ABSTRACT

Title of dissertation: EMPIRICAL ESSAYS ON THE ECONOMICS OF NEONATAL INTENSIVE CARE

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The number of neonatal intensive care units (NICUs) in smaller community hospitals increased greatly during the 1980s and 1990s, attracting deliveries away from hospitals with the most sophisticated NICUs. This pattern of "deregionalization" has caused concern because previous studies find higher mortality rates for high-risk infants born in hospitals with less sophisticated NICUs relative to those born in hospitals with the highest care level. In this dissertation, I provide causal estimates of the effect of deregionalization on infant health outcomes and treatment intensity.

In Chapter 2, I argue that previous estimates of the relationship between the level of care at a high-risk infant's birth hospital and mortality may be biased by unobserved selection. To estimate a causal relationship, I use an instrumental variable strategy that exploits exogenous variation in distance from a mother's residence to hospitals offering each level of care. My instrumental variable estimates are bounded well below ordinary least squares estimates and are not statistically different from

zero. These results suggest that relocating patients to hospitals with the highest level of care prior to delivery may not lead to improved mortality outcomes, because infants currently born in lower level facilities have higher unobserved mortality risk. I also provide suggestive evidence that inter-hospital transfer after birth is one mechanism by which infants born at the lowest levels of care achieve similar outcomes to those born at higher level hospitals.

In Chapter 3, I test whether additional neonatal intensive care supply leads to excess neonatal intensive care utilization. I exploit within hospital-month variation in the number of vacant NICU beds in an infant's birth hospital the day prior to birth as a source of exogenous variation in supply. I find that the effect of empty beds on NICU admission is positive but very small for the highest risk infants as measured by very low birth weight. However, it is larger for infants with birth weights above this threshold. These results suggest that additional supply of neonatal intensive care resources can lead to increased utilization of intensive care for infants above the very low birth weight threshold.

EMPIRICAL ESSAYS ON THE ECONOMICS OF NEONATAL INTENSIVE CARE

by

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Dedication

To my wife Krista for her never ending love, support, and encouragement throughout this entire process.

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Chapter 1

Introduction

Rising health care costs are a fundamental problem facing the United States economy. Health care currently accounts for about 16% of GDP and is projected to grow to about 19% percent by 2019.[1] This rapid cost growth was one of the primary motivations behind the health reform passed in 2010. One of the major factors behind these rising costs are new medical technologies and service offerings. On average, most of these new technologies have been worthwhile due to the overwhelming improvements in health that they are able to provide (Cutler and McClellan, 2001; Hall and Jones, 2007; Luce et al., 2006; Murphy and Topel, 2003). However, there is often concern that these services are not allocated optimally. The Dartmouth Atlas Project has documented large geographic variation in health expenditures which does not appear to be correlated with health outcomes (Baicker et al., 2006; Baicker and Chandra, 2004b; Fisher et al., 2003a,b; Fuchs, 2004), providing some evidence of "flat-of-the-curve" medicine, in which treatment is provided to the point where the marginal return is below the marginal cost (or even zero).

This dissertation examines the organization of one particular medical service that displays these characteristics: Neonatal Intensive Care Units (NICUs). A NICU is a unit of the hospital that is separate from the traditional newborn nursery and is specially equipped to care for sick, preterm, and underweight infants. The original NICUs of the late 1960s and early 1970s provided incubation and sometimes mechanical ventilation. Since this time, technological innovations have greatly changed medical care for sick infants, and the most sophisticated NICUs are now able to

[1] According to the Centers for Medicare & Medicaid Services: `http://www.cms.gov/NationalHealthExpendData/downloads/proj2009.pdf`, last accessed on May 16, 2010.

provide extensive monitoring, proper nutrition, artificial surfactant, extracorporeal membrane oxygenation (ECMO), and various diagnostic tests and surgical procedures.[2] These innovations have clearly led to improved outcomes for high-risk infants. For example, the 28-day mortality rate for infants weighing 1,000 to 1,499 grams (2.2 and 3.3 pounds) dropped from 52.2% to 6.7% between 1960 and 1990 (Cutler and Meara, 2000).[3]

Recent decades have seen a trend towards "deregionalization" of neonatal intensive care in which many smaller hospitals have adopted NICUs. Despite the large average gains in infant health that have been attributed to NICUs, this trend has worried organizations such as the March of Dimes and the American Academy of Pediatrics because previous studies have found higher mortality rates for high-risk infants born in hospitals with these smaller, less sophisticated NICUs compared to those born in hospitals with "Regional" NICUs (e.g., Cifuentes et al., 2002; Phibbs et al., 2007, 1996). However, the many potential effects of this deregionalization are not well understood. First, in terms of the first-order question of the effects on the health of the high-risk infants NICUs are intended to treat, the previous estimates may in fact be biased by unobserved patient selection into hospitals. Depending on the mechanisms behind and the direction of this selection, the effect of the level of neonatal intensive care at an infant's birth hospital on mortality could be biased in either direction; deregionalization could be more or less detrimental to infant mortality than previously thought. Second, there may be other effects of deregionalization beyond the quality of care delivered to high-risk infants. These effects could include changes in the quality of care of lower risk infants, differences in

[2]Mechanical ventilation assists infants whose lungs have not fully developed to breath. Artificial surfactant treats respiratory distress syndrome by helping the lungs to develop. ECMO machines pump blood out of the infant, oxygenate it, and pump it back into the infant if the infant's heart and lungs are too weak to oxygenate the blood on its own.

[3]Accounting for the costs of these innovations and the value of both lives saved and quality of life for surviving infants, Cutler and Meara (2000) calculate a 510% rate of return to spending on infant health care between 1960 and 1990.

the intensity and cost of care, composition changes in who receives care, and utility gains for mothers who can choose more convenient hospitals offering NICUs.

Given all of these potential effects, understanding the full welfare consequences of deregionalization would be a very difficult undertaking. In this dissertation, I tackle two pieces of this puzzle. In Chapter 2, I revisit the question of how deregionalization has impacted mortality for very low birth weight infants. By exploiting exogenous variation in the distance from where mothers live to the nearest hospital offering each level of neonatal intensive care, I account for potential unobserved selection and estimate the causal effect of the level of care at the birth hospital on very low birth weight infant mortality. Chapter 3 considers the effect of the supply of neonatal intensive care on the level of utilization of these resources. I provide preliminary estimates of the effect of the number of empty NICU beds just prior to birth on the probability an infant is admitted to the NICU. I then examine how this effect varies across the birth weight distribution to differentiate how available supply affects utilization differently for high-risk and low-risk newborns. The remainder of this chapter provides further background information about neonatal intensive care and summarizes the results of Chapters 2 and 3.

As neonatal intensive care developed in the 1970s, few doctors and nurses were trained in neonatology. As a result, specialists were located in regional care centers, typically associated with large teaching hospitals. In 1976 a March of Dimes report recommended that hospitals offering delivery services be classified into three categories with the lowest providing no intensive care, and the highest providing the most complex care and acting as regional referral centers for high-risk mothers and infants (Committee on Perinatal Health, 1976).[4]

[4] In general, Level I nurseries describe hospitals that provide basic birthing service and care for healthy infants. They have the facilities and staff required for neonatal resuscitation, but must stabilize and transfer ill newborns to other facilities for further treatment. Level II nurseries treat moderately ill infants, and Level III units treat infants who are extremely premature, critically ill, or in need of surgery. In many cases, Level II and Level III units are further subdivided based on their abilities to provide mechanical ventilation, surgery, or ECMO. Additionally, units are often

Also in the late 1970s, the Robert Wood Johnson Foundation began the Regional Perinatal Care Program. This program was intended to explore the effects and feasibility of encouraging regional perinatal care encompassing pre- and post-birth care of mothers and infants, including neonatal intensive care. The program consisted of grants to eight sites across the country. The grants provided funds to improve record keeping, create a referral and transportation system, and conduct education and outreach. Anecdotally, these networks functioned well. Unfortunately, this program was difficult to evaluate because many forces were leading to nationwide reductions in infant mortality rates and regionalization was occurring outside the study sites (Holloway, 2000).

Over time the technologies and trained specialists necessary to operate NICUs became more prevalent, and NICU adoption became feasible for a wider array of hospitals (McCormick and Richardson, 1995). Despite the Regional Perinatal Care Program and the March of Dimes' recommendations of a regionalized system, exactly the opposite began to occur over the 1980s and 1990s: there was a drastic increase in the number of NICUs, and many of the new entrants were smaller units in community hospitals (e.g., McCormick and Richardson, 1995; Schwartz, 1996; Schwartz et al., 2000). Moreover, while births increased by 17.6% between 1980 and 1995 in Metropolitan Statistical Areas (MSA), the number of hospitals with NICU beds doubled, the number of NICU beds more than doubled, and the number of neonatologists more than tripled (Howell et al., 2002).[5] Additionally, American Hospital Association data reveal that 89% of the new NICUs that opened between 1980 and 1996 were lower level NICUs, as opposed to only 46% of the units established before 1980 (Baker and Phibbs, 2002).

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labeled as Intermediate, Community, or Regional units. In Section 2.3 I describe how I classify level of care for my study.

[5] Improving quality of care over time did lead to more infants surviving and spending longer periods of time in the NICU; however, Howell et al. (2002) calculate that by 1995 the number of available NICU bed-days exceeded medically necessary bed-days by a factor of 2.5.

Haberland et al. (2006) document that new lower level NICUs have in fact shifted deliveries of high-risk infants from Regional hospitals to the lower level hospitals in California. In a difference-in-differences framework, they show that becoming closer to a mid-level NICU, as a result of a new unit opening near a mother's zip code of residence, increases the probability that a very low birth weight infant is born in a hospital with a mid-level NICU by 17 percentage points and decreases the probability of being born in a hospital with a Regional NICU by 15 percentage points.[6] Based on evidence that mortality rates are higher for infants born in hospitals with lower level NICUs, discussed in detail in Chapter 2, and the course of deregionlization, the March of Dimes reaffirmed its recommendations in 1993 (Committee on Perinatal Health, 1993). The American Academy of Pediatrics provided similar recommendations for more regionalized care in 2004 including recommendations for consistent definitions of care levels and the need for high-risk infants to be born in higher level facilities. (Committee on Fetus and Newborn, 2004).

It has been hypothesized that so many community hospitals adopted NICUs in order to compete for profitable obstetric patients (McCormick and Richardson, 1995). Neonatal intensive care is typically generously reimbursed, and even managed care organizations have been hesitant to limit infant care, so NICUs can be profit centers for hospitals (Horwitz, 2005, see online appendix). Beyond NICUs themselves, hospitals are particularly interested in attracting obstetric patients, since mothers are typically young, healthy, and likely to return to the hospital for the later care of their families if they have a positive birth experience (Friedman et al., 2002). Almost all births in the United States are covered by some form of public or private insurance (Russell et al., 2007), limiting hospitals' ability to compete through prices. Therefore, hospitals may compete by trying to attract this desirable

[6] I confirm these results in Chapter 2 by showing that mothers living closer to hospitals with lower level NICUs are more likely to choose such hospitals and less likely to choose hospitals with higher level NICUs.

patient pool through signals of quality, such as the availability of a NICU.

This type of competition is not unique to neonatal intensive care. Theoretically, the effects of non-price competition on hospital behavior and patient welfare are ambiguous but can potentially lead to over-provision of services known as a "medical arms race" (Gaynor, 2006). Dranove et al. (1992) find that decreases in market concentration lead to increases in the number of hospitals offering various high tech services in that market. Others have shown that hospitals expand their capacity to perform certain procedures in order to deter other hospitals from adopting that procedure (Dafny, 2005a), and hospitals adopt particular technologies in order to steal business from their competitors (Schmidt-Dengler, 2006). In contrast, comparing the effect of competition on costs and mortality for heart attack patients, Kessler and McClellan (2000) find that competition led to improvements in patient welfare during the 1990s. My work sheds light on how the organization of neonatal intensive care markets affects the quantity and quality of care provided.

In Chapter 2 I revisit the question of how mortality outcomes for high-risk infants, as measured by being very low birth weight, differ by the level of neonatal intensive care available at the hospital of birth. As briefly discussed above and in more detail in Chapter 2, previous studies have found that very low birth weight infants born in hospitals with lower level NICUs experience higher mortality rates than those born in hospitals with the most sophisticated, Regional NICUs. Most of these previous studies utilize high-quality linked hospital inpatient, birth certificate, and death certificate data allowing them to control for many important clinical and demographic characteristics associated with infant mortality. However, there may be important unobserved differences between mothers who choose hospitals with varying levels of neonatal intensive care.

On the one hand, it may be the case that those very low birth weight infants born in *higher* level hospitals are unobservably less healthy than those born in lower level hospitals. For example, mothers who deliver in higher level hospitals may be referred there by their physicians because of predetermined risk factors that are not perfectly measured in the data. On the other hand, it may be the case that those very low birth weight infants born in *lower* level hospitals are unobservably less healthy. One could imagine that mothers of very low birth weight infants who choose lower level hospitals are less well informed, less likely to plan ahead, or less risk averse than those who choose to deliver in the higher level hospitals, and these characteristics may be correlated with worse infant health outcomes. By examining the observable characteristics of my sample, I show evidence consistent with the predictions of both of these selection mechanisms. Depending on which mechanism dominates, previous estimates of the mortality gradient could be biased upwards or downwards, suggesting that deregionalization may be more or less detrimental to very low birth weight mortality than previously thought.

I assess this concern by using an instrumental variable strategy to isolate exogenous variation in the level of neonatal intensive care available at the hospital in which the mother of a very low birth weight infant chooses to deliver her newborn. In the spirit of McClellan et al. (1994), I use the distances from the center of the mother's zip code of residence to the nearest hospital offering each level of neonatal intensive care as instruments for the level of care at the hospital in which she delivers her newborn. The validity of these instruments is motivated three factors: the hypothesis that NICUs have been adopted in order to compete for patients instead of to address local health needs; previous evidence showing that NICU location is not correlated with infant health measures; and evidence in my sample that distance is not correlated with observable demographic and health characteristics. I also show that distance is an important predictor of the level of care chosen by mothers of very low birth weight infants. Additionally, consistent with hospitals adopting NICUs to compete for patients across the risk distribution, I show that mothers of infants with higher birth weights are more likely to choose a hospital with a NICU when they live closer to such a hospital as well

My instrumental variables estimates indicate that very low birth weight infants born in hospitals with lower levels of neonatal intensive care do not have statistically significantly different mortality rates from those born in hospitals with the highest level of care. Furthermore, these instrumental variable estimates are bounded away from my ordinary least squares estimates, suggesting that even if the true effects are not zero, these more traditional ordinary least squares estimates exaggerate the mortality differences. The interesting implication of this result is that very low birth weight infants born in hospitals with lower level NICUs have higher unobserved mortality risk than those born in hospitals with higher level NICUs. This finding suggests that relocating deliveries to higher level hospitals prior to birth would not improve mortality outcomes because it would be relocating the deliveries of infants from the higher risk portion of the health distribution.

However, these results do not imply that the higher level NICUs are of no value. In fact, very low birth weight infants born in hospitals with lower level NICUs are very likely to be transferred to higher level hospitals after birth, and I show that the probability of being transferred is not affected by my measures of distance. This finding suggests that, while the location of NICUs impacts where very low birth weight infants are delivered, it does not impact where they ultimately receive care. Post-birth inter-hospital transfers appear to be an effective tool to equalize mortality outcomes for infants born in hospitals with varying levels of neonatal intensive care.

My findings suggest that limiting the trend of deregionalization is not necessary to minimize very low birth weight infant mortality. However, networks between hospitals to facilitate post-birth transfers are instrumental in ensuring that infants eventually receive appropriate care. If hospitals coordinate sufficiently post-birth, market competition that leads to NICU adoption is not detrimental to mortality.

That being said, it is important to recognize that mortality is not the only contributor to social welfare. Even if competition between hospitals in this market does not lead to lower quality of care, it may or may not lead to less efficient allocation of neonatal intensive care resources.

Chapter 3 of this dissertation considers one way in which neonatal intensive care resources may not be allocated efficiently. An important issue in the provision of health care is whether the mere presence of the supply of medical services leads to excessive utilization of these resources, and I examine this question in the context of neonatal intensive care. Such a relationship could occur through two main mechanisms related to two important information asymmetries prevalent in health care markets. First, the physician often has more information about the patient's health than the patient himself. Given this information gap, physicians may take advantage of their agency over patients to increase income by prescribing additional treatment beyond what is necessary. Because the physician is able to influence the patient's demand for medical care, this behavior is called "supplier-induced demand" (Evans, 1974; Fuchs, 1978; McGuire, 2000; Pauly, 1981). The second mechanism that may cause excessive utilization when more supply is available is moral hazard in insurance, which acts through the patient's information advantage over the insurer. Because insurance lowers the price of consuming health care, and the insurer cannot fully know the patient's true health status, insurance can lead to the patient consuming more than the optimal amount of health care (Arrow, 1963; Cutler and Zeckhauser, 2000; Pauly, 1968). Of course, moral hazard cannot increase the amount of health care utilization if supply is not available; thus, additional supply can lead to excessive utilization of services by opening the door for latent moral hazard to be realized.

Cross sectional comparisons between available supply and utilization are not sufficient to identify if this relationship exists, because there are many factors that may be correlated with the availability of health resources that could lead to additional utilization. Methodologically, the innovation of Chapter 3 of this dissertation is to find an exogenous source of variation in available supply. I conduct a first examination of the effect of the number of empty NICU beds available in the birth hospital on the day prior to birth on the probability that an infant is admitted to the NICU. The key to the identification strategy is the use of hospital-specific month fixed effects. With these fixed effects I identify the relationship between NICU supply and utilization from within hospital-month variation in the number of empty NICU beds. The fixed effects allow me to flexibly control for characteristics of patients who choose a particular hospital, long run trends and short run seasonality of infant health, and any hospital-specific trends or seasonality. I argue in the chapter that conditional on observable characteristics and these fixed effects, a particular infant's unobserved health characteristics are unlikely to be correlated with the unobserved health characteristics of infants born just prior to the infant, which is what determines the number of available empty NICU beds. While this identification strategy accounts for unobserved correlates between NICU supply and utilization, NICU admission is measured with error in the data that I utilize. Therefore, results in Chapter 3 are best viewed as preliminary, and I intend to verify these results using other data sources in future research.

I find that on average an additional empty NICU beds increases the probability of being admitted to the NICU by 1.11%. Not surprisingly, the effect of empty beds on NICU admission varies across the birth weight distribution. When I estimate regressions separately for subsamples stratified by birth weight, I find that the effect is very small for very low birth weight infants.[7] However, the effect size jumps discretely for infants above the very low birth weight threshhold and is largest for

[7]The effect of empty beds on NICU admission is especially small for this group when I account for the fact that very low birth weight infants are likely to be transferred if NICU beds are not available for them at the birth hospital.

infants close to the top of the low birth weight range and infants with high birth weights. These two groups are likely to be on the margin of needing neonatal intensive care. These results imply that the availability of empty NICU beds increases the utilziation of neonatal intensive care resources, particularly in the birth weight ranges where hospitals would have the most discretion over admission decisions.

This analysis is quite relevant in the context of deregionalization. With the diffusion of neonatal intensive care resources, the potential for excess supply grows. This chapter estimates the effects of short term variation in empty NICU beds, but this variation is likely to be related to the long term trends in availability associated with deregionalization. Interestingly, I also find that the effect of empty beds on NICU admission is the largest in hospitals with lower level NICUs as compared to hospitals with the most sophisticated NICUs. As these lower level NICUs are those units most associated with deregionalization, this finding suggests that deregionalization may have the scope to lead to additional intensive care utilization for lower risk infants.[8]

This chapter also provides an important contribution to the literature on neonatal intensive care markets by considering infants throughout the birth weight distribution. Much of the previous literature focuses on the effect of deregionalization on mortality outcomes for high-risk infants. It is also important to consider the implications of neonatal intensive care markets for healthier infants, and my findings suggest that excess supply contributes to lower risk infants receiving additional treatment. Because care in the NICU is more expensive than care in the traditional nursery, additional supply has likely increased the cost of care for low-risk infants.[9]

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[8]It is also not surprising that the effects are smaller in higher level NICU hospitals since many high-risk infants are transferred from hospitals with lower level NICUs to these higher level hospitals. Therefore, there is likely to be less discretion and less incentive for responding to excess capacity in these higher level hospitals.

[9]There may be other costs associated with excessive NICU utilization including psychic costs to the parents of seeing their infant in intensive care and the potential for hospital borne infections that are prevalent in NICUs.

Overall, this dissertation finds that deregionalization has likely not been as detrimental to very low birth weight infant mortality as previously thought, but additional NICU supply contributes to increased utilization of care for lower risk infants. These two findings represent two important contributions to understanding the welfare effects of deregionalization and open the door for further research about other aspects of the welfare calculation. Some important avenues of future research include the effect on broader health measures than the blunt consideration of mortality, the utility implications for mothers who are able to choose more convenient hospitals with some level of neonatal intensive care, a more specific understanding of costs including the fixed costs of adopting a NICU and the costs of maintaining and operating a NICU, and the determinants of NICU adoption from the hospital point of view.

Chapter 2

The Effect of Deregionalization on Health Outcomes: Evidence from Neonatal Intensive Care

2.1 Introduction

Technological innovations over the past half century have greatly changed medical care for sick infants. Over this time Neonatal Intensive Care Units (NICU) have been developed to administer treatments such as mechanical ventilation, artificial surfactant, and extracorporeal membrane oxygenation (ECMO)[1] to sick, preterm, and underweight infants, and they have clearly lead to improved outcomes for these groups. For example, the 28-day mortality rate for infants weighing 1,000 to 1,499 grams (2.2 and 3.3 pounds) dropped from 52.2% to 6.7% between 1960 and 1990 (Cutler and Meara, 2000).[2]

Despite these long run gains, there is concern that NICUs have not diffused optimally. The 1980s and 1990s saw a large increase in the number of NICUs in smaller, community hospitals that provide less sophisticated care compared to the original NICUs in large, regional hospitals (e.g., McCormick and Richardson, 1995; Schwartz, 1996; Schwartz et al., 2000). This trend of "deregionalization" has

[1] Mechanical ventilation assists infants whose lungs have not fully developed to breath. Artificial surfactant treats respiratory distress syndrome by helping the lungs to develop. ECMO machines pump blood out of the infant, oxygenate it, and pump it back into the infant if the infant's heart and lungs are too weak to oxygenate the blood on its own.

[2] I do not focus on costs in this chapter, but anecdotally, opening a new NICU can cost between \$125,000 and \$200,000 per bed (Baker and Phibbs, 2002). Hospital costs for very low birth weight (VLBW) infants, those weighing less than 1,500 grams or 3.3 pounds, averaged \$136,000 in California in 2000 (Schmitt et al., 2006). Nationwide, it is estimated that medical care services for high-risk infants cost \$16.9 billion in 2005 (`http://www.marchofdimes.com/peristats/slidesets/slideset_6_99.ppt`, last accessed on October 6, 2009). In the long run Cutler and Meara (2000) calculate a 510% rate of return to spending on infant health care between 1960 and 1990, accounting for the value of both lives saved and quality of life for surviving infants.

worried policy makers because previous studies have found higher mortality rates for infants born in hospitals with these Community NICUs compared to those born in hospitals with Regional NICUs, conditional on observable demographic and health characteristics (e.g., Cifuentes et al., 2002; Phibbs et al., 2007, 1996). Based on this evidence, organizations such as the March of Dimes and the American Academy of Pediatrics have advocated for a stronger regional system where high-risk mothers are referred to hospitals with Regional NICUs prior to delivery in order to minimize mortality.

This chapter seeks to estimate the causal effect on mortality of the level of care available at the hospital in which a very low birth weight (VLBW) infant – under 1,500 grams or 3.3 pounds – is born. As an empirical matter, it is not clear that the worse outcomes experienced by infants born in hospitals with lower level NICUs are attributable to the hospital type per se. Even conditional on observable characteristics, infants born in different hospitals may have different underlying risk factors. Depending on the mechanisms behind any unobserved selection, conventional estimates of mortality differences by level of care could be biased in either direction. If infants born in hospitals with lower level NICUs have *lower* underlying mortality risk than those born in Regional NICUs, previous estimates will have *understated* the mortality penalty associated with being born in lower level hospitals. Alternatively, if infants born in hospitals with lower level NICUs have *higher* underlying risk factors, previous estimates will have *overstated* the mortality differences. Any bias implies the system of deregionalization might actually be more harmful or less harmful than currently believed. While deregionalization may affect many factors other than mortality, understanding the causal effect of level of care on mortality of high-risk infants is of first-order importance to making policy decisions about the organization of neonatal care.

I propose an instrumental variables strategy to overcome selection issues associated with a mother's choice of hospital. I exploit the distance a mother must travel to the nearest hospital of each level of care as a source of quasi-experimental variation in the type of hospital chosen. Distance is an important determinant of hospital choice for many medical treatments such as cardiac and cancer surgery (e.g., Cutler, 2007; Kessler and McClellan, 2000; McClellan and Newhouse, 1997; Tay, 2003) and for expectant mothers as well (Phibbs et al., 1993). I also provide evidence that distance is likely to be exogenous to unobserved health outcomes in my data set, which is not surprising given evidence that NICU location is not correlated with the geographic variation in underlying infant health conditions (Goodman et al., 2001). Using detailed data on all California VLBW births between 1991 and 2001, I estimate how the birth hospital's level of care causally effects VLBW mortality.

My ordinary least squares (OLS) analysis yields estimates of 7.6%, 13.4%, and 31.8% higher risk-adjusted mortality rates for infants born at hospitals offering three lower levels of care relative to those born in hospitals offering the highest level of care. These results are consistent with the previous literature, but my instrumental variable estimates provide evidence that these OLS estimates are biased upward. The instrumental variables estimates are bounded well below the OLS estimates and are not statistically different from zero. My results are robust to including zip code level controls, such as population density and racial characteristics, or zip code fixed effects.

Comparing the OLS and the instrumental variable estimates reveals that infants born in hospitals with lower levels of care are negatively selected. This selection could occur if, for example, more uninformed mothers choose lower levels of care and have unobservably less healthy infants. This finding implies that relocating births to Regional NICU hospitals prior to delivery would not lead to lower mortality rates because the relocated infants would have higher unobserved mortality risk. In terms of mortality, deregionalization does not appear to have caused worse outcomes for high-risk infants.

It is also possible that the instrumental variable estimates represent a local average treatment effect. I find that my estimates are not heterogeneous across demographic sub-samples, but there still may be heterogeneous effects along unobservable dimensions. If this is the case, instrumental variables would estimate the effect of level of care on mortality for an unobserved subgroup of infants whose mothers' choices of level of care are affected by the distance instruments. However, because variation in the instruments is directly related to deregionalization, any local effect is precisely the policy relevant effect. My estimates would imply that infants of mothers who choose to give birth in hospitals with lower level NICUs because these NICUs are available – the marginal group of infants whose delivery hospitals are impacted by deregionalization – do not experience higher mortality rates.

While my results indicate that mortality does not differ by level of care at the hospital in which an infant is *born*, they do not imply that Regional NICUs are of no value. In fact, I show evidence that infants born in hospitals with the lowest levels of care are likely to be transferred to Regional NICU hospitals after birth, and the geographic distribution of hospitals does not impact the probability of transfer. It is difficult to compare outcomes to the counterfactual world that experiences deregionalization but does not allow for post-birth transfer, but my findings suggest that mortality is not causally affected by the level of care at the birth hospital because high-risk infants eventually receive care in higher level hospitals if necessary.

The remainder of this chapter is structured as follows. Section 2.2 reviews the previous literature. Section 2.3 describes the data and summary statistics. Section 2.4 provides the empirical framework. Section 2.5 presents the results, followed by robustness checks in Section 2.6. Section 2.7 concludes.

2.2 Previous Literature

2.2.1 Previous Estimates of Mortality Differences by Level of Care

Multiple authors have estimated how risk-adjusted mortality varies by level of neonatal intensive care at the hospital of birth, and many of these studies use the same California inpatient data set as this chapter. The typical methodology includes a logistic regression of mortality on level of care indicators, controlling for demographic characteristics and health status. The specific results depend on the precise categorization of hospitals, but in general these studies find higher mortality as level of care decreases for groups of high-risk infants that NICUs are intended to care for. Phibbs et al. (1996) find that VLBW infants born in hospitals with the largest Regional NICUs have statistically lower mortality rates than the lower categories, but the lower categories, including hospitals with no NICU, do not differ from each other. Cifuentes et al. (2002) use a population of infants below 2,000 grams (4.4 pounds) and find that all levels except for the largest Community NICUs have higher mortality rates than Regional NICUs. As they restrict their sample to smaller and smaller birth weight groups, the gradient becomes steeper. Similarly, Gould et al. (2002) find higher mortality rates at all levels relative to Regional NICUs except for those Community NICUs that are licensed under the California Children's Services Program.

Finally, in the most recent study on the relationship between level of care and mortality, Phibbs et al. (2007) distinguish mortality rates by very narrow level and volume interactions. While not necessarily statistically significant within each level, they find decreasing mortality across levels and by volume within levels. Based on their estimates, they conclude that if 90% of VLBW deliveries in California urban areas had been relocated to hospitals with the largest Regional NICUs, 21% of

VLBW deaths in 2000 could have been avoided.[3]

However, while high-quality hospital inpatient data sets allow the ability to control for many important covariates, mothers may select into different delivery hospitals based on characteristics not observed in the data. Such unobserved selection would lead to biased estimates of the mortality differences by level of care, and the direction of the bias would depend on the direction of the selection.

One typical form of selection that biases estimates of the effect of health treatments on outcomes is selective referral. If mothers and physicians have additional information about the mother's health status, and higher risk mothers are referred to hospitals with Regional NICUs, mothers would be positively selected into lower levels of care. Therefore, the mortality differences relative to Regional NICUs would be underestimated. On the other hand, if mothers negatively select into lower levels of care over hospitals with Regional NICUs, the mortality differences would be overestimated. This case might arise if more uninformed mothers are more likely to choose hospitals with lower levels of care over hospitals with Regional NICUs and infants of these uninformed mothers have higher unobserved mortality risk.

2.2.2 Natural Experiments in Health Research

This chapter is also related to the health economics literature that uses natural experiments to determine the marginal effects of medical treatments and technology. As with neonatal care, time series evidence suggests that most new technologies have led to vast improvements in health outcomes over time and the monetized benefits have outweighed the costs (Cutler and McClellan, 2001; Hall and Jones, 2007; Luce et al., 2006; Murphy and Topel, 2003). However, comparisons of health care expenditures and outcomes across geographic regions have found that higher spending

[3]They calculate this number only considering the sample of infants for whom they deem relocation geographically feasible and note that such relocation would require new large NICUs and the closure of some smaller NICUs.

areas do not achieve better outcomes (Baicker et al., 2006; Baicker and Chandra, 2004b; Fisher et al., 2003a,b; Fuchs, 2004). Given this contradiction, researchers have taken advantage of quasi-experimental variation to better compare individuals who differ only in their treatment and not in other unobserved dimensions to estimate causal effects of treatment. Here I highlight two portions of this literature that are most related to this chapter: research on the effects of infant health care and research using a similar identification strategy to that used in this chapter.

Studies that use natural experiments to estimate the returns to incremental units of infant health care find mixed results. Almond and Doyle (2008) exploit a California policy extending minimum length of hospital stays following delivery and the discontinuity in stay length for infants born just before and just after midnight. They find no effect of increased stay length on health outcomes for uncomplicated infants. Evans et al. (2008) exploit the same policy and find similar results for uncomplicated infants, but they do find that longer length of stay leads to reduced hospital readmission rates for more complicated cases. Using a regression discontinuity design, Almond et al. (2008) find that infants just below the VLBW cutoff receive more treatment and experience lower mortality rates than those just above the VLBW cutoff. Taken together, these studies imply that, at least for high-risk infants, increased treatment can be beneficial. My research adds to this literature by estimating whether the facilities available at the hospital in which a high-risk infant is born affect mortality.

McClellan et al. (1994) and Cutler (2007) use a similar identification strategy to this chapter's strategy in order to estimate the effect of catheterization and revascularization, respectively, following a heart attack on mortality. As with infant care, there are two selection concerns in this context, although the mechanisms are slightly different. First, the healthiest patients may have less need for these intensive surgeries. Second, the sickest patients may forego surgery due to a higher risk of dying during the procedure. To account for selection, both papers use distance to the nearest hospital providing surgery as an instrument for whether a patient receives surgery.[4] Both studies find that instrumental variable estimates of the benefit of intensive surgery are substantially lower than the ordinary least squares estimates, although Cutler (2007) finds that the monetized benefits still outweigh the costs.

2.3 Data

2.3.1 Linked Birth Data

My empirical analysis requires detailed data describing infants' hospitalizations and outcomes. The primary data set I utilize is the Linked Patient Discharge Data/Birth Cohort File (LPDD/BCF) created by the California Office of Statewide Health Planning and Development (OSHPD). This data set includes records of all births in non-Federal hospitals in the state of California. I have obtained data files for the years 1991 to 2001, comprising approximately six million births. In addition to including observations of all births from a large state, the main advantage of this data set is that it links additional data to an infant's hospital discharge record. First, it links an infant's delivery hospital discharge record to the mother's discharge record and all subsequent records resulting from transfers or readmissions to California hospitals within the first year of life. For each hospitalization, the data set includes detailed diagnosis and treatment variables, summary variables such as length of stay and hospital charges, and patient information including zip code of residence. Second, the hospital discharge data are linked to vital statistics data on births and infant deaths within the first year of life, which include gestation, birth weight, number of prenatal care visits, month prenatal care began, and demograph-

[4] Other authors have also used distance as a source of exogenous variation to predict patient flows in order to estimate the effect of volume (Gowrisankaran et al., 2006) and competition (Gowrisankaran and Town, 2003; Kessler and McClellan, 2000; Tay, 2003) on health outcomes.

ics, such as the mother and father's race, ethnicity, and education. Additionally, these records provide information on infant mortality within the first year of life, even if death occurred outside of the hospital.

The main analysis sample that I consider includes VLBW infants, defined as weighing between 500 and 1,500 grams (1.1 and 3.3 pounds) at birth. Of the initial 6.1 million birth observations with non-missing birth weight, 72,275 fall in this birth weight range.[5] To obtain my analysis sample, I first exclude observations with a missing zip code of residence, a zip code of residence outside the state of California, a missing hospital identification number, or that are delivered in a hospital without a delivery unit. The remaining sample contains 65,567 birth observations.

I then make three restrictions to maintain a sample that is as broad as possible but that excludes observations with an unusual hospital choice set. I first drop 2,704 observations where the mother's county of residence is "non-metro" according to the Office of Management and Budget.[6] This restriction excludes a small group of infants from the most rural areas for whom access to neonatal care is quite different from other residents of the state. Additionally, the previous literature has focused on deregionalization and the effect of level of care on outcomes in metropolitan areas (Howell et al., 2002; Phibbs et al., 2007) where policy recommendations about delivery relocation would be most feasible. Second, I drop 7,627 infants delivered in Kaiser owned hospitals. Mothers who choose a Kaiser hospital for delivery must be covered by Kaiser insurance, and mothers covered by Kaiser insurance must deliver in a Kaiser owned hospital; therefore, choice of hospital is restricted for this group.[7] Third, I exclude 4,113 observations diagnosed with a congenital anomaly.

[5]The full data set includes 6,221,001 births of which 1.54% of the observations have a missing birth weight.

[6]Based on 1993 USDA Rural-Urban Continuum Codes that are calculated from the 1990 Census. Source: `http://www.ers.usda.gov/briefing/rurality/ruralurbcon/priordescription.htm`.

[7]In my analysis sample, 88% of mothers with Kaiser coverage deliver in a Kaiser hospital, and 97% of mothers who deliver in a Kaiser hospital have Kaiser coverage. In results not shown here, regressions similar to the first stage regressions discussed below for the sample of Kaiser insured

This restriction is consistent with the previous literature (Phibbs et al., 2007), and it also excludes observations most likely to be selectively referred to higher levels of care due to a diagnosis during the prenatal period.

I also exclude 8,115 observations of fetal deaths, which are infants who die prior to delivery and, therefore, never receive neonatal care (Phibbs et al., 2007). Finally, because I cluster standard errors at the zip code level and estimate models with zip code fixed effects, I exclude 96 observations for which the mother's zip code of residence has no other observations remaining in the data. In Section 2.6, I show that my results are robust to each of these sample restrictions.

I choose my sample of high-risk infants using birth weight as the health proxy in order to be comparable to previous literature, and because it is the best measure of an infant's health stock at birth (Almond et al., 2005; Cutler and Meara, 2000). Relative to gestation, another summary of health status at birth, Almond et al. (2008) note that birth weight is more accurately recorded, less likely to be missing in the data, and less likely to be manipulated by delaying birth because it is not possible to know birth weight *ex ante*.[8]

VLBW infants are the population most of interest because they contribute disproportionately to costs and mortality. Schmitt et al. (2006) document that VLBW infants make up 0.9% of births but account for 36% of newborn hospital costs, and tabulations of hospital charges for my sample lead to similar figures. Mean charges for my VLBW sample are $209,000, compared to $21,000 for low birth weight infants (1500 to 2500 grams or 3.3 to 5.5 pounds) and $2,630 for normal birth weight infants (above 2500 grams or 5.5 pounds). Likewise, length of stay after birth averages 50.6 days for VLBW infants, 9.2 days for low birth weight infants,

mothers show that distance has very little power in predicting the level of care chosen for delivery. This is in contrast to the strong predictive power of distance for the analysis sample discussed in Section 2.5.

[8]Additionally, Almond et al. (2008) find empirical evidence that the recording of birth weight is not manipulated by physicians.

3.0 days for normal birth weight infants. Additionally, VLBW infants make up the vast majority of infant mortality. The main outcome I focus on in this chapter is neonatal mortality, defined as mortality within twenty-eight days of birth or within one year if an infant is continuously hospitalized since birth. VLBW infants have a neonatal mortality rate of 15.7%, compared to 0.7% for low birth weight infants and 0.1% for normal birth weight infants. Therefore, changes in how infant care is delivered has the most scope to affect outcomes for VLBW infants.

2.3.2 Hospital Data

My empirical analysis also requires data describing the level of neonatal care offered by each hospital that delivers infants. I obtain data from the authors of Phibbs et al. (2007) that differentiate hospitals into six levels of neonatal care based on the treatments each hospital provides in a given year. First, they use OSHPD hospital financial data to determine which hospitals have neonatal intensive care beds. Second, they use ICD-9 CM procedure codes in the hospital inpatient data to identify which hospitals perform particular procedures. As a guide, they define levels of care consistent with the six levels outlined in the American Academy of Pediatrics 2004 report.[9] Table 2.1 lists the six levels and their corresponding procedures. Third, the authors confirmed level of care designations through conversations with hospital personnel.

I collapse these detailed categories into four levels of care, which I refer to as No NICU, Intermediate NICU, Community NICU, and Regional NICU hospitals. No NICU hospitals provide birthing services and well-baby care, but no neonatal intensive care (Level I in Table 2.1). Intermediate NICUs care for mildly ill infants but do not provide mechanical ventilation (Level II). Community NICUs include

[9]The authors utilize the draft version of the American Academy of Pediatrics report because the final version does not include a category that provides unrestricted ventilation but no surgery, a level of service many CA units provide.

any unit that provides mechanical ventilation and either does not provide major surgery or provides surgery but treated less than 50 VLBW infants in 1991 (IIIA, small IIIB, and small IIIC).[10] Finally, Regional NICUs include those that provide major surgeries and treated greater than 50 VLBW infants in 1991, or any unit that provides cardiac bypass and/or ECMO, the two most specialized surgical procedures, regardless of size (large IIIB, large IIIC, and all IIID).

This categorization results in 161 No NICU, 58 Intermediate, 41 Community, and 36 Regional NICU hospitals at the beginning of my sample in 1991. These numbers change during my sample period as deregionalization progressed through the decade. Table 2.2 shows the number of hospitals by level and year between 1991 and 2001. The total number of hospitals providing any birthing services falls from 296 in 1991 to 269 in 2001. In contrast, the number of Community NICUs increases from 35 to a peak of 57 in 1999. 10 hospitals open new NICUs at the Community level and 21 hospitals upgrade an Intermediate NICU to the Community level. As a result of these upgrades, the aggregate number of Intermediate NICUs actually decreases from 58 to 45 over the sample period; however, there are also 15 hospitals that open new NICUs at the Intermediate level. Not surprisingly, the number of Regional NICUs, the largest, most well established, and most expensive units, remains relatively constant over the sample period.

2.4 Empirical Framework

This section describes my empirical approach to estimating the effect of level of neonatal care at the birth hospital on mortality. I first discuss an ordinary least squares regression that estimates average mortality differences between infants born in No NICU, Intermediate NICU, or Community NICU hospitals and those born

[10] I use the number of VLBW infants treated in 1991 to identify this classification to prevent hospitals from changing levels due to changes in demand during my sample period.

in Regional NICU hospitals, conditional on a rich set of control variables. This estimation strategy is comparable to the methodology of the previous literature and provides "risk-adjusted" mortality differences. I then discuss how these estimates could be upwards or downwards if mothers choose hospitals based on unobserved characteristics not included in the risk adjustment. Lastly, I discuss my instrumental variables strategy to account for unobserved selection and estimate the causal effect of level of care.

2.4.1 Baseline Model

I begin by estimating the average difference in mortality rates by level of care at the delivery hospital, controlling for observable characteristics of the mother and infant. The regression equation is as follows:

$$y_{izt} = \alpha + N_{izt}\beta^N + I_{izt}\beta^I + C_{izt}\beta^C + \mathbf{X}_{izt}\mathbf{\Gamma} + \varepsilon_{izt} \tag{2.1}$$

The unit of observation is infant i, whose mother resides in zip code z, born in year t. The dependent variable, y_{izt}, is a neonatal mortality indicator that is equal to one if an infant dies within 28 days of birth or within one year if continually hospitalized since birth, and zero otherwise.[11] $\mathbf{X}_{izt}$ is a vector of observable determinants of infant izt's health. These controls include time (year, month, and day of week indicators); mother's demographics such as age, race, ethnicity, and insurance coverage;[12] and health related controls such as the infant's sex, birth weight, and diagnoses.[13]

[11] In Section 2.6 I show that results are robust to measuring mortality across different time frames.

[12] Specific demographic controls are age, age squared, and indicators for black, other race, Hispanic, Medicaid, HMO, and self-pay.

[13] Specific health controls are parity, sex, multiple birth status, an indicator for having a clinical condition, indicators for small and large for gestational age, birth weight dummies at 100 gram increments, the number of prenatal care visits, and the month in which prenatal care began. The clinical condition indicator is equal to one for infants having at least one of the following conditions identified in Phibbs et al. (2007): hydrops due to isoimmunization, hemolytic disorders, fetal distress, fetus affected by maternal condition, oligohydramnios, other high-risk maternal

The three explanatory variables of interest, N_{izt}, I_{izt}, C_{izt}, are indicators equal to one if infant izt is born in a hospital with No NICU, an Intermediate NICU, or a Community NICU, respectively. Being born in a hospital with a Regional NICU is the excluded group, so the β^j coefficients are interpreted as the difference in mortality when born in a hospital with level of care j relative to being born in a Regional NICU hospital.[14]

For this specification to estimate the causal effect of level of care on mortality, hospital choice must be uncorrelated with unobserved determinants of mortality captured by the error term, ε_{izt}, conditional on the observable characteristics, $\mathbf{X}_{izt}$ ($E\left[\mathbf{H}'_{izt}\varepsilon_{izt}|\mathbf{X}_{izt}\right] = \mathbf{0}$, where $H_{izt} = [N_{izt}, I_{izt}, C_{izt}]$). If this condition is not met, and unobserved mortality, conditional on observables, is different for infants born in hospitals with different levels of care the OLS estimates of the β^js will be biased. If infants born in lower level hospitals are unobservably healthier (lower unobserved mortality), consistent with physicians referring the highest risk mothers to Regional NICU hospitals, OLS estimates will understate the true mortality difference between being born in lower level hospitals and Regional NICU hospitals. On the other hand, if infants born in lower level hospitals are unobservably less healthy (higher unobserved mortality), consistent with more uninformed mothers choosing lower levels of care and having higher risk infants, OLS estimates will overstate these mortality difference.

Sample means by level of care in Table 2.3 show that there are clear unconditional differences in mortality rates by level of care at the hospital in which an infant is born. Neonatal mortality rates fall from 21.9% for VLBW infants born in No NICU hospitals, to 16.9% in Intermediate NICU hospitals, 15.5% in Com-

conditions, placenta hemorrhage, premature rupture of membrane, and prolapsed cord.

[14]It is important to point out that I am estimating mortality differences based on the hospital in which the infant is born. This framework does not take into account whether or not the infant was actually treated in the NICU or whether they were transferred to and treated in another hospital. In this context, my estimates can be thought of as intent-to-treat effects.

munity NICU hospitals, and 14.7% in Regional NICU hospitals. However, there are also differences in important observable characteristics. OLS regressions control for these observable characteristics, but these differences motivate the concern that there may be differences in unobservable dimensions as well. Mothers' demographic characteristics differ by level of care, but not monotonically. For example, 9.8% of mothers giving birth in No NICU hospitals, 20.5% in Intermediate NICUs, 12.8% in Community NICUs, and 18.6% in Regional NICUs are black. The percentage of mothers covered by Medicaid and the percentage without any college education decreases substantially from No NICU, to Intermediate NICU, and to Community NICU hospitals, but the percentage in Regional NICU hospitals is higher than the percentage in Community NICU hospitals. These large differences indicate selection into level of care by mothers' demographics, but the direction of the selection is ambiguous. Furthermore, these demographic characteristics are likely to be correlated with mortality risks. For example, Singh and Kogan (2007) show persistent infant mortality disparities by mothers' education and socioeconomic status.

There are also clear patterns of selection on infant health characteristics. Consistent with selection of healthier infants into lower levels of care, infants born at lower levels are less likely to be multiple births, have slightly higher birth weight and longer gestation, are less likely to have a clinical diagnosis, are less likely to be small or large for their gestational age, and experience lower hospital charges and shorter lengths of stay. Given the differences in observed characteristics by level of care, there are likely differences in unobserved characteristics as well (Altonji et al., 2005). Therefore, accounting for non-random selection is important, though the direction of the bias is again unclear *ex ante*.

2.4.2 Estimating Causal Effects

To understand the effects of deregionalization on VLBW infant outcomes, it is necessary to estimate the causal effect of level of care on neonatal mortality. Because OLS estimates may not be able to control for all determinants of mortality, I utilize instrumental variables to overcome unobserved selection. With three endogenous explanatory variables, at least three instruments are necessary to identify the empirical model. I construct three instruments based on the distance from a mother's residence to each level of care, which I define in more detail below. For a 3×1 vector of instruments, $\mathbf{D}_{zt}$, instrumental variables estimates of β^N, β^I, and β^C will be consistent if the instruments are uncorrelated with the error term in Equation (2.1) ($E[\mathbf{D}'_{zt}\varepsilon_{izt}|\mathbf{X}_{izt}] = \mathbf{0}$) and are strong determinants of the type of hospital a mother chooses, conditional on the other observable characteristics. This second condition is similar to saying that the coefficients on the instruments are non-zero in the following set of first stage regression equations of each level of care indicator on the vector of instruments and all other covariates:[15]

$$\begin{aligned} N_{izt} &= \delta^N + \mathbf{D}_{zt}\mathbf{\Pi}^N + \mathbf{X}_{izt}\mathbf{\Gamma}^N + \mu^N_{izt} \\ I_{izt} &= \delta^I + \mathbf{D}_{zt}\mathbf{\Pi}^I + \mathbf{X}_{izt}\mathbf{\Gamma}^I + \mu^I_{izt} \\ C_{izt} &= \delta^C + \mathbf{D}_{zt}\mathbf{\Pi}^C + \mathbf{X}_{izt}\mathbf{\Gamma}^C + \mu^C_{izt} \end{aligned} \tag{2.2}$$

Notation is as above with each $\mathbf{\Pi}^j$ representing a vector of three first stage coefficients and each μ^j_{izt} representing a first stage error term.

The parameter estimates of Equation (2.2) are used to obtain the predicted probability of choosing each level of care for each observation, and two stage least squares (2SLS) estimates are computed by estimating Equation (2.1) with these

[15] More formally, it must be the case that the instruments are sufficiently linearly related to $\mathbf{H}_{izt}$ that $E[\mathbf{Z}'_{izt}\mathbf{H}_{izt}]$ is of full column rank, where $\mathbf{Z}_{zt} = [\mathbf{D}_{zt}, \mathbf{X}_{izt}]$. It is also necessary for the instruments to be sufficiently linearly independent so that $E[\mathbf{Z}'_{izt}\mathbf{Z}_{zt}]$ has full column rank (Wooldridge, 2001).

predicted probabilities in place of the level of care indicators.[16] Therefore, identification of β^N, β^I, and β^C in Equation (2.1) comes from comparing mortality for otherwise identical infants who are born at different levels of care because they live at different distances from each level of care. For example, β^C is identified from differences in mortality outcomes between infants who are and are not born in hospitals with Community NICUs because their mothers live within close or far proximity to a hospital offering a Community NICU. Intuitively, this comparison emphasizes the importance of the assumption that $E[\mathbf{D}'_{zt}\varepsilon_{izt}|\mathbf{X}_{izt}] = \mathbf{0}$. In order for instrumental variables to provide causal estimates, it is crucial that mothers living at different distances from each level of care not have infants that differ in unobserved determinants of mortality.

Since this strategy requires the location of NICUs to be exogenous to VLBW infant health, it is worth briefly re-emphasizing the process by which NICUs have diffused and discussing how mothers choose hospitals. Most importantly, diffusion has been driven by many factors unrelated to the health of VLBW infants. Over time the technologies and trained specialists necessary to operate NICUs became more prevalent, and therefore, NICU adoption became feasible for community hospitals. It has been hypothesized that so many hospitals adopted lower level NICUs in order to compete for profitable obstetric patients (McCormick and Richardson, 1995). Ninety-seven percent of births are covered by private or public insurance (Russell

[16]Both the dependent variable and the endogenous explanatory variables in this model are binary. Bhattacharya et al. (2006) point out that two stage least squares can lead to inconsistent estimates when the mean probability of the binary dependent variable is close to zero or one, or when there is more than one endogenous binary treatment variable. They advocate a multivariate probit model which assumes that the error terms from Equations (2.1) and (2.2) follow a multivariate normal distribution. On the other hand, Angrist (2001) argues that linear models still provide good approximations of average causal effects, parameter estimates directly correspond to the relevant average treatment effects, and nonlinear models depend on the distributional assumptions and are inconsistent if these assumptions are incorrect. Wooldridge (2001) points out that some of the assumptions behind average treatment effects are not precisely true with binary outcomes, but linear methods may still produce reasonable average treatment effect estimates. I have estimated my OLS specifications with both probit and logit models and find marginal effects that are almost identical to the OLS coefficient estimates presented in Section 2.5. Future work will verify that the 2SLS estimates are not biased by the linear functional form.

et al., 2007), so most families are shielded from the full cost of infant care. One way for hospitals to compete for these patients is to invest in signals of quality, which might attract risk-averse mothers. Hospitals are particularly motivated to attract obstetric patients, since mothers are typically young, healthy, and likely to return to the hospital for the later care of their families if they have a positive birth experience (Friedman et al., 2002), and NICUs themselves can be profitable (Horwitz, 2005, see online appendix).

Most preterm labor is spontaneous, and in 50% of cases, doctors are not even able to determine the cause *ex post*. Forty to fifty percent of cases with an identified cause are traced to an infection, but often mothers show no signs of these infections prior to labor.[17] As detailed by an Institute of Medicine report, there are a variety of documented correlates of preterm delivery. These correlates range from behavioral factors such as tobacco use and nutrition, to psychosocial factors such as stress, personal resources, and social support, to medical conditions of the mother or pregnancy such as obesity or multiple births, to other factors such as exposure to environmental toxins, genetics, etc. Interrelated with many of these characteristics, there are demographic differences in preterm birth rates as well. Mothers at either extreme of the age distribution, unmarried mothers, black mothers, and mothers with low income or low educational attainment are all known to have higher rates of preterm delivery. Despite these correlates, this report emphasizes that there is in fact little understanding of what conditions and events can be used to predict and diagnose preterm labor before it occurs (Behrman and Butler, 2007).

As a result of this unpredictability, a NICU is likely an effective tool for attracting patients of all risk levels. Expectant mothers usually deliver in the hospital where their obstetrician has delivery privileges, so they in effect choose their delivery hospital when they choose their obstetrician early in their pregnancy. If risk-averse

[17] Source: `www.marchofdimes.com/peristats`, last accessed on September 29, 2009.

mothers plan ahead when choosing their obstetrician and delivery hospital, the presence of a NICU is likely to factor into their decision. A mother likely considers travel time, convenience for family members, perceived quality of care, and the possibility of transfer if higher quality care is needed. If utility is increasing in perceived quality of care and decreasing in travel time, a community hospital with a NICU can attract nearby mothers willing to trade additional perceived quality at a further Regional NICU in favor of the increased convenience of choosing the nearby hospital. Furthermore, if mothers tend to choose local obstetricians who are likely to have priveldges in local hospitals, mothers will be more likely to choose nearby hospitals.

Of course, location relative to hospitals with NICUs is not the only determinant of hospital choice. Phibbs et al. (1993) estimate hospital choice models separately for high- and low-risk mothers and for publicly and privately insured mothers within each risk category. Not surprisingly, overall, mothers prefer closer hospitals, hospitals with higher quality, and hospitals with neonatal intensive care units. Despite the fact that many high-risk deliveries are unexpected, the authors do find some differences in hospital choices among high- and low-risk mothers. For example, high-risk mothers prefer hospitals with higher measures of quality, including higher level neonatal intensive care units. This finding is consistent with my sample means above that find higher-risk infants born in hospitals with higher levels of care. The authors also find some important differences in hospital choice between publicly and privately insured mothers. While distance has a similar effect on hospital choice for both groups, publicly insured mothers deliver in hospitals with worse health outcomes and are less likely to deliver in hospitals with NICUs. These findings suggest possible restrictions on access to care for publicly insured mothers.[18]

As discussed above, I restrict the sample to exclude Kaiser insured patients

[18] Additionally, during my sample period California began adopting Medicaid managed care plans on a county by county basis. These plans potentially provide further restrictions on the hospitals in which some Medicaid mothers can deliver.

who have little choice of delivery hospital, but the findings of Phibbs et al. (1993) suggest there are likely to be other groups with varying degrees of choice restrictions including publicly insured patients. Patients with other managed care insurance are likely to be restricted somewhat as well, though to varying degrees as compared to Kaiser. That being said, the motives to compete for healthy, risk-averse mothers, evidence that growth of neonatal resources has outpaced medical need, and findings that the location of neonatal intensive care resources are uncorrelated with markers of need such as occurrences of VLBW or preterm births (Goodman et al., 2001), support the exogeneity of NICU location to VLBW infant health. In the next subsection, I provide further evidence from my data supporting this claim. To the extent that some patients have restricted choice, the only effect would be to weaken the power of the instrument as long as these factors are not correlated with distance, which appears to be the case.

It is important to point out that under the assumptions of the empirical model, the instrumental variables estimates of β^N, β^I, and β^C in Equation (2.1) are structural parameters and provide causal estimates of the effect of level of care at the hospital of birth on infant mortality rates. In contrast, the first stage relationships in Equation (2.2) are reduced form equations where the endogenous level of care indicators are regressed on all of the model's exogenous variables. These equations do not necessarily provide structural parameters of the neonatal intensive care level demand function.[19]

[19] As discussed above, one previous study has attempted to estimate hospital demand parameters for delivery hospitals. Phibbs et al. (1993) estimate McFadden conditional logit models of hospital choice, and their model includes features such as distance from a mother's residence and presence of a neonatal intensive care unit. Additional work in this area is left to future research, as estimating such demand functions is important for understanding how mothers choose hospitals and why hospitals choose to provide various levels of care. Additionally, many hospitals now advertise heavily about not only the quality of care, but also amenities available for expectant mothers, such as private rooms, jacuzzis, etc. Goldman and Romley (2008) find that Medicare pneumonia patients in Los Angeles place a high value on non-medical amenities when choosing a hospital for treatment. Such amenities may also be an important tool for hospitals competing for maternity patients.

2.4.3 The Instruments

In this section, I describe how I calculate the three distance instruments and discuss why they are likely to be exogenous to unobserved VLBW mortality. I first calculate the straight line distance from the center of each patient's zip code of residence to each hospital using GIS software. Hospital location is obtained from OSHPD's publicly available geocoded data of hospital latitude and longitude.[20] I then construct three instruments that represent the differential distance between the nearest hospital of a given level of care or higher and the nearest hospital with a Regional NICU, as follows:

$$NoDist_{zt} = D(Reg_{zt}) - min[D(No_{zt}), D(Int_{zt}), D(Com_{zt}), D(Reg_{zt})] \tag{2.3a}$$

$$IntDist_{zt} = D(Reg_{zt}) - min[D(Int_{zt}), D(Com_{zt}), D(Reg_{zt})] \tag{2.3b}$$

$$ComDist_{zt} = D(Reg_{zt}) - min[D(Com_{zt}), D(Reg_{zt})] \tag{2.3c}$$

The $D(\cdot)$ operator indicates the distance from zip code z at time t to the nearest hospital offering a particular level of care. These measures can be thought of as the number of miles *saved* by choosing the nearest hospital with at least a particular level of care over the nearest hospital with the highest level of care, and therefore get larger as an individual lives closer to a hospital offering the particular level of care.

When using differential distance, the hospital choice decision is modeled as a function of distance to each lower level of care relative to distance to Regional NICU hospitals.[21] It emphasizes the fact that mothers make a trade off when choosing a lower level of care at a closer hospital – they forego potentially higher quality care

[20] OSHPD only provides this data for currently existing facilities. For those facilities for which I do not have exact location, I use the center of the hospital's zip code obtained in the OSHPD State Utilization File of Hospitals.

[21] Cutler (2007) and McClellan et al. (1994) also use differential distance as their instruments by subtracting distance to the nearest hospital from distance to the nearest hospital offering heart surgery.

in exchange for a shorter travel time.[22] Also, these three measures will always take on values greater than or equal to zero due to the $min[\cdot]$ operator in (2.3), and they equal zero if an individual lives closer to a Regional NICU than one of the lower levels of care. This specification captures the fact that if a hospital nearby offers a particular level of care, a mother can also receive lower level care by traveling to the same hospital.

These distances are not the only way one could specify exposure to NICUs. I utilize this method to best proxy for the cost of obtaining each level of care; although, one could also specify distance based on the distance to the nearest hospital of a specific level of care (instead of the nearest hospital with a particular level or higher). Other potential measures of exposure include hospital market shares or the number of hospitals of each level within a given radius. I choose distance so as not to impose potentially endogenous market definitions. As mentioned above, the goal is not to estimate structural parameters of hospital choice, but instead to exploit the exogenous variation in distance that directs patients to different levels of care.

Table 2.4 provides summary statistics of the four distance measures used to construct the instruments and of the three instruments themselves. On average, mothers of VLBW infants in my sample live 3.7, 5.7, 8.1, and 14.8 miles from the nearest hospital offering any birthing services, at least Intermediate care, at least Community care, and Regional care, respectively. The average number of miles saved by traveling to the nearest hospital with no NICU or higher relative to the nearest Regional NICU is 11.2. The average number of miles saved traveling to the nearest hospital with at least an Intermediate NICU or at least a Community NICU is 9.1 and 6.8 miles, respectively. These measures have wide variation, each with

[22] A model with four instruments based on distance to each of the four levels of care would achieve the same goal, as it would condition on distance to the nearest Regional NICU in each first stage regression. Using differential distance is equivalent to including all four distance measures separately, but restricting the coefficient on the Regional distance variable. 2SLS results, not shown here, without this functional form assumption are almost identical to those presented in Section 2.5.

standard deviations around 20 miles, or two to three times their means.

I now provide a set of summary statistics supporting the assumption that differential distance is uncorrelated with the error term in Equation (2.1) and is therefore independent of unobservable determinants of VLBW mortality. Table 2.5 lists sample means of observable characteristics by the three instruments. If a detailed list of observable characteristics are independent of differential distance, it is likely to be the case that unobservable characteristics are as well (Altonji et al., 2005). For each instrument the table shows sample means for three groups: those observations with zero differential distance and those with differential distance below and above the median, conditional on non-zero differential distance.

The first three rows show that those individuals living in zip codes below the median typically save between one and five miles by traveling to each of the three lower levels of care, and those with values above the median save between 16 and 32 miles. Other than the proportion of mothers who are black, which is about twice as large for observations at zero differential distance compared to individuals above the median for all three instruments, mothers' demographics show little variation by distance. For example, the percent of mothers covered by Medicaid ranges from 48.3% to 52.1% for the Community distance groups. In contrast, this figure had a gap of 13.6 percentage points between No NICU and Community NICU hospitals in Table 2.3. Most importantly, infant health characteristics do not differ much across distance groups. While the number of prenatal visits is slightly lower for those with zero miles saved, the month prenatal care began is similar across groups and there are no large differences in parity, multiple births, birth weight, or gestation.

Most individual observable characteristics do not appear to differ by distance, but there may be other important characteristics that do. The bottom portion of the table presents means of zip code level characteristics. These variables are collected from the 2000 census and merged to the mother's zip code of residence, and I present means treating each birth as an observation. Here, there are some potentially important differences by differential distance as median household income increases, percent urban decreases, and population density decreases across columns for each distance variable.[23]

Despite these differences, the variation in differential distance is not driven by population density alone. Figure 2.1 displays a map of California and plots the location of Intermediate, Community, and Regional NICUs in 1991. The light gray lines outline counties in the San Francisco Bay, Los Angeles Metro, and San Diego Metro areas. NICUs are clearly clustered around these metropolitan areas, but there does not appear to be any systematic difference in where each level of care is located. The geographic distribution of the community distance variable at its 1991 baseline is displayed in Figure 2.2, with Panel A showing the whole state and Panel B zooming in on the five counties comprising the Los Angeles Metro area. The lightest colored zip codes have no births in the VLBW sample and the other zip codes are shaded by the three groups discussed above: those saving zero miles, and those above and below the median conditional on non-zero differential distance. The darker zip codes that have the largest differential distances, and are therefore closer to Community NICU hospitals, are more likely to be in outlying areas, but there is variation both within the major metropolitan areas and in the suburban areas with many neighboring zip codes of varying distances. Figure 2.3 shows similar maps plotting Intermediate distance.

Overall, summary statistics indicate that differential distance is uncorrelated with most major observable demographic and infant health characteristics. To the extent that any differences in urban concentration are not captured by the individual controls, I examine the robustness of my results to the inclusion of zip code level controls and estimate models with zip code fixed effects in Section 2.6.

[23] Cutler (2007) and McClellan et al. (1994) also find that areas with higher differential distance to hospitals offering heart surgery are less urban.

2.5 Results

Comparisons of sample means in Section 2.4 revealed unconditionally higher neonatal mortality for VLBW infants born in hospitals with lower levels of care compared to those born in Regional NICU hospitals. However, there are also important differences in observable characteristics by level of care. This section estimates OLS specifications of the effect of level of care on mortality controlling for these observable characteristics and 2SLS estimates that account for any other unobservable determinants of mortality that may be correlated with hospital choice.

2.5.1 OLS Estimates

Table 2.6 presents OLS coefficient estimates of β^N, β^I, and β^C. Moving across the columns, I progressively add control variables. To account for likely similarities in health conditions and hospital choices at local levels, and because the instruments vary at the zip code level when I estimate 2SLS models, standard errors of all regression estimates in this chapter are clustered by zip code. This clustering allows for arbitrary correlation of the error term within zip codes.

The estimates in Column 1 reflect the unadjusted mortality differences by level of care with no additional covariates and replicate the differences in sample means from Table 2.3. VLBW infants born in No NICU, Intermediate NICU, and Community NICU hospitals are 7.2, 2.2, and 0.8 percentage points more likely to die, respectively, than those born in Regional NICU hospitals. The Community NICU coefficient is statistically significant at the 10% level and the other two coefficients are statistically significant at the 5% level. Column 2 adds controls for long term mortality trends in the form of year dummies and within year mortality cycles in the form of eleven month-of-year dummies and six day-of-week dummies. The estimated effect of being born in a hospital with a Community NICU increases to

1.3 percentage points and is now statistically significant at the 5% level; the other two estimates are similar to the previous column.

Column 3 adds controls for mother's demographic characteristics. The coefficient estimates decrease from Column 2 but are still positive and precisely estimated. Column 4 adds controls for the infant's baseline health characteristics and prenatal care. These covariates control for underlying health risk and are similar to controls used in previous studies. This specification estimates "risk-adjusted" mortality differences by level of care and will be treated as the baseline OLS estimates for the remainder of the paper. Except for the No NICU coefficient, the estimates in Column 4 are slightly larger than those in the previous column, and the coefficients imply that on average, infants born in hospitals with Community NICUs, Intermediate NICUs, or No NICUs are 1.2, 2.1, or 5.0 percentage points more likely to die than those born in hospitals with Regional NICUs, respectively. Relative to the sample mean mortality rate of 15.7%, these coefficients imply effects of 7.6%, 13.4%, and 31.8%, respectively.

OLS estimates lead to the conclusion that infants born in lower level hospitals experience higher risk-adjusted mortality rates, confirming the previous literature. Infants born in No NICU hospitals have the highest risk-adjusted mortality rate, and most relevant to the trend towards deregionalization, infants born in Intermediate and Community NICU hospitals experience statistically and qualitatively higher mortality rates than those born in Regional NICU hospitals. However, the finding that the coefficient estimates are sensitive to controls implies that observed determinants of mortality are correlated with level of care. The fact that the coefficient estimates increase or decrease depending on which controls are added reinforces that the direction of any selection is ambiguous. Evidence of selection on the observables emphasizes the importance of accounting for any potential unobserved selection as well.

2.5.2 First Stage Estimates

This section presents the first stage estimates of the effect of distance on level of care chosen specified in Equation (2.2). I provide evidence that the three distance measures are strong instruments and further evidence that they satisfy the exclusion restriction. Table 2.7 presents the results, building up to the baseline specification by progressively adding controls across the columns for each outcome. The coefficient estimates and standard errors show little to no change across columns. This finding implies little correlation between distance and observable characteristics and further supports the hypotheses that the instruments are uncorrelated with unobserved characteristics as well.

Columns 4, 8, and 12, present the main first stage specifications with all controls included. All of the first stage coefficient estimates are strongly statistically significant and show the expected substitution patterns. Individuals living closer to a particular level of care are more likely to choose that level of care and less likely to choose the other levels of care. For example, a ten mile increase in $ComDist$, associated with living ten miles closer to a Community NICU or higher, decreases the probability of choosing a No NICU hospital and an Intermediate NICU hospital by 2.5 and 2.7 percentage points, respectively, and increases the probability of choosing a Community NICU hospital by 7.4 percentage points.[24] These coefficient estimates are equivalent to 33%, 24%, and 31% changes relative to their respective level of care indicator sample means. These are large effects given the standard deviations of the distance instruments are around twenty. Qualitatively, distance is an important determinant of the level of care a mother chooses.

Below the estimates in each panel I report F-Statistics testing the null that

[24]Though not a part of the estimation, there is implicitly a fourth relationship between the probability of choosing a hospital with a Regional NICU and distance. While not shown in the table, this same change decreases the probability of choosing a hospital with a Regional NICU by 2.2 percentage points, confirming the findings of Haberland et al. (2006) that lower level NICUs divert high-risk births from Regional NICUs.

the three distance coefficients are jointly equal to zero. The F-Statistics for the main specifications with the full set of controls are 32.46, 44.56, and 38.35, all well above the rule-of-thumb cutoff of 10 typically used to assess finite sample bias from weak instruments. Additionally, the fact that each instrument is significant in all three equations and has a particularly large coefficient estimate in the equation corresponding to its respective level of care, suggests that each of the three instruments provide independent variation to identify the model.

2.5.3 2SLS Estimates

Table 2.8 reports the 2SLS results. Column 1 repeats the baseline OLS results with all controls from Table 2.6. All three 2SLS coefficient estimates in Column 2 are substantially lower than their counterparts in Column 1. The coefficient of the No NICU indicator decreases from 0.050 to -0.030, the coefficient of the Intermediate NICU indicator decreases from 0.021 to 0.009, and the coefficient of the Community NICU indicator decreases from 0.012 to -0.063. The No NICU and Community NICU coefficient estimates actually change signs and the Intermediate NICU coefficient estimate falls by half, but the standard errors increase by a factor of between three and nine. The Community NICU coefficient estimate is marginally statistically significant (at the 10% level), but neither of the other two estimates in Column 2 are statistically significant.[25]

Despite the large standard errors, the 2SLS estimates are clearly different from and bounded below the OLS estimates. First, I conduct a Hausman test of the null hypothesis that both the OLS and 2SLS estimates are consistent against the

[25]One might be concerned that some of the infant health and prenatal care controls are endogenous. This would be a concern if, for example, mothers who live close to Regional NICUs also have access to higher quality prenatal care, or if hospitals with differing levels of care have different propensities to diagnose various health conditions. To account for this, I also estimate 2SLS regressions excluding this set of controls. The results are similar to those reported in Column 2 of Table 2.8 with coefficient (standard error) estimates of -0.021 (0.037), 0.014 (0.018), and -0.041 (0.042) for No NICUs, Intermediate NICUs, and Community NICUs, respectively.

alternative that only the 2SLS estimates are consistent.[26] The p-value of this test is 0.031, so the null is rejected at the 5% significance level. This test implies that the 2SLS coefficient estimates are statistically different from the OLS estimates and provide more consistent estimates.

Second, even the upper bounds of the 2SLS estimates imply much lower quantitative and qualitative effects on mortality than the OLS estimates, at least for the No NICU and Community NICU coefficients. Figure 2.4 plots the OLS and 2SLS coefficient estimates scaled by mean neonatal mortality. It also plots one and two standard deviation intervals above the 2SLS coefficient estimates. The OLS coefficient estimate of the No NICU coefficient implies 31.8% higher mortality relative to being born in a Regional NICU hospital. The 2SLS coefficient estimate is large and negative, one standard deviation above the 2SLS coefficient estimate is still below zero, and even two standard deviations above implies an effect of 17.9% – 44% lower than the OLS estimate. Similarly, one standard deviation above the Community NICU coefficient estimate is still far below zero, and two standard deviations above implies an effect of 4% – 46% lower than the OLS effect of 7.4%. One standard deviation above the Intermediate NICU 2SLS coefficient estimate is above the OLS estimate, but the point estimate is still 55% lower than the OLS point estimate.

The 2SLS estimates are not statistically different from zero and are small in magnitude compared to OLS estimates. This finding provides evidence that the OLS estimates of higher mortality at the three lower levels of care relative to Regional NICU hospitals are overstated. The dominant form of selection is unobservably higher risk births occurring in lower level hospitals. These results imply that policy

[26]The usual Hausman test also assumes that the OLS estimates are efficient under the null hypothesis. However, clustered standard errors result in a covariance matrix that is not asymptotically efficient. Therefore, I construct the Hausman test statistic using a paired bootstrap strategy that samples at the zip code level. My sample has 1,144 zip codes, so I construct 5,000 random samples of my data that each draw 1,144 zip codes with replacement. For each bootstrap sample, I run my OLS and 2SLS regressions and save the coefficient estimates. I then construct the estimated variance-covariance matrix of the difference between the OLS and 2SLS coefficients based on the distribution of these 5,000 estimates. See Cameron and Trivedi (2005, p. 378) for details.

measures aimed at reversing the effects of deregionalization are likely to have a limited impact on mortality. Relocating mothers who would have chosen to give birth in lower level hospitals to Regional NICU hospitals prior to birth would not improve mortality rates because the relocated deliveries would be from the less healthy portion of the distribution.

It is important to emphasize that I am estimating how the level of care at the hospital in which an infant is *born* impacts mortality. My results do not imply that being *treated* in a hospital with a higher level NICU has no effect on outcomes. In fact, a likely mechanism behind my results is that infants born in hospitals with lower levels of care achieve similar outcomes to those born in hospitals with higher levels of care because the former group will be transferred to a higher level hospital after birth if necessary. In my sample 66% of VLBW infants born in hospitals with No NICUs or Intermediate NICUs are transferred to a higher level hospital after birth, and 85% of those that are transferred are sent to a Regional NICU hospital.

In order to explore whether the probability of transfer is systematically impacted by distance, I regress an indicator for whether or not an infant is transferred to a Regional NICU hospital on the three distance instruments for the sample of VLBW infants born in No NICU or Intermediate NICU hospitals. To run this regression, I select the sample based on an endogenous variable, but statistically insignificant coefficients on the three distance instruments would suggest that hospitals do not selectively transfer infants based on distance. In other words, this kind of finding would imply that transfers occur when medically necessary and are not impacted by where a mother lives in relation to where NICUs are located.

Results of this regression do reveal a positive and statistically significant coefficient of 0.029 on the No NICU distance variable, but the coefficient estimates on the other two distance instruments are very small and statistically insignificant (-0.00008 and 0.008, respectively). The positive coefficient on No NICU differential distance implies that as a mother lives closer to a hospital with No NICU or higher or farther from the nearest Regional NICU, her infant is more likely to be transferred to a Regional NICU. When I instead regress the transfer indicator on all four distance measures instead of the three differential distance measures, I find that this coefficient is being driven by the distance to the nearest Regional NICU hospital. This finding is likely a result of using the selected sample of infants born in No NICU or Intermediate NICU hospitals. Infants of mothers who live close to Regional NICU hospitals, but choose not to deliver in the Regional NICU hospital are likely to have healthier infants and less medical need for transfer. Overall, these results suggest that I find no gradient between level of care at the birth hospital and mortality because VLBW infants are transferred to hospitals with higher levels of care when medically necessary, and the location of lower level NICUs does not change the probability of eventually being treated in a hospital with a higher level NICU.

2.6 Robustness Tests

In this section I further test the assumptions that lead to my conclusions and explore the robustness of my findings to various alternative specifications. I also examine whether the effect of level of care on mortality differs among different subsamples of the VLBW infant population and discuss implications of local average treatment effects.

2.6.1 Additional Tests of Instrument Validity

The distance instruments are motivated by the supposition that NICUs are not located according to medical need and are likely adopted to attract low-risk obstetric patients. Table 2.9 provides further evidence of this hypothesis by presenting "first stage" estimates of the effect of distance on mothers' hospital choice for infants above the VLBW threshold. I display estimates for low birth weight infants (1,500 to 2,500 grams or 3.3 to 5.5 pounds), those just above the low birth weight cutoff (2,500 to 3,000 grams or 5.5 to 6.6 pounds), and the remaining normal birth weight group (3,000 to 4,500 grams 6.6 to 9.9 pounds).[27] Distances are strong predictors of level of care for these samples, and the coefficient estimates and F-Statistics actually increase in absolute value as birth weight increases. This evidence supports the anecdotes that NICUs attract all mothers and the assumption that NICU location is exogenous to the unobserved determinants of VLBW mortality.

As a final test of the validity of the instruments, I estimate reduced form regressions of the effect of the distance instruments on neonatal mortality, and examine their sensitivity to the addition of controls. The stability of the first stage estimates in Table 2.7 provides evidence in favor of the exclusion restriction. A similar exercise for the reduced form estimates provides a sharper test because it provides evidence on how observable characteristics are correlated with the portion of distance that predicts neonatal mortality. If selection on the unobservables is similar to selection on the observables, and the reduced form estimates are insensitive to controls, the exclusion restriction that $E[\mathbf{D}'_{zt}\varepsilon_{izt}|\mathbf{X}] = \mathbf{0}$ is likely to hold.

Table 2.10 presents the results. The first column includes no controls, the second column adds time dummies, and the final two columns add demographic and health characteristics. The estimates are quite stable across specifications. The $NoDist$ and $IntDist$ coefficient estimates change slightly as controls are added, but they are quite small and statistically insignificant across all four columns. The $ComDist$ coefficient estimate is very stable across specifications and in the final column is estimated as a statistically significant -0.004.

These reduced form point estimate are also small in magnitude. For example,

[27] All samples are subject to the same restrictions described in Section 2.3.

a one standard deviation change in miles saved to the nearest Community NICU or higher (18.4 miles) only leads to a 0.8 percentage point decrease in mortality. As a comparison, a one standard deviation increase in mother's age (6.9 years) reduces mortality by 4.5 percentage points, and a one standard deviation increase in number of prenatal care visits (6.1 visits) reduces mortality by 1.8 percentage points. An increase in birth weight from the category just below the mean (900 to 999 grams) to the category just above the mean (1,000 to 1,099 grams) decreases mortality by 3.9 percentage points. All of these effects are much larger in magnitude than the effects of distance on neonatal mortality. These quantitatively and qualitatively small reduced form estimates are consistent with the small 2SLS estimates of the effect of level of care on mortality. 2SLS estimates scale the reduced form estimates by the size of the effect of distance on hospital choice. If distance affects the level of care chosen but not mortality, level of care cannot affect mortality for the population that chooses level of care as a result of distance.

2.6.2 Alternative Specifications

2.6.2.1 Zip Code of Residence Controls

Sample means by differential distance in Table 2.5 showed that individuals living closer to each of the three lower levels of care relative to Regional NICU hospitals live in zip codes with lower population density and higher income. Though I control for many individual level covariates in my main results, if these zip code level characteristics are conditionally correlated with distance and infant health, 2SLS estimates would be biased. Therefore, I test the robustness of my estimates to controlling for zip code level population density; percent black; percent Hispanic; percent of the population over 25 with no college, some college, a college degree, and more than a college degree; and median household income.[28]

Additionally, distances may factor into the hospital choice decision differently in urban and suburban areas. For example, five miles in downtown Los Angeles may have a much different travel time than five miles in a suburban area. Furthermore, hospitals are located closer to each other in more urban areas than less urban areas. Using differential distance and controlling for all three distance variables captures some of these features, but the fist stage regression may have more predictive power if the effect of distance is allowed to vary with population density. I therefore estimate models with interactions of the distance measures and zip code population density added to the instrument set.

Table 2.11 presents first stage estimates with the baseline specification repeated in Panel A. In this table each row lists coefficient estimates from one first stage regression. When zip code level controls are added in the first three rows of Panel B, the magnitudes of the first stage coefficient estimates change slightly, but they are very similar, highly statistically significant, and the F-Statistics are of similar magnitudes to Panel A. The second portion of Panel B interacts the distance instruments with population density. The coefficient estimates of the three distance measures decrease a bit in magnitude, but are still highly statistically significant. The density interactions are almost all statistically significant with positive diagonal elements and negative off diagonal elements, matching the pattern of signs of the distance main effects. Thus, the effect of distance becomes stronger as population density increases, as would be expected if travel times are longer or travel is more expensive in more densely populated areas. The three added instruments result in similar F-Statistics for the No NICU and Intermediate NICU regressions, but a lower F-Statistic in the Community NICU regression that is still well above 10.

The corresponding panels of Table 2.12 present the OLS and 2SLS squares

[28] All zip code level variables are calculated from the 2000 Census. Unfortunately, the 1990 census does not provide comparable data at the zip code level.

results, with each row listing coefficient estimates from one regression. The OLS and 2SLS coefficient and standard error estimates in specifications controlling for zip code level characteristics are very similar to the baseline estimates. Controlling for differences between urban and suburban zip codes does not impact the results. The last row of Panel B presents results when the instrument set includes interactions with population density. The standard errors of these estimates are very similar to the specification with zip code level controls and the baseline specification; however, the coefficients all move towards zero and none are statistically significant. If anything, allowing the effect of distance to differ with population density results in point estimates that are even closer to zero.

2.6.2.2 Zip Code of Residence Fixed Effects

Next, I estimate models with zip code of residence fixed effects to control for any other characteristics that are constant within a zip code, but not accounted for by the census data controls. Identification with these fixed effects comes from changes over time in a zip code's distances to each level of care caused by new, upgraded, or closed NICUs nearby during the sample period. Thus, the variation in distance is directly driven by deregionalization during the sample period. 25% of the VLBW sample lives in a zip code that at some point between 1991 and 2001 experiences a change in Intermediate Distance, and the average change is 4.5 miles. 32% lives in a zip code that experiences a change in Community Distance, and the average change is 3.9 miles. Figure 2.5 maps zip codes that become no closer, slightly closer (changes below the median), and much closer (changes above the median) to Community NICUs, with Panel A showing the whole state and Panel B focusing on the Los Angeles metro area. Zip codes with large changes in distance are more likely to be in outlying areas, but there are many neighboring zip codes experiencing different changes in both urban and suburban areas. Figure 2.6 shows similar maps for Intermediate distance.

With fixed effects the instruments are valid if zip code level changes in distance are uncorrelated with zip code level changes in unobserved mortality.[29] Even if zip codes at different distances differ systematically, identification will only be threatened if unobserved mortality trends are conditionally correlated with changes in distance. Figure 2.7 shows that at least trends in mean observable demographic and underlying health variables do not systematically differ between zip codes experiencing different changes in distance. This finding of parallel trends is not surprising given the evidence that deregionalization has not been driven by the health needs of high-risk infants.

Panel C of Tables 2.11 and 2.12 show first stage and second stage results with zip code fixed effects, respectively. The instruments are still strong predictors of level of care chosen with large, positive, and statistically significant coefficients along the diagonal. The F-Statistics are lower than in the cross sectional specifications, but they are all above 16 without population density interactions and above 11 with the interactions. OLS results in Table 2.12 are similar to the cross sectional results. The 2SLS estimates are again not statistically significant. The fixed effects lead to much larger standard errors and more negative point estimates of the No NICU and Community NICU coefficients, but the qualitative results are similar: negative or small point estimates, indicating no difference in mortality outcomes by level of care at the birth hospital. When the instruments are allowed to vary with population density, the negative point estimates of the No NICU and Community NICU coefficients are cut by about two thirds and move towards zero as in the specifications without fixed effects. These specifications confirm that the main results are robust to the most complete possible controls for local characteristics. They also show that the cross sectional 2SLS specifications estimate similar effects to specifications

[29]Formally, the assumption is $E[\ddot{D}'_{zt}\ddot{\varepsilon}_{izt}|\ddot{\mathbf{X}}_{izt}] = \mathbf{0}$, where the dots indicate variables in deviation-from-zip-code-mean-form.

identified directly from changes in distance related to deregionalization.

2.6.2.3 Pooling No NICUs and Intermediate NICUs

I also estimate models where I pool No NICU and Intermediate NICU hospitals into one category. Only 7.6% and 11.1% of the VLBW sample are born in these two types of hospitals, respectively. Thus combining them into one group may provide more precision. Additionally, some of the first stage predictions of these indicators are outside the unit interval. Pooling these two groups reduces the percentage of observations with at least one of their first stage predictions outside the unit interval from 12.8% to 2.7%.

It is also likely medically reasonable to pool these two groups. Neither of these types of hospitals is designed to care for VLBW infants, and neither provides mechanical ventilation. Additionally, infants born at both levels of care have very similar transfer patterns. About 60% of VLBW infants born at these two levels of care are transferred to Regional NICUs. In contrast, only 20% of infants born in Community NICU hospitals are transferred to Regional NICUs. Given the likely similarity of care, it is not surprising that a χ^2 test that the 2SLS No NICU and Intermediate NICU coefficient estimates from the main specification are the same does not reject the null hypothesis (p-value=0.24).

Panel D of Tables 2.11 indicates that distance is still a strong predictor of level of care with even larger F-statistics than in the original estimation. In Panel D of Table 2.12 OLS estimates are as expected, with a similar Community NICU coefficient estimate to the baseline specification and coefficient estimates of the pooled No/Intermediate NICU coefficient between the original No NICU and Intermediate NICU coefficient estimates. The precision gains in the 2SLS estimates are not large, but the point estimates are closer to zero, and none of them are statistically significant negative estimates.

2.6.2.4 Alternative Control Variables and Clustering

Table 2.13 presents estimate from six other alternative specifications, with the baseline specification repeated in Column 1. Columns 2 through 5 test whether the results change of I include various different health related controls. Column 2 adds an indicator for whether the infant was delivered by cesarean section or not. I do not include this control in the main specification because, as a treatment decision, it may be endogenous to the level of neonatal intensive care at the birth hospital. Despite this concern, adding it as a control variable does not appreciably change the OLS or 2SLS estimates. Column 3 and 4 provide evidence that my results are not sensitive to how I control for birth weight. In these two columns, I interact the birth weight indicators with the male dummy and re-specify the birth weight indicators in 50-gram increments instead of 100-gram increments, respectively. Both alternative specifications lead to OLS and 2SLS estimates that are similar to the baseline estimates. Column 5 replaces the dummy indicating whether an infant has any of the defined clinical conditions with a full set of indicators for each of the nine different conditions.[30] Again, the results are quite similar to the baseline estimates.

The last two columns of Table 2.13 explore whether the standard error estimates change if the level of clustering is changed. To this point, standard error estimates have been clustered at the zip code level to allow unobserved mortality to be correlated within zip codes. Column 6 allows for more conservative geographic correlation by clustering at the HSA (Hospital Service Area) level. These HSAs are collections of zip codes for which most of their Medicare patients receive care from the same hospital.[31] While these areas are calculated only with Medicare patients, they are likely good proxies for general health care markets. My sample includes

[30]The nine 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 (Phibbs et al., 2007).

[31]Source: `http://gonzo.dartmouth.edu/faq/data.shtm`, last accessed May 17, 2010.

1,144 zip codes which are grouped into 192 HSAs. The standard error estimates remain virtually unchanged when clustering at this larger geographic level. If anything the 2SLS estimate of the community NICU coefficient becomes a bit more precise.[32] Column 7 clusters standard errors by hospital instead of by geography. Allowing unobserved mortality to be correlated within hospitals does slightly inflate the standard errors beyond those allowing unobserved mortality to be correlated within geographic areas.

2.6.2.5 Alternative Mortality Measures

Results to this point indicate that OLS estimates overstate differences in neonatal mortality by level of care. This definition of mortality includes all deaths within 28 days of birth or within one year if an infant is continuously hospitalized since birth. It may be the case that results differ for shorter or longer term measures of mortality. In Table 2.14 I present OLS and 2SLS estimates of the effect of level of care on 1-day, 28-day, and 1-year mortality, regardless of hospitalization time. In general results are similar to the baseline specification, repeated in Column 1. OLS estimates reveal higher mortality in lower level hospitals. The point estimates increase as the mortality window increases, but increases in the mean mortality rate as the window lengthens imply the relative magnitudes are similar for each outcome. For all three additional mortality outcomes 2SLS estimates are well below the OLS estimates and statistically insignificant. The finding that OLS estimates overstate differences in mortality is robust to these alternative outcome measures.

[32] Unreported estimates reveal very similar standard error estimates when clustering at the county level. As a caveat, the asymptotics for clustered standard errors require the number of clusters to approach infinity while the cluster size is fixed. There are only 39 counties in the data, so this specification has a small number of large clusters.

2.6.3 Heterogeneity and Local Average Treatment Effects

Throughout the paper I have assumed a homogeneous effect of level of care on mortality for all VLBW infants. However, it is possible that the effect may vary by the infant's characteristics. This is particularly important with instrumental variable estimates because they only estimate the impact of level of care on mortality for the sub-group of infants whose mothers choose level of care based on the instruments. If these "compliers," who choose their level of care because of distance, are different from the rest of the sample, these estimates will represent a local average treatment effect (LATE) (Angrist et al., 1996; Imbens and Angrist, 1994). I cannot directly observe the compliers in my data, but one might be concerned that the 2SLS estimates are driven by a particular group of observations if these compliers differ from the general population. I therefore estimate my OLS and 2SLS regression equations on various sub-samples based on observable characteristics to ensure the estimates are not being driven by any particular groups. Understanding any heterogeneity in this effect is also important for policy implications. If there are sub-groups for whom there is a gradient between level of care and mortality, interventions may be warranted to target these specific groups and ensure they are able to deliver in higher level hospitals.

Table 2.15 presents results for various subsamples with the baseline estimation from Table 2.8 repeated in Column 1. Overall, the OLS and 2SLS coefficient estimates are similar across all reported sub-groups. OLS estimates are positive and statistically significant, and 2SLS estimates are small and mostly statistically insignificant. Column 2 shows the results for infants of Hispanic mothers. The 2SLS estimate of the effect of being born in an Intermediate NICU hospital (0.025) is close to the OLS coefficient estimate (0.028) for this group, but still statistically insignificant. The other two 2SLS coefficient estimates are negative, statistically insignificant, and similar to the baseline sample.

Column 3 excludes infants of black mothers from the estimation. Infants with black mothers make up a small subset of the sample, so I do not estimate the regressions for them alone, but excluding them does not have much effect on the estimates. This sample also provides a useful robustness check because of the difference in percent black by differential distance reported in Table 2.5. The estimates for the population of infants with mothers covered by Medicaid in Column 4 are similar to the baseline specification, indicating the results are similar by insurance coverage. The sample of infants whose mothers have no college education in Column 5 has a 2SLS Intermediate NICU coefficient that is the same as the OLS estimate (0.027), but again it is not statistically significant and the other 2SLS coefficient estimates are negative.

In the previous section I show the results are robust to controlling for population density and allowing the effect of distance to differ by population density. One might also be concerned that the results are driven by either urban or suburban areas, which I address in Column 6. This column presents estimates for the subsample whose zip code population density is below the median. Again, the 2SLS coefficient on being born in an Intermediate NICU hospital (0.022) is similar to the OLS coefficient (0.028), but the other estimates are similar to the baseline specification, indicating the results are similar for individuals in urban and suburban areas.

Finally, the effect of level of care may have changed over time. Mortality rates for VLBW infants decreased during the early 1990s, but leveled off during the latter part of the decade (Horbar et al., 2002).[33] Also, Table 2.2 shows that the diffusion of NICUs leveled off during the second half of the decade. It is possible that the gradient between level of care and mortality changed during this time period if technology improved, if new NICUs improved over time due to learning, or if the

[33] In my sample, mean neonatal mortality fell from 20.08% to 14.80% between 1991 and 1995, but only fell to 13.62% by 2001.

propensity for lower level units to transfer infants to higher levels changed over time. Column 7 presents results for births occurring during the first half of the sample from 1991 to 1995. The OLS gradient between level of care is greater during this time period as compared to results for the full time period, but because mean mortality was higher during the earlier period, the relative effects are similar. The 2SLS estimates are similarly small and statistically insignificant as compared to the baseline estimation. There is no evidence of a differential effect of level of care on mortality over time.

Despite evidence that the effect of level of care on mortality does not vary by observable characteristics, there still may be unobserved heterogeneity. If there are heterogeneous treatment effects that vary by unobservables, 2SLS would estimate a LATE for a group of compliers that are not identifiable in the data. That being said, because the compliers are the infants whose mothers choose their delivery hospital based on distance, the LATE would in fact be the policy relevant effect. Even if the 2SLS estimates do not represent the effect of level of care on mortality for the entire population, they still imply that the population that would be impacted by policy measures regarding the geographic distribution of NICUs does not experience different mortality rates by level of care.

2.6.4 Sample Selection

In this section I ensure that my estimates are not sensitive to the sample restrictions discussed in Section 2.3. The first column of Table 2.16 repeats the estimates from the main specification in Table 2.8. Columns 2 through 5 report results including various groups that were excluded from the main analysis sample. Column 2 includes infants in the most rural counties, Column 3 includes infants born in Kaiser hospitals, Column 4 includes infants diagnosed with a congenital anomaly, and Column 5 includes fetal deaths.

These estimates reveal that the OLS and 2SLS estimates are not appreciably affected by these sample restrictions. If anything, including rural residents results in 2SLS estimates that are closer to zero, although excluding these observations is still probably best, since they are likely to live furthest from all hospitals and may be unobservably different from those living close to all hospitals. Including deliveries in Kaiser hospitals has little effect on the estimation as well. Results of first stage regressions for this sample alone, not shown here, reveal that these added observations do not choose hospitals based on distance; therefore, they do not contribute to the 2SLS estimates, so it is not surprising that the results are not affected by including them.

Including infants with congenital anomalies leads to higher coefficient estimates in the OLS specification, but similar 2SLS estimates to the baseline specification. Finally, including observations of infants who die before delivery approximately doubles the magnitude of both the OLS and 2SLS coefficient estimates. The mean mortality rate for this sample is almost twice that of the main analysis sample, so the relative effects are very similar. This finding indicates that differences in level of care do not differentially impact the probability of death prior to delivery.

2.7 Conclusion

This chapter estimates the causal effects of level of neonatal intensive care at the birth hospital on VLBW mortality. The issue of deregionalization – the increasing number of smaller, community hospitals opening NICUs – has gained attention in the health policy community. Evidence of higher risk-adjusted mortality rates for VLBW infants born in hospitals with lower level NICUs has led advocates to suggest high-risk mothers be referred to more sophisticated hospitals prior to delivery. However, these estimates could be biased in either direction by unobserved selection. To overcome selection concerns, I utilize an instrumental variables strategy that exploits exogenous variation in distance from a mother's residence to the nearest hospital of each level of care. NICU location has been driven by factors unrelated to the health of VLBW infants, and I provide evidence in my data that distance is uncorrelated with health conditions.

My OLS estimates confirm the previous literature and imply 7.6%, 13.4%, and 31.8% higher risk-adjusted mortality rates for VLBW infants born in Community, Intermediate, and No NICU hospitals, respectively, relative those born in Regional NICUs hospitals. However, my instrumental variables estimates imply that these mortality differences are overstated. 2SLS estimates are not statistically different from zero and are small in magnitude. The No NICU and Community NICU 2SLS coefficient estimates are bounded well below their OLS counterparts, with even two standard deviations above the 2SLS estimates lying about 50% below the OLS estimates. The Intermediate NICU 2SLS coefficient estimate is not clearly bounded below the OLS estimate, but the point estimate is half the magnitude. My results are robust to controlling for zip code level characteristics and zip code level fixed effects. I also find no evidence that the effect of level of care on mortality is heterogeneous by demographics. Even if the effect varies on other unobservable dimensions, any unobserved heterogeneity would lead to a local average treatment effect directly identified from infants impacted by deregionlization.

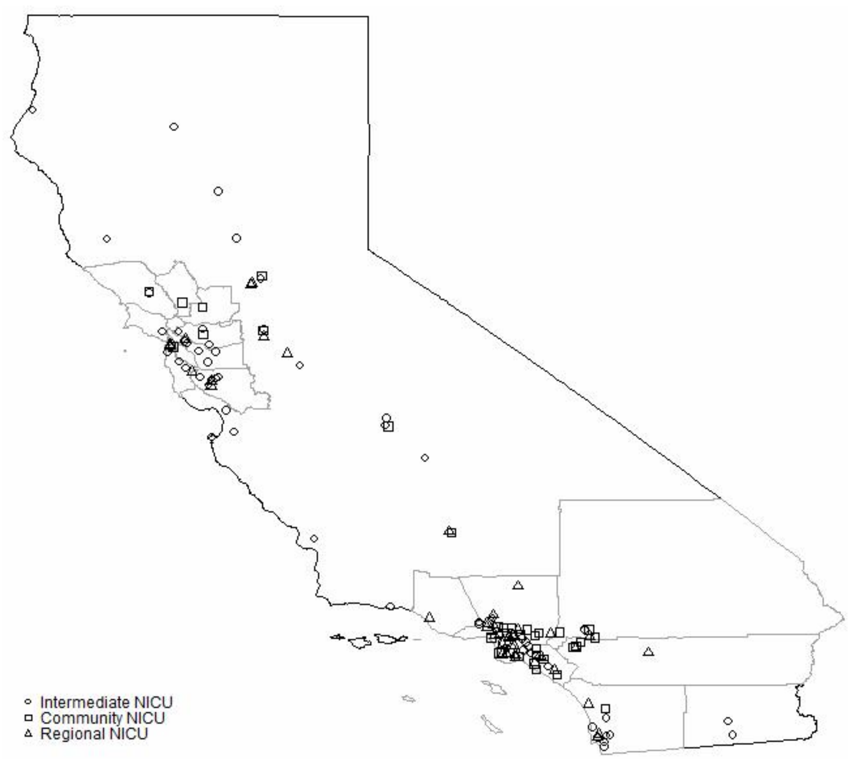
The fact that the 2SLS estimates are below the OLS estimates, reveals that mothers with higher unobserved risk select into hospitals with lower levels of care. In terms of mortality, these results imply that relocating high-risk deliveries to Regional NICU hospitals prior to birth will not result in improved health outcomes. Instead, Regional hospitals would be treating new patients with higher unobserved acuity. I also show evidence that level of care at the birth hospital does not impact mortality because infants born in No NICU and Intermediate NICU hospitals are often transferred to Regional NICU hospitals, and these transfers are independent of how close mothers live to lower level facilities. Deregionalization does not appear to prevent infants born in No NICU or Intermediate NICU hospitals from eventually receiving care in Regional NICUs.

This analysis has addressed the first-order question of how deregionalization has impacted VLBW mortality. Future research is needed to understand the full welfare impacts of this trend. First, while mortality may not vary by level of care at the birth hospital, there may be important differences in cost of care. If larger hospitals achieve economies of scale, they may be more efficient in treating sick infants. Inter-hospital transfers may also be costly, both monetarily and emotionally. Alternatively, more sophisticated facilities may provide more costly procedures with little marginal return. Second, there may be important effects of deregionalization on quality and cost of care for healthier infants. Chapter 3 examines one aspect of this question and finds that additional short term NICU supply leads to a higher probability of NICU admission for infants above the very low birth weight threshold.

Third, if mothers value shorter travel time and more convenient visitation of family members, access to at least some level of intensive care at nearby hospitals may increase utility. Also, more competition in the neonatal intensive care market may lead to lower prices. Ho et al. (2007) study the market for Whipple surgery, a treatment for pancreatic cancer, and find that regionalizing this treatment by consolidating it to the hospitals with the highest volume leads to substantial price increases.[34] Finally, further research is warranted to understand the determinants of NICU adoption by hospitals and whether hospitals are able to recoup their fixed costs by attracting profitable patients.

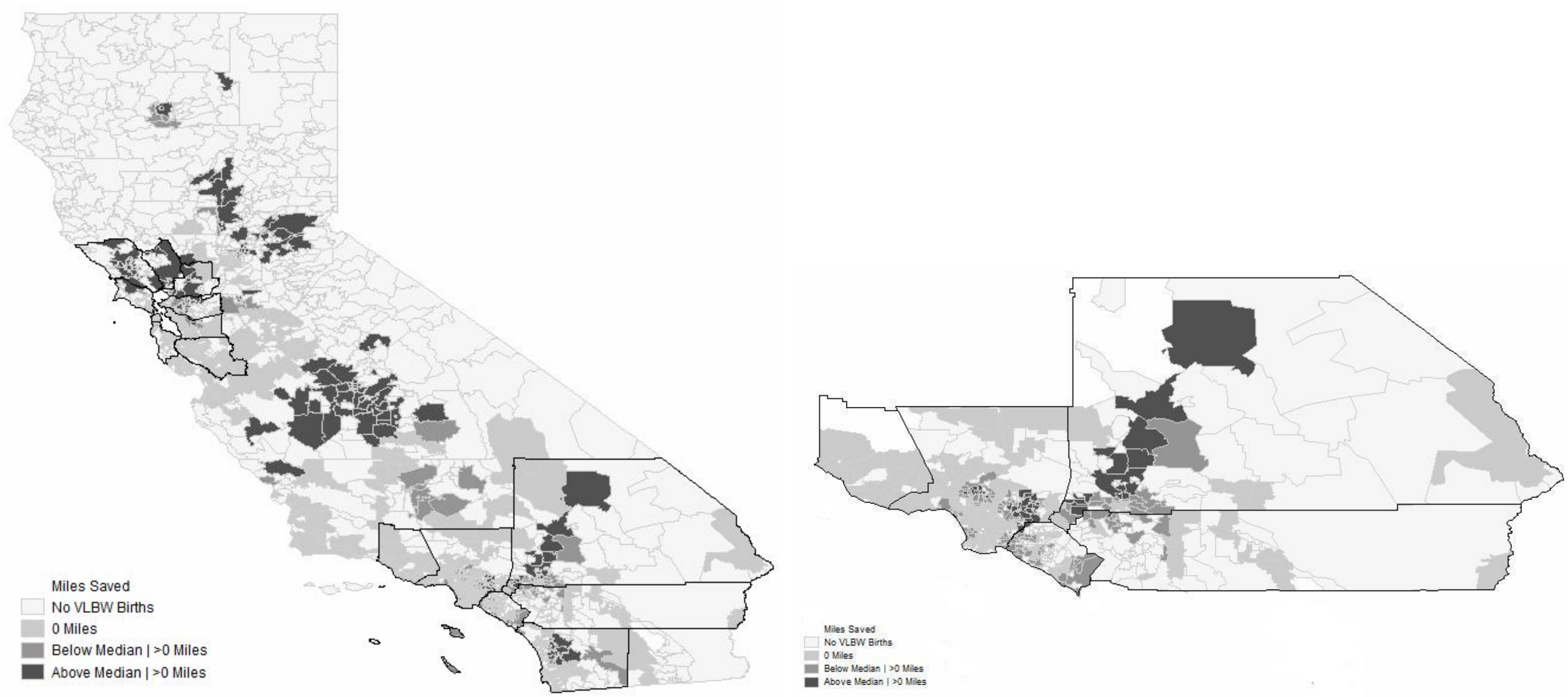
[34] These authors do find that regionalization of Whipple surgery can reduce mortality, but price increases cancel out over half of the increased consumer surplus.

Figure 2.1: NICU Location by Level of Care in 1991



Notes: The light gray lines outline counties in the San Fransisco Bay, Los Angeles Metro, and San Diego Metro areas. See text for definitions of the levels of care.

Figure 2.2: Miles Saved to Nearest Community NICU or Higher, 1991

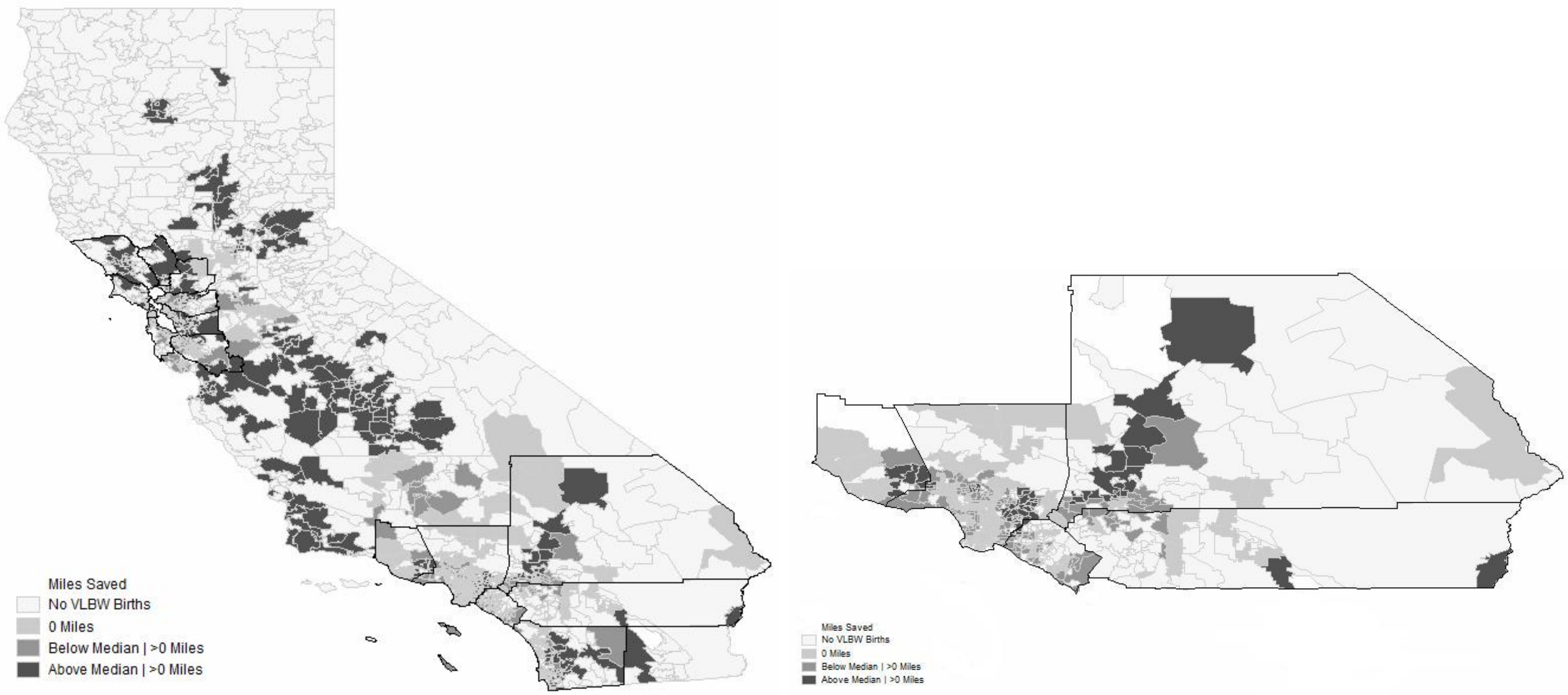


(a) Full State

(b) LA Metro Area

Notes: These figures shade zip codes based on the number of miles a mother living at the center of the zip code saves by choosing the nearest Community NICU or higher over the nearest Regional NICU. Zip codes shaded in white indicate no very low birth weight births in my analysis sample. Remaining zip codes are divided into three groups: those saving zero miles, and those above and below the median conditional on non-zero differential distance. The dark lines in Panel A outline counties in the San Fransisco Bay, Los Angeles Metro, and San Diego Metro areas.

Figure 2.3: Miles Saved to Nearest Intermediate NICU or Higher, 1991

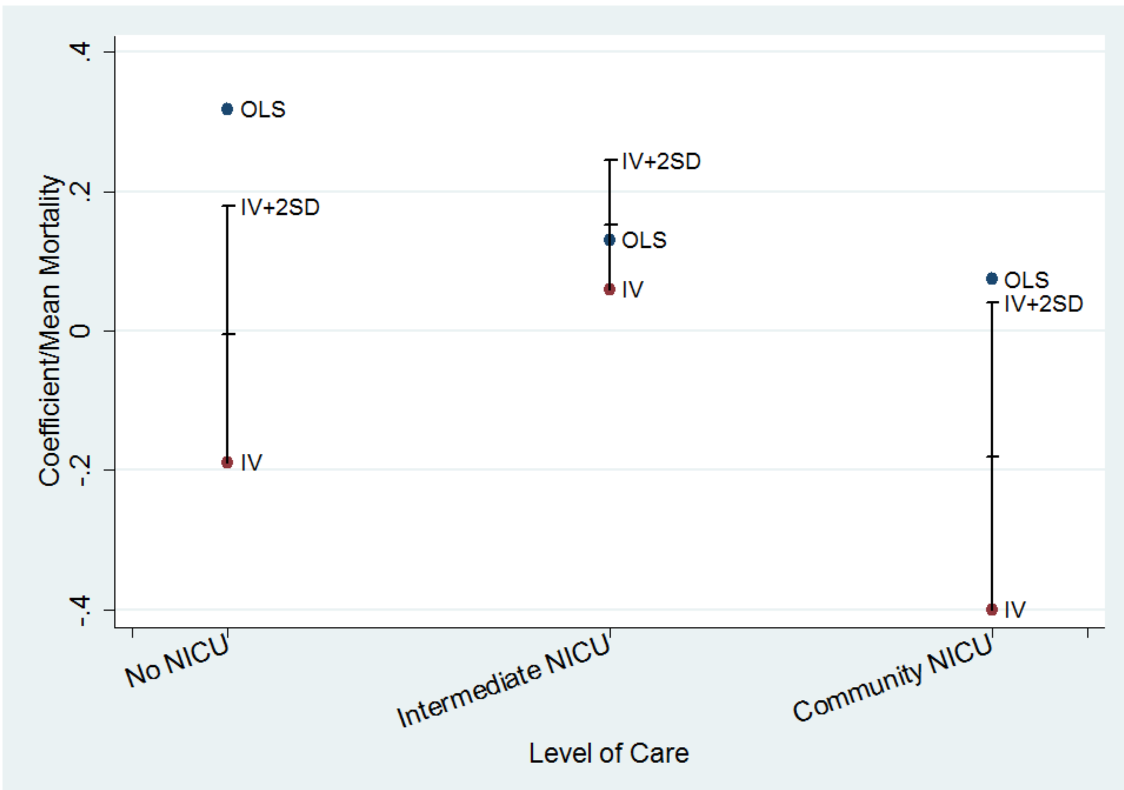


(a) Full State

(b) LA Metro Area

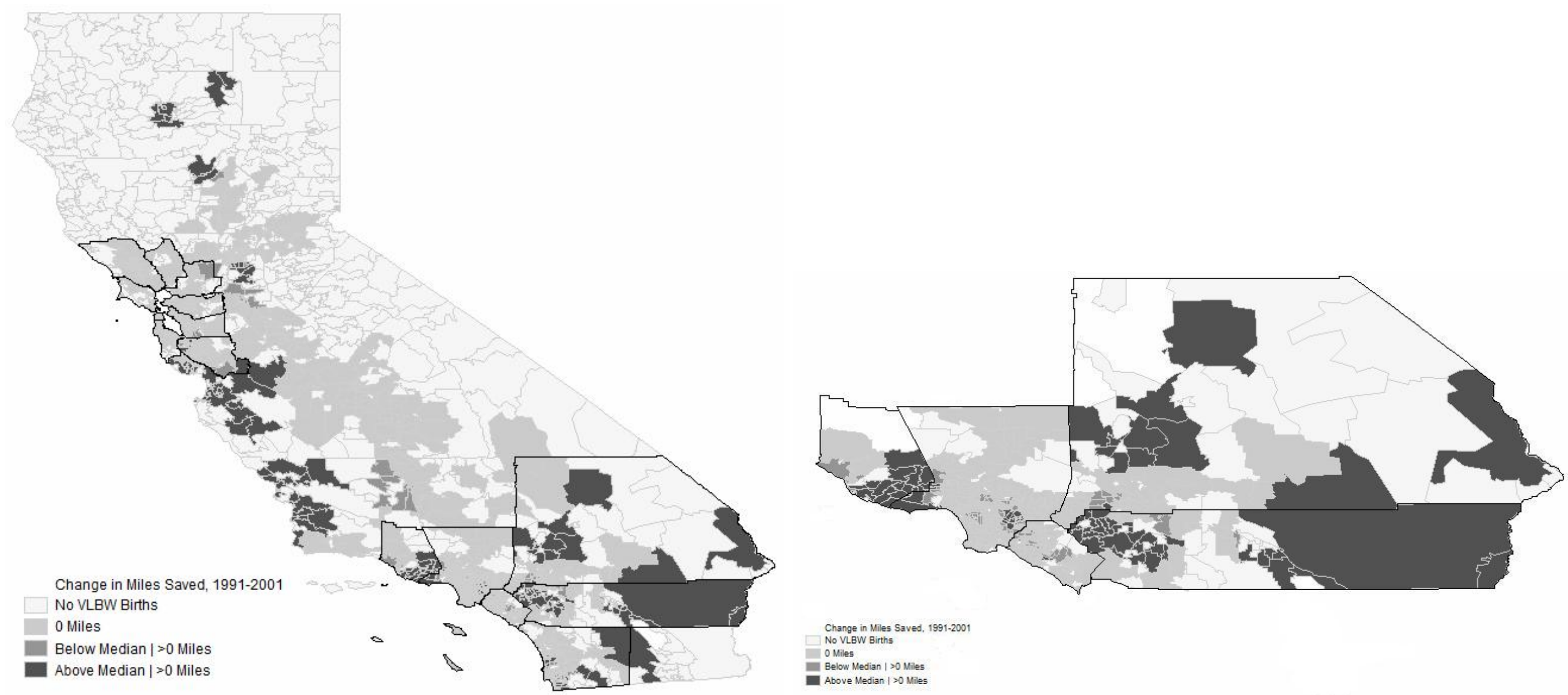
Notes: These figures shade zip codes based on the number of miles a mother living at the center of the zip code saves by choosing the nearest Intermediate NICU or higher over the nearest Regional NICU. Zip codes shaded in white indicate no very low birth weight births in my analysis sample. Remaining zip codes are divided into three groups: those saving zero miles, and those above and below the median conditional on non-zero differential distance. The dark lines in Panel A outline counties in the San Fransisco Bay, Los Angeles Metro, and San Diego Metro areas.

Figure 2.4: Coefficient Estimate Magnitudes



Notes: This figure plots the OLS and 2SLS coefficient estimates from Table 2.8 divided by mean neonatal mortality (15.7%). The dashed points indicate one and two standard deviation intervals above the 2SLS coefficient estimates.

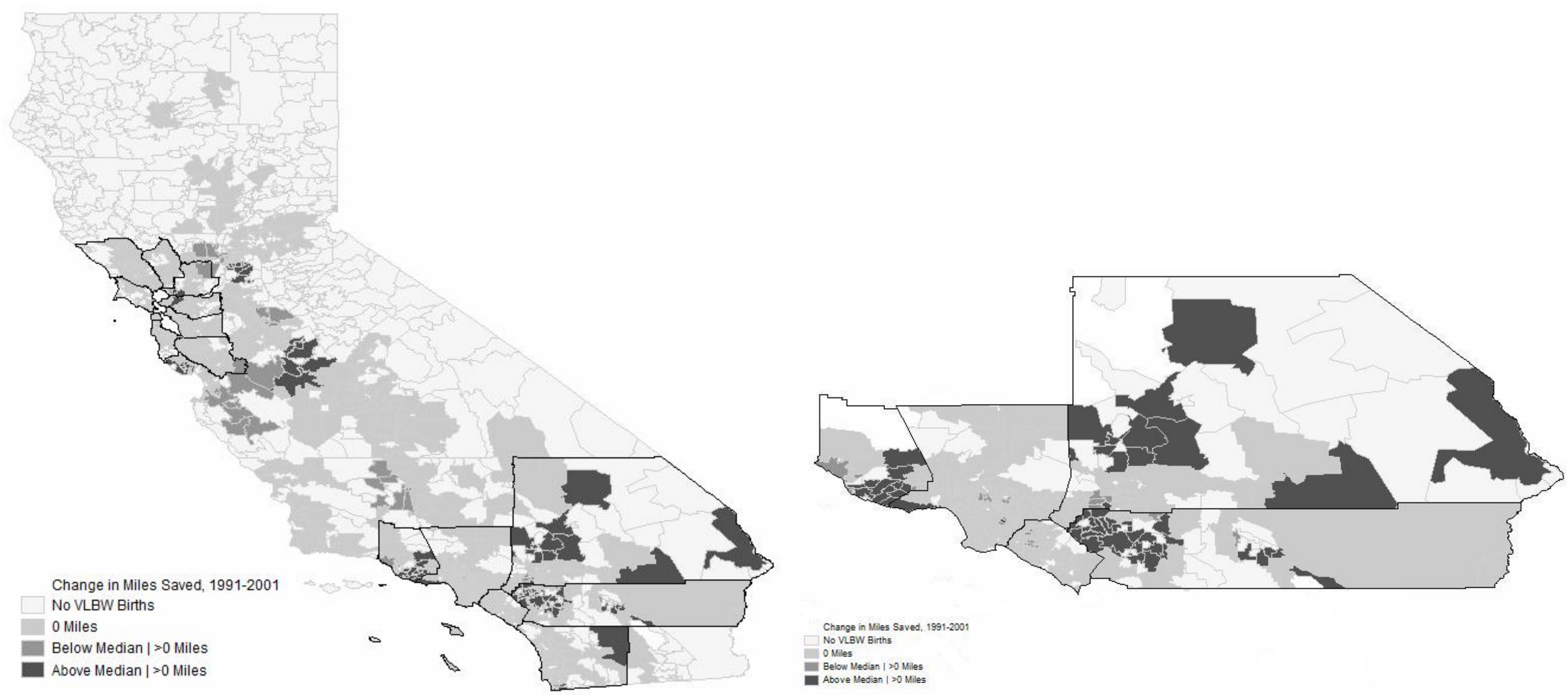
Figure 2.5: Changes in Community Distance, 1991 to 2001



(a) Full State (b) LA Metro Area

Notes: These figures shade zip codes based on changes from 1991 to 2001 in the number of miles a mother living at the center of the zip code saves by choosing the nearest Community NICU or higher over the nearest Regional NICU. Zip codes shaded in white indicate no very low birth weight births in my analysis sample. Remaining zip codes are divided into three groups: those that become no closer, slightly closer (changes below the median), and much closer (changes above the median). The dark lines in Panel A outline counties in the San Fransisco Bay, Los Angeles Metro, and San Diego Metro areas.

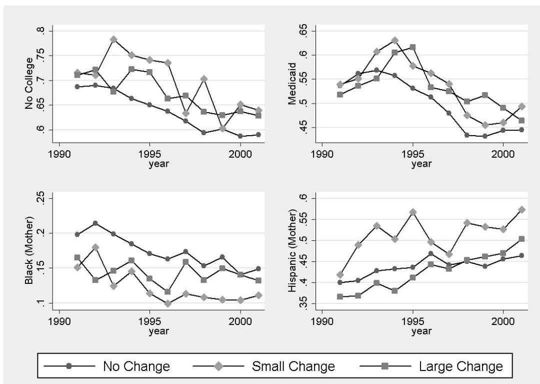
Figure 2.6: Changes in Intermediate Distance, 1991 to 2001



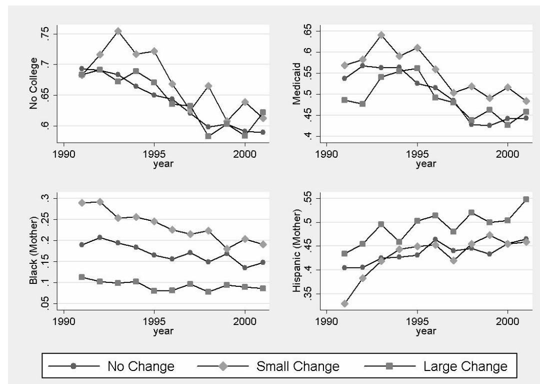
(a) Full State (b) LA Metro Area

Notes: These figures shade zip codes based on changes from 1991 to 2001 in the number of miles a mother living at the center of the zip code saves by choosing the nearest Intermediate NICU or higher over the nearest Regional NICU. Zip codes shaded in white indicate no very low birth weight births in my analysis sample. Remaining zip codes are divided into three groups: those that become no closer, slightly closer (changes below the median), and much closer (changes above the median). The dark lines in Panel A outline counties in the San Fransisco Bay, Los Angeles Metro, and San Diego Metro areas.

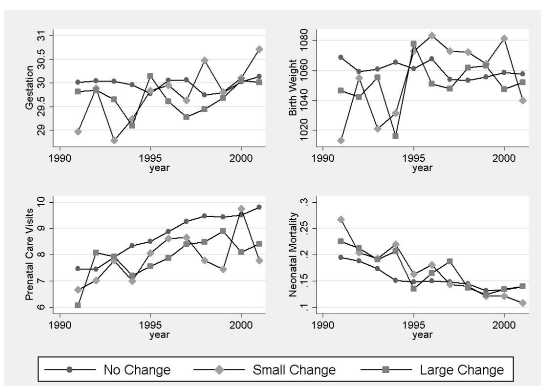
Figure 2.7: Demographic and Health Trends by Changes in Distance



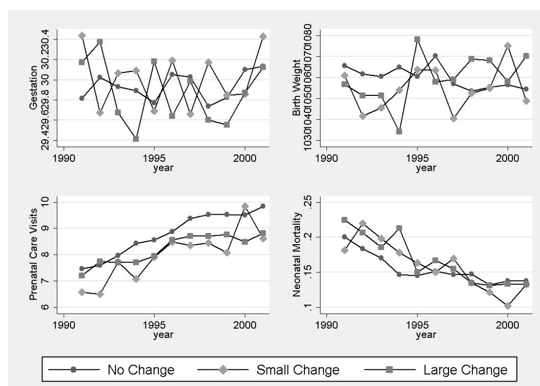
(a) Demographics by Δ IntDist



(b) Demographics by Δ ComDist



(c) Health Characteristics by Δ IntDist



(d) Health Characteristics by Δ ComDist

Notes: These figures plot means of mothers' demographic and infants' health characteristics by changes in differential distance to Intermediate and Community NICUs. Observations are divided into three groups based on whether the zip code of residence becomes no closer, slightly closer (changes below the median), or much closer (changes above the median) to the respective level of care between 1991 and 2001. N=42,912.

Table 2.1: Detailed Level of Care Definitions

Level	Care Provided
I	Basic neonatal care for healthy infants No Intensive Care Unit
II	Have an intensive care unit Care for midly ill infants Do not provide mechanical ventilation
IIIA	Provide mechanical ventilation with restrictions (e.g., only for less than 96 hours, or only for infants weighing above 1,000 grams)
IIIB	Provide mechanical ventilation without restrictions
IIIC	Provide major neonatal surgery excluding cardiac surgery requiring bypass and/or extracorporeal membrane oxygenation (ECMO)
IIID	Provide cardiac surgery requiring bypass and/or ECMO

Notes: Level of neonatal care definitions from Phibbs et al. (2007). There are three ICD-9 CM codes indicating mechanical ventilation: <96 hours, >96 hours, and duration unknown. Hospitals with NICU beds that do not have occurrences of any of these codes are labeled as Level II. In distinguishing between Level IIIA and IIIB, Phibbs et al. (2007) count units that never provide ventilation for more than 96 hours as IIIA. For units that provide both types but do not provide any surgery, they examine the patterns of ventilation by duration and birth weight to distinguish which appear to have restrictions.

Table 2.2: California Obstetric Hospitals by Year and Level of Care

Year	No NICU	Intermediate NICU	Community NICU	Regional NICU	Total
1991	161	58	35	42	296
1992	153	52	43	44	292
1993	149	53	45	45	292
1994	147	56	45	45	293
1995	148	49	51	46	294
1996	140	48	54	46	288
1997	141	47	55	46	289
1998	139	45	58	46	288
1999	135	44	60	46	285
2000	130	45	57	45	277
2001	122	45	57	45	269

Notes: Author's tabulations based on data from Phibbs et al. (2007) and OSHPD Annual Utilization Files. See level of care definitions in text.

Table 2.3: Sample Means by Level of Care at Birth Hospital

	No NICU	Intermediate NICU	Community NICU	Regional NICU
Mother's Demographics				
Age	25.781	26.861	27.939	28.084
Black	0.098	0.205	0.128	0.186
Hispanic	0.567	0.374	0.454	0.434
Medicaid	0.591	0.546	0.455	0.508
HMO	0.148	0.213	0.276	0.212
Self Pay	0.095	0.045	0.031	0.022
No College	0.788	0.679	0.643	0.654
Some College	0.151	0.199	0.195	0.183
College	0.061	0.122	0.162	0.163
Infant Characteristics				
Month Prenatal Care Began	2.323	2.321	2.190	2.202
# of Prenatal Visits	6.692	8.209	8.718	8.873
Parity	2.349	2.358	2.209	2.289
Male	0.542	0.521	0.512	0.511
Multiple Birth	0.167	0.210	0.218	0.244
Birth Weight (Grams)	1067.017	1063.166	1064.203	1055.371
Gestation (Weeks)	30.079	30.083	29.836	29.928
Clinical Condition	0.153	0.192	0.237	0.306
Small for Gest.	0.034	0.049	0.065	0.055
Large for Gest.	0.008	0.009	0.012	0.021
Treatment				
Total Length of Stay	39.179	44.197	50.828	53.319
Total Charges ($1,000s)	156.595	158.987	204.456	228.216
Charges/Day ($1,000s)	1.656	2.894	4.059	4.136
Ventilation	0.136	0.235	0.571	0.556
Transfer	0.706	0.638	0.209	0.114
Outcomes				
28 Day Mortality	0.202	0.150	0.139	0.131
1 Year Mortality	0.235	0.185	0.167	0.160
Neonatal Mortality	0.219	0.169	0.155	0.147
28 Day Readmission	0.043	0.036	0.011	0.007
1 Yr Readmission	0.223	0.240	0.204	0.198
Observations	3,268	4,788	10,136	24,720
# of Hospitals	142	49	51	45

Notes: Columns display sample means for infants delivered in hospitals at four levels of care. Total Length of Stay and Total Charges sum length of stay and hospital charges over all contiguous hospitalizations prior to first being discharged home or dying. Neonatal mortality is mortality within twenty-eight days of birth or within one year if an infant is continuously hospitalized since birth. Number of hospitals indicates the average number of hospitals providing each level of care over the 11-year sample. See Table 2.2 for number of hospitals by year.

Table 2.4: Summary Statistics of Distance Variables

	Mean	SD
D(No+)	3.673	4.206
D(Int+)	5.709	8.065
D(Com+)	8.064	11.983
D(Reg)	14.830	22.991
NoDist	11.156	21.723
IntDist	9.120	20.249
ComDist	6.766	18.446
N	42,912	

Notes: The first four rows show the mean and standard deviation of distance to the nearest hospital offering each level of care or higher. The next three rows show the mean and standard deviation of differential distance to the nearest hospital offering each level of care or higher relative to the nearest Regional NICU.

Table 2.5: Sample Means by Distance

	By Miles Saved to Nearest No +			By Miles Saved to Nearest Int +			By Miles Saved to Nearest Com +		
	0	<Median	>Median	0	<Median	>Median	0	<Median	>Median
Distance									
Miles Saved No+	0.000	1.982	26.378	1.605	3.137	28.981	4.010	4.500	32.040
Miles Saved Int+	0.000	1.488	21.695	0.000	2.000	25.604	2.177	3.108	28.958
Miles Saved Com+	0.000	0.919	16.277	0.000	1.257	19.219	0.000	1.584	25.420
Mother's Demographics									
Age	27.826	27.913	27.517	27.787	27.936	27.490	27.752	28.038	27.411
Black	0.227	0.196	0.107	0.208	0.194	0.102	0.207	0.163	0.095
Hispanic	0.434	0.457	0.431	0.475	0.414	0.436	0.438	0.431	0.462
Medicaid	0.519	0.517	0.488	0.524	0.501	0.493	0.521	0.483	0.499
HMO	0.213	0.223	0.227	0.205	0.239	0.224	0.211	0.242	0.224
Self Pay	0.031	0.030	0.035	0.034	0.030	0.033	0.033	0.034	0.030
No College	0.684	0.668	0.650	0.694	0.648	0.650	0.676	0.645	0.661
Some College	0.170	0.181	0.198	0.171	0.189	0.197	0.178	0.188	0.197
College	0.146	0.151	0.152	0.135	0.163	0.153	0.147	0.166	0.142
Infant Characteristics									
Mth Prenatal Care Began	2.240	2.214	2.220	2.241	2.213	2.210	2.244	2.203	2.194
# of Prenatal Visits	8.196	8.733	8.679	8.277	8.792	8.728	8.537	8.628	8.683
Parity	2.390	2.247	2.259	2.370	2.229	2.246	2.316	2.228	2.270
Male	0.502	0.514	0.522	0.506	0.515	0.522	0.512	0.511	0.523
Multiple Birth	0.223	0.224	0.236	0.218	0.231	0.236	0.227	0.233	0.226
Birth Weight (Grams)	1056.087	1059.704	1060.439	1056.993	1061.276	1059.441	1060.140	1060.478	1056.107
Gestation (Weeks)	30.033	29.909	29.905	29.980	29.963	29.859	29.975	29.925	29.860
Clinical Condition	0.281	0.279	0.242	0.273	0.286	0.236	0.287	0.276	0.212
Small for Gest.	0.061	0.055	0.051	0.058	0.057	0.050	0.057	0.057	0.050
Large for Gest.	0.018	0.019	0.013	0.018	0.018	0.013	0.018	0.016	0.013
Zip Code Characteristics									
Med HH Income ($1,000)	40.511	43.736	46.599	40.221	45.705	46.690	43.265	45.441	44.694
Percent Urban	0.986	0.987	0.924	0.978	0.984	0.925	0.973	0.980	0.923
Population Density	8683.053	9752.449	3162.460	9185.203	8225.657	3312.801	8142.096	7983.427	3456.633
Observations	9,247	16,776	16,889	14,585	14,147	14,180	21,440	10,719	10,753

Notes: The first three columns display sample means by differential distance to the nearest hospital with any obstetric services, the second three columns by differential distance to the nearest Intermediate NICU or higher, and the final three columns by differential distance to the nearest Community NICU or higher. For each set of columns, the sample is divided into three groups: those with zero differential distance, and those above and below the median conditional on non-zero differential distance.

Table 2.6: Neonatal Mortality by Level of Care, OLS Estimates

	Dependent Variable: Neonatal Mortality			
	(1)	(2)	(3)	(4)
I(No NICU)	0.072**	0.072**	0.054**	0.050**
	(0.009)	(0.008)	(0.009)	(0.007)
I(Intermediate NICU)	0.022**	0.021**	0.017**	0.021**
	(0.007)	(0.006)	(0.006)	(0.005)
I(Community NICU)	0.008*	0.013**	0.010**	0.012**
	(0.004)	(0.004)	(0.004)	(0.004)
Time FE		X	X	X
Demographics			X	X
Health Controls				X

Notes: Each column lists estimates with standard errors in parentheses (clustered at the zip code level) from separate regressions of neonatal mortality on indicators for delivery in a hospital with No NICU, an Intermediate NICU, and a Community NICU. Regional NICU is the excluded group. The columns successively add controls. Time fixed effects include year dummies, month-of-year dummies, and day-of-week dummies. Demographics include age, age squared, race, ethnicity, and insurance coverage. Health controls include number of prenatal care visits, month in which prenatal care began, parity, sex, multiple birth status, an indicator for having a clinical condition, indicators for small and large for gestational age, and birth weight dummies at 100 gram increments. N = 42,912; * $p<.10$, ** $p<.05$

Table 2.7: Level of Care by Distance, First Stage Estimates

Dep. Var.:	I(No NICU)				I(Intermediate NICU)				I(Community NICU)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
NoDist	0.116**	0.115**	0.112**	0.111**	-0.044**	-0.044**	-0.042**	-0.043**	-0.034**	-0.031**	-0.033**	-0.034**
	(0.013)	(0.013)	(0.013)	(0.012)	(0.008)	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)
IntDist	-0.087**	-0.087**	-0.085**	-0.084**	0.126**	0.126**	0.124**	0.125**	-0.025**	-0.026**	-0.024**	-0.024**
	(0.015)	(0.015)	(0.015)	(0.014)	(0.013)	(0.014)	(0.013)	(0.013)	(0.009)	(0.009)	(0.009)	(0.009)
ComDist	-0.025**	-0.025**	-0.025**	-0.025**	-0.027**	-0.027**	-0.027**	-0.027**	0.077**	0.073**	0.074**	0.074**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
F-Stat	33.47	33.22	32.47	32.46	43.53	43.69	44.54	44.56	40.91	37.18	38.48	38.35
Time FE		X	X	X		X	X	X		X	X	X
Demog.			X	X			X	X			X	X
Health				X				X				X

Notes: Each column lists coefficient estimates with standard errors in parentheses (clustered at the zip code level) from separate regressions of a level of care indicator on the distance instruments. See notes to Table 2.6 for details of control variables. N = 42,912; * p<.10, ** p<.05

Table 2.8: Neonatal Mortality by Level of Care, 2SLS Estimates

Dependent Variable:	OLS	2SLS
Neonatal Mortality	(1)	(2)
I(No NICU)	0.050**	-0.030
	(0.007)	(0.029)
I(Intermediate NICU)	0.021**	0.009
	(0.005)	(0.015)
I(Community NICU)	0.012**	-0.063*
	(0.004)	(0.035)

Notes: This table lists OLS and 2SLS coefficient estimates with standard errors in parentheses (clustered at the zip code level) of neonatal mortality on indicators for delivery in a hospital with No NICU, an Intermediate NICU, and a Community NICU. Regional NICU is the excluded group. All controls described in the notes to Table 2.6 are included in both columns. N = 42,912; * p<.10, ** p<.05

Table 2.9: Level of Care by Distance for Heavier Infants

	1,500 to 2,500 Grams			2,500 to 3,000 Grams			3,000 to 4,500 Grams		
Dependent Var:	I(No NICU)	I(Inter)	I(Comm)	I(No NICU)	I(Inter)	I(Comm)	I(No NICU)	I(Inter)	I(Comm)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
NoDist	0.199**	-0.069**	-0.059**	0.240**	-0.081**	-0.071**	0.251**	-0.083**	-0.077**
	(0.019)	(0.010)	(0.008)	(0.023)	(0.011)	(0.009)	(0.023)	(0.010)	(0.009)
IntDist	-0.173**	0.188**	-0.009	-0.221**	0.209**	0.002	-0.239**	0.209**	0.011
	(0.021)	(0.016)	(0.010)	(0.025)	(0.018)	(0.011)	(0.025)	(0.017)	(0.012)
ComDist	-0.022**	-0.074**	0.086**	-0.015*	-0.087**	0.082**	-0.005	-0.090**	0.080**
	(0.008)	(0.010)	(0.007)	(0.009)	(0.012)	(0.007)	(0.007)	(0.010)	(0.007)
F-Stat	43.27	51.50	57.03	41.99	51.42	59.40	46.84	49.76	64.78
N	237,488	237,488	237,488	751,750	751,750	751,750	3,705,006	3,705,006	3,705,006

Notes: Each column lists coefficient estimates with standard errors in parentheses (clustered at the zip code level) from separate regressions of delivery hospital level of care indicators on No NICU distance, Intermediate distance, and Community distance. Each panel presents estimates for a different sample, stratified by birth weight. All regressions include all controls described in the notes to Table 2.6. * p<.10, ** p<.05

Table 2.10: Reduced Form Estimates

	Dependent Variable: Neonatal Mortality			
	(1)	(2)	(3)	(4)
NoDist	0.003	0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.003)
IntDist	0.002	0.002	0.004	0.005
	(0.005)	(0.005)	(0.005)	(0.003)
ComDist	-0.005**	-0.003	-0.003	-0.004**
	(0.003)	(0.003)	(0.003)	(0.002)
Time FE		X	X	X
Demographics			X	X
Health Controls				X

Notes: Each column lists OLS coefficient estimates with standard errors in parentheses (clustered at the zip code level) from separate regressions of neonatal mortality on No NICU distance, Intermediate distance, and Community distance. Columns successively add controls. See notes to Table 2.6 for details of control variables. N = 42,912; * p<.10, ** p<.05

Table 2.11: Alternative Specifications: First Stage Estimates

	NoDist		IntDist		ComDist		NoDistXDensity		IntDistXDensity		ComDistXDensity		F-Stat
A. Baseline													
I(No NICU)	0.111**	(0.012)	-0.084**	(0.014)	-0.025**	(0.007)							32.46
I(Intermediate)	-0.043**	(0.008)	0.125**	(0.013)	-0.027**	(0.009)							44.56
I(Community)	-0.034**	(0.006)	-0.024**	(0.009)	0.074**	(0.008)							38.35
B. Zip Code Level Controls													
I(No NICU)	0.105**	(0.012)	-0.079**	(0.014)	-0.025**	(0.007)							29.83
I(Intermediate)	-0.038**	(0.007)	0.120**	(0.013)	-0.026**	(0.009)							44.01
I(Community)	-0.045**	(0.008)	-0.015	(0.010)	0.073**	(0.008)							38.30
I(No NICU)	0.097**	(0.012)	-0.066**	(0.014)	-0.025**	(0.008)	0.020**	(0.005)	-0.022**	(0.005)	0.002	(0.002)	26.60
I(Intermediate)	-0.023**	(0.007)	0.080**	(0.013)	-0.010	(0.011)	-0.011**	(0.004)	0.034**	(0.009)	-0.020**	(0.008)	46.54
I(Community)	-0.046**	(0.007)	0.009	(0.010)	0.053**	(0.008)	-0.007	(0.006)	-0.012*	(0.007)	0.018**	(0.005)	17.92
C. Zip Code Level Fixed Effects													
I(No NICU)	0.085**	(0.016)	-0.111**	(0.017)	-0.009	(0.006)							16.06
I(Intermediate)	-0.000	(0.025)	0.124**	(0.017)	-0.045**	(0.005)							46.08
I(Community)	0.021	(0.036)	0.074**	(0.028)	0.072**	(0.011)							21.50
I(No NICU)	0.075**	(0.019)	-0.108**	(0.018)	-0.005	(0.006)	0.012**	(0.005)	-0.006	(0.006)	-0.005	(0.003)	11.92
I(Intermediate)	-0.001	(0.025)	0.107**	(0.020)	-0.041**	(0.005)	-0.008	(0.005)	0.021**	(0.007)	-0.005	(0.005)	28.94
I(Community)	-0.004	(0.037)	0.048*	(0.028)	0.060**	(0.011)	-0.002	(0.011)	0.029**	(0.012)	0.012*	(0.007)	13.61
D. Pooling No and Intermediate NICUs													
No/Interm	0.093**	(0.007)			-0.037**	(0.008)							114.74
Community	-0.048**	(0.005)			0.065**	(0.006)							60.04

Notes: Each row lists coefficient estimates with standard errors in parentheses (clustered at the zip code level) from separate regressions of delivery hospital level of care indicators on No NICU distance, Intermediate distance, and Community distance. All regressions include all controls described in the notes to Table 2.6. N = 42,912; * p<.10, ** p<.05

Table 2.12: Alternative Specifications: OLS & 2SLS Estimates

	I(No NICU)		I(Intermediate NICU)		I(Community NICU)	
A. Baseline						
OLS	0.050**	(0.007)	0.021**	(0.005)	0.012**	(0.004)
2SLS	-0.030	(0.029)	0.009	(0.015)	-0.063*	(0.035)
B. Zip Code Level Controls						
OLS	0.051**	(0.007)	0.022**	(0.005)	0.012**	(0.004)
2SLS	-0.041	(0.037)	0.008	(0.015)	-0.072*	(0.038)
2SLS (Density Interaction)	-0.003	(0.029)	0.003	(0.013)	-0.027	(0.033)
C. Zip Code Fixed Effects						
OLS	0.055**	(0.007)	0.034**	(0.006)	0.014**	(0.004)
2SLS	-0.186*	(0.108)	0.005	(0.069)	-0.086	(0.056)
2SLS (Density Interaction)	-0.074	(0.098)	0.064	(0.065)	-0.028	(0.045)
D. Pooling No NICU and Intermediate NICU						
OLS			0.032**	(0.004)	0.011**	(0.004)
2SLS			-0.001	(0.012)	-0.034	(0.031)

Notes: Each row lists OLS and 2SLS coefficient estimates with standard errors in parentheses (clustered at the zip code level) of neonatal mortality on indicators for delivery in a hospital with No NICU, an Intermediate NICU, and a Community NICU. Regional NICU is the excluded group. All regressions include all controls described in the notes to Table 2.6. N = 42,912; * p<.10, ** p<.05

Table 2.13: Alternative Control Variables and Clustering

	Baseline	C-Section Control	BW Dummies × Male	BW Dummies in 50 Grams	Specific Clin. Cond. Controls	Cluster at HSA	Cluster at Hospital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OLS Estimates							
I(No NICU)	0.050**	0.049**	0.049**	0.050**	0.049**	0.050**	0.050**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
I(Intermediate NICU)	0.021**	0.020**	0.021**	0.020**	0.020**	0.021**	0.021**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.009)
I(Community NICU)	0.012**	0.013**	0.011**	0.012**	0.012**	0.012**	0.012*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.007)
2SLS Estimates							
I(No NICU)	-0.030	-0.028	-0.030	-0.025	-0.030	-0.030	-0.030
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.030)	(0.034)
I(Intermediate NICU)	0.009	0.009	0.010	0.009	0.010	0.009	0.009
	(0.015)	(0.015)	(0.014)	(0.014)	(0.015)	(0.015)	(0.023)
I(Community NICU)	-0.063*	-0.058*	-0.063*	-0.057*	-0.060*	-0.063**	-0.063
	(0.035)	(0.035)	(0.034)	(0.034)	(0.035)	(0.029)	(0.043)

Notes: Each column lists OLS and 2SLS coefficient estimates with standard errors in parentheses (clustered at the zip code level) of neonatal mortality on indicators for delivery in a hospital with No NICU, an Intermediate NICU, and a Community NICU. Regional NICU is the excluded group. All regressions include all controls described in the notes to Table 2.6 plus additional controls noted in column headings. N = 42,912; * p<.10, ** p<.05

Table 2.14: Alternative Mortality Measures

	Neonatal Mortality	Mortality within: 1 Day	28 Days	1 Year
	(1)	(2)	(3)	(4)
OLS Estimates				
I(No NICU)	0.050**	0.026**	0.048**	0.052**
	(0.007)	(0.005)	(0.006)	(0.007)
I(Intermediate NICU)	0.021**	0.017**	0.018**	0.024**
	(0.005)	(0.004)	(0.005)	(0.006)
I(Community NICU)	0.012**	0.003	0.011**	0.012**
	(0.004)	(0.003)	(0.004)	(0.004)
2SLS Estimates				
I(No NICU)	-0.030	-0.054**	-0.030	-0.014
	(0.029)	(0.020)	(0.026)	(0.027)
I(Intermediate NICU)	0.009	0.011	-0.003	0.021
	(0.015)	(0.011)	(0.014)	(0.014)
I(Community NICU)	-0.063*	-0.099**	-0.097**	-0.037
	(0.035)	(0.027)	(0.033)	(0.035)
Mean Mortality	0.157	0.063	0.140	0.170

Notes: Each column lists OLS and 2SLS coefficient estimates with standard errors in parentheses (clustered at the zip code level) of neonatal mortality on indicators for delivery in a hospital with No NICU, an Intermediate NICU, and a Community NICU. Regional NICU is the excluded group. All regressions include all controls described in the notes to Table 2.6. * p<.10, ** p<.05

Table 2.15: Heterogeneity

	Baseline	Hispanic	Non-Black	Medicaid	No College	Suburban	1991 - 1995
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OLS Estimates							
I(No NICU)	0.050**	0.049**	0.056**	0.042**	0.048**	0.050**	0.059**
	(0.007)	(0.009)	(0.007)	(0.009)	(0.008)	(0.008)	(0.010)
I(Intermediate NICU)	0.021**	0.028**	0.022**	0.018**	0.027**	0.028**	0.017**
	(0.005)	(0.009)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
I(Community NICU)	0.012**	0.010*	0.013**	0.007	0.011**	0.010*	0.013**
	(0.004)	(0.005)	(0.004)	(0.006)	(0.005)	(0.005)	(0.006)
2SLS Estimates							
I(No NICU)	-0.030	-0.023	-0.039	-0.017	-0.018	-0.029	-0.024
	(0.029)	(0.036)	(0.030)	(0.036)	(0.033)	(0.034)	(0.042)
I(Intermediate NICU)	0.009	0.025	0.012	-0.001	0.027	0.022	0.002
	(0.015)	(0.022)	(0.015)	(0.020)	(0.020)	(0.019)	(0.022)
I(Community NICU)	-0.063*	-0.040	-0.080**	-0.047	-0.090**	-0.059	-0.078
	(0.035)	(0.059)	(0.035)	(0.049)	(0.045)	(0.041)	(0.060)
N	42,912	18,974	35,705	21,707	27,651	21,464	19,861
Mean Mortality	0.157	0.168	0.159	0.160	0.162	0.157	0.175

Notes: Each column lists OLS and 2SLS coefficient estimates with standard errors in parentheses (clustered at the zip code level) of neonatal mortality on indicators for delivery in a hospital with No NICU, an Intermediate NICU, and a Community NICU. Regional NICU is the excluded group. All regressions include all controls described in the notes to Table 2.6. * $p<.10$, ** $p<.05$

Table 2.16: Robustness to Sample Restrictions

	Baseline	Include Rural	Include Kaiser	Include Cong. Anom.	Include Fetal Death
	(1)	(2)	(3)	(4)	(5)
OLS Estimates					
I(No NICU)	0.050**	0.054**	0.052**	0.058**	0.110**
	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
I(Intermediate NICU)	0.021**	0.023**	0.026**	0.029**	0.056**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
I(Community NICU)	0.012**	0.013**	0.013**	0.011**	0.023**
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
2SLS Estimates					
I(No NICU)	-0.030	-0.006	-0.017	-0.000	-0.066**
	(0.029)	(0.028)	(0.028)	(0.029)	(0.028)
I(Intermediate NICU)	0.009	0.005	0.013	0.018	0.014
	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)
I(Community NICU)	-0.063*	-0.020	-0.055*	-0.050	-0.093**
	(0.035)	(0.033)	(0.033)	(0.036)	(0.036)
N	42,912	44,937	49,018	47,121	51,123
Mean Mortality	0.157	0.157	0.158	0.167	0.291

Notes: Each column lists OLS and 2SLS coefficient estimates with standard errors in parentheses (clustered at the zip code level) of neonatal mortality on indicators for delivery in a hospital with No NICU, an Intermediate NICU, and a Community NICU. Regional NICU is the excluded group. All regressions include all controls described in the notes to Table 2.6. * p<.10, ** p<.05

Chapter 3

The Effect of Neonatal Intensive Care Availability on Utilization

3.1 Introduction

Amid rising health care costs and the political debate over health reform, excessive utilization of health care resources is an important topic. One concern is that the availability of supply directly leads to additional utilization of health resources. The large amount of regional variation in health expenditures, which appears not to be correlated with health outcomes (Fisher et al., 2003a,b), provides some evidence that health care is consumed to the point where the marginal benefit is below the marginal cost. That said, it is difficult to identify whether a determinant of over utilization is that simply the availability of medical resources leads to additional utilization. Theoretically, physicians and hospitals face financial incentives to provide additional care. Additionally, moral hazard in insurance can lead to over utilization when supply is available and patients are insulated from the full cost of their care. However, as Fuchs (2004) points out, empirically testing the hypothesis that availability itself leads to excessive utilization requires variation in supply that is uncorrelated with variation in demand, and cross sectional comparisons are likely to be insufficient.

In this chapter, I conduct a first examination of the effect of supply on utilization in the context of neonatal intensive care. I estimate the effect of the number of empty beds available in the NICU the day prior to birth on the probability that an infant is admitted to the NICU. To overcome the endogeneity between supply and utilization, I estimate regressions with hospital-specific month fixed effects. These fixed effects allow me to flexibly control for many unobserved factors that might be correlated with NICU admission and exploit within-hospital month variation in the availability of NICU beds. Within hospital-month deviations in the availability of NICU beds is unlikely to be correlated with changes in the health of infants born in that hospital, and I provide empirical evidence to support this identifying assumption.

Neonatal intensive care is an important and interesting health care market to examine the effect of availability on utilization. It has been claimed that the increase in the supply of NICUs has outpaced demand, and in particular, the growth in the number of small NICUs in community hospitals has been unnecessary (e.g., Baker and Phibbs, 2002; Howell et al., 2002; Schwartz, 1996; Schwartz et al., 2000). In order for a NICU to directly provide revenue to the hospital and income to the physician, the beds must be utilized; therefore, in the context of this deregionalization, there may be particularly large scope for available supply to increase utilization. Furthermore, almost all births in the United States are covered by insurance, so risk-averse parents, insulated from the full cost of their infant's care, may prefer additional care for their infant if it is available.

If the availability of neonatal intensive care directly leads to additional utilization of neonatal intensive care, there are a variety of important costs that would be incurred. First, there is the economic cost associated with using care beyond the point where the marginal benefit outweighs the marginal cost. On its face this cost is a transfer from the insurers to the hospital and physician. But, it will also be passed on to consumers in the form of higher premiums or less coverage and to the government and taxpayers in the case of Medicaid covered infants. In terms of social welfare it is a cost to use these resources beyond their efficient level. Furthermore, 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 relative to insurance reimbursements, hospitals have incentives to increase utilization to recoup these fixed costs. As a result, looking for an effect of available capacity on NICU admission is one way to get a sense of how hospitals attempt to recoup these fixed costs.

There are also psychic costs associated with an infant being cared for in a NICU. The birth of a child is a stressful time for parents, and seeing an infant in intensive care and thinking he or she may have health problems provides additional stress and worry. Additionally, there are potentially *negative* health effects of unnecessary care in the NICU. In particular, there is a literature in epidemiology documenting the role of nosociomal, or hospital borne, infections. These infections lead to mortality, morbidity, and longer lengths of stay, are difficult to predict and diagnose, and their prevalence has risen over recent decades (e.g., Benjamin et al., 2000; Clark et al., 2004; Kossoff et al., 1998).[1] Increased exposure to such infections could be one potential cost of spending time in the NICU.

Also, if the supply of neonatal intensive care affects utilization, it is likely to be strongest for healthier infants just on the margin of needing intensive care. These relatively healthier patients are an important population in terms of the share of NICU case-loads and costs, though they are an understudied group in the literature on neonatal intensive care and its deregionalization. A study by Kirkby et al. (2007) finds that moderately preterm infants with 32-34 weeks of gestation comprise 23.8% of NICU admissions and 21.6% of NICU costs. In one large regional NICU in Massachusetts, Gray et al. (1996) find that normal birth weight infants (those weighing more than 5.5 pounds) account for 35.4% of NICU admissions. Furthermore, 41% requires only monitoring and no intensive treatments, suggesting that perhaps these are in fact marginal NICU cases. These infants also likely represent the group for which the marginal cost of NICU admission is low relative to the

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

marginal return to the hospital from a per diem insurance reimbursement. To the extent that deregionalization provides scope for excess capacity to increase utilization, this chapter provides a first look at how deregionalization might affect the care of healthier infants.

I find that on average, more empty beds on the day prior to an infant's birth does increase the probability of NICU admission. Disaggregating the effects by birth weight categories reveals that the effects are small for very low birth weight infants (those weighing 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% 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. These results suggest that empty beds have the smallest effect for the sickest infants who necessitate intensive care regardless of external factors such as supply, and have the largest effect for low birth weight and high birth weight infants, two groups likely to be on the margin of needing intensive care.

It is possible that the effect of availability on utilization is at least partially driven by NICUs that are capacity constrained and must turn away patients when crowded. I cannot completely rule out this possibility, but I argue that, while this mechanism may be present, it is unlikely to be driving the result. A hospital can transfer an infant to another hospitals when its NICU is crowded and has little incentive not to do so. The birth hospital cannot receive a reimbursement for caring for the infant in the NICU if it does not have capacity. When I allow for the fact that infants who are not admitted to the NICU at the birth hospital may be transferred to other hospitals, I find that the effect of empty beds on utilization becomes very small for VLBW infants but shows little change for infants above the

VLBW threshold. This finding suggests that VLBW infants are transferred when the NICU is crowded, but higher birth weight infants admitted to the NICU when more beds are available is likely to represent over-utilization. I also show that the effect of empty beds on NICU admission is still present when I remove hospitals from my sample that are often facing capacity constraints.

In addition to birth weight, I explore heterogeneity of the effect of empty beds on NICU admission along other dimensions. Of particular interest in the context of NICU deregionalization, I find that the effect of empty beds on NICU admission is positive in hospitals at all three levels of care, but is larger in Intermediate and Community NICU hospitals as compared to Regional NICU hospitals. After taking into consideration that lower level NICU hospitals are more likely to transfer infants who need NICU care and differences in the variation in empty beds across levels of care, the differences in the effect size across hospital types decreases but still prevails. This finding suggests that the NICUs most associated with deregionalization have the most scope to increase utilization in response to availability.

It is worth pointing out three limitations of this study. First, while my identifications strategy accounts for many issues associated with understanding the effect of supply on utilization, the data used in this analysis does contain some complicated measurement error. The data I utilize does not explicitly identify which infants are treated in the NICU. I therefore impute NICU admission based on its most likely predictors – length of stay and hospital charges. As a result, both the dependent and independent variables in my analysis are measured with error. Because the measurement error affects the dependent and independent variables in ways that are clearly correlated with each other, this measurement error is non-classical. As a result, it is difficult to account for and difficult to even understand the full effects, though I discuss some of the ramifications below. This chapter provides preliminary estimates of the effect of NICU availability on utilization, although these results may be biased by this measurement error. In future research I will verify my NICU admission algorithm and estimate the effect of empty beds on NICU admission in other data sets that do specify which infants are treated in the NICU. In particular I intend to examine this research question using hospital inpatient data from the state of New York collected by the Statewide Planning and Research Cooperative System (SPARCS). This data set includes accommodation codes that indicate when an infant is cared for in the NICU.

A second important caveat is that my analysis models the decision to admit an infant to the NICU as a function of the conditions in the NICU on the previous day. I do not take into account the health of other infants born in the hospital on the same day, and I treat these observations as independent. Future work is warranted to take these issues into account in a more sophisticated model.

Third, I am not able to tease out the mechanism behind the effect of empty beds on utilization. As discussed in more detail below, there are two potential mechanisms stemming from two different information asymmetries. One mechanism is "supplier-induced demand." This behavior occurs when the physician takes advantage of his agency and information advantage over the patient and prescribes additional care beyond what the patient needs in order to increase revenue. The second mechanism is moral hazard in insurance. The physician and the patient both know more about the patient's true health than the insurance provider. Because insurance insulates the patient from the full cost of care and insurers cannot perfectly monitor the physician and patient's care decisions, patients may over consume medical care.[2]

The remainder of this chapter is organized as follows. Section 3.2 provides a brief review of the previous literature on excessive utilization, including a discussion

[2]Throughout the paper, references to moral hazard will indicate *ex post* moral hazard, as opposed to *ex ante* moral hazard which states that individuals insulated from the price of medical care by insurance will perform more risky behaviors leading to a higher need for care.

of moral hazard and induced demand. Section 3.3 describes the data and the algorithm to impute NICU admission. Section 3.4 discusses the empirical framework. Section 3.5 presents results including a discussion of heterogeneity, and Section 3.6 concludes.

3.2 Previous Literature

There are two sets of very influential papers that provide explanations for the potential over-utilization of health care resources. Moral hazard in insurance, first formalized by Arrow (1963) and Pauly (1968), describes the tendency for patients to spend more on medical care when these expenditures are partially or fully paid by the insurer than they would if they were paying the full cost. In essence, moral hazard is the substitution effect on the part of the patient towards a lower priced good. Insurers are unable to optimally design policies to prevent moral hazard because they cannot fully observe the individuals medical needs. Thus, moral hazard takes place as a result of "hidden action" by the agent (the individual) when the principle (the insurer) cannot fully monitor behavior (Cutler and Zeckhauser, 2000).

Empirically identifying moral hazard is difficult because individuals who need more medical care may demand more insurance. In order overcome this endogeneity, the RAND Health Insurance Experiment was designed to explore the extent of moral hazard. By randomizing coinsurance rates across study participants, they were able to show that more insurance coverage leads to higher health expenditures and estimate that the price elasticity of medical care is around -0.2 (Manning et al., 1987). This study remaines the best empirical evidence to date that health insurance leads to moral hazard (Cutler and Zeckhauser, 2000).

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

The second mechanism behind excessive utilization of health resources is supply (or physician) induced demand as first defined by Evans (1974), Fuchs (1978), and Pauly (1981). Induced demand occurs when the physician, acting as the agent, exploits his information advantage over the patient, who is the principle, and provides excess treatment to increase revenue. 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 in the amount of care provided) and decreasing in inducement. The physician will induce demand to the point where the marginal return to inducement is equal to the marginal utility cost of inducement.[4]

There are two major empirical literatures that attempt to identify demand inducement, one related to income effects and the other substitution effects. The

[3]In the dataset analyzed below, 96% of deliveries in the state of California between 1992 and 2001 were paid for by some form of insurance. Russell et al. (2007) report a similar percentage for infants in the 2001 National Inpatient Sample.

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

early literature on income effects was problematic as it looked for cross sectional relationships between the number of physicians (or physician-to-population ratios) and utilization, the idea being that when there are additional providers in a market, each individual provider's income decreases (McGuire, 2000). The most convincing study of income effects is Gruber and Owings (1996), who look at the decrease in demand associated with decreasing fertility during the 1970s. They find that a 10% decrease in fertility leads to a 0.97 percentage point increase in the rate of cesarean sections, which are more generously reimbursed than normal deliveries. Their result implies that doctors respond to the negative income shock associated with a decrease in demand by altering treatment practices to maintain income. The other major empirical literature considers substitution effects by examining physician responses to fee differentials between complementary treatments. For example, Gruber et al. (1999) show that increases in Medicaid fee differentials between cesarean and vaginal deliveries increase the cesarean delivery rate.[5]

Similarly to induced demand, when reimbursements are determined by groups of diagnoses, it may be the case that physicians diagnose patients with more generously reimbursed conditions. Dafny (2005b) examines the effect of a policy change that leads to large changes in reimbursement rates for Medicare patients. Medicare reimbursements are based on Diagnosis Related Groups (DRGs), and she finds that in response to changes in DRG specific reimbursement rates, hospitals tend to "up-code" patients to the diagnosis codes with the largest price increases.[6] As discussed above, Gray et al. (1996) find that many normal birth weight infants admitted to the NICU require only monitoring and no intensive treatments. It would be interesting

[5]In another line of research relevant to induced demand, Afendulis and Kessler (2007) examine the tradeoff that occurs with the integration of diagnosis and treatment. They find that paitients diagnosed by cardiologists who also perform angioplasty are more likely to recieve angioplasty than those diagnosed by cardiologists who do not themselves provide treatment.

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

for future research to determine if marginal infants admitted to the NICU when there are more empty beds available actually receive more intensive treatment, or if admitting them to the NICU provides an opportunity for hospitals to "upcode" the infants in order to receive a higher reimbursement.

Baras and Baker (2009) is an example of a study that attempts to identify the causal effect of supply on utilization. They examine the effect of the availability of MRI scanners on MRI use for lower back pain, a condition for which the use of MRIs is controversial.[7] They include geographic market fixed effects to control for cross sectional differences in unobserved preferences and health and identify that increases in MRI availability lead to increases in usage and increases in surgery rates. These results are valid so long as changes in demand or changes in underlying health are not associated with changes in the availability of MRI machines.

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

[7] Baras and Baker (2009) point out that using MRIs to diagnose lower back pain is controversial because it often detects and leads to surgery for lower back abnormalities that are not necessarily the cause of the pain

Profit et al. (2007) is the study most directly related to this paper. The authors examine the effect of the number of NICU patients on discharge patterns. In a population of infants between 30 weeks and 34 weeks gestation in ten NICUs in Massachusetts and California the authors 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. They document that discharges are 20% less likely than expected on days in the lowest census quintile and 32% more likely than expected on days in the highest census quintile. There are no significant differences between actual and expected discharges in the second through fourth quintiles. My paper differs by examining the decision to admit an infant to the NICU. Both margins are likely important drivers of expenditures and have different implications. If capacity affects the intensive margin through the timing of discharge and therefore length of stay, it may be the case that infants who need care are receiving more care than necessary. However, if capacity affects the extensive margin by changing who is admitted to the NICU it could impact infants who are not in need of intensive care.

3.3 Data

3.3.1 Data Sources

This chapter uses data sources that were discussed in Chapter 2, but I will briefly review their main features here. In particular, this chapter uses two California data sets provided by the Office of Statewide Health Planning and Development (OSHPD): the Linked Patient Discharge Data/Birth Cohort File and the State Utilization Data File of Hospitals. I utilize data from 1991 through 2001, although for reasons discussed below, the analysis sample will include 1992 through 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 is organized with one observation for each hospitalization within the first year of life, so I can follow each infant from its birth record through all transfers and readmissions within the first year. 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, detailed information about diagnoses and treatment, and further information about the hospitalization such as length of stay and charges. The Utilization Data File contains annual hospital level data on capacity and utilization. Particularly useful for this chapter are variables indicating a hospital's annual number of NICU beds and NICU discharges.

3.3.2 Imputing NICU Admission

A limitation of the Linked Discharge Data File is that it does not identify if an infant is admitted to the NICU. I thus impute whether an infant is admitted to the NICU based on a set of observables in each record. Phibbs et al. (1996) use earlier years of this same data set and a criteria based on Diagnosis Related Group (DRG) codes, birth weight, length of stay, and diagnoses to identify the population of infants most likely to have been cared for in the NICU.[8] I take guidance from their procedure along with input from a neonatologist that I interviewed in creating my NICU admission selection algorithm.

A key difference between my approach and Phibbs et al. (1996) is that, while these authors study the population of those most likely to be admitted to the NICU, I am using NICU admission itself as an outcome and using it to build my key explanatory variable. Therefore it is important for me to be more precise in assigning NICU admission. To maximize the accuracy with which I assign admission, I calibrate my approach to match the number of NICU admissions reported in the Utilization Data

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

File. For each hospital-year pair, this target number of admissions is equal to the sum of the number of NICU discharges and the number of infants transferred from the NICU to another ward within the hospital.[9] Also, because I am imputing both the dependent variable and the independent variable of interest, I must calculate them in a way that does not "assume" induced demand or moral hazard. Therefore, unlike Phibbs et al. (1996), I do not use variables such as diagnoses in my imputation because induced demand and moral hazard motives may lead to inaccurate recording of such variables. I predominantly use measures of hospital resource utilization to impute NICU admission.

First, I divide observations into three types of records: births, transfers, and readmissions.[10] Second, I prevent NICU admission for three types of records: (1) readmission records more than 8 weeks after birth if the DRG at birth had indicated a normal newborn, (2) readmission records more than 8 weeks after birth if the most recent hospitalization was greater than 4 weeks prior to the readmission; and (3) all subsequent transfer and readmission records following these two types of records. According to the neonatologist that I interviewed, readmitted infants can be cared for in the NICU, but not if they are readmitted long after birth, particularly if they were healthy at birth. Healthy infants at birth will likely be too large for the NICU bassinets if readmitted long after birth.

All other birth, transfer, or readmission observations not described above are considered candidates for NICU admission. Phibbs et al. (1996) impute likely admission for infants with a length of stay greater than five days. I find in my data set that a threshold of 5 days is too inclusive and in many hospitals would impute admissions for more infants than my target allows. Therefore, the third step of my

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

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

procedure assigns NICU admission to all infants with a length of stay greater than 10 days. This threshold still overshoots the target in some hospitals, but by far less than when using a five-day threshold. Fourth, I impute the rest of the admissions necessary to meet the target number in each hospital-year by selecting infants with the highest charges per day. NICU stays are extremely expensive, so it is very likely that the most expensive babies have accumulated their charges in the NICU.[11] This claim was confirmed by the neonatologist that I interviewed. 26.59% of admissions are imputed based on stays longer than 10 days. The remaining 73.41% are chosen based on charges per day.

Once admission has been imputed within my sample, I derive the daily census for each NICU by counting how many patients are present based on their hospital admission date and length of stay.[12] It is important to note that I must assume an infant admitted to the NICU spends it's entire hospital stay in the NICU, so I may be overestimating the number of patients in the unit on a given day. In Section 3.4 I discuss the ramifications of this inherent measurement error.

3.3.3 Analysis Sample

I begin with the sample of all birth, transfer, and readmission records in the Linked Discharge Data File from 1991 through 2001 merged with the Utilization Data File at the hospital-year level. This initial sample includes 6,221,001 birth observations and 7,053,804 total observations from an average of 387.27 hospitals per year. I proceed to make a series of restrictions on the sample, which are summarized in Table 3.1. The first set of restrictions is at the hospital-year level. First, I restrict my sample to observations in hospitals with NICUs by limiting to hospitals that report a positive number of neonatal intensive care beds in the Utilization Data File

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

[12]For birth records, birth date and hospital admission date are synonymous.

and then to hospitals that report a positive number of NICU patients (the sum of NICU discharges and within hospital NICU transfers).[13]

Second, I eliminate hospitals that either report zero births in the Utilization Data File or have no birth records present in the Linked Discharge Data File. This restriction in effect eliminates children's hospitals from the sample. I am focusing on the NICU admission decision at the hospital of birth, so I do not want to consider children's hospitals that do not provide delivery services and only receive neonatal intensive care patients via transfer or readmission. I also eliminate hospital years for which the number of births reported by the Utilization Data File and the number of births in the Linked Discharge Data File differ by more than 10% in order to remove hospitals with discrepancies between the two data sets. As seen in Table 3.1 this restriction only eliminates 4 hospitals per year on average.

I next eliminate hospitals for which all patients are missing charge data. Without data on hospital charges, I cannot assign NICU admission for infants in these hospitals. It is worth pointing out that this restriction excludes Kaiser owned hospitals because they do not report hospital charges in the data.[14] Therefore, the results of this paper are not relevant to Kaiser owned hospitals. One interesting question that I cannot address is whether incentives to fill empty beds are weaker in Kaiser hospitals where physicians are not directly paid for the quantity of care provided. In Section 3.5 I do examine effects separately for patients with managed care insurance other than Kaiser, but this group is less well defined than the Kaiser population would be.

The sample that remains contains an average of 121.91 hospitals per year and 4,028,735 observations of which 3,566,527 are birth records. At this point I perform

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

[14]All hospitals excluded by this restriction are in fact Kaiser hospitals. No other hospitals are missing charges for all patients. In my final sample only 1,208 or 0.04% of individual infants are missing charge data.

the NICU admission imputation algorithm on all remaining observations. In some cases where many infants had a length of stay greater than 10 days, this algorithm leads to too many admissions as compared to the target number of discharges. I drop all observations for a hospital-year in which the target number of discharges differs from the number of imputed admissions by more than 10%. This restriction only removes 1.27 hospitals per year and 1.9% of the birth observations.

Finally, I make three restrictions at the infant level. I drop a very small number of observations for which the admission date is missing. For such observations, I cannot know the census on their date of birth nor can I count them in the census during their time in the hospital. I also drop a very small number of observations with missing birth weight information. This only eliminates 1% of the birth records. Finally, I exclude observations from 1991 from my analysis sample, because I do not observe the stock of infants in a NICU at the beginning of the sample. The 99th percentile of length of stay for NICU admitted patients is 91 days, so excluding one year of data should be sufficient to allow the stock of patients to be accurate after one year. The final analysis sample includes an average of 121.1 hospitals per year and 3,131,948 birth observations.

3.4 Empirical Framework

The basic empirical strategy is to estimate a linear probability model where the probability of NICU admission is a function of characteristics of the infant and the number of empty NICU beds on the day prior to birth. The analysis is conducted on the 3,131,948 birth records to examine admission patterns at the birth hospital. The number of empty beds is measured as the difference between the number of NICU beds reported in the Utilization Data File and the number of infants that I count as being patients in the NICU on a given day. 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.[15]

I use ordinary least squares to estimate regression equations of the following form:

$$admit_{ith} = \alpha + EmptyBeds_{t-1,h}\beta + \mathbf{X}_{ith}\mathbf{\Gamma} + \delta_{th} + \varepsilon_{ith} \tag{3.1}$$

$admit_{ith}$ is an indicator equal to 1 if infant i, born at time t, in hospital h is admitted to the NICU. $EmptyBeds_{t-1,h}$ is the measure of how many empty beds are available in the NICU of hospital h at time $t-1$ (the day prior to the infant's birth). $\mathbf{X}_{ith}$ is a vector of characteristics specific to the infant which I describe in more detail in Section 3.5. δ_{th} are hospital-specific month fixed effects[16] and ε_{ith} is a random error term. All standard errors are clustered at the hospital level to allow unobserved determinants of NICU admission to be correlated within hospitals but maintain the assumption that they are independent across hospitals. It is likely that infants within a hospital would have correlated unobservable characteristics as a result of factors such as geography, socioeconomic status, etc.

The key to identifying the causal effect of empty beds on NICU admission, β, are the hospital-specific month effects, δ_{th}, which allow the unobserved probability of admission to vary for each hospital within each month. These fixed effects flexibly control for unobserved differences across hospitals in treatment styles and patient populations, long run trends and seasonality of infant health, and any differences in these trends and seasonality across hospitals.

Clearly, it is desirable to control for differences across hospitals in the types of patients they attract and their treatment practices. While Equation (3.1) controls

[15] If I had more detailed information such as exactly when infants are admitted and discharged, I could construct my measure of empty beds just prior to admission. Because I must assume that an infant is admitted on the day of birth and leaves the NICU on the day of discharge, the best measure I can use is excess capacity on the day prior to birth.

[16] In practice I construct a variable that takes on a unique value for every hospital month pair and estimate the regressions "absorbing" these fixed effects.

for patient level observables. Chapter 2 makes clear that infants born at different hospitals differ in unobservable characteristics as well. In my data it is also clear that hospitals vary greatly in their use of neonatal intensive care. Figure 3.1 plots the density of hospital level NICU admission rates and shows a great deal of heterogeneity. The mean hospital has a NICU admission rate of 12.97%, the median has an admission rate of 10.75%, and the standard deviation is 9.50%. Furthermore, differences in hospital treatment styles will directly affect the dependent and independent variables. For example, a high intensity hospital will likely have a higher NICU admission rate and may maintain a higher NICU census as a result. In fact, at the hospital level the correlation coefficient of the NICU admission rate and the number of empty beds faced by the average infant is -0.24. Taking scale into account, the correlation between the NICU admission rate and the percent of empty beds faced by the average infant is -0.37.

In addition, it is important to control for the fact that characteristics of mothers giving birth and the health of their infants are quite cyclical. Buckles and Hungerman (2008) show that mothers of infants born during the winter months have lower socioeconomic characteristics relative to those giving birth in the spring and summer months.[17] They also examine measures of infant health and find that infants born during the winter months have lower birth weights and lower APGAR scores[18] than those born during the spring and summer months. Figure 3.2 confirms this seasonal relationship in my data by plotting the fraction of births that are very low birth weight, the neonatal mortality rate of very low birth weight infants, and the NICU admission rate by quarter over my 11 year sample. In general, the VLBW rate and the NICU admission rate increase over time while VLBW mortality decreases over time. In addition to these general trends, there is a large amount of

[17]For example, mothers giving birth during the winter are more likely to be teenagers, less likely to have a college degree, more likely to be black, and less likely to be married.

[18]APGAR scores measure an infant's health at birth based on a test performed immediately after birth

quarter to quarter variation in all three rates.

However, including only time dummies in the empirical model is potentially insufficient if these cycles are heterogeneous and vary across hospitals. Serial correlation in infant health within a hospital would lead to downward biased estimates of β because times with few empty beds would also be times with few subsequent NICU admissions. To the extent that hospital-specific month effects flexibly control for these cycles separately for each hospital, I will be able to purge this serial correlation from the regression.

With these fixed effects the identifying variation comes from within hospital-month variation in the number of empty beds. The estimate of β represents how within hospital-month deviations from the hospital-month average number of empty beds changes the NICU admission probability of subsequent newborns. Therefore, the identifying assumption is that these deviations are uncorrelated with unobserved NICU admission probability. This assumption is likely to be valid since unobserved changes in newborns' baseline health are unlikely to be correlated with unobserved changes in the health of the infants born in the immediate past who determine the number of empty NICU beds available in the hospital. While this assumption is untestable in practice, I provide supportive evidence of its validity in Section 3.5.

While these fixed effects are necessary to identify the effect of empty beds on NICU admission, they lead to identifying the effect from a very specific source of variation – unexpected changes in 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, to the extent that I find an effect of hospital-month deviations in empty beds on NICU admissions, it likely implies that there is scope for available capacity to affect utilization on other margins as well. If patients and hospitals respond to short term deviations in available capacity, they likely respond to broader variation in available capacity as well. Short term effects of capacity on utilization imply additional economic, psychic, and health costs themselves, but any potential broader effects would greatly magnify these costs.

If the number of empty NICU beds affects the NICU admission decision, the effect is likely to vary by characteristics of the infant. Presumably the care decisions of the sickest infants will be independent of excess capacity in the NICU, especially if hospitals can transfer these infants when the NICU is full. 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 (3.1) for the full sample, I estimate it for subsamples stratified by birth weight: very low birth weight (VLBW) infants weighing less than 1,500 grams (3.33 pounds), low birth weight (LBW) infants between 1,500 and 2,500 grams (3.33 to 5.5 pounds), two groups of normal birth weight (NBW) infants, one ranging from 2,500 to 3,250 grams (5.5 to 7.15 pounds) and the other from 3,250 to 4,000 grams (7.15 to 8.81 pounds), and high birth weight (HBW) infants above 4,000 grams (8.81 pounds). I also present results that trace out the effect more flexibly by estimating Equation (3.1) for subsamples stratified at half pound increments.[19]

It is important to note that that imputing NICU admission 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. For example, suppose over a certain period of time in a give hospital, the actual NICU patients are less sick than usual and therefore ac-

[19] As discussed in Chapter 2 birth weight is the best measure of an infant's health stock at birth (Almond et al., 2005; Cutler and Meara, 2000) and is measured more accurately than gestation. In Section 3.11 I examine the robustness of my results to stratifying by gestation instead of birth weight

cumulate 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 admission in that hospital-year's quota. In this case, estimates of β would be biased upward. Unfortunately, there is no way of telling to what extent and in which direction measurement error occurs. To the extent that these errors are constant within a hospital-month, they would only shift the mean number of empty beds and mean admission probability in a hospital-month and be absorbed by the hospital-specific month fixed effects. However, this error may not be constant within a hospital-month. As a result, the estimates in this chapter should be viewed as preliminary until verified using other data sources more suited to measuring NICU admission. As discussed above, I will utilize data from New York that specifically reports NICU admission to improve this analysis.

3.5 Results

3.5.1 Summary Statistics

Before presenting the estimation results, this section discusses summary statistics of the analysis sample. Table 3.2 describes the six analysis samples, listing means for all newborns in each sample and those admitted to the NICU in each sample. The differences across these samples in mean NICU admission rates further motivate providing estimates separately for each. While 13.5% of newborns are admitted to the NICU, 76.9% of VLBW newborns are admitted and 52.3% of LBW newborns are admitted. This number falls to 11.4% for the first NBW group and 9.2% for the second NBW group, before rising slightly to 12.3% for the HBW group. With such large differences in baseline admission rates, it is more informative to analyze these samples separately.

As discussed in Chapter 2 some demographic characteristics are associated with preterm delivery, but for the most part these high risk births are often unexpected. As a result, there are some differences in demographic characteristics across the samples, but for the most part these differences are not very large. The three lightest groups are more likely to be covered by Medicaid and have lower education than the two heaviest groups. There are large differences in the fraction of infants whose mothers are black with VLBW and LBW infants having a much higher fraction than the heavier groups. On the other hand, the heavier groups have higher proportions of Hispanic mothers.[20]

There are more noticeable differences across samples in health related characteristics. Not surprisingly, infants born at lower birth weights are more likely to be multiple births and have slightly higher parities. Mothers of lighter infants have received slightly less prenatal care; although, this difference is likely mechanical, as shorter gestational age limits the possible number of visits. This is confirmed by the fact that there are are very small differences in the month prenatal care began across birth weight samples. Heavier infants are less likely to have congenital anomalies, less likely to be diagnosed with a clinical condition[21] (except for HBW infants) and have longer gestation.

Finally there are large differences in health outcomes across these samples. While I do note examine outcomes directly, these differences further motivate the

[20]This is consistent with the well documented "Hispanic paradox" that Hispanics typically have lower socioeconomic status but better health outcomes

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

need to explore the birth weight samples separately. As discussed in Chapter 2, neonatal mortality rates drop drastically with birth weight from 23.0% for VLBW infants[22] to 1.2% for LBW infants and well below 1% for heavier infants. Finally, lighter infants are much more likely to be transferred and incur much more intensive treatment in the form of much longer lengths of stays and hospital charges. For example, VLBW infants spend an average of 48.626 days in the hospital, LBW infants spend 10.469 days in the hospital and higher birth weight infants spend around 3 days in the hospital.

Table 3.3 provides summary statistics of the NICU environment on the day prior to birth for the full sample of newborns and each of the birth weight subsamples. On average, newborns are born in a hospital with 21.470 NICU beds, though this varies widely, as the standard deviation is 17.278 beds. On average, there are 1.897 empty beds available in the NICU, and the standard deviation is 9.049. At the 25th percentile there are -3 empty beds and at the 75th percentile there are 7 empty beds.[23] VLBW infants, and to a lesser extent LBW infants, are typically born in hospitals with larger NICUs. Also, at the mean, lighter infants face slightly fewer available empty NICU beds when they are born.

While these numbers give a sense of the baseline NICU environment, the identification strategy is based on variation after partialing out the hospital-specific month fixed effects. For each birth weight subsample, the third row of Table 3.3 summarizes the variation in the residuals from a regression of the number of empty beds on these fixed effects. In other words, it summarizes how the number of empty beds deviates from the within hospital-month mean number of empty beds. By construction the mean of this variable is zero. The standard deviation is 3.097. At

[22]Note that the neonatal mortality rate is higher than in Chapter 2 because this sample includes infants weighing less than 500 grams

[23]The number of empty beds can be negative because my NICU admission algorithm is based on the annual number of infants treated in the NICU, and because I must assume that an infant spends their entire hospital stay in the NICU

the 25th percentile newborns face 1.682 less empty beds than the hospital-month average, and at the 75th percentile they face 1.695 more beds than the hospital-month average. When discussing estimation results, I will refer to these measures in order to discuss the magnitude of my results. As such, it is helpful to see that this variation does not differ drastically across birth weight samples. The standard deviations are slightly larger for the lighter infants, likely as a result of often being born in hospitals with larger NICUs. The differences, however, are small with the standard deviations ranging from 3.037 beds to 3.158 beds. The difference between the 25th and 75th percentiles ranges from 3 beds to 3.4 beds and does not follow a monotonic pattern by birth weight. As a result of these similarities across subsamples, I will discuss my results in the context of changing the number of empty beds by 3.

The identifying assumption for my framework to estimate the causal effect of the number of empty beds on NICU admission is that unobserved within hospital-month deviations in admission probability are uncorrelated with unobserved within hospital-month deviations in the number of empty beds. Table 3.4 provides supportive evidence of this claim by comparing observable characteristics by the number of empty beds available. For each of the birth weight samples, this table divides observations by whether the residual number of empty beds the day prior to birth is above or below the median. For simplicity, I present means of the observable characteristics without partialing out the fixed effects. There is some evidence that VLBW infants born on days with above median residual empty beds are less healthy than those born on below median days, but these differences are quite small. For example, they are slightly more likely to be multiple births and more likely to have a congenital anomaly. Otherwise, there are little to no differences in demographic, pregnancy, or infant characteristics on days with above or below median residual empty beds for the six samples, suggesting unobserved determinants of NICU admission are likely not associated with residual empty beds. For each sample, the NICU admission probability is higher for infants born on days with higher residual empty beds. These differences range from 0.7 percentage points for the NBW2 sample to 6 percentage points for the VLBW sample. This table provides preliminary evidence that NICU admission rates are higher on days with more empty beds.

3.5.2 The Effect of Empty Beds on NICU Admission

The previous section provides evidence that newborns are more likely to be admitted to the NICU on days with higher than average empty bed counts. In this section, I discuss the regression estimates of the effect of empty beds on NICU admission controlling for various observable characteristics and hospital-specific month fixed effects as described by Equation (3.1).

The main regression results are presented in Table 3.5 where each row lists coefficient estimates for a different birth weight sample. For reference, Column 1 repeats the mean NICU admission rate and the number of observations for each sample. Moving across Columns 2 through 7, I present separate regression estimates of the empty bed coefficient subsequently adding control variables. All estimates include the hospital-specific time fixed effects. Column 2 presents estimates with no other controls included. For all six samples, the coefficient estimates are positive and very precisely estimated. Before discussing the magnitudes, notice that the only control variables that appreciably impact any of the coefficient estimates are the birth weight dummies. These control variables decrease the size of the coefficients for the full, VLBW, and LBW samples. However, after adding birth weight controls, the coefficient estimates are quite insensitive to the addition of day of week dummies, demographic characteristics, pregnancy characteristics, and infant characteristics.[24]

[24]Demographic characteristics mother's age, mother's age squared, education indicators (some college, college degree, more than a college degree), insurance status indicators (Medicaid and managed care), and race and ethnicity indicators (black, other race, and Hispanic). Pregnancy

If there are any differences in health characteristics associated with empty beds, they appear to be fully accounted for by including birth weight controls, and the stability of these coefficient estimates to the addition of all other controls further supports the evidence presented above that empty beds are not correlated with observable characteristics after conditioning on hospital-specific time effects.[25]

Focusing on the main results with all controls included in Column 7, an additional empty bed leads to a 0.15 percentage point increase in the probability of NICU admission. Relative to the overall mean rate of admission, this represents an effect of 1.11% as reported in Column 8. Estimates in the subsequent rows imply that there is important heterogeneity in this effect. The coefficient estimates are highest for the VLBW and LBW samples (0.34 and 0.49 percentage points, respectively) and lower for the NBW samples, before increasing slightly for the HBW sample (0.14, 0.09, an 0.17 percentage points, respectively). However, these magnitudes are difficult to compare because of the large differences in admission rates by birth weight. In Column 8 I compare the results relative to mean admission probabilities for each sample. Here the relative effect is actually smallest for VLBW infants at 0.40%. This effect increases to 0.94% for LBW infants, 1.21% and 1.00% for the two NBW groups, and 1.34% for the HBW group.

As expected, the smallest relative effect is among the VLBW infants. To get a better sense of the magnitude of these effects, it is useful to scale them by a measure of the actual variation in the number of empty beds. As discussed above, the standard deviation of the residual number of empty beds and the difference

characteristics include sex, parity, a multiple birth indicator, month prenatal care began, and number of prenatal care visits. Infant characteristics include indicators for having a congenital anomaly, a clinical condition, being small for gestational age, and being large for gestational age.

[25]Though not reported in the table, regression estimates that control for whether or not the infant is delivered by cesarean section are identical to those in Column 7. While a cesarean section is an important risk factor, I prefer not to include it in the regressions. Since it is a treatment decision, it is potentially endogenous to the number of empty beds, as the number of empty NICU beds may weigh into a physicians decision on if and when to schedule a cesarean delivery. Below I examine the robustness of my results to excluding infants delivered by cesarean section.

between the 25th and 75th percentiles of these residuals are around 3 for all of the birth weights subsamples. So, even for three bed change, the VLBW estimate implies an effect of only 1.2%. While it appears that the number of empty beds impacts the probability that VLBW infants are admitted to the NICU, the effect seems quite small for this group, which is the group one would expect the smallest impact of external factors on treatment choices. A three bed change in the number of empty beds leads to an increase in the NICU admission probability by 2.82%, 3.63%, 3.00% and 4.02% for LBW, NBW1, NBW2, and HBW infants respectively. Considering that a one standard deviation change or moving from the 25th to the 75th percentile of empty beds is a large shock, it could be argued that these effects are relatively small. However, as discussed above in Section 3.4, these estimates are based on a very specific source of variation – unexpected hospital-month deviations in the number of empty NICU beds. To the extent that I find a measurable effect of these short term deviations on NICU utilization, we might expect that NICU utilization responds to NICU capacity at other margins as well. While the estimates do not directly imply that additional NICU capacity in aggregate leads to additional NICU utilization, these results suggest that there is wide scope for available capacity to effect utilization at broader levels as well. In particular, increases in capacity associated with deregionalization may provide opportunities for additional NICU utilization.

To further disaggregate the effect I estimate Equation (3.1) for subsamples at 250-gram birth weight increments. The coefficient estimates and 95% confidence intervals are plotted by birth weight in Figure 3.3a, and the percentage effect relative to each subsample's mean NICU admission probability is plotted in Figure 3.3b. In these figures, the birth weight along the horizontal axis represents the upper bound of each subsample. For all subsamples, the coefficient estimates are positive, and except for the 750 to 1000 subgroup, they are all statistically significant at the 5%

level.

Focusing on the relative effects in Figure 3.3b reveals an interesting pattern. The relative effects are flat and small for infants below 1500 grams. There is then a discrete increase moving from just below 1500 grams to just above 1500 grams as the size of the effect jumps from 0.22% to about 0.66%. While the absolute effect declines through the LBW range, the relative effect, which takes into account the declining admission probability through this range, rises to a peak of about 1.38% for infants between 2250 and 2500 grams. The effect then drops to about 0.9% for infants above 2750 grams and remains flat through most of the NBW range. Finally, the relative effect size begins to climb again through the HBW range – those above 4000 grams.

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 health infants as measured by VLBW, discretely jumps above this threshold and climbs through the group of LBW infants who are likely on the margin of needing NICU care, decreases for the most health NBW infants, and increases again for another potentially marginal group of HBW infants.

The finding that the effect of empty beds on NICU admission probability increases from just below to just above the VLBW threshold is interesting in the context of Almond et al. (2008). These authors use the VLBW "rule of thumb" to identify the effect of additional treatment on mortality. They find discretely higher charges but lower mortality rates for infants just below this threshold. It is interesting that this rule of thumb also seems to affect how physicians respond to empty beds. Below the rule of thumb, there appears to be little room for judgment, and empty beds have little effect on NICU admission. Above the rule of thumb, there appears to be more room for external factors to impact admission decisions.

3.5.3 The Mitigating Effects of Inter-Hospital Transfer

A positive effect of empty NICU beds on NICU admission does not necessarily imply that excess capacity leads to excessive utilization of NICUs. It may be the case that this effect is at least partially driven by infants being denied NICU care when a NICU is full and therefore capacity constrained. However, infants who need neonatal intensive care are often transferred to other hospitals. In my sample, 19% of VLBW infants are transferred from their birth hospital to another hospital and 8% are transferred on the day of birth. If infants are more likely to be transferred when the NICU at the birth hospital is crowded, I will overestimate the effect of the number of empty beds on NICU admission as these transferred infants (and eventual NICU patients) will be considered to not be admitted. To understand the extent to which this occurs, Table 3.6 provides estimates of the effect of empty beds on an indicator for whether or not an infant is transferred to another hospital. In Column 1, I consider the effect of empty beds on whether an infant is ever transferred, and in Column 2, I consider the effect on whether the infant is transferred on the day he is born. If an infant is being transferred due to capacity constraints in the NICU, it is likely to occur soon after birth.

The number of empty NICU beds has a negative and statistically significant impact on the probability of ever being transferred and transferred on the first day for all subsamples except HBW infants. However, the effect size is extremely small in magnitude for infants above the low birth weight threshold, consistent with the fact from Table 3.2 that heavier infants have mean transfer rates under 1% and transfer rates on the first day of less than 0.3%.

For VLBW infants, transfers mitigate a large portion of the effect of empty beds on NICU admissions. To show this directly, Columns 3 and 4 show estimates of the effect of empty beds on an indicator variable that is equal to one if the infant is admitted to the NICU *or* transferred. Compared to the baseline estimates in the last column of table 3.5 the effect is cut by one third for VLBW infants from 0.31 percentage points to 0.20 percentage points. Transfers also have a small mitigating effect for LBW infants, decreasing the effect from 0.49 percentage points to 0.45 percentage points. Not surprisingly, transfers do not mitigate the effect of empty beds for heavier infants. These results suggest that the effect of empty beds on the ultimate treatment received by the sickest infants is negligible, while healthier infants who are more likely to be on the margin of needing intensive care are impacted more. Furthermore, it suggests that if hospitals transfer infants when it is medically necessary, much of the effect of empty beds on NICU admission for infants above the VLBW threshold is likely due to excessive utilization of services as opposed to binding capacity constraints.

3.5.4 Hospital Level Heterogeneity

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

In the context of neonatal intensive care deregionalization, an immediate question is how the effect of empty beds on NICU admission differs by the level of NICU available at a hospital. It has been documented that the diffusion of NICUs has outpaced medical need, and by 1995 the number of available NICU bed-days exceeded medically necessary bed-days by a factor of 2.5 (Howell et al., 2002). Additionally, most of the new NICUs established during the 1980s and 1990s where lower level NICUs in community hospitals (e.g., Baker and Phibbs, 2002; McCormick and Richardson, 1995; Schwartz, 1996; Schwartz et al., 2000). As discussed in Chapter

2, in my data covering California over the period 1991 through 2001, 31 hospitals opened new Intermediate or Community NICUs while the number of hospitals with the most sophisticated Regional NICUs remained constant. If deregionalization has in fact led to excess supply of neonatal intensive care, it suggests that there is a large amount of scope to admit marginal infants to the NICU. In particular, if lower level NICUs represent excess capacity in aggregate, they may be more likely to respond to the incentives associated with empty beds as they attempt to recoup fixed costs and maintain revenue.

The first three columns of Table 3.7 presents results for subsamples based on the level of NICU available at the newborn's birth hospital. For all subsamples, the point estimate of the effect of empty beds on NICU admission is highest in hospitals with Intermediate NICUs and lowest in hospitals with Regional NICUs. This result suggests that hospitals with lower level NICUs respond more strongly to the prevalence of empty NICU beds. Also, the effect is still positive and statistically significant in hospitals with Regional NICUs, suggesting these hospitals respond to the incentives associated with empty beds as well. Table 3.8 replicates these results with NICU admission or transfer on the first day as the dependent variable. The point estimates show little change for infants above the LBW threshold, but the point estimates for VLBW and LBW infants are lower when considering the mitigating effect of transfers. Also, consistent with the fact that hospitals with lower level NICUs are more likely to transfer high-risk infants, considering transfers has a larger proportional effect on Intermediate hospitals. For VLBW infants, the effect falls by over a half in Intermediate NICU hospitals and by about a third in both Community and Regional NICU hospitals. For LBW infants, the effect size falls by a fifth in Intermediate NICU hospitals but only by about a tenth in Community and Regional NICU hospitals.

In addition to comparing the effects of a one-bed change in the number of empty beds, it is also important to consider the fact that the variation in empty beds differs by hospital type. Therefore, I compare the effects of a one standard deviation change in residual empty beds from separate regressions of empty beds on hospital-specific month fixed effects from each birth weight/NICU level subsample. With this additional scaling, the effect of empty beds on NICU admission is still largest in Intermediate and Community NICU hospitals and smallest in Regional NICU hospitals, but the gradient is less steep. For example, in the LBW sample, a one standard deviation change in empty beds in an Intermediate NICU hospital (1.97) leads to a 1.9 percentage point increase in NICU admission. In Community NICU hospitals a one standard deviation change (2.34) leads to a 0.86 percentage point increase in NICU admission, and in Regional NICU hospitals, a one standard deviation change (3.92) leads to a 0.59 percentage point increase in NICU admission. While Intermediate NICU hospitals appear to respond more strongly to the number of empty beds, considering differences in the variation in empty beds decreases the discrepancy between levels of care.

Given the fact that variation in empty beds differs across hospitals, it is worthwhile to also separate them by NICU size as well. With the hospital-specific month fixed effects, my estimates are identified from deviations from the within hospital-month mean number of empty beds. While these fixed effects inherently control for the NICU size, they do not account for the fact that, for example, NICUs with 10 beds only have scope to deviate from their mean by a small number of beds, whereas NICUs with 50 beds can have much wider deviations from their means. Not surprisingly, the standard deviation of residual empty beds from a regression of empty beds on the hospital-specific month effects is 2.068 for the sample of infants born in hospitals with less than 20 NICU beds and 3.744 in hospitals with more than 20 NICU beds. In Columns 4 and 5 of Table 3.7 and Table 3.8 I separate hospitals by NICU size and present estimates for infants born in hospitals with less than 20

NICU beds and greater than or equal to 20 NICU beds. While number of beds is highly correlated with level of care – at the hospital-year level Intermediate NICUs average 6.4 beds, Community NICUs 12.5 beds, and Regional NICUs 32.0 beds – there is heterogeneity within each category. Only 2.8% of Intermediate NICUs have greater than 20 beds, but 15.6 % of Community NICUs have greater than 20 beds, and 22.3% of Regional NICUs have fewer than 20 beds.

The results indicate that empty beds have a higher absolute effect on NICU admission in hospitals with smaller NICUs across all birth weights, though the very large discrepancies for VLBW and LBW infants are partially offset in Table 3.10 where the outcome is NICU admission or transfer. Scaling by the standard deviation of residual empty beds for each group decreases the gradient. For example, for the LBW group, the estimates imply that a one standard deviation change in empty beds in small NICUs (2.282) increases the probability of NICU admission or transfer by 1.7 percentage points while a one standard deviation change in empty beds in large NICUs (3.915) increases the probability of NICU admission or transfer by 1.2 percentage points. Taking these differences into consideration reveals that the effect of empty beds is larger in hospitals with smaller NICUs, though not by a substantial amount when considering relative size. These results also indicate that the main estimates are not being driven by empty bed variation from NICUs of a particular size.

The final set of columns in Table 3.7 and Table 3.8 show the effect of empty beds on NICU admission by hospital characteristic that may directly affect financial incentives: ownership status. There is an important literature in health economics on the differences between for-profit and not-for profit hospitals. In particular, Duggan (2000) shows that privately owned not-for-profit hospitals are similarly responsive to financial incentives as for-profit hospitals, while government owned hospitals are less responsive, and Duggan (2002) finds that not-for-profit hospitals behave more similarly to for-profit hospitals when they compete more directly with other for-profit hospitals. On the other hand Dafny (2005b) finds that for-profit hospitals are more likely to upcode patients to "with complication" DRGs when price differences between DRGs increase.

Columns 6-8 of Table 3.7 show the effect of empty beds on NICU admission for government owned, privately owned not-for-profit, and for-profit hospitals, respectively. The effects are positive and statistically significant for all three hospital types in all birth weight subsamples except for VLBW infants in for-profit hospitals, suggesting that hospitals with all three ownership structures respond to available capacity. The point estimates are generally largest for infants born in for-profit hospitals, although this is not true for VLBW infants where the effect is not statistically significant in for-profit hospitals and is largest in not-for-profit hospitals. However, when considering the role of transfers in Columns 6-8 of Table 3.8 the effect for VLBW infants in Government owned hospitals falls to zero, and the effect for LBW infants in Government owned hospitals becomes very similar to those in not-for-profit hospitals. Comparing these results suggests that government-owned hospitals do not respond to available capacity for VLBW infants. While they are still responsive for LBW infants, the effect is similar to not-for-profit hospitals and smaller than for-profit hospitals. Generally these results suggest that empty beds lead to additional NICU utilization for all three ownership types, but consistent with Dafny (2005b) the effect is strongest in for-profit hospitals.

3.5.5 Individual Level Heterogeneity

In this section, I examine the effect of empty beds on admission probability separately by individual characteristics. Table 3.9 presents results for subsamples based on individual characteristics, with the baseline results repeated in Column 1. Table 3.10 presents the same sample cuts with NICU admission or transfer on the first day as the dependent variable. While coefficient estimates are lower in the second table, the patterns across columns are similar, so I will mainly discuss the estimates in Table 3.9.

On one hand, physicians may be less able to take advantage of their agency and induce demand when the patient is more informed (McGuire, 2000). On the other hand, more informed, higher educated individuals may be more prone to moral hazard.[26] Columns 2 through 4 explore these possibilities by separating the samples by demographic characteristics. I show results for infants with Hispanic mothers, black mothers, and mothers with no college education. The point estimate for VLBW infants with Hispanic mothers is 0.24, which is lower than the baseline estimate of 0.31. For all other birth weight groups, the estimates for infants of Hispanic mothers are very similar to the baseline samples. The fact that the effect of empty beds on NICU admission does not differ much for Hispanic infants is not informative about the level of induced demand or moral hazard for this group relative to other groups.

Results for infants with black mothers are presented in Column 3. These point estimates reveal that the effect of empty beds on NICU admission is higher for VLBW black infants than the VLBW baseline, lower for LBW black infants relative to the LBW baseline, and very similar for black infants and the baseline at higher birth weights. One possible explanation for the very high point estimate for VLBW black infants is that these infants are more prone to face capacity constraints that prevent them from receiving treatment; however, the estimate for VLBW black infants when considering the outcome of NICU admission or transfer is still over twice the size of the baseline point estimate. Future research is warranted to understand why the treatment of VLBW black infants is more likely to be impacted by

[26] In a related context Fang et al. (2008) find that higher educated and higher income individuals purchase more insurance despite having lower health risks. This relationship is labeled "advantageous selection" as it relates to individuals buying more insurance, but it may be the case that these individuals would utilize health care resources at a higher level as well.

available NICU capacity.

Column three presents results for samples where the mother has no college education. If non-college educated individuals show a larger effect, it may be the case that induced demand is stronger for less well informed patients. On the other hand, if the effect is smaller, it would suggest higher educated individuals are more risk averse. The point estimate suggest a slightly higher effect of empty beds on NICU admission for VLBW infants whose mothers have no college education, but no differences at the other birth weights. Again, the lack of heterogeneity in the effect by education does not distinguish between induced demand and moral hazard.

Columns 5 through 7 of Table 3.9 considers that there may be direct differences in financial incentives by insurance status. Among the privately insured, managed care organizations provide fewer financial incentives that are directly tied to the type of treatment administered and provide tighter monitoring of behaviors that might be considered moral hazard. In fact, managed care has been shown to slow the adoption of neonatal intensive care units (Baker and Phibbs, 2002). However, anecdotally, managed care has been hesitant to limit reimbursement of infant care (Horwitz, 2005, see online appendix), so it is an empirical question whether the effect of supply on utilization differs between these two groups of patients. Almost all births that are not covered by private insurance are covered by Medicaid. While Medicaid typically does not reimburse as generously as private insurance, there is still scope for induced demand and moral hazard in this population.

The results in Columns 5 through 7 reveal little difference in the point estimates between private non-managed care patients and managed care patients except for VLBW infants where there is a larger effect for managed care patients. This result is in line with the fact that managed care may be hesitant to limit infant health care. As a caveat, my sample excludes all Kaiser hospitals. Kaiser likely provides the weakest scope for induced demand and moral hazard, but unfortunately, I cannot test the effect of empty beds on NICU admission for this strongest form of managed care. The results for Medicaid patients are similar to privately insured patients except for a slightly higher point estimate among VLBW Medicaid patients relative to VLBW privately insured patients. Even if Medicaid reimburses neonatal intensive care less generously than private insurance companies, the availability of empty beds still increases the probability of NICU admission for Medicaid patients.

Finally, in the last two columns of Table 3.9 I present estimates separately for the period from 1991 through 1995 and the period from 1996 through 2001. As discussed in Chapter 1, deregionalization continued through the early part of the 1990s but leveled off after 1996. The point estimates are slightly larger for the later period, but the effect seems relatively stable over time.

3.5.6 Robustness

In this section I discuss the robustness of my results to various specification and sample considerations. First, I estimate my regressions by gestational age subgroups instead of birth weight to ensure that measuring health by gestation leads to similar conclusions. Figure 3.4a plots coefficient estimates and 95% confidence intervals for subsamples at one-week of gestation intervals. Figure 3.4b plots these coefficient estimates relative to the mean NICU admission probability in each one-week subgroup. While not as distinct as with the birth weight specifications, a similar pattern occurs. The coefficient estimates are all positive and statistically significant at the 5% level. The relative effects are small for infants with low gestational ages. Gestation of less than 32 weeks is considered very preterm, 32-36 weeks moderately preterm, and 37 weeks or higher term. The effect size increases substantially between 33 and 34 weeks – in the middle of the moderately preterm range. After this threshold, the pattern is a bit noisy, but relatively flat until increasing for infants with long gestational ages.

Results of various other robustness checks are presented in Table 3.11, with the baseline results from Table 3.5 repeated in Column 1. As discussed above, adding birth weight dummies to the regression of NICU admission on the empty beds variable and the hospital-specific month fixed effects changed the empty beds coefficient. Here I examine whether including more specific birth weight controls impacts the results. Column 2 of Table 3.11 presents results including birth weight dummies in 50-gram increments instead of the relatively crude 250-gram increments. For all of the birth weight subgroups, results are almost identical whether I include birth weight controls in 50 or 250-gram increments.

One potential behavior on the part of hospitals and physicians that could bias my results is the ability to time a delivery through either delaying or inducing labor or scheduling cesarean sections. If physicians attempt to deliver high risk infants when NICUs are less crowded, my estimates would be biased towards finding a possitive effect of empty beds on NICU admission. To consider this, Column 3 presents results excluding cesarean deliveries, one group for which timing of birth may be endogenous. This restriction leads to a slightly larger effect for VLBW infants and a slightly smaller effect for the other groups. Overall, potentially planned cesareans do not appear to bias the results.

The final two columns of Table 3.11 relate to my NICU admission algorithm. After performing my algorithm, I find that some hospitals experience many days where the number of empty beds is negative indicating that the NICU is over capacity. In fact 18% of hospital-year level observations are over capacity for more than 60% of the year. By assuming that infants are admitted to the NICU immediately when born and leave the NICU at discharge, I can expect to overestimate the number of infants in the NICU on a given day, but this assumption is likely not driving this large number of days over capacity. The Utilization Data File also lists the number of total days infants spent in the NICU for a given year. When dividing this number by the number of bed-days available (the number of beds times the number of days they are available to be used), I find that the utilization data also indicate that NICUs are operating over capacity, with bed-days used exceeding bed-days available in 24% of hospital year observations. Also, the ratio of utilized bed-days to available bed-days is highly correlated with the percentage of days a hospital is over capacity under my admission algorithm. The correlation coefficient between these two measures is 0.69.

If these facts are taken to indicate that some hospitals often operate with very full NICUs, it is worth ensuring they are not driving the results, and that the estimates are instead driven by hospitals with empty bed variation in ranges away from their capacity constraints. In Column 4 of Table 3.11 I drop all observations on days where the NICU is greater than 5% over capacity, in other words, days where the number of NICU occupants by my algorithm exceeds the number of NICU beds by more than 5%. In Column 5 I drop all observations for hospital-years in which the number of NICU occupants exceeded NICU beds for more than 60% of the year. If anything, excluding these two sets of observations increases the coefficient estimates for VLBW, LBW, and HBW infants, while they are virtually unchanged for NBW infants. This finding again suggests that the results are not being driven by the denial of neonatal intensive care when NICUs are crowded.

3.6 Conclusion

The effect of the availability of medical resources on their rate of utilization is a difficult to identify parameter. I provide a preliminary examination of this question in the context of neonatal intensive care, an important and interesting context due to the increase in the number of hospitals offering NICUs. To identify the effect of availability on utilization, I estimate the effect of the number of empty beds available in the NICU the day prior to an infant's birth on the probability that the infant is admitted to the NICU. Including hospital-specific month fixed effects in my regressions allows me to exploit within hospital-month variation in NICU availability. I am therefore able to flexibly control for factors correlated with an individual's choice of hospital, hospital treatment style, serial correlation in infant health, and serial correlation in infant health specific to each hospital.

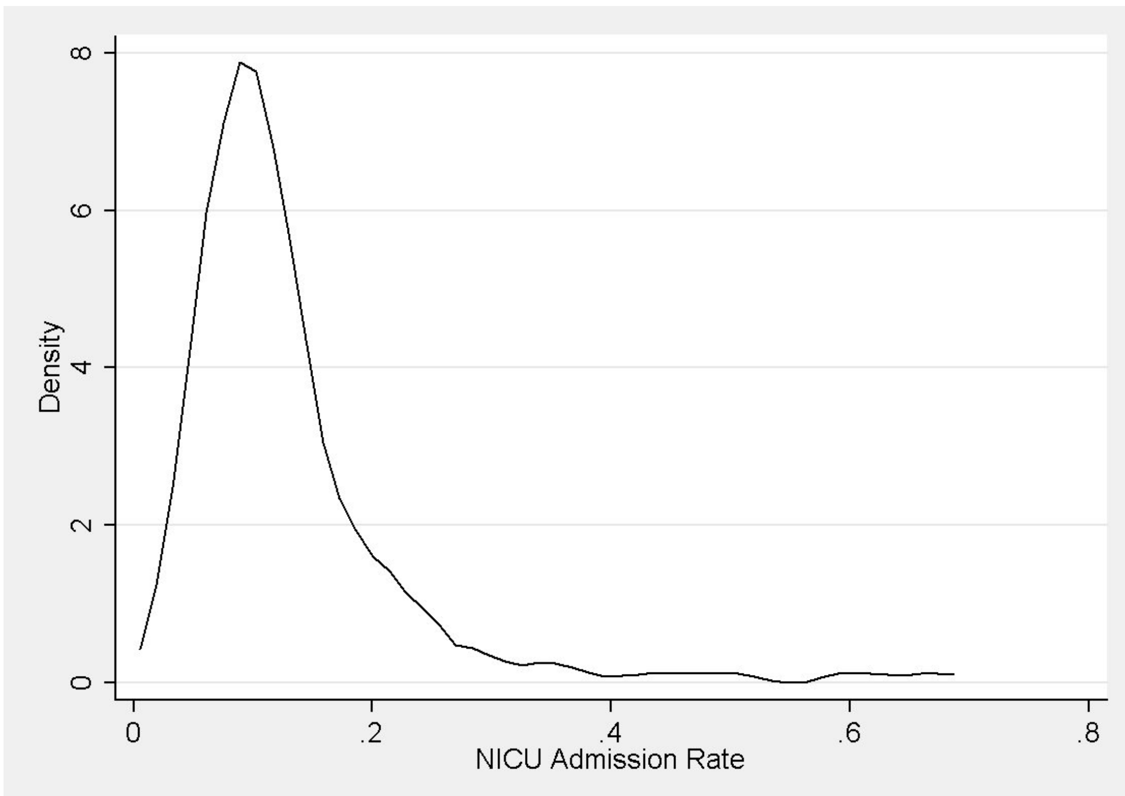
I find that on average, an increase in the number of empty NICU beds of 3 (an approximately one standard deviation change) increases the probability of being admitted to the NICU by 3.33%. The magnitude of the effect is smallest for VLBW infants (1.2%) and larger for infants above this threshold. For example, for LBW infants, the effect of a 3 bed change is 2.82%. Allowing for the fact that many VLBW infants are transferred on their first day in the hospital decreases the effect for this group by about a third. When I estimate the effect separately for narrower birth weight groups, I find that the effect size is negligible for all VLBW infants, and jumps discretely after this threshold is crossed. The effect appears to be the largest for the heaviest of the LBW group and for HBW infants, two groups likely on the margin of needing and not needing neonatal intensive care.

In the context of the deregionalization 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. Additionally, I find that the effect of empty beds on NICU admission is largest in Intermediate NICU hospitals even after accounting for the fact that these hospitals are more likely to transfer infants to other hospitals when there are fewer empty beds available. It appears that lower level NICU hospitals, which are those hospitals most associated with deregionalization, are more prone to admitting infants to the NICU in response to empty NICU beds.

These estimates suggest that the availability of neonatal intensive care beds leads to additional neonatal intensive care utilization. I also provide evidence that this effect is not being driven by crowded NICUs denying care. This finding has important implications. In particular, it implies additional economic costs, psychic costs, and potential health costs for unnecessarily treated infants. Future research is warranted to better understand these costs, particularly the health effects of being admitted to the NICU because empty beds were available. This is a difficult question to answer because the stock of infants in the NICU may have other effects on health outcomes through avenues independent of NICU admission. For example, nurses and doctors may be able to provide better care for all infants when there are fewer patients to attend to.

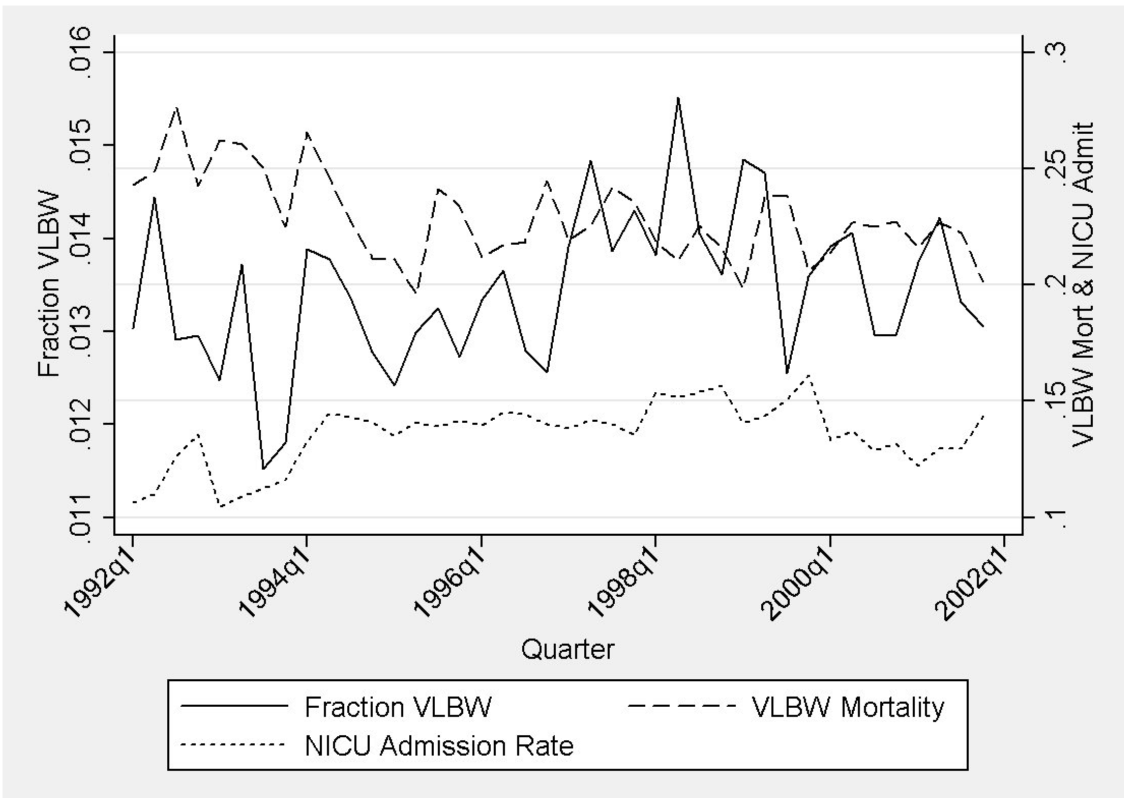
These results open the door to other avenues of future research as well. First, it is important to verify these results using data that specifically identify which infants are admitted to the NICU. Second, it may be interesting and informative to model the NICU decision in a more sophisticated manner. Relevant to the concerns about the direct effects of having fewer infants in the NICU on health outcomes, a particular infant's outcomes and NICU admission decision may also be a function of the number and health of other infants born the same day as a given infant. Finally, while I document that empty beds lead to additional utilization of NICUs, I am not able to identify the mechanism behind this result. It is likely that both information asymmetries between physicians and patients and information asymmetries between patients and insurers contribute, but understanding the relative contributions is an important question for future research.

Figure 3.1: Hospital Level NICU Admission Density



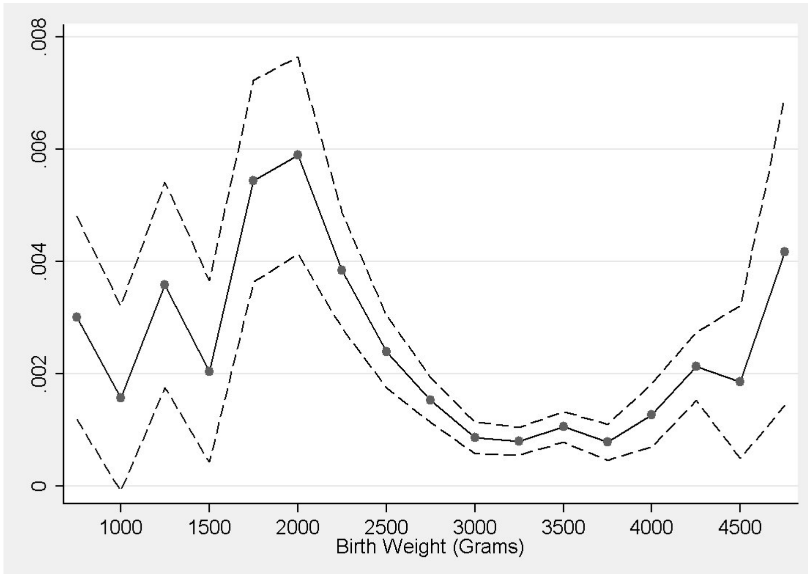
Notes: This figure plots the kernel density of NICU admission rates at the hospital level. For each hospital, the NICU admission rate is calculated as the fraction of infants admitted to the NICU.

Figure 3.2: Very Low Birth Weight, Mortality, and NICU Admission Over Time

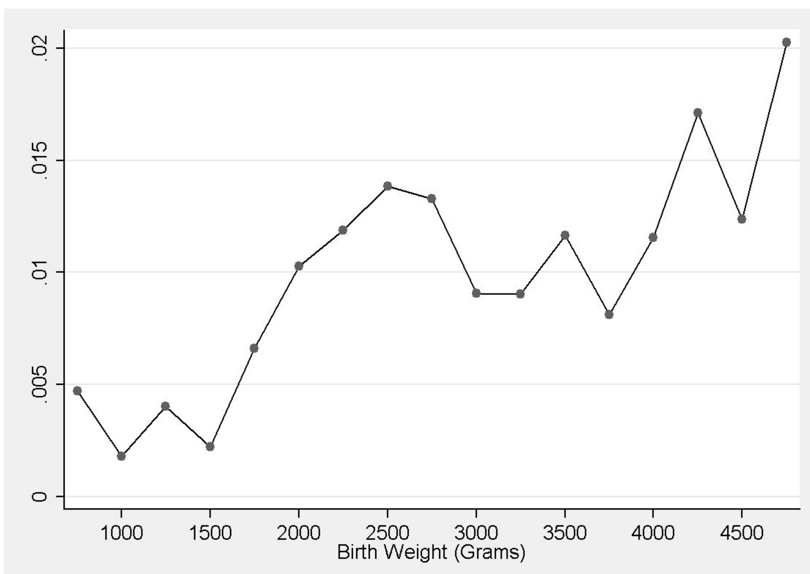


Notes: This figure plots trends in the fraction of VLBW infants, the fraction of VLBW infants that die in 28 days or within one year if hospitalized continuously since birth, and the fraction of infants admitted to the NICU by quarter from the analysis sample. VLBW infants are measured on the left hand side axis while Mortality and NICU admission are measured on the right hand side axis.

Figure 3.3: Effect of Empty Beds on NICU Admission by Birth Weight



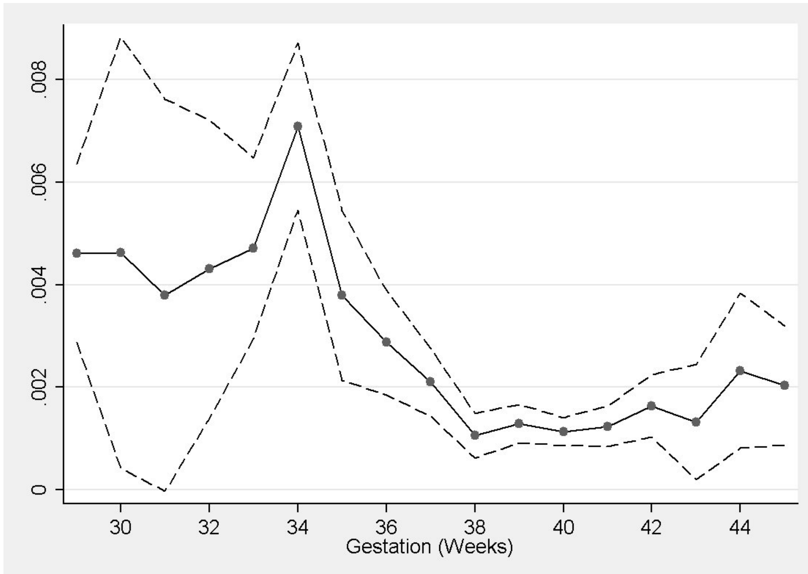
(a) Coefficients and 95% Confidence Intervals



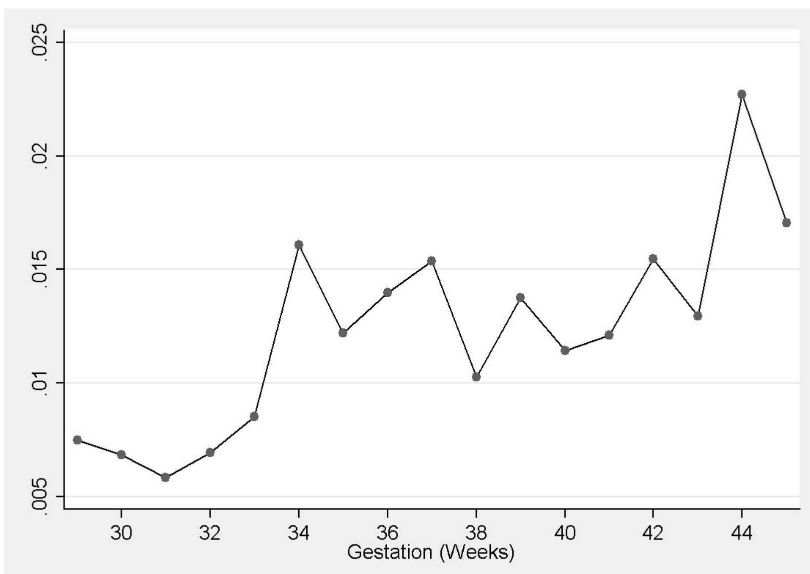
(b) Coefficients Normalized by NICU Admission Rate

Notes: The top panel plots coefficient estimates and 95% confidence intervals from separate regressions of NICU admission on the number of empty beds the day before birth for samples stratified by birth weight in 250-gram increments. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. All standard errors are clusterd at the hospital level. The bottom panel plots these coefficient estimates divided by the NICU admission rate with each birth weight subgroup.

Figure 3.4: Effect of Empty Beds on NICU Admission by Gestation



(a) Coefficients and 95% Confidence Intervals



(b) Coefficients Normalized by NICU Admission Rate

Notes: The top panel plots coefficient estimates and 95% confidence intervals from separate regressions of NICU admission on the number of empty beds the day before birth for samples stratified by gestation in 1-week increments. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. All standard errors are clusterd at the hospital level. The bottom panel plots these coefficient estimates divided by the NICU admission rate with each gestation subgroup.

Table 3.1: Constructing Analysis Sample

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

Notes: This table lists the number of observations and average number of hospitals per year after the imposition of each sample restriction. The sample starts with all infants in the Discharge Data File and then eliminates hospitals with 0 NICU beds, hospitals with 0 NICU patients, hospitals with no births reported in the Utilization Data File or no births in the Discharge Data File, hospitals where the number of births differs by more than 10% between the two data sets, hospitals with charges missing for all infants, and hospitals for which the number of NICU patients derived from my admission algorithm differs from the number of target admissions by more than 10%. It then eliminates infant observations missing an admission date, missing birth weight, and from 1991.

Table 3.2: Sample Means

	Full Sample		VLBW		LBW	
	All	NICU	All	NICU	All	NICU
NICU Admission	0.135	1.000	0.769	1.000	0.523	1.000
Mother's Demographics						
Age	27.482	27.752	27.931	28.187	27.690	27.939
Medicaid	0.475	0.493	0.495	0.483	0.512	0.512
Managed Care	0.385	0.367	0.338	0.350	0.349	0.346
Self Pay	0.037	0.033	0.029	0.018	0.035	0.030
No College	0.623	0.629	0.656	0.639	0.643	0.641
Some College	0.183	0.184	0.189	0.194	0.180	0.182
College	0.110	0.104	0.089	0.095	0.098	0.098
College Plus	0.083	0.083	0.067	0.072	0.078	0.079
Black	0.074	0.110	0.168	0.165	0.136	0.138
Other Race	0.125	0.118	0.100	0.104	0.132	0.115
Hispanic	0.474	0.449	0.440	0.435	0.427	0.423
Pregnancy Characteristics						
Multiple Birth	0.028	0.085	0.238	0.246	0.219	0.241
Parity	2.150	2.180	2.257	2.250	2.225	2.284
Mnth. Pren. Beg.	2.450	2.394	2.148	2.175	2.425	2.376
# of Pren. Visits	11.533	11.329	8.599	9.152	10.968	10.746
Male	0.512	0.555	0.510	0.503	0.478	0.520
Infant Characteristics						
Congenital Anom.	0.010	0.045	0.090	0.099	0.035	0.052
Clinical Condition	0.107	0.224	0.268	0.295	0.166	0.225
Small for Gest.	0.004	0.019	0.058	0.070	0.044	0.052
Large for Gest.	0.067	0.099	0.016	0.018	0.018	0.025
Birth Weight (G)	3337.54	2951.86	993.00	1071.44	2171.40	2056.66
Gestation (Wks)	39.464	37.877	29.381	30.063	36.341	35.247
Treatment & Outcomes						
Neonatal Mort.	0.005	0.014	0.230	0.107	0.012	0.012
28 Day Readmit	0.032	0.033	0.010	0.007	0.034	0.026
1 Year Readmit	0.105	0.140	0.193	0.217	0.139	0.151
Transfer	0.009	0.034	0.173	0.135	0.037	0.041
Transfer Day 1	0.003	0.000	0.069	0.001	0.014	0.000
Length of Stay	3.868	10.986	39.393	50.837	9.605	15.234
Charges	6.522	37.744	175.217	226.201	24.846	44.727
Charges/Day	0.847	2.625	4.097	4.419	1.799	2.655
Total LOS	4.118	11.958	48.626	57.492	10.469	16.148
Total Charges	7.636	42.083	213.979	254.629	28.243	48.268
N	3,131,948	423,840	42,040	32,340	173,895	90,967

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Table 3.2 – *continued from previous page*

	NBW1		NBW2		HBW	
	All	NICU	All	NICU	ALL	NICU
NICU Admission	0.114	1.000	0.092	1.000	0.123	1.000
Mother's Demographics						
Age	26.967	27.385	27.552	27.590	28.623	28.637
Medicaid	0.502	0.508	0.461	0.478	0.432	0.465
Managed Care	0.364	0.356	0.397	0.385	0.419	0.396
Self Pay	0.040	0.038	0.037	0.034	0.032	0.030
No College	0.642	0.640	0.614	0.614	0.595	0.616
Some College	0.176	0.179	0.185	0.186	0.197	0.191
College	0.104	0.102	0.114	0.111	0.119	0.107
College Plus	0.078	0.080	0.087	0.089	0.090	0.086
Black	0.092	0.124	0.059	0.083	0.044	0.060
Other Race	0.158	0.139	0.113	0.116	0.076	0.085
Hispanic	0.469	0.435	0.485	0.470	0.471	0.485
Pregnancy Characteristics						
Multiple Birth	0.034	0.047	0.003	0.004	0.000	0.001
Parity	2.061	2.129	2.153	2.092	2.370	2.348
Mnth. Pren. Beg.	2.500	2.458	2.440	2.406	2.394	2.384
# of Pren. Visits	11.336	11.343	11.704	11.926	12.017	12.242
Male	0.454	0.531	0.530	0.580	0.628	0.651
Infant Characteristics						
Congenital Anom.	0.009	0.045	0.006	0.032	0.007	0.030
Clinical Condition	0.052	0.144	0.065	0.165	0.437	0.611
Small for Gest.	0.002	0.006	0.000	0.001	0.000	0.001
Large for Gest.	0.013	0.037	0.036	0.082	0.422	0.578
Birth Weight (G)	2973.22	2927.34	3577.76	3584.03	4273.25	4324.34
Gestation (Wks)	39.211	38.659	40.033	39.921	40.426	40.296
Treatment & Outcomes						
Neonatal Mort.	0.002	0.007	0.001	0.003	0.001	0.003
28 Day Readmit	0.034	0.040	0.031	0.036	0.030	0.036
1 Year Readmit	0.108	0.140	0.098	0.122	0.097	0.117
Transfer	0.006	0.025	0.004	0.019	0.005	0.019
Transfer Day 1	0.002	0.000	0.001	0.000	0.002	0.000
Length of Stay	3.106	6.025	2.914	4.924	3.206	5.353
Charges	3.393	17.521	2.674	13.762	3.346	14.649
Charges/Day	0.757	2.375	0.724	2.479	0.794	2.364
Total LOS	3.220	6.498	2.977	5.241	3.285	5.669
Total Charges	3.974	19.959	3.008	15.485	3.746	16.266
N	1,032,399	117,619	1,560,110	143,068	323,504	39,846

Notes: This table presents sample means for the full sample and each of the five birth weight subsamples. For each sample, the first column includes all infants and the second includes those admitted to the NICU. Total LOS and Total Charges sum length of stay and hospital charges over all contiguous hospitalizations prior to first being discharged home or dying. Neonatal mortality is mortality within twenty-eight days of birth or within one year if an infant is continuously hospitalized since birth.

Table 3.3: Summary Statistics of Empty Beds

	Mean	St. Dev.	25th Pct.	50th Pct.	75th Pct.
Full Sample (N=3,131,948)					
NICU Beds	21.471	17.278	8.000	16.000	28.000
Empty NICU Beds	1.897	9.049	-3.000	2.000	7.000
Residual Empty Beds	0.000	3.097	-1.682	0.063	1.695
VLBW (N=42,040)					
NICU Beds	27.923	19.489	12.000	22.000	40.000
Empty NICU Beds	1.716	10.484	-4.000	2.000	8.000
Residual Empty Beds	0.000	3.158	-1.500	0.000	1.500
LBW (N=173,895)					
NICU Beds	23.675	18.193	10.000	19.000	32.000
Empty NICU Beds	1.723	9.621	-3.000	2.000	7.000
Residual Empty Beds	0.000	3.181	-1.706	0.000	1.708
NBW1 (N=1,032,399)					
NICU Beds	21.381	17.055	9.000	16.000	28.000
Empty NICU Beds	1.815	9.092	-3.000	2.000	7.000
Residual Empty Beds	0.000	3.079	-1.660	0.054	1.683
NBW2 (N=1,560,110)					
NICU Beds	21.147	17.148	8.000	16.000	28.000
Empty NICU Beds	1.945	8.932	-3.000	2.000	7.000
Residual Empty Beds	0.000	3.066	-1.659	0.066	1.669
HBW (N=323,504)					
NICU Beds	21.297	17.551	8.000	16.000	28.000
Empty NICU Beds	2.042	8.951	-2.000	2.000	7.000
Residual Empty Beds	0.000	3.037	-1.600	0.500	1.621

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

Table 3.4: Sample Means by Residual Empty Beds

	Full Sample		VLBW		LBW	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
NICU Admission	0.128	0.143	0.741	0.804	0.497	0.549
Mother's Demographics						
Age	27.469	27.496	27.873	28.004	27.675	27.706
Medicaid	0.475	0.474	0.491	0.500	0.511	0.514
Managed Care	0.385	0.385	0.341	0.335	0.350	0.347
Self Pay	0.037	0.037	0.033	0.025	0.036	0.034
No College	0.624	0.622	0.654	0.658	0.645	0.641
Some College	0.183	0.183	0.191	0.186	0.179	0.182
College	0.109	0.111	0.088	0.089	0.097	0.100
College Plus	0.084	0.083	0.067	0.067	0.079	0.078
Black	0.074	0.074	0.166	0.171	0.135	0.136
Other Race	0.125	0.125	0.099	0.102	0.134	0.131
Hispanic	0.475	0.474	0.439	0.441	0.427	0.426
Pregnancy Characteristics						
Multiple Birth	0.026	0.030	0.230	0.247	0.210	0.228
Parity	2.149	2.151	2.234	2.285	2.219	2.231
Mnth. Pren. Beg.	2.450	2.450	2.136	2.162	2.431	2.419
# of Pren. Visits	11.538	11.527	8.452	8.781	10.962	10.973
Male	0.512	0.512	0.516	0.502	0.476	0.481
Infant Characteristics						
Congenital Anom.	0.009	0.010	0.087	0.095	0.034	0.036
Clinical Condition	0.106	0.109	0.261	0.276	0.163	0.169
Small for Gest.	0.004	0.004	0.056	0.060	0.044	0.044
Large for Gest.	0.067	0.067	0.016	0.017	0.018	0.019
Birth Weight (G)	3344.34	3330.74	988.77	998.23	2181.30	2161.46
Gestation (Wks)	39.493	39.436	29.312	29.466	36.410	36.272
Treatment & Outcomes						
Neonatal Mort.	0.005	0.005	0.242	0.215	0.013	0.012
28 Day Readmit	0.032	0.032	0.010	0.011	0.034	0.033
1 Year Readmit	0.104	0.105	0.188	0.199	0.137	0.141
Transfer	0.009	0.009	0.188	0.155	0.039	0.035
Transfer Day 1	0.004	0.003	0.084	0.051	0.016	0.013
Length of Stay	3.742	3.995	37.555	41.662	9.102	10.110
Charges	5.986	7.059	166.925	185.444	23.263	26.435
Charges/Day	0.820	0.874	4.098	4.097	1.759	1.838
Total Length of Stay	3.982	4.253	47.731	49.731	10.020	10.921
Total Charges	7.056	8.215	210.349	218.462	26.903	29.589
N	1,566,023	1,565,925	23,231	18,809	87,131	86,764

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Table 3.4 – *continued from previous page*

	NBW1		NBW2		HBW	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
NICU Admission	0.110	0.118	0.088	0.095	0.118	0.129
Mother's Demographics						
Age	26.961	26.973	27.543	27.560	28.610	28.635
Medicaid	0.502	0.502	0.460	0.461	0.432	0.432
Managed Care	0.364	0.364	0.397	0.397	0.419	0.419
Self Pay	0.040	0.041	0.036	0.037	0.032	0.033
No College	0.643	0.640	0.615	0.613	0.595	0.594
Some College	0.176	0.176	0.185	0.185	0.197	0.196
College	0.104	0.105	0.113	0.115	0.118	0.120
College Plus	0.078	0.078	0.087	0.087	0.090	0.089
Black	0.092	0.091	0.059	0.059	0.044	0.043
Other Race	0.159	0.158	0.113	0.113	0.076	0.076
Hispanic	0.469	0.468	0.485	0.486	0.473	0.470
Pregnancy Characteristics						
Multiple Birth	0.033	0.034	0.003	0.003	0.000	0.000
Parity	2.062	2.061	2.152	2.153	2.368	2.371
Mnth. Pren. Beg.	2.500	2.499	2.437	2.442	2.398	2.390
# of Pren. Visits	11.334	11.338	11.708	11.700	12.003	12.031
Male	0.454	0.454	0.530	0.529	0.628	0.627
Infant Characteristics						
Congenital Anom.	0.009	0.009	0.006	0.006	0.007	0.007
Clinical Condition	0.051	0.052	0.065	0.065	0.435	0.440
Small for Gest.	0.002	0.002	0.000	0.000	0.000	0.000
Large for Gest.	0.013	0.013	0.036	0.036	0.420	0.424
Birth Weight (G)	2973.57	2972.86	3578.02	3577.51	4271.97	4274.53
Gestation (Wks)	39.221	39.202	40.037	40.030	40.424	40.429
Treatment & Outcomes						
Neonatal Mort.	0.002	0.002	0.001	0.001	0.001	0.001
28 Day Readmit	0.034	0.035	0.031	0.031	0.030	0.031
1 Year Readmit	0.108	0.108	0.097	0.098	0.097	0.096
Transfer	0.006	0.006	0.004	0.004	0.005	0.005
Transfer Day 1	0.003	0.002	0.001	0.001	0.002	0.002
Length of Stay	3.071	3.140	2.899	2.928	3.182	3.229
Charges	3.237	3.548	2.578	2.770	3.190	3.501
Charges/Day	0.744	0.770	0.707	0.742	0.771	0.817
Total Length of Stay	3.188	3.252	2.959	2.995	3.263	3.307
Total Charges	3.830	4.117	2.894	3.122	3.612	3.881
N	516,209	516,190	780,057	780,053	161,876	161,628

Notes: This table presents sample means for the full sample and each of the five birth weight subsamples by residual empty beds. The residuals are from separate regressions of empty beds on hospital-specific month fixed effects for each subsample. Total LOS and Total Charges sum length of stay and hospital charges over all contiguous hospitalizations prior to first being discharged home or dying. Neonatal mortality is mortality within twenty-eight days of birth or within one year if an infant is continuously hospitalized since birth.

Table 3.5: Effect of Empty Beds on NICU Admission

	P(Admit) and Sample Size	Regression Coefficients *Dependent Var: NICU Admission*						Relative Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full Sample	0.135	0.0024**	0.0015**	0.0015**	0.0015**	0.0015**	0.0015**	1.11%
	3,131,948	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	
VLBW	0.769	0.0047**	0.0034**	0.0034**	0.0033**	0.0032**	0.0031**	0.40%
(0 to 1,499 G)	42,040	(0.0007)	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0006)	
LBW	0.523	0.0075**	0.0052**	0.0052**	0.0051**	0.0051**	0.0049**	0.94%
(1,500 to 2,499 G)	173,895	(0.0008)	(0.0006)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	
NBW1	0.114	0.0014**	0.0014**	0.0014**	0.0014**	0.0014**	0.0014**	1.21%
(2,500 to 3,249 G)	1,032,399	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	
NBW2	0.092	0.0009**	0.0009**	0.0010**	0.0010**	0.0010**	0.0009**	1.00%
(3,250 to 4,000 G)	1,560,110	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	
HBW	0.123	0.0017**	0.0017**	0.0017**	0.0017**	0.0017**	0.0017**	1.34%
(4,000+ G)	323,504	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	
Hospital-specific Month FE		X	X	X	X	X	X	
Birth Weight FE			X	X	X	X	X	
Day of Week FE				X	X	X	X	
Demographics					X	X	X	
Pregnancy Characteristics						X	X	
Infant Characterisitcs							X	

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

Table 3.6: Mitigating Effects of Inter-Hospital Transfers

Dependent Var:	Transfer	Transfer Day 1	Admit or Transfer	Admit or Transfer Day 1
	(1)	(2)	(3)	(4)
Full Sample	-0.0001**	-0.0001**	0.0014**	0.0014**
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
VLBW	-0.0015**	-0.0011**	0.0020**	0.0020**
(0 to 1,499 G)	(0.0005)	(0.0003)	(0.0005)	(0.0005)
LBW	-0.0008**	-0.0005**	0.0044**	0.0045**
(1,500 to 2,499 G)	(0.0002)	(0.0001)	(0.0005)	(0.0005)
NBW1	-0.0001**	-0.0001**	0.0013**	0.0013**
(2,500 to 3,249 G)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
NBW2	-0.0000*	-0.0000*	0.0009**	0.0009**
(3,250 to 4,000 G)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
HBW	-0.0000	-0.0000	0.0017**	0.0016**
(4,000+ G)	(0.0000)	(0.0000)	(0.0002)	(0.0002)

Notes: Each row presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions for the full sample and each of the five birth weight subsamples. In Column 1 the dependent variable is whether the infant is ever transfered, Column 2 whether the infant is transfered on the first day, Column 3 whether the infant is admitted to the NICU or ever transferred, and Column 4 whether the infant is admitted to the NICU or transferred on the first day. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. * p<.10, ** p<.05

Table 3.7: Heterogeneous Effects by Hospital Characteristics – NICU Admission

Dependent Var:	NICU Level			NICU Size		Hospital Ownership		
NICU Admission	Inter.	Comm.	Reg.	Less Than 20 Beds	Greater Than 20 Beds	Gov.	Non-Profit	For-Profit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full Sample	0.0032**	0.0021**	0.0011**	0.0024**	0.0011**	0.0017**	0.0014**	0.0022**
	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0004)
N	564,035	1,205,629	1,347,250	1,785,118	1,346,830	546,232	2,219,680	371,924
Very Low Birth Weight	0.0125**	0.0065**	0.0023**	0.0074**	0.0022**	0.0015*	0.0036**	0.0010
(0 to 1,499 Grams)	(0.0025)	(0.0015)	(0.0005)	(0.0014)	(0.0047)	(0.0009)	(0.0007)	(0.0013)
N	4,223	12,482	25,283	17,463	24,577	7,425	30,221	4,426
Low Birth Weight	0.0118**	0.0088**	0.0034**	0.0092**	0.0034**	0.0054**	0.0047**	0.0065**
(1,500 to 2,499 Grams)	(0.0016)	(0.0009)	(0.0004)	(0.0009)	(0.0004)	(0.0008)	(0.0006)	(0.0016)
N	27,192	60,221	85,933	89,595	84,300	31,687	123,318	19,151
Normal Birth Weight 1	0.0030**	0.0017**	0.0011**	0.0022**	0.0010**	0.0014**	0.0013**	0.0020**
(2,500 to 3,249 Grams)	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0003)
N	185,486	397,938	444,189	586,234	446,165	181,525	728,132	124,497
Normal Birth Weight 2	0.0022**	0.0012**	0.0007**	0.0013**	0.0007**	0.0014**	0.0007**	0.0017**
(3,250 to 4,000 Grams)	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0004)
N	285,974	610,950	655,201	902,873	657,237	268,026	1,108,103	187,106
High Birth Weight	0.0030**	0.0026**	0.0012**	0.0022**	0.0014**	0.0018**	0.0016**	0.0019**
(4,000+ Grams)	(0.0008)	(0.0004)	(0.0002)	(0.0003)	(0.0003)	(0.0005)	(0.0002)	(0.0007)
N	61,160	124,038	136,644	188,953	134,551	57,569	229,906	36,744

Notes: Each cell presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of NICU admission on the number of empty beds. Each row presents estimates from the full sample and each of the five birth weight subsamples. Columns 1-3 present estimates from samples defined by the level of NICU, Columns 4-5 by the number of NICU beds, and Columns 6-8 by hospital ownership. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. * p<.10, ** p<.05

Table 3.8: Heterogeneous Effects by Hospital Characteristics – NICU Admission or Transfer

Dependent Var:	NICU Level			NICU Size		Hospital Ownership		
NICU Admission or Transfer Day 1	Inter.	Comm.	Reg.	Less Than 20 Beds	Greater Than 20 Beds	Gov.	Non-Profit	For-Profit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full Sample	0.0030**	0.0020**	0.0010**	0.0022**	0.0011**	0.0016**	0.0013**	0.0021**
	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0004)
N	564,035	1,205,629	1,347,250	1,785,118	1,346,830	546,232	2,219,680	371,924
VLBW	0.0061**	0.0044**	0.0015**	0.0042**	0.0016**	-0.0001	0.0025**	0.0010
(0 to 1,499 G)	(0.0011)	(0.0011)	(0.0005)	(0.0010)	(0.0005)	(0.0011)	(0.0005)	(0.0012)
N	4,223	12,482	25,283	17,463	24,577	7,425	30,221	4,426
LBW	0.0098**	0.0082**	0.0031**	0.0083**	0.0031**	0.0046**	0.0043**	0.0059**
(1,500 to 2,499 G)	(0.0013)	(0.0009)	(0.0004)	(0.0008)	(0.0004)	(0.0008)	(0.0006)	(0.0015)
N	27,192	60,221	85,933	89,595	84,300	31,687	123,318	19,151
NBW1	0.0029**	0.0017**	0.0010**	0.0022**	0.0009**	0.0013**	0.0013**	0.0019**
(2,500 to 3,249 G)	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0004)	(0.0001)	(0.0004)
N	185,486	397,938	444,189	586,234	446,165	181,525	728,132	124,497
NBW2	0.0022**	0.0012**	0.0007**	0.0013**	0.0007**	0.0014**	0.0007**	0.0017**
(3,250 to 4,000 G)	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0004)
N	285,974	610,950	655,201	902,873	657,237	268,026	1,108,103	187,106
HBW	0.0029**	0.0026**	0.0012**	0.0022**	0.0014**	0.0018**	0.0016**	0.0020**
(4,000+ G)	(0.0008)	(0.0004)	(0.0002)	(0.0003)	(0.0003)	(0.0005)	(0.0002)	(0.0007)
N	61,160	124,038	136,644	188,953	134,551	57,569	229,906	36,744

Notes: This table is structured identically to Table 3.7, except the dependent variable is an indicator that is equal to one if the infant is admitted to the NICU or transferred on the first day. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. * p<.10, ** p<.05

Table 3.9: Heterogeneous Effects by Individual Characteristics – NICU Admission

Dependent Var:		Demographics			Insurance Status			Time	
NICU Admission	Baseline	Hispanic	Black	No College	Private/Non-Managed Care	Managed Care	Medicaid	1991 – 1995	1996 – 2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Full Sample	0.0015**	0.0014**	0.0015**	0.0015**	0.0014**	0.0013**	0.0016**	0.0013**	0.0016**
	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0002)
N	3,131,948	1,486,098	231,056	1,952,067	323,065	1,206,000	1,486,377	1,325,225	1,806,723
VLBW	0.0031**	0.0024**	0.0068**	0.0038**	0.0016**	0.0027**	0.0037**	0.0028**	0.0033**
(0 to 1,499 G)	(0.0006)	(0.0010)	(0.0014)	(0.0007)	(0.0008)	(0.0011)	(0.0007)	(0.0007)	(0.0008)
N	42,040	18,491	7,080	27,573	5,763	14,228	20,820	17,225	24,815
LBW	0.0049**	0.0050**	0.0033**	0.0047**	0.0050**	0.0046**	0.0047**	0.0043**	0.0055**
(1,500 to 2,499 G)	(0.0005)	(0.0006)	(0.0007)	(0.0004)	(0.0012)	(0.0009)	(0.0005)	(0.0005)	(0.0008)
N	173,895	74,188	23,579	111,801	18,042	60,606	89,117	73,311	100,584
NBW1	0.0014**	0.0013**	0.0013**	0.0013**	0.0015**	0.0012**	0.0013**	0.0013**	0.0014**
(2,500 to 3,249 G)	(0.0001)	(0.0002)	(0.0003)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
N	1,032,399	483,914	94,563	662,321	96,980	375,625	518,062	437,107	595,292
NBW2	0.0009**	0.0010**	0.0006**	0.0010**	0.0006**	0.0008**	0.0010**	0.0008**	0.0010**
(3,250 to 4,000 G)	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
N	1,560,110	757,033	91,753	957,999	164,556	619,948	718,624	658,382	901,728
HBW	0.0017**	0.0017**	0.0006	0.0016**	0.0016**	0.0017**	0.0017**	0.0015**	0.0018**
(4,000+ G)	(0.0002)	(0.0003)	(0.0014)	(0.0003)	(0.0006)	(0.0002)	(0.0003)	(0.0004)	(0.0003)
N	323,504	152,472	14,081	192,373	37,724	135,593	139,754	139,200	184,304

Notes: Each cell presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of NICU admission on the number of empty beds. Each row presents estimates from the full sample and each of the five birth weight subsamples. Column 1 repeats the baseline estimates from Table 3.5, Columns 2-4 from samples defined by demographic characteristics, Columns 5-7 by insurance status, and Columns 8-9 by time. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. * p<.10, ** p<.05

Table 3.10: Heterogeneous Effects by Individual Characteristics – NICU Admission or Transfer

Dependent Var:		Demographics			Insurance Status			Time	
NICU Admission or Transfer Day 1	Baseline	Hispanic	Black	No College	Private/Non-Managed Care	Managed Care	Medicaid	1991 – 1995	1996 – 2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Full Sample	0.0014**	0.0013**	0.0013**	0.0014**	0.0014**	0.0013**	0.0014**	0.0012**	0.0015**
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
N	3,131,948	1,486,098	231,056	1,952,067	323,065	1,206,000	1,486,377	1,325,225	1,806,723
VLBW	0.0020**	0.0015	0.0049**	0.0027**	0.0008	0.0016*	0.0026**	0.0015**	0.0023**
(0 to 1,499 G)	(0.0005)	(0.0009)	(0.0014)	(0.0006)	(0.0007)	(0.0009)	(0.0008)	(0.0007)	(0.0006)
N	42,040	18,491	7,080	27,573	5,763	14,228	20,820	17,225	24,815
LBW	0.0045**	0.0044**	0.0028**	0.0042**	0.0048**	0.0042**	0.0040**	0.0038**	0.0050**
(1,500 to 2,499 G)	(0.0005)	(0.0006)	(0.0007)	(0.0004)	(0.0012)	(0.0009)	(0.0005)	(0.0005)	(0.0008)
N	173,895	74,188	23,579	111,801	18,042	60,606	89,117	73,311	100,584
NBW1	0.0013**	0.0012**	0.0013**	0.0013**	0.0014**	0.0012**	0.0013**	0.0012**	0.0014**
(2,500 to 3,249 G)	(0.0001)	(0.0002)	(0.0003)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
N	1,032,399	483,914	94,563	662,321	96,980	375,625	518,062	437,107	595,292
NBW2	0.0009**	0.0009**	0.0006**	0.0009**	0.0006**	0.0008**	0.0010**	0.0008**	0.0010**
(3,250 to 4,000 G)	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
N	1,560,110	757,033	91,753	957,999	164,556	619,948	718,624	658,382	901,728
HBW	0.0016**	0.0016**	0.0006	0.0016**	0.0016**	0.0017**	0.0017**	0.0014**	0.0018**
(4,000+ G)	(0.0002)	(0.0003)	(0.0014)	(0.0003)	(0.0006)	(0.0002)	(0.0003)	(0.0004)	(0.0003)
N	323,504	152,472	14,081	192,373	37,724	135,593	139,754	139,200	184,304

Notes: This table is structured identically to Table 3.9, except the dependent variable is an indicator that is equal to one if the infant is admitted to the NICU or transferred on the first day. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. * p<.10, ** p<.05

Table 3.11: Robustness Checks

Dependent Var: *NICU Admission*	Baseline	50 G BW Dummies	Exclude C-Sections	< 5% Over Capacity	< 60% of Days Over Capacity
	(1)	(2)	(3)	(4)	(5)
Full Sample	0.0015**		0.0012**	0.0017**	0.0017**
	(0.0001)		(0.0001)	(0.0002)	(0.0002)
N	3,131,948		2,428,260	2,118,001	2,269,669
VLBW	0.0031**	0.0030**	0.0036**	0.0042**	0.0042**
(0 to 1,499 Grams)	(0.0006)	(0.0006)	(0.0008)	(0.0007)	(0.0007)
N	42,040	42,040	18,915	26,865	28,483
LBW	0.0049**	0.0049**	0.0047**	0.0058**	0.0060**
(1,500 to 2,499 Grams)	(0.0005)	(0.0005)	(0.0005)	(0.0008)	(0.0008)
N	173,895	173,895	107,492	113,986	122,061
NBW1	0.0014**	0.0014**	0.0011**	0.0014**	0.0015**
(2,500 to 3,249 Grams)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
N	1,032,399	1,032,399	830,504	691,937	741,901
NBW2	0.0009**	0.0009**	0.0008**	0.0010**	0.0010**
(3,250 to 4,000 Grams)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
N	1,560,110	1,560,110	1,245,920	1,062,425	1,138,587
HBW	0.0017**	0.0017**	0.0014**	0.0021**	0.0020**
(4,000+ Grams)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
N	323,504	323,504	225,429	222,788	238,637

Notes: Each cell presents coefficient estimates with standard errors in parenthesis (clustered at the hospital level) from separate regressions of NICU admission on the number of empty beds. Each row presents estimates from the full sample and each of the five birth weight subsamples. Column 1 repeats the baseline estimates from Table 3.5, Column 2 includes birth weight controls at 50-gram increments, Column 3 excludes infants delivered by cesarean section, Column 4 excludes observations from hospital-days in which the number of NICU patients exceeds the number of beds by more than 5%, and Column 5 excludes hospital-years in which the number of NICU patients exceeds the number of NICU beds for more than 60% of the year. Specifications include all control variables described in the notes to Table 3.5, including hospital-specific month fixed effects. * $p<.10$, ** $p<.05$

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