates only account for hospital costs and not physician costs. That being said, they are still 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 \$609,176 (95% CI of \$227,837 to \$990,516). This is still much lower than the value of a life, but would be even lower if capacity did not lead to additional utilization for higher birth weight infants.

Again, these results should be interpreted with caution. They may not fully represent costs and benefits of the NICU itself if available beds impact overall utilization and health outcomes through channels other than NICU admission. Additionally, hospital charges, even deflated by cost-to-charge ratios, may not represent true resource utilization if hospitals admit marginal infants to the NICU in order to increase reimbursements without providing much actual care.

VI. Conclusion

The effect of the availability of medical resources on their rate of utilization is difficult to identify. This paper examines this question in the context of neonatal intensive care, an important and interesting setting due to recent increases 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, the cyclicity and trends in infant health, and the cyclicity and trends specific to each hospital.

I find that, on average, an additional empty NICU bed increases the probability of NICU admission by about 1% in both states. The magnitude of the effect is small for VLBW

²⁷Due to data availability, I am only able to calculate these ratios beginning in 1995. Mortality and charges estimates for the sample with cost information are similar to the full sample results.

infants in California and not statistically significant in New York, where there is less measurement error in NICU admissions and empty beds. Allowing for the fact that many VLBW infants are transferred 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.

I also provide evidence that my estimates are not being solely driven by crowded NICUs denying care and that the effect of empty beds on admission is larger when financial incentives are expected to be the most salient. I provide suggestive evidence that available beds increase length of stay and charges. While I do find that available beds decrease mortality rates for low and very low birth weight infants, these estimates may reflect health effects through channels other than NICU admission.

These estimates suggest that the availability of NICU beds leads to additional NICU 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. The results of this paper also suggest that the positive correlation between capacity and utilization found in aggregate summary statistics of the NICU market and in other contexts is likely not fully spurious. Given these marginal responses to available capacity, these results speak to the dynamic incentives for continued investment in high-fixed-cost technologies. This paper provides motivation for further research on how the ability to profitably utilize available capacity provides incentives for potential further overinvestment in additional capacity.

While these findings are interesting in the context of neonatal intensive care, they may apply to any type of care in which there is a choice about the “level of care” a patient should receive, particularly when the more intense level of care is provided in a separate unit of the hospital with its own defined capacity. For example, these results may apply to adult intensive care admissions or admission to the hospital from the emergency department. These results may also apply to contexts in which hospitals invest in a high fixed cost technology and then face incentives to utilize the technology rather than let it sit idle. Such technologies could range from MRI machines, for which Baras and Baker (2009) have found a correlation between regional changes in supply and utilization, to recent high cost innovations such as robotic surgery equipment and proton therapy centers. My results motivate further research regarding the relationship between supply and utilization in other areas of hospital care.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Figure 1.

AGGREGATE TRENDS IN NICU CAPACITY AND UTILIZATION

Note: This figure plots the annual number of total NICU beds and total patients discharged from NICUs in each state. The number of beds is measured on the left-hand-side axis, while NICU discharges are measured on the right-hand-side axis.

Source: Statistics are calculated from the California State Utilization Data File of Hospitals and the New York Institutional Cost Reports.

Figure 2.

EFFECT OF EMPTY BEDS ON NICU ADMISSION BY BIRTH WEIGHT

Note: Panel A 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 3, including hospital-specific month fixed effects. All standard errors are clustered at the hospital level. Panel B plots these coefficient estimates divided by the NICU admission rate of each birth weight subgroup.

Table 1

Sample Means by Birth Weight Sample

	All	Very Low Birth Weight	Low Birth Weight	Normal Birth Weight 1	Normal Birth Weight 2	High Birth Weight
Panel A: California						
NICU Admission	0.155	0.772	0.551	0.133	0.110	0.145
Mother's Demographics						
Age	27.5	27.9	27.7	27.0	27.6	28.6
Medicaid	0.475	0.495	0.512	0.502	0.461	0.432
Self Pay	0.037	0.029	0.035	0.040	0.037	0.032
Black	0.074	0.168	0.136	0.092	0.059	0.044
Hispanic	0.474	0.440	0.427	0.469	0.485	0.471
Pregnancy Characteristics						
Male	0.512	0.510	0.478	0.454	0.530	0.628
Multiple Birth	0.028	0.238	0.219	0.034	0.003	0.000
Infant Characteristics						
Congenital Anomaly	0.010	0.090	0.035	0.009	0.006	0.007
Clinical Condition	0.107	0.268	0.166	0.052	0.065	0.437
Birth Weight (Grams)	3338	993	2171	2973	3578	4273
Gestation (Weeks)	39.5	29.4	36.3	39.2	40.0	40.4
N	3,131,948	42,040	173,895	1,032,399	1,560,110	323,504
Panel B: New York						
NICU Admission	0.132	0.858	0.532	0.097	0.072	0.112
Mother's Demographics						
Age	28.7	28.5	28.1	28.1	28.9	29.9
Medicaid	0.378	0.421	0.412	0.413	0.359	0.312
Self Pay	0.052	0.044	0.052	0.057	0.050	0.046
Black	0.197	0.319	0.258	0.225	0.174	0.146
Hispanic	0.105	0.085	0.101	0.110	0.105	0.096
Pregnancy Characteristics						
Male	0.513	0.502	0.476	0.457	0.536	0.635
Multiple Birth	0.039	0.267	0.251	0.041	0.004	0.001
Infant Characteristics						
Congenital Anomaly	0.013	0.103	0.039	0.011	0.007	0.010
Clinical Condition	0.086	0.193	0.114	0.028	0.047	0.448
Birth Weight (Grams)	3281	998	2152	2963	3572	4289
Gestation (Weeks)	—	—	—	—	—	—
N	687,086	13,764	47,515	236,578	323,340	65,889

Note: This table presents sample means for the full sample and for each of the five birth weight subsamples in each state. Subsamples include very low birth weight (VLBW, below 1,500g), low birth weight (LBW, 1,500-2,500g), normal birth weight 1 (NBW1, 2,500-3,250g), normal birth weight 2 (NBW2, 3,250-4,000g), and high birth weight (HBW, above 4,000g). Dashes indicate variables not available in the New York SPARCS inpatient data set.

Table 2

Summary Statistics of Empty Beds

	Mean	Standard Deviation	25th Percentile	50th Percentile	75th Percentile
California Full Sample (N=3,131,948)					
NICU Beds	21.469	17.278	8.000	16.000	28.000
Empty NICU Beds	2.547	8.887	-2.000	2.000	7.000
Residual Empty Beds	0.000	3.073	-1.667	0.059	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

Note: 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

Effect of Empty Beds on NICU Admission

	P (Admit) and Sample Size	Regression Coefficients <i>Dep. Var.: NICU Admission</i>			Relative Effect
	(1)	(2)	(3)	(4)	(5)
Panel A: California					
Full Sample	15.5 3,131,948	0.250 ** (0.021)	0.162 ** (0.014)	0.157 ** (0.014)	1.01%
VLBW (0 to 1,499 G)	77.2 42,040	0.492 ** (0.075)	0.346 ** (0.056)	0.309 ** (0.053)	0.40%
LBW (1,500 to 2,499 G)	55.1 173,895	0.749 ** (0.085)	0.525 ** (0.060)	0.497 ** (0.056)	0.90%
NBW1 (2,500 to 3,249 G)	13.3 1,032,399	0.162 ** (0.014)	0.159 ** (0.014)	0.153 ** (0.013)	1.15%
NBW2 (3,250 to 4,000 G)	11.0 1,560,110	0.097 ** (0.013)	0.097 ** (0.013)	0.095 ** (0.012)	0.86%
HBW (4,000+ G)	14.5 323,504	0.174 ** (0.023)	0.170 ** (0.023)	0.163 ** (0.023)	1.13%
Panel B: New York					
Full Sample	13.2 687,086	0.305 ** (0.043)	0.151 ** (0.034)	0.141 ** (0.034)	1.07%
VLBW (0 to 1,499 G)	85.8 13,764	0.194 ** (0.088)	0.135 (0.085)	0.128 (0.084)	0.15%
LBW (1,500 to 2,499 G)	53.2 47,515	0.791 ** (0.124)	0.501 ** (0.099)	0.494 ** (0.095)	0.93%
NBW1 (2,500 to 3,249 G)	9.67 236,578	0.157 ** (0.035)	0.160 ** (0.034)	0.134 ** (0.032)	1.39%
NBW2 (3,250 to 4,000 G)	7.19 323,340	0.055 (0.041)	0.054 (0.041)	0.054 (0.041)	0.76%
HBW (4,000+ G)	11.2 65,889	0.121 ** (0.056)	0.119 ** (0.055)	0.095 ** (0.055)	0.84%
Hospital-Month FE		X	X	X	
Birth Weight FE			X	X	
All Controls				X	

Note: Each row presents coefficient estimates in percentage points 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. All controls include day-of-week fixed effects, mother's age, mother's age squared, race, ethnicity, insurance coverage, number of prenatal care visits (CA only), month in which prenatal care began (CA only), parity (CA only), sex, multiple birth status, an indicator for having a congenital anomaly, an indicator for having a clinical condition, and indicators for small and large for gestational age.

*Significant at the 10 percent level

**
Significant at the 5 percent level

Table 4

Empty Beds and Observed Characteristics

<i>Dependent Variable</i>	Panel A: California		Panel B: New York	
	Coefficient	Standard Error	Coefficient	Standard Error
Mother's Demographics				
Age	0.002 **	(0.001)	0.005	(0.003)
Medicaid	0.012	(0.009)	−0.014	(0.016)
Self Pay	−0.004	(0.003)	−0.010	(0.008)
Black	0.005	(0.005)	−0.005	(0.014)
Hispanic	0.003	(0.008)	0.005	(0.009)
Pregnancy Characteristics				
Male	0.005	(0.010)	0.011	(0.021)
Multiple Birth	0.031 ** ^a	(0.005)	0.039 ** ^a	(0.008)
Infant Characteristics				
Congenital Anomaly	0.006 **	(0.002)	0.012 **	(0.004)
Clinical Condition	0.028 ** ^a	(0.004)	0.010	(0.010)
Birth Weight (Grams)	−0.024 *	(0.014)	−0.065	(0.045)
Gestation (Weeks)	−0.002 **	(0.001)	—	—

Note: Each row presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of each observed characteristic on the number of empty beds for the full sample in each state. Coefficients from models with binary dependent variables are in percentage points. All specifications include hospital-specific month fixed effects and birth weight fixed effects.

** Significant at the 5 percent level

* Significant at the 10 percent level

^a Significant at the 5% level after a Bonferroni correction based on the full set of 30 regressions in California and 22 regressions in New York.

Table 5

Inter-Hospital Transfers

<i>Dependent Variable:</i>	Panel A: California		Panel B: New York	
	Transfer	Admit or Transfer	Transfer	Admit or Transfer
	(1)	(2)	(1)	(2)
Full Sample	−0.014 ** (0.003)	0.145 ** (0.013)	−0.003 (0.002)	0.138 ** (0.034)
VLBW (0 to 1,499 G)	−0.141 ** (0.058)	0.199 ** (0.045)	−0.011 (0.082)	0.103 (0.080)
LBW (1,500 to 2,499 G)	−0.086 ** (0.018)	0.451 ** (0.053)	−0.015 (0.024)	0.487 ** (0.094)
NBW1 (2,500 to 3,249 G)	−0.007 ** (0.003)	0.145 ** (0.013)	−0.003 (0.003)	0.132 ** (0.031)
NBW2 (3,250 to 4,000 G)	−0.003 * (0.002)	0.091 ** (0.012)	0.001 (0.002)	0.053 (0.042)
HBW (4,000+ G)	−0.003 (0.003)	0.162 ** (0.023)	0.001 (0.004)	0.096 * (0.055)

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

** Significant at the 5 percent level

* Significant at the 10 percent level

Table 6

Excluding Hospitals Frequently Near Capacity

<i>Dependent Variable: NICU Admission</i>	California	New York
	(1)	(2)
Full Sample	0.205 ^{**} (0.021)	0.159 ^{**} (0.026)
N	1,455,665	550,130
VLBW (0 to 1,499 Grams)	0.439 ^{**} (0.110)	0.132 (0.099)
N	16,413	10,823
LBW (1,500 to 2,499 Grams)	0.631 ^{**} (0.097)	0.402 ^{**} (0.106)
N	75,224	37,596
NBW1 (2,500 to 3,249 Grams)	0.185 ^{**} (0.023)	0.152 ^{**} (0.030)
N	473,466	190,578
NBW2 (3,250 to 4,000 Grams)	0.135 ^{**} (0.016)	0.081 ^{**} (0.022)
N	735,319	258,979
HBW (4,000+ Grams)	0.209 ^{**} (0.043)	0.146 ^{**} (0.060)
N	155,243	52,154

Note: Each row presents coefficient estimates in percentage points 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. Samples in this table exclude hospital-year pairs in which the daily median fraction of empty beds is less than 20%. Specifications include all control variables described in the notes to Table 3, including hospital-specific month fixed effects.

*Significant at the 10 percent level

**
Significant at the 5 percent level

Table 7**Heterogeneity by Hospital and Patient Characteristics**

	Hospital Ownership	Teaching Status	NICU Size	Payer	Medicaid Managed Care
	(1)	(2)	(3)	(4)	(5)
Panel A: California					
Empty Beds	0.140 ** (0.015)	0.180 ** (0.018)	0.285 ** (0.023)	0.193 ** (0.030)	0.193 ** (0.023)
Empty Beds X					
Government Owned	0.038 (0.032)				
For Profit Owned	0.133 ** (0.047)				
Non-Profit Teaching Hospital		-0.084 ** (0.028)			
Medium NICU			-0.119 ** (0.027)		
Large NICU			-0.181 ** (0.028)		
Managed Care (HMO in NY)				-0.057 ** (0.023)	
Medicaid				-0.028 (0.036)	
Other				-0.015 (0.045)	
Medicaid Managed Care					-0.082 ** (0.021)
N	3,131,948	2,760,024	3,131,948	3,131,948	403,276
Panel B: New York					
Empty Beds	0.122 ** (0.038)	—	0.051 (0.218)	0.144 ** (0.033)	0.203 ** (0.033)
Empty Beds X					
Government Owned	0.091 (0.087)				
For Profit Owned	0.583 ** (0.041)				
Non-Profit Teaching Hospital		—			
Medium NICU			0.249 (0.233)		
Large NICU			0.087 (0.220)		
Managed Care (HMO in NY)				-0.006 (0.012)	
Medicaid				-0.009 (0.012)	
Other				0.019 (0.035)	
Medicaid Managed Care					0.041 (0.028)
N	687,086	—	687,086	687,086	259,646

Note: Each column presents coefficient estimates in percentage points with standard errors in parenthesis (clustered at the hospital level) in which empty beds is interacted with various hospital and patient characteristics. Each row includes the full sample except Column 2, which excludes for-profit hospitals, and Column 5, which includes Medicaid covered patients in years that Medicaid managed care is observable in the data. Specifications include all control variables described in the notes to Table 3, including hospital-specific month fixed effects.

*Significant at the 10 percent level

**
Significant at the 5 percent level

Table 8

Effect of Empty Beds on Length of Stay, Hospital Charges, and Health Outcomes

	Panel A: California				Panel B: New York	
	Length of Stay	Charges	1-Year Mortality	28-Day Readmission	Length of Stay	Charges
	(1)	(2)	(3)	(4)	(1)	(2)
Full Sample	0.017 ** (0.002)	78.7 ** (9.23)	−0.006 ** (0.001)	0.007 ** (0.003)	0.021 ** (0.003)	75.5 ** (11.6)
VLBW (0 to 1,499 Grams)	0.218 ** (0.043)	959 ** (272)	−0.166 ** (0.051)	0.009 (0.013)	0.340 ** (0.110)	1,159 ** (399)
LBW (1,500 to 2,499 Grams)	0.093 ** (0.011)	307 ** (55.3)	−0.039 ** (0.010)	0.001 (0.014)	0.087 ** (0.017)	226 ** (46.7)
NBW1 (2,500 to 3,249 Grams)	0.010 ** (0.001)	42.9 ** (9.13)	0.002 (0.002)	0.007 (0.005)	0.011 ** (0.003)	45.0 ** (7.90)
NBW2 (3,250 to 4,000 Grams)	0.005 ** (0.001)	17.4 ** (4.45)	−0.0003 (0.001)	0.007 * (0.004)	0.006 ** (0.001)	22.9 ** (4.46)
HBW (4,000+ Grams)	0.007 ** (0.002)	30.1 ** (6.89)	−0.0002 (0.003)	0.008 (0.014)	0.005 (0.006)	25.6 (17.9)

Note: 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. Coefficients in Columns 3 and 4 are in percentage points. Specifications include all control variables described in the notes to Table 3, including hospital-specific month fixed effects.

** Significant at the 5 percent level

* Significant at the 10 percent level