

# Estimating marginal returns to medical care: Evidence from at-risk newborns\*

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## Abstract

We estimate marginal returns to medical care for at-risk newborns by comparing health outcomes and medical treatment provision on either side of common risk classifications, most notably the “very low birth weight” threshold at 1500 grams. First, using data on the census of US births in available years from 1983-2002, we find evidence that newborns with birth weights just below 1500 grams have *lower* one-year mortality rates than do newborns with birth weights just above this cutoff, even though mortality risk tends to decrease with birth weight. One-year mortality falls by approximately one percentage point as birth weight crosses 1500 grams from above, which is large relative to mean one-year mortality of 5.5% just above 1500 grams. Second, using hospital discharge records for births in five states in available years from 1991-2006, we find evidence that newborns with birth weights just below 1500 grams have discontinuously higher charges and frequencies of specific medical inputs. We estimate a \$4,000 increase in hospital costs as birth weight approaches 1500 grams from above, relative to mean hospital costs of \$40,000 just above 1500 grams. Taken together, these estimates suggest that the cost of saving a statistical life of a newborn with birth weight near 1500 grams is on the order of \$500,000 to \$650,000 in 2006 dollars.

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# I. INTRODUCTION

Medical expenditures in the United States are high and increasing. A key policy question is whether the benefits of additional medical expenditures exceed their costs, yet several challenges complicate the empirical measurement of returns to medical spending. One fundamental challenge is that we expect less healthy patients to receive more medical inputs, likely leading to a downward bias in estimates of returns. To overcome this challenge, a variety of studies have used cross-sectional, time-series, and panel data techniques to identify patients who are arguably similar in terms of underlying health status but receive different levels of medical spending. The results of such studies are mixed. On one hand, time-series and panel data studies that compare increases in spending and improvements in health outcomes over time have argued that increases in costs have been less than the value of the associated benefits, at least for some technologies.<sup>1</sup> On the other hand, cross-sectional studies that compare “high-spending” and “low-spending” geographic areas tend to find large differences in spending yet remarkably similar health outcomes.<sup>2</sup>

The lack of consensus in these studies may not be surprising since these studies often estimate returns to different types of medical spending. The return to a dollar of medical spending likely differs across medical technologies and across populations, and the return to the first dollar of medical spending in any given context may differ from the return to the last dollar of medical spending. The time-series studies often estimate returns to large changes in treatments that occur over long periods of time. The cross-sectional studies, on the other hand, estimate returns to additional, incremental spending that occurs in some areas but not others.<sup>3</sup> While estimates of returns to large changes in medical spending are useful summaries of changes over time, estimates of marginal returns are needed to inform policy decisions over whether to increase or decrease the level of care in a given context.

The main innovation in this paper proposes a novel research design for more direct estimation of the marginal costs and marginal benefits to medical spending, which we can combine

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<sup>1</sup>See, for example, Cutler & McClellan (2001), Cutler *et al.* (1998), Cutler *et al.* (2006), Luce *et al.* (2006), McClellan (1997), Murphy & Topel (2003), and Nordhaus (2002).

<sup>2</sup>See, for example, Baicker & Chandra (2004), Fisher *et al.* (1994), Fuchs (2004), Kessler & McClellan (1996), O’Connor *et al.* (1999), Pilote *et al.* (1995), Stukel *et al.* (2005), and Tu *et al.* (1997).

<sup>3</sup>Some have interpreted the cross-sectional results as evidence that marginal returns to spending may be low in many contexts; see, for example, Meltzer (2003).

to estimate marginal returns. Our research design is applicable in settings with an observable, continuous measure of health risk and a diagnostic threshold based on this risk variable that generates a discontinuous probability of receiving additional treatment. In such empirical settings, we can use a regression discontinuity framework: as long as other factors are smooth across the threshold (an assumption we investigate in several empirical tests), individuals within a small bandwidth on either side of the threshold should differ only in their probability of receiving additional health-related inputs and not in underlying health. This research design allows us to estimate marginal returns to medical spending for patients near such thresholds in the following sense: conditional on estimating that, on average, patients on one side of the threshold incur additional medical costs, we can estimate the associated benefits by examining average differences in health outcomes across the threshold. Combining these cost and benefit estimates then allows us to calculate the return to this increment of additional spending, or “average marginal returns.”

We apply our research design to estimate marginal returns to medical spending on “at risk” newborns, a population that is of interest for several reasons. First, even relatively small reductions in mortality for newborns can be magnified in terms of the total number of years of life saved. Second, technologies for treating at-risk newborns have expanded tremendously in recent years, at very high cost. For example, a 2005 Agency for Healthcare Research and Quality study finds that that top two most expensive diagnoses (regardless of age) are “infant respiratory distress syndrome” (average charges of \$91,000) and “premature birth and low birthweight” (average charges of \$79,300). Third, although existing estimates suggest that the benefits associated with large changes in spending on at risk newborns over time have been greater than their costs (Cutler & Meara, 2000), there is a dearth of evidence on the returns to incremental spending in this context. Fourth, studying newborns allows us to focus on a large portion of the health care system, as child birth is one of the most common reasons for hospital admission in the US. This patient population also provides samples large enough to detect effects of additional treatment on infant mortality.

This paper focuses on the “very low birth weight” (VLBW) classification at 1500 grams (just under 3 pounds, 5 ounces)—a threshold that is commonly cited in the medical literature. We also consider other classifications based on birth weight and alternative measures of newborn health.

From an empirical perspective, birth weight-based thresholds provide an attractive basis for a regression discontinuity design for several reasons. First, they are unlikely to represent breaks in underlying health risk. A 1985 *Institute of Medicine* report, for example, notes: “...designation of very low birth weight infants as those weighing 1,500 grams or less reflected convention rather than biologic criteria.” Second, it is generally agreed that birth weight cannot be predicted in advance of delivery with the accuracy needed to change (via birth timing) the classification of a newborn from being just above 1500 grams to being just below 1500 grams. Thus, although we empirically investigate our assumption that the position of a newborn just above 1500 grams relative to just below 1500 grams is “as good as random,” the medical literature suggests this assumption is also intuitively plausible.

To preview our main results, using data on the census of US births in available years from 1983-2002, we find that one-year mortality increases by approximately one percentage point as birth weight crosses the VLBW threshold from above, which is large relative to mean one-year mortality of 5.5% just above 1500 grams. This increase in mortality is in stark contrast to the overall decline in mortality as birth weight increases, and to the extent that heavier newborns are healthier in unobservable ways, the increase in mortality that we observe is all the more striking. Second, using hospital discharge records for births in five states in available years from 1991-2006, we estimate a \$4,000 increase in hospital costs for infants just below the 1500 gram threshold, relative to mean hospital costs of \$40,000 just above 1500 grams. Weaker evidence suggests analogous effects at other cutoffs, such as the “extremely low birth weight” threshold of 1000 grams and the “low birth weight” threshold of 2500 grams. Taken together, our estimates suggest that the cost of saving a statistical life for newborns near 1500 grams is on the order of \$500,000 to \$650,000 in 2006 dollars, which as we discuss in Section VIII can be compared to a variety of benchmarks.

The remainder of the paper is organized as follows. Section II discusses the available evidence on the costs and benefits of medical care for at-risk newborns, gives a brief background on the origins of the at-risk newborn classifications we study, and discusses why these classifications could affect the provision of medical treatment. Section III describes our data and analysis sample, and Section IV outlines our empirical framework, estimation strategy, and bandwidth selection. Section V presents our main results, and Section VI presents several robustness and

specification checks. Section VII examines variation in our estimated treatment effects over time and across hospitals. In Section VIII we combine our main estimates to calculate two-sample estimates of marginal returns, and for comparison to the previous literature also compare our estimates to a time series estimate (in the spirit of Cutler & Meara (2000)) of the returns to large changes in spending over time for newborns in our bandwidth. Section IX discusses evidence from other thresholds, and Section X concludes.

## II. BACKGROUND

### A. Costs and benefits of medical care for at-risk newborns

Technologies for treating at-risk newborns have expanded tremendously in recent years - including the development of improved ventilators and an expansion in the number of neonatal intensive care units. Many of these medical advances have been very expensive. For example, in 2005 the US Agency for Healthcare Research and Quality estimated that the two most expensive hospital diagnoses (regardless of age) were “infant respiratory distress syndrome” and “premature birth and low birth weight.”<sup>4</sup> Russell *et al.* (2007) estimated that in the US in 2001, preterm and low-birth weight diagnoses accounted for 8% of newborn admissions, but 47% of the costs for all infant hospitalizations (at \$15,100 on average).<sup>5</sup> Despite their high and highly-concentrated costs, use of new neonatal technologies has continued to expand. An example related to our threshold of interest is provided by the Oxford Health Network’s 362 hospitals, where the use of high-frequency ventilation among VLBW infants tripled between 1991 and 1999 (Horbar *et al.*, 2002).

These high costs naturally motivate the question of what these medical advances have been “worth” in terms of improved health outcomes. Anspach (1993) and others discuss the paucity of randomized controlled trials which measure the effectiveness of neonatal intensive care. In the absence of such evidence, some have questioned the effectiveness of these increasingly intensive treatment patterns (Enthoven, 1980; Grumbach, 2002; Goodman *et al.*, 2002).<sup>6</sup> On the other

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<sup>4</sup>See <http://www.ahrq.gov/data/hcup/factbk6/factbk6.pdf> (accessed 29 October 2008).

<sup>5</sup>It is worth noting that these costs may disproportionately fall on the public sector. Russell *et al.* (2007) note that of all preterm/low birth weight infant stays, 42% designated Medicaid as the expected payer, relative to 37.5% for “uncomplicated” newborns.

<sup>6</sup>Grumbach (2002), for example, argues: “Neonatal intensive care units are profit-making centers for hospitals, commanding high payments from private and public insurance plans,” and that “uncontrolled growth” in

hand, Cutler & Meara (2000) examine time-series variation in birth weight-specific treatment costs and mortality outcomes and argue that medical advances for newborns have had large returns.<sup>7</sup>

## B. “At risk” newborn classifications

We focus on a commonly-used newborn risk classification: the “very low birth weight” (VLBW) classification at 1500 grams (just under 3 pounds, 5 ounces). Weaker evidence is found for other classifications, including the “extremely low birth weight” (ELBW) classification at 1000 grams (just over 2 pounds, 3 ounces) and the “low birth weight” (LBW) classification at 2500 grams (just over 5 pounds, 8 ounces). We also explore gestational-age based measures such as the “prematurity” classification at 37 gestational weeks - where gestational weeks are defined as the number of weeks since the first day of the mother’s last menstrual period. This subsection provides some background on these three classifications.<sup>8</sup>

Physicians had begun to recognize and assess the relationships among inadequate growth (low birth weight), shortened gestation (prematurity), and mortality at least by the early 1900s. The 2500 gram low birth weight classification, for example, has existed since at least 1930, when a Finnish pediatrician advocated 2500 grams as the birth weight below which infants were at high risk of adverse neonatal outcomes. This recommendation was formally adopted by the World Health Organization (WHO) on two separate occasions, in 1948 and 1950.<sup>9</sup> A 1985

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neonatology “has less to do with the true need of communities for effective clinical services than with the financial incentives.” Going a step further, Goodman *et al.* (2002) ask whether “infants might be harmed by the availability of higher levels of resources.”

<sup>7</sup>Cutler and Meara’s empirical approach assumes that all within-birth weight changes in survival have been due to improvements in medical technologies. This approach is motivated by the argument that conditional on birth weight, the overwhelming factor influencing survival for low birth weight babies is medical care in the immediate postnatal period (Paneth, 1995; Williams & Chen, 1982). However, others have noted that it is possible that underlying changes in the health status of infants within each weight group (due to, for example, improved maternal nutrition) are responsible for neonatal mortality independent of newborn medical care (United States Congress, Office of Technology Assessment, 1981).

<sup>8</sup>The discussion in this section draws heavily from United States Institute of Medicine (1985).

<sup>9</sup>The first recommendation was adopted at the First World Health Assembly in 1948: “For the purposes of this classification, an immature infant is a liveborn infant with a birth weight of 5-1/2 pounds (2500 grams) or less, or specified as immature. In some countries, however, this criteria will not be applicable. If weight is not specified, a liveborn infant with a period of gestation of less than 37 weeks or specified as ‘premature’ may be considered as the equivalent of an immature infant for purposes of this classification.” The second recommendation was adopted at the WHO Expert Group on Prematurity in 1950: “The Expert Group on Prematurity recognizes the necessity for uniform technology for international usage. Since the primary goal is to lower fetal and neonatal mortality, the aim can best be achieved by providing specialized care for infants of low birth weight. The group suggests a premature infant be defined as one whose birth weight is 2500 grams (5-1/2 pounds) or less. The limitations of this criteria are recognized, however, since data on birth weight will not always be available and other criteria of prematurity must be used, for example, gestation.”

Institute of Medicine report notes that both recommendations indicated that the use of a birth weight marker served as a shorthand notation for a variety of interrelated physiologic processes affecting fetal growth and the duration of gestation. The 1985 IOM report notes that over time, interest increased in the outcomes of subgroups of low birth weight infants, in particular the fate of the smallest infants, and that conventionally “very low birth weight” infants were defined as those born weighing 1500 grams or less.

The key to our empirical strategy is that these cutoffs appear to truly reflect convention rather than strict biologic criteria. For example, the 1985 Institute of Medicine report notes:

*“Birth weight is a continuous variable and the limit at 2,500 grams does not represent a biologic category, but simply a point on a continuous curve. The infant born at 2,499 grams does not differ significantly from one born at 2,501 grams on the basis of birth weight alone...As with the 2,500 gram limit, designation of very low birth weight infants as those weighing 1,500 grams or less reflected convention rather than biologic criteria.”*

The 1961 WHO Expert Committee on Maternal and Child Health issued further recommendations emphasizing the importance of the 2500 gram and 37 gestational week classifications. While gestational age is a natural consideration when determining treatment for newborns with low birth weights, a comparison of treatments by gestational age has the added complication that gestational age is a choice variable that is likely affected by the prematurity thresholds. Gestational age is known to women in advance of giving birth, and women can choose to time their birth (for example, through an induced vaginal birth or through a C-section) based on gestational age. Thus, *a priori* we would expect that mothers who give birth prior to 37 gestational weeks may be different than mothers who give birth after 37 gestational weeks on the basis of factors other than gestational age. It is thought that birth weight, on the other hand, cannot be predicted in advance of birth with the accuracy needed to change (via birth timing) the classification of a baby from being just above 1500 grams to being just below 1500 grams; this assertion has been confirmed from conversations with physicians,<sup>10</sup> as well from studies such

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<sup>10</sup>We use the phrase “conversations with physicians” somewhat loosely throughout the text of the paper to reference discussions with several physicians as well as readings of the relevant medical literature and references such as the *Manual of Neonatal Care* for the Joint Program in Neonatology (Harvard Medical School, Beth Israel Deaconess Medical Center, Brigham and Women’s Hospital, Children’s Hospital Boston) (Cloherty & Stark,



as Pressman *et al.* (2000). Gestational age is also thought to be considerably more difficult to measure than is birth weight (see, for example, the 1985 IOM report); however, to the extent that we as researchers observe the same measure of gestational age that women and doctors observe, this is not “measurement error” in the traditional sense and should not be problematic for our research design.

To define treatment of observations occurring exactly at the relevant cutoffs, we rely on definitions listed in the International Statistical Classification of Diseases and Related Health Problems (ICD-9) codes. According to the ICD-9 codes, very low birth weight is defined as having birth weight strictly less than 1500 grams, and analogously (with a strict inequality) for the other thresholds we examine.

### C. Should these cutoffs matter?

Of course, birth weight and gestational age are not the only factors used to assess newborn health. For example, respiratory rate, color, APGAR score (an index of newborn health), head circumference, and presence of congenital anomalies could also affect physicians’ initial health assessments of infants (Cloherty & Stark, 1998). This implies that we should expect our cutoffs of interest to be “fuzzy” rather than “sharp” discontinuities (Trochim, 1984): that is, we do not expect the probability of a given treatment to fall from 1 to 0 as one moves from 1499 grams to 1501 grams, but rather expect a change in the likelihood of treatment for newborns classified into a given risk category.

From an empirical perspective, the fact that we observe a first stage in terms of a discontinuity in treatment provision around 1500 grams suggests that hospitals or physicians do use these cutoffs to determine treatment either through hospital protocols or as rules of thumb.<sup>11</sup> For example, diagnostic ultrasounds (also known as cranial ultrasounds) are used to check for bleeding or swelling of the brain as signs of intraventricular hemorrhages (IVH) - a major concern for at-risk newborns. The 1500 gram threshold is commonly cited as a point below which diagnostic

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1998). The medical doctors we spoke with include Dr. Christopher Almond from Children’s Hospital Boston (Boston, MA); Dr. Burak Alsan from Harvard Brigham and Women’s/Children’s Hospital Boston (Boston, MA); Dr. Munish Gupta from Beth Israel Deaconess Medical Center (Boston, MA); Dr. Chafen Hart from the Tufts Medical Center (Boston, MA); and Dr. Katherine Metcalf from Saint Vincent Hospital (Worcester, MA). We are very grateful for their time and feedback, but they are of course not responsible for any errors in our work.

<sup>11</sup>As we will discuss more in Section IV, we may not observe all relevant first stage inputs. That said, we do find evidence of a first stage for summary measures of treatment, as well as for some particular procedures.



ultrasounds should be used. In particular, the neonatal care manual used by medical staff at the Longwood Medical Area (Boston, MA) notes: “We perform routine ultrasound screens in infants with birth weight <1500gm (Cloherty & Stark, 1998).” We will investigate differences in procedure use, including diagnostic ultrasounds, below.

These cutoffs could affect treatment provision through such hospital protocols. The ICD-9 codes, for example, list separate diagnosis codes (V21.30-V21.35) that depend on birth weight in grams: 0-500, 500-999, 1000-1499, 1500-1999, 2000-2500, *etc.* Current Procedural Terminology (CPT) codes that are used for billing purposes also separately identify newborns weighing greater or less than 1500 grams, thus potentially allowing for a different level of billing. Such reporting differences may naturally lead to different protocols in hospitals. Note that in theory if prices differ across our threshold of interest, then any discontinuous jump in charges could in part be due to changes in prices rather than changes in quantities. In practice, however, we find evidence of discontinuities in quantities, such as length of stay, implying that changes in prices do not explain our entire measured discontinuities in charges. We unfortunately do not observe prices in any of our hospital discharge record datasets.

Another explanation for why these cutoffs may affect treatment provision is that they may be used as informal “rules of thumb” by physicians.<sup>12</sup> Discussions with physicians suggest that these potential discontinuities are well-known, salient cutoffs below which babies may be at increased consideration for receiving additional treatments. Clinicians (as well as researchers) frequently collapse continuous measures of health (such as birth weight) into binary measures such as “low birth weight” versus “normal birth weight.” Some have argued that such heuristics can influence medical treatment by acting as “silent adjudicators of clinical practice” (McDonald, 1996). Others have noted that rules of thumb in medicine can be “useful and even necessary shortcuts guiding search and choice under uncertainty and time constraint” (Andre *et al.*, 2002).

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<sup>12</sup>Medical malpractice environments could also be one force affecting adherence to either formal rules or informal rules of thumb.

### III. DATA

#### A. Data description

Our empirical analysis requires data with information on birth weight and some welfare-relevant outcome, such as medical care expenditures or health outcomes. Our primary analysis uses three data sets: first, the National Center for Health Statistics (NCHS) birth cohort linked birth/infant death files; second, a longitudinal research database of linked birth record-death certificate-hospital discharge data from California; and third, hospital discharge data from several states in the Healthcare Cost and Utilization Project (HCUP) state inpatient databases.

The NCHS birth cohort linked birth/infant death files, hereafter the “nationwide data,” include data for a complete census of births occurring each year in the US, for the years 1983-1991 and 1995-2002.<sup>13</sup> The data include information reported on birth certificates linked to information reported on death certificates for infants who die within one year of birth. The birth certificate data offers a rich set of covariates (for example, mother’s age and education), and the death certificate data includes a cause of death code. Beginning in 1989, these data include some treatment variables - namely, indicators for use of a ventilator for less than or (separately) greater than thirty minutes after birth.

Our other two data sources offer treatment variables beyond ventilator use. The California research database is the same dataset used in Almond & Doyle (2008). These data were created by the California Office of Statewide Health Planning and Development, and include all live births in California from 1991-2002 - approximately 6 million records. The data include hospital discharge records linked to birth and death certificates. The hospital discharge data include diagnoses, course of treatment, length of hospital stay, and charges incurred during the hospitalization. The data are longitudinal in nature and track hospital readmissions for up to one year from birth as long as the infants enter a California hospital. This longitudinal aspect of the data allows us to examine charges and length of stay even if the newborn is transferred to another hospital.<sup>14</sup>

The HCUP state inpatient databases allow us to analyze the universe of hospital discharge abstracts in four other states that include the birth weight variable necessary for our analysis.<sup>15</sup>

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<sup>13</sup>NCHS did not produce linked birth/infant death files from 1992-1994.

<sup>14</sup>The current version of our data only tracks a newborn through at most one transfer.

<sup>15</sup>The State Inpatient Data (SID) we analyze contain the universe of inpatient discharge records from partic-

Specifically, we use HCUP data from Arizona for 2001-2006, New Jersey for 1995-2006, Maryland for 1995-2006, and New York for 1995-2000 (see Appendix Table A3).<sup>16</sup> The HCUP data includes variables similar to those available in the California discharge data, but unlike the California data are not linked to mortality records or hospital records for readmissions or transfers. Although we cannot link these discharge data with mortality data directly, we can examine mortality outcomes for these newborns through using a sub-sample of our nationwide data, as our nationwide data and the HCUP discharge data relate to the same births.<sup>17</sup> In much of our analysis, we pool the California and HCUP data to create a “five-state sample.”

## B. Analysis sample

Sample selection issues are minimal. In our main specifications, we pool data from all available years, although we do separately examine results across time periods. For the main results, we limit the sample to those observations with non-missing, non-imputed birth weight information.<sup>18</sup> Fortunately, given the demands of our empirical approach, these data provide relatively large samples: over 200,000 newborns fall within our pilot bandwidth of 3 ounces within the 1500 gram threshold in the nationwide data, and we have over 30,000 births in the same interval when we consider the hospital discharge data. We discuss bandwidth selection below.

# IV. EMPIRICAL FRAMEWORK AND ESTIMATION

## A. Empirical framework

Consider the following structural equation for the VLBW classification:

$$Y = f(Z, I, e), \quad Z \in [Z^* - h, Z^* + h] \quad (1)$$

ipating states. (Other HCUP databases, such as the National Inpatient Sample, are a sub-sample of the SID data.)

<sup>16</sup>At present, 39 states participate in the SID. Of these 39 states, 10 report the birth weight of newborns. We have obtained HCUP data for the top four of the ten states with birth weight (with the exception of North Carolina, we have discharge data for the top four states by number of births: New York, New Jersey, Maryland, and Arizona). In ongoing work, we intend to supplement our analysis with additional years of HCUP data from these four states.

<sup>17</sup>Note that our nationwide data include births that took place outside of hospitals, whereas our California and HCUP discharge data by construction only capture deliveries taking place in hospitals. In practice, 99.2% of deliveries in our national sample occurred in a hospital. In some specifications we limit our nationwide data to the sample of hospitalized births, for greater comparability.

<sup>18</sup>In practice this sample selection criteria excludes a very small number of our observations. For the full NCHS data, for example, dropping observations with missing or imputed birth weights drops only 0.12% of the sample.

where  $Y$  is the outcome variable (for example, one year mortality),  $Z$  is the running variable (birth weight),  $I$  are inputs that affect mortality (such as medical treatments and parental inputs),  $e$  is unobserved heterogeneity, and  $h$  is a bandwidth around  $Z^*$ . In an instrumental variables framework, we define the instrumental variable  $Z^*$  as:

$$Z^* = \begin{cases} 1 & \text{if birth weight} < 1500 \text{ grams} \\ 0 & \text{if birth weight} \geq 1500 \text{ grams} \end{cases}$$

Our first stage equation can then be written as:

$$I = g(Z^*, Z, v), \quad Z \in [Z^* - h, Z^* + h] \quad (2)$$

where  $v$  is unobserved heterogeneity. In order for  $Z^*$  to be a valid instrument, the two usual instrumental variables conditions must hold. First, there must exist a first stage relationship between  $Z^*$  and  $I$ ; note that this relationship will be conditional on our running variable  $Z$ . Second, the exclusion restriction requires that the only mechanism through which the instrument  $Z^*$  affects our outcome variable (here, mortality), conditional on  $Z$  within the bandwidth, is through its effect on the input  $I$ . If we observed and were able to measure all relevant inputs in  $I$ , then we could argue for the validity of this exclusion restriction. However, for any given  $I$  that we observe, it is likely that there exists some additional health-related input that we do not observe.<sup>19</sup> It is unclear how important such unobserved inputs are in practice, but to the extent they are important, a first stage variable  $I$  such as medical expenditures would violate the exclusion restriction.

On one hand, our reduced form estimate of the direct impact of our instrumental variable  $Z^*$  on mortality is itself interesting and policy relevant, as this estimate includes the effects of all relevant inputs. On the other hand, our instrumental variable estimate is also of substantive interest, and thus having a sense of the potential magnitude of the effects of unobserved inputs is useful. To the extent that medical inputs are much more important relative to parental inputs in

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<sup>19</sup>As an example, consider the case in which the length of time which parents hold their newborns has a direct effect on mortality, that this variable is unobserved, and that this variable varies discontinuously across our cutoff (which could be the case if the 1500 gram classification is salient to parents).

the very short run after birth (say, within twenty-four hours of birth), we can test for impacts on short run mortality measures and be somewhat assured that other unobserved parental inputs are not likely to affect these estimates. As we will discuss in Section V, we do indeed find effects on short run mortality measures.

We examine a number of different first stage variables, which we discuss at length below. Broadly, we examine first stage variables in two categories: “summary treatment measures” (such as charges and length of stay) that capture many aspects of hospital treatment, and “mechanism variables” (such as ventilation) that could be the margins through which discontinuities in summary treatments occur.

In Section V, we report first stage and reduced form estimates separately. In Section VIII, we combine these estimates into two-sample estimates in which the numerator is the reduced form estimate and the denominator is the first stage estimate.<sup>20</sup>

## B. Estimation

To estimate the size of the discontinuity in outcomes and treatment, we follow standard methods for regression discontinuity analysis (as in, for example, Imbens & Lemieux (2008)). First, we estimate and report a local-linear regression. This estimate incorporates information from a bandwidth of 3 ounces (85 grams) above and below the threshold. We describe the selection of this bandwidth in the next subsection. We use a triangle kernel so that the weight on each observation decays with the distance from the threshold, and we report asymptotic standard errors (Cheng *et al.*, 1997; Porter, 2003).<sup>21</sup>

In addition, within the bandwidth  $h$ , we estimate the following model for infant  $i$  weighing  $g$  grams in year  $t$ :

$$Y_i = \alpha_0 + \alpha_1 VLBW_i + \alpha_2 VLBW_i * (g_i - 1500) + \alpha_3 (1 - VLBW_i) * (g_i - 1500) + \alpha_4 t_i + \alpha_5 s_i + X_i' \alpha_6 + \epsilon_i \quad (3)$$

where  $Y$  is an outcome such as one-year mortality, and  $VLBW$  is an indicator that the newborn was classified as very low birth weight (that is, strictly less than 1500 grams). We include

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<sup>20</sup>Without covariates, the two-sample estimate is equivalent to the Wald and two stage least squares estimates, given our binary instrumental variable. Even though the first stage and reduced form estimates come from different data sources, we can standardize the samples and covariates to produce the same estimates that we would attain from a single data source.

<sup>21</sup>We are grateful to Doug Miller for providing code from Ludwig & Miller (2007).

separate gram trend terms above and below the cutoff, parameterized so that a test of whether the trend is the same above and below the threshold is simply a matter of testing whether  $\alpha_2 = \alpha_3$ . In some specifications, we include year indicators  $t_i$ , state indicators  $s_i$ , and newborn characteristics,  $X'_i$ . The newborn characteristics that are available for all of the years in the nationwide data include an indicator that the mother was born outside the state where the infant was born, indicators for mother's age, education, father's age, the newborn's sex, gestational age indicators, race, and plurality. We estimate this model by OLS with heteroskedastic-robust standard errors. Probit results for our binary dependent variables give very similar results, as described below.

### C. Bandwidth selection

Our pilot bandwidth includes babies with birth weights within 3 ounces (85 grams) of 1500 grams, or from 1415 grams to 1585 grams. We chose this pilot bandwidth by a cross-validation procedure where the relationships between the main outcomes of interest and birth weight were estimated with local linear regressions and compared to a 4-th order polynomial model. These models were estimated separately above and below the 1500 gram threshold. The bandwidth that minimized the sum of squared errors between these two estimates between 1200 and 1800 grams tended to be between 60 and 70 grams for the mortality outcomes. For the treatment measures, the bandwidth tended to be closer to 40 grams. Given that we are estimating the relationship at a boundary, a larger bandwidth is generally warranted. We chose to use a pilot bandwidth of 85 grams - 3 ounces<sup>22</sup> - for the main results. This larger bandwidth incorporates more information that can improve precision, but of course including births further from the threshold departs from the assumption that newborns are nearly identical on either side of the cutoff. That said, our local linear estimates allow the weight on observations to decay with the distance from the threshold. In addition, the results are qualitatively similar across a wide range of bandwidths (see Table 6). To give a clearer sense of how our data look graphically, our figures report means for a slightly wider bandwidth - namely, the 5 ounces above and below.

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<sup>22</sup>As discussed in the next section, our birth weight variable has pronounced reporting heaps at gram equivalents of ounce intervals. We specify the bandwidth in ounces to ensure that the sample sizes are comparable above and below the discontinuity, given these trends in reporting.

## V. RESULTS

### A. Frequency of births by birth weight

Figure 1 reports a histogram of births between 1400 grams and 1600 grams in the nationwide sample, which has several notable characteristics.<sup>23</sup> First, there are pronounced reporting heaps at the gram equivalents of ounce intervals. Although there are also reporting heaps at “round” gram numbers (such as multiples of one hundred), these heaps are much smaller than those observed at gram equivalents of ounce intervals. Discussions with physicians suggest that birth weight is frequently measured in ounces, although typically also measured in grams at some point for purposes of billing and dosage recommendations for pharmaceuticals. Given the nature of the variation inherent in the reporting of our birth weight variable largely in ounces, our graphical results will focus on data which has been collapsed into one-ounce bins.<sup>24</sup>

Second, we do not observe irregular reporting heaps around our 1500 gram threshold of interest, consistent with women being unable to predict birth weight in advance of birth with the accuracy necessary to move from just above 1500 grams to just below 1500 grams. The lack of heaping also suggests that physicians or hospitals do not manipulate reported birth weight so that, for example, more newborns fall below the 1500g cutoff and justify higher reimbursements. Comfortingly, the frequency of births at 1500 grams is very similar to the frequency of births at 1400 grams and at 1600 grams, and the ounce markers surrounding 1500 grams have similar frequencies to other ounce markers.

More formally, McCrary (2008) suggests a direct test for possible manipulation of the running variable - in our case, birth weight. We implement his test by collapsing our nationwide data to the gram level - keeping a count of the number of newborns classified at each gram - and then regressing that count as the outcome variable in the same framework as our regression discontinuity estimates. Using this test, we find no evidence of manipulation of the running variable around the VLBW threshold.<sup>25</sup>

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<sup>23</sup>See Appendix Figure A1A for a wider set of births.

<sup>24</sup>Specifically, we construct one-ounce bins radiating out from our threshold of interest (*e.g.* 0-28 grams from the threshold, 29-56 grams from the threshold, *etc.*).

<sup>25</sup>Specifically, for 1500 grams we estimate a coefficient of -2,100 (*s.e.*=1500, *p*=0.1898).



## B. Health outcomes

Figure 2 reports mean mortality for all infants in one-ounce bins close to the VLBW threshold. Mortality falls with birth weight, but we find a discontinuous increase in mortality just after the VLBW threshold. Note that the one-year mortality rate is relatively high for this at-risk population: close to 6%. The figure shows that even within our relatively small bandwidth, there is a general reduction in mortality as birth weight increases, reflecting the health benefits associated with higher birth weight. The increase in mortality observed just above 1500 grams appears to be a level shift, with the slope generally similar above and below the threshold.<sup>26</sup> The mean mortality rate in the ounce bin just above the threshold is 6.15%, which is 0.46 percentage points larger than mean mortality just below the threshold of 5.69%. We see a similar .48 percentage point difference for 28-day mortality - between 4.39% above the threshold and 3.91% below the threshold. This suggests that most of the observed gains in 28-day mortality persist to one year.

Table 1 reports the main results. The first reported outcome is one-year mortality, and the local-linear regression estimate is -0.0121. This implies a 22% reduction in mortality compared to a mean mortality rate of 5.53% in the 3 ounces above the threshold (the “untreated” group in this regression discontinuity design). OLS point estimates are slightly smaller, but still large: -0.0095. The estimates are similar when probit models are used.<sup>27</sup>

For our OLS models, we report heteroskedastic-robust standard errors. To address potential concerns about discreteness in our dependent variable, we perform the standard error correction suggested by Card & Lee (2008). In our application, this correction amounts to clustering at the gram-level. Doing so increases the magnitude of our standard errors, but our main results remain statistically significant at conventional levels.<sup>28</sup>

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<sup>26</sup>Note that in this graph there is also a smaller, visible “jump” in mortality around 1600 grams, an issue we address in several ways. First, if we construct graphs analogous to Figure 2 which focus on 1600 grams as a potential discontinuity, there is no visible jump at 1600 grams. Exploration of this issue reveals that the slightly different groupings which occur when one-ounce bins are radiated out from 1500 grams relative to when one-ounce bins are radiated out from 1600 grams explain this difference, implying that small-sample variation is producing this visible “jump” at 1600 grams in Figure 2. Reassuringly, the “jump” at 1500 grams is also visible in the graph which radiates one-ounce bins from 1600 grams, suggesting that small-sample variation is not driving the visible discontinuity at 1500 grams. Finally, when we estimate a discontinuity in a formal regression framework at 1600 grams we find no evidence of either a first stage or a reduced form effect at 1600 grams.

<sup>27</sup>A probit model predicts a difference of -0.0095 in a model with no controls other than the trend terms (evaluated at the cutoff), and -0.0065 in a model with full controls evaluated at the cutoff and the mean of the other control variables.

<sup>28</sup>For example, our OLS estimate of -0.0095 (*s.e.*=0.0022) for one-year mortality has a standard error of 0.0048

The gram-trend terms reflect the overall downward slope in mortality. The point estimates suggest a steeper slope after the threshold, but the difference is not statistically significant.

In terms of covariates, the largest impact on our main coefficient of interest is found when we introduce year indicators, likely because medical treatments and associated survival rates have changed so much over time. The estimated change in mortality around the threshold in the specification with the year indicators decreases to -0.0076. Again, this remains a large effect, and we consider heterogeneity across time periods below. When we include the full set of covariates, the results are largely unchanged.<sup>29</sup> To be conservative, in the rest of our analysis, we always report a specification that includes the full set of covariates.

The remaining outcomes in Table 1 are mortality measures at shorter time intervals. The 28-day mortality coefficient is similar in magnitude to the one-year mortality coefficient, despite a smaller mean mortality rate of .0383%. Given different mean mortality rates, the estimate implies a 23% reduction in 28-day mortality as compared to a 17% reduction in one-year mortality. As discussed above, the similarity between the one year and 28-day mortality rates implies that any effects of being categorized as very-low birth weight are seen in the first month of life - a time when these infants are largely receiving medical care (as described more below in our length of stay results). Similarly, 7-day and 24-hour mortality rates are lower for those who are very-low birth weight compared to those just above the threshold, with differences of between 16% and 19% compared to the mean mortality rate for infants above the threshold. Finally, 1-hour mortality rates are also higher for those born just above the threshold, as well.<sup>30</sup>

The following two subsections consider the extent to which newborns classified as very low birth weight receive discontinuously more medical treatments relative to newborns just above 1500 grams. While the universe of births in the natality file allows us to consider mortality effects with a large sample, these data do not include summary measures of treatment. As described above, we are able to examine summary measures of treatment in our hospital discharge data

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when we cluster at the gram level.

<sup>29</sup>The estimated coefficients on many of these covariates as well as one of the year indicators are reported in Appendix Table A1. Characteristics that are associated with lower mortality include prenatal visits, mother's age, and African American. Higher mortality is associated with mother's first birth, males, longer gestational age (where greater age in this bandwidth suggests that the newborn is particularly small for gestational age), and singleton births where the low birth weight in this bandwidth is not explained by the presence of multiple births. We only show a sub-set of the coefficients on these covariates in order to keep the table to one page

<sup>30</sup>In a probit model with full controls, the main marginal effect of interest, evaluated at the cutoff and the mean of the other control variables, is -0.0012 (s.e.=0.0005) compared to a mean 1-hour mortality rate of 0.0055 just above the threshold.

from five states (Arizona, California, Maryland, New Jersey, and New York). These states appear to have broadly representative mortality outcomes. When we estimate our mortality results separately within each state and rank them by the estimated coefficient scaled by mean mortality just above the threshold, each of the states in our 5-state sample falls toward the middle of the distribution.<sup>31</sup>

Our mortality results are similar when limit our nationwide data to only these five states. On this sample of nearly 50,000 births, we estimate that mortality falls by 1.2 percentage points (*s.e.*=0.42) compared to a mean of 5.4% (as reported in Table 4).<sup>32</sup>

### C. Differences in summary measures of treatment

Figure 3A reports mean hospital charges in one-ounce bins. The measure appears fairly flat at \$94,000 for the three ounces prior to the threshold, then falls discontinuously to \$85,000 after the threshold and continues on a downward trend, consistent with fewer problems among relatively heavier newborns. As the large mean charges suggest, this measure is right skewed. The results are similar, however, when we estimate the relationship using median comparisons and when the charges are transformed by the natural logarithm to place less weight on large charge amounts, as described below.<sup>33</sup>

As noted above, if prices differ across our threshold of interest, then any discontinuous jump in charges could in part be due to changes in prices rather than changes in quantities. One way to test whether differences in quantities of care are driving the main results is to consider a quantity measure that is consistently measured across hospitals: length of stay in the hospital. Figure 3B shows that average length of stay drops from just over 27.3 days immediately prior to the threshold to 25.7 days immediately after the threshold. Of course, length of stay and charges

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<sup>31</sup>Specifically, we estimate separate models with no control variables in all available years in the nationwide data in each of the 50 states and the District of Columbia. With the most negative value of the estimated coefficient scaled by mean mortality just above the threshold ranked as 1, Arizona ranks 25 (-0.0101/.0618), California ranks 21 (-.0185/.0682), Maryland ranks 19 (-.0148/.0511), New Jersey ranks 16 (-.0153/.0423), and New York ranks 15 (-.0208/.0569). New Jersey has the lowest mean mortality below the threshold, but Arizona, California, New Jersey, and New York are more to the middle of the distribution, ranking 26, 30, 7, and 13 respectively.

<sup>32</sup>The mortality outcomes were also considered in these five states from 1991-2002 - the overlap of the years between the state data and the nationwide linked birth-death certificate data. As expected, the results are imprecisely estimated with the smaller sample, and the point estimates are lower as well.

<sup>33</sup>Note that our sample sizes vary somewhat when looking at charges variables in levels or in logs. In levels, we are missing data on charges for some observations which are births at hospitals which do not charge for inpatient care but rather charge based on monthly capitated payment schedules (such as Kaiser hospitals in California). In logs, we also lose observations with zero charges, which occur for births at hospitals which do not charge for inpatient care (such as Shriners hospitals in California).

are not independent measures, as longer stays accrue higher charges both in terms of room charges and are associated with a greater number of services provided. We further investigate the differences in these measures below.

As in the reduced-form results for infant mortality, we compare treatment measures above and below the threshold using a local-linear regression and a bandwidth of 3 ounces (85 grams). In addition, we estimate OLS models following equation 3, where the outcome,  $Y$ , now represents hospital charges or length of stay. Figure 2 shows that the linear trend controls within 3 ounces above and below the threshold appear reasonable. Again, results are similar when we estimate alternative models, such as count models for length of stay.<sup>34</sup>

Table 2 reports the regression results. The first column reports estimates from the local linear regression and hospital charges are \$9,450 higher just before the threshold. This is relatively large compared to the mean charges of \$82,000 above the threshold. The remaining columns report the OLS results. The first column mimics the local linear regression but now places equal weight on the observations up to 3 ounces on either side of the threshold. Without controls, the estimate decreases somewhat to \$9,021; with full controls the estimated increase in charges for infants categorized as very low birth weight is largely unchanged (\$9065, s.e.=\$2,297). These estimates imply a difference of approximately 11% compared to the charges accrued by infants above the threshold.

We report the coefficients on the controls in Appendix Table A1. There are fewer controls in the 5-state sample than there are in the nationwide sample, as the discharge data do not include the birth certificate data. Results are qualitatively similar in a separate analysis of California, which allows for a wider set of controls from the linked birth-certificate data. The coefficients suggest that preterm births, multiple births, and c-section births accrue higher charges.

In related models of length of stay, we find that newborns weighing just under 1500 grams have stay lengths that are between 1.5 and 2 days longer, depending on the model.<sup>35</sup> This represents a difference of 6-8% compared to the mean length of stay of 25 days above the threshold. In California, when the length of stay from a transfer to another hospital is included

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<sup>34</sup>The estimated incidence-rate ratio for the indicator of being just below the threshold is 1.076 (s.e.=0.018) in a model with year indicators and 1.068 (s.e.=0.18) in a model with full controls.

<sup>35</sup>Note that we replace our length of stay variable with "length of stay plus one" so as to include observations in our log length of stay variable that are less than one day.

as well, the estimated difference is closer to 11%.<sup>36</sup>

To ensure that our results are not driven by a small number of newborns with large charge amounts, we investigated median differences in charges, and the natural logarithm of charges, which places less weight on large amounts and greater weight on smaller ones. Appendix Figure A2 reports these measures separately for the five-state sample and California - a state where longitudinal data allows calculations of treatment measures across hospitals if the newborn were transferred. In particular, the relationship is similar to the mean comparisons when median charges are considered: the ounce just below the threshold has median charges of \$68,000 and the ounce just after the threshold has median charges of \$59,000: \$9,000 or 15% of median charges above the threshold. Further, Table 4 shows results from a median regression with full controls, and median charges are found to be \$9,415 lower just after the threshold.

Figure A2B reports the means for log charges, which are relatively noisy around 10.6 log points from 5 ounces above the threshold, with an increase just prior to the threshold. This upward slope is largely driven by newborns with few charges.<sup>37</sup> Further, when data from hospitals where newborns were transferred are included using the longitudinal data from California (Appendix Figure A2D), log charges are relatively flat at 11.17 in the one-ounce bins just before the threshold and drop to 11.00 after the threshold. Appendix Figures A2E and A2F report similar estimates to the main results when median log charges are compared in both the 5-state sample and in the California sample (differences of 14 and 12 log points, respectively.)

Table 4 reports results when the dependent variable is log charges. When we impose a linear trend in grams, charges are found to be 28 log points higher just before the threshold. If we do not extrapolate using a linear trend, as the upward trend prior to the threshold within our bandwidth likely reflects noise rather than a true increase, then a difference between 10.77 log points just prior to the threshold and a return to 10.6 just after the threshold would suggest an alternative estimate of 17 log points. Further, when we use the longitudinal data from

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<sup>36</sup>Note that our first stage variables could be truncated in the sense that newborns just above 1500 grams have higher mortality rates (as shown above) and thus may mechanically receive less medical treatment. This effect would make our first stage effect look less conservative, but would make the cost per year of life saved estimates we derive below more conservative (since larger first stage effects will make our marginal return estimates look less cost effective).

<sup>37</sup>Specifically, accrued charges less than \$3,000 against birth weight revealed a fairly noisy series. An OLS model with the standard controls suggests that such low charge amounts are less likely for newborns just below the threshold, with an estimated coefficient on birth weight less than 1500g of -0.042 (s.e. = 0.0065), compared to a mean above the threshold of 0.093.

California to calculate total charges for newborn treatment including charges associated with hospital transfers, an upward trend prior to the threshold is not present, and the estimate is 22 log points (Table 4). Meanwhile, when log length of stay is the dependent variable, newborns weighing just below the threshold are found to stay 14 log points fewer days in care, and 11 log points when length of stay including transfers is compared in the California data.

An alternative measure to hospital charges is a measure of hospital costs.<sup>38</sup> This measure is known to introduce noise into the results, as the hospital-level cost to charge ratios are a rough measure of the difference between costs and charges and the ratios are volatile over time within hospitals. That said, the estimated cost is closer to the marginal cost of interest. While these costs may not represent social costs for such care—the nurses, physicians, and capital expenditures may not be affected by the births of a small number of very low birth weight infants - they represent our best summary measurement of the difference in treatment that VLBW classification affords. For this reason, we use costs rather than charges in our interpretation of the social costs of medical treatment for these newborns, as in Section VIII below.

The figures for hospital costs in levels and in logs are similar to the main results, with a trend that is somewhat more steep after the threshold when levels are considered. As reported in Table 4, the cost measure is \$4,200 higher just before the threshold in a model without controls, and \$3,800 higher in a model with controls, compared to an average cost of nearly \$40,000 just above the threshold, or approximately 10%. A median regression suggests a difference of \$4,800 compared to a median level of \$30,100 within 3 ounces above the threshold: a 16% difference.

In summary, we find differences in summary treatment measures of approximately 10-15% with some variation in the estimate depending on the treatment measure. In terms of charges, the difference across the discontinuity is approximately \$9,500. When we deflate charges by a cost-to-charge ratio, this difference is closer to \$4,000 when means are compared and \$5,000 when we compare median costs.

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<sup>38</sup>The Centers for Medicare and Medicaid Services (CMS) report cost-to-charge ratios for each hospital in each year beginning in 1996 and the data are available through 2005. Our charge data are available from 1991-2006 (see Appendix Table A3). To include the information from all of these years, the 2000 cost-to-charge ratios were used to deflate charges in all states but New York where the first year of data is 2001 and the 2001 cost-to-charge ratio is used. Further, we followed a CMS suggestion to replace the hospital's cost-to-charge ratio with the state median if the cost-to-charge ratio is beyond the 5th or 95th percentile of the state's distribution. Results were similar, though noisier, when the sample was restricted to 1996-2005 and each hospital-year cost-to-charge ratio was employed.

#### D. Mechanisms: Differences in types of care

The discharge data include procedure codes that can be used to investigate the types of care that differ for infants on either side of the very low birth weight threshold. We explore the data for such differences, with a special focus on common perinatal procedures. Like the mortality differences, however, the smaller 5-state sample tends to make such differences difficult to find. Table 3 and Figure 4 present differences for measures of common procedures.

One of the most common procedures is some form of ventilation.<sup>39</sup> Figure 4A does not offer compelling evidence of a meaningful difference in ventilation by birth weight, however. Further, the nationwide data include ventilation measures, so sample size considerations do not play a role, but in those data we still find little evidence of an increase in ventilation among these newborns.

Another common measure of resource utilization that *a priori* seems to be a likely source of the differences we observe in our summary treatment measures is admission to a neonatal intensive care unit (NICU). Since care provided in such units is very costly, it seems plausible that the threshold could be used to gain entry into such a unit. We find little difference on this margin in our data, however. First, we examine the California data, which includes a variable on whether or not the infant spent at least 24 hours in a NICU or died in the NICU in less than 24 hours. We include newborns born in hospitals that did not have a NICU for comparability to our main results, which also include such newborns. Table 3 suggests a modest increase in NICU use (approximately 3 percentage points as compared to a mean just above the threshold of 44 percentage points), but a graph of the measure across birth weight ounces shows little evidence of a discontinuous change. Second, we examine the Maryland HCUP data, which records the number of days in the NICU, but again we find little evidence of a difference at the threshold.<sup>40</sup>

Perhaps the most compelling differences we find are for two relatively common procedures: diagnostic ultrasound of the infant and operations on the heart. As noted above, diagnostic

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<sup>39</sup>We observe several measures of assisted ventilation, including continuous positive airway pressure (CPAP) ventilation, a procedure which can be thought of as less serious than a traditional ventilator; intubation, which involves an endotracheal breathing tube being inserted through a newborn's mouth into her lungs; and several other measures of ventilator use. Some oxygen may be provided before birth weight is measured, although to the best of our knowledge we are not able to separate this from ventilation provided after birth weight is measured in our data. Within these subcategories, we found little support for any discontinuous change.

<sup>40</sup>New Jersey data include a field for NICU charges, but this variable proves unreliable: the fraction of newborns with non-missing NICU charges for this at-risk population is only 2%. Recent nationwide birth certificate data include an indicator for NICU admission for a handful of states. We do not see a visible discontinuity in these data, albeit potentially due to the small sample of births in the years for which we observe this variable.



ultrasounds are used to check for bleeding or swelling of the brain and some physician manuals cite 1500 grams as a threshold below which diagnostic ultrasounds are suggested. Figure 4C suggests a jump in ultrasounds of roughly 2 percentage points compared to a mean of approximately 25%. Table 3 suggests a similar estimate of between 2 and 3 percentage points.

The pattern of the “operations on the heart” indicator shows an upward pre-trend in the procedure prior to the threshold and what appears to be a discontinuous drop after the threshold. Table 3 suggests that the jump is between 1.5 and 2.4 percentage points, or roughly 8% higher than the mean rate for those born above the threshold in this sample. If we do not extrapolate the pre-trend, Figure A2 shows a cardiac surgery rate of 30.0% prior to the threshold and 26.7% after the threshold, a difference of 3.3 percentage points.

In summary, we examine several possible treatment mechanisms at the discontinuity. The strongest evidence that we find is for operations on the heart and diagnostic ultrasounds, for which we estimate an approximate 10% increase in usage just prior to the VLBW threshold. We show that these differences are statistically significant when the usual heteroskedastic-robust standard errors are used for comparability to the main results, but these standard errors could be corrected to account for the search across procedures. When we apply a bonferroni correction for the different types of procedures, our estimates remain statistically significant.<sup>41</sup> We find little evidence of differences in NICU usage or other common procedures such as ventilation, however, including an examination of ventilation in the nationwide data with much larger sample sizes.

## VI. ROBUSTNESS & SPECIFICATION CHECKS

In this section, we test for evidence of differences in covariates across our VLBW threshold (sub-section A), discuss potential sample selection issues (sub-section B), discuss the sensitivity of our results to alternative bandwidths (sub-section C), and examine our mortality results by cause of death (sub-section D).

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<sup>41</sup>We searched for differences among 2 summary measures of cardiac care, 7 different measures of ventilation procedures, and diagnostic ultrasound. The p-values on diagnostic ultrasound and cardiac care using the reported standard errors are 0.002 and 0.018, respectively. Multiplying these p-values by 10 implies that the estimates are significant at the 2% and 18% levels; multiplying these p-values by 3 for the 3 categories of exploration, would imply that the estimates are significant at the 1% and 4% levels.

## A. Testing for evidence of differences in covariates across 1500 grams

As discussed above, it is thought that birth weight cannot be predicted in advance of birth with the accuracy needed to change (via birth timing) the classification of a baby from being just above 1500 grams to being just below 1500 grams. As such, we expect that the newborns will be similar above and below the threshold in both observable and unobservable characteristics. Moreover, any strategic recategorization of newborns based on birth weight around 1500 grams should be detectable in our histograms of birth frequencies by gram of birth weight (as discussed above). That said, it is still of interest to directly compare births on either side of our threshold based on observable characteristics.

Table A2 compares means of observable characteristics above and below the threshold, controlling for linear trends in grams from the threshold, as in the main analysis. A summary measure is simply the predicted mortality rate from a probit model of mortality on all of the controls. Most of the comparisons show similar levels across the threshold, with few that appear to be meaningfully different. Given the large sample size, however, some of the differences are statistically significant.

To further consider these differences, Figure 5 compares covariates of interest in the 5 ounces around the VLBW threshold.<sup>42</sup> Here, the comparisons appear even more stable across the threshold. In particular, gestational age, which is particularly related to birth weight, is generally smooth through the threshold. Prenatal visit rates appear different in the linear-trend specifications in Table A2, but Figure 5J reveals little difference. While Table A2 suggests a statistically significant difference in predicted mortality, Figure 5, which is in the same scale as actual mortality Figure 2, suggests little difference across the threshold. It appears that newborns are nearly identical regardless of whether they weighed in at a level just below or just above the VLBW threshold.

## B. Sample selection

One possible source of sample selection is the possibility that very sick infants are discontinuously reported more frequently as fetal deaths across our cutoffs of interest. Although we would expect such sample selection to be apparent in the births histogram we examined above, we can also test

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<sup>42</sup>The list was selected for ease of presentation and includes the major covariates of interest. Similar results were found for additional covariates as well.

for such sample selection directly using data on fetal death reports from the National Center for Health Statistics (NCHS) perinatal mortality data for 1995 to 2002. There are two types of fetal death classifications in these data: greater than or equal to 20 gestational weeks, and less than 20 gestational weeks. In practice, the frequency of fetal deaths for less than 20 gestational weeks is negligible for all birth weights above around 1000 grams, and thus near our discontinuities only the greater than or equal to 20 gestational week fetal deaths are relevant. As in the births data, there are reporting spikes for fetal deaths at ounce-equivalent gram intervals, but graphical analyses of these data does not suggest any spikes in fetal death reporting that are discontinuous across our cutoffs of interest.

Another possible source of sample selection in the 5-state sample is the possibility that newborns just below the cutoff were transferred to another hospital. Our results are similar when such transfers are included in the treatment measures in California, however, as shown in Table 4.<sup>43</sup> Further, those born just below the threshold are slightly less likely to experience a transfer to another hospital (Table 4). This difference in transfers would tend to understate the treatment of newborns just below the threshold and bias the results away from finding an increase in care for these infants.

### C. Bandwidth sensitivity

The local-linear regression results are qualitatively similar for a wide range of bandwidths (see Table 6). The magnitude of the mortality estimates decreases with the bandwidth, suggesting that our relatively large bandwidth is conservative. When the bandwidth includes only one ounce on either side of the threshold ( $h = 30$  grams), the difference in 1-year mortality is -2.7%; when  $h = 150$  grams, the estimate decreases to -0.8%, which is similar to our main results.

In terms of the treatment measures in the 5-state sample, the hospital charges are estimated to decrease by approximately \$8,000 when 1 ounce is used as the bandwidth and \$6500 when 150 grams is used, compared to our estimate of just over \$9,000. When hospital costs or length of stay are examined, our estimates are smaller than the 1-ounce bandwidth: a similar \$4,300 for hospital costs and 2.4 days. When  $h = 150$  grams, the cost difference is estimated to be

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<sup>43</sup>When the dependent variable is length of stay including transfers, the main coefficient of interest is 1.36 (s.e.=0.561) in a model with year indicators and 1.24 (s.e.=0.553) in a model with full controls, compared to a mean of 27.0 for those born just above the threshold.

\$2600 and 1.14 days, respectively. Meanwhile, when log charges are considered (not shown), the estimates decrease as the bandwidth increases.

#### D. Causes of death

For several reasons, it is of interest to ask which causes of death appear to account for our observed mortality effect. First, if our mortality effect appeared to be driven by so-called “external” causes of death (such as accidents), this would be concerning since it would be difficult to link deaths from those causes to differences in medical inputs. Second, from a policy perspective, it is of substantive interest to know which causes of death appear to be driving our results.

On the first point, a graphical examination of trends in “external” causes of death as a share of all deaths across our discontinuities does not suggest any spikes in external deaths that are discontinuous across our cutoffs of interest. Formalizing this in our regression framework, we find no statistically significant change in external deaths across our cutoffs of interest (see Appendix Table A4).

On the second point, we examine cause of death for newborns around 1500 grams as follows. We group causes of death into broad, mutually exclusive categories that are consistent over time based on the 9th and 10th revisions of the ICD cause of death classes.<sup>44</sup> Graphically, a visually discernable jump is primarily noticeable for deaths due to perinatal conditions (such as jaundice and respiratory distress syndrome) and, to a lesser extent, for deaths due to nervous system and sense organ disorders. These results support the notion that differences in care received in the hospital are likely driving the main results, as these conditions are known to be relatively common neonatal infections. Formalized regression estimates (see Appendix Table A4) confirm an economically and modestly statistically significant jump in perinatal conditions, as well as for nervous system and sense organ disorders.<sup>45</sup>

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<sup>44</sup>In particular, the 9th-revision 61 infant cause of death and 10th-revision 130 infant cause of death recode groups were used to create the following categories: (1) infectious and parasitic diseases (such as meningococcal diseases); (2) neoplasms (that is, cancers); (3) endocrine, nutritional, metabolic, immunity, and blood disorders (such as cystic fibrosis); (4) nervous system and sense organ disorders (such as meningitis); (5) respiratory system disorders (such as pneumonia); (6) digestive system disorders (such as gastritis); (7) congenital anomalies (such as congenital malformations of the heart); (8) perinatal conditions (such as jaundice); (9) symptoms, signs, and ill-defined conditions (such as SIDS, or sudden infant death syndrome); (10) other causes of death (such as external deaths); and (11) missing cause of death codes (a very small share of deaths).

<sup>45</sup>The nervous system and sense organ disorder category includes meningitis, which is a common form of morbidity and mortality among newborns - particularly in the first month of life (Levene *et al.*, 2008).

We also examine a few individual causes of death - namely, respiratory distress syndrome (RDS), sudden infant death syndrome (SIDS), and jaundice. We find no visible discontinuities for RDS or SIDS (nor any statistically significant regression estimates). For jaundice, the graphical results are noisy but consistent with an increase in deaths due to jaundice above 1500 grams, a result confirmed in regression estimates which find a modestly statistically significant jump in jaundice deaths when moving from just below to just above 1500 grams.<sup>46</sup>

## VII. VARIATION IN TREATMENT EFFECTS

Several potential sources of heterogeneity in our estimated treatment effects are of interest. In this section, we discuss over time (sub-section A) and across hospitals (sub-section B).

### A. Time period & technology changes

It is possible that there is heterogeneity in outcomes and treatment across time - due, for example, to technological changes in medical treatments for at-risk newborns over the time period we examine. For example, one major technological innovation occurring during our sample is the development of artificial surfactant. Respiratory problems in newborns are frequently caused by insufficient pulmonary surfactant, a “wetting” protein on the surface of the alveoli (Beers, 2003). Surfactant helps to keep the alveoli open, and a lack of surfactant is often referred to as respiratory distress syndrome (RDS). The use of artificial surfactant began in the early 1990s, but unfortunately we do not observe the use of surfactant in our primary datasets. However, even without data on surfactant use we can still examine differences in our first stage and reduced form results across different time periods to test for general evidence of a “surfactant” effect.<sup>47</sup>

Table 4 reports 1-year mortality results for four time periods available in our data: 1983-1987; 1988-1991; 1995-1998; and 1999-2002. In general, we find a reduction in mortality associated

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<sup>46</sup>Neonatal jaundice is a very common problem among newborns (Levene *et al.*, 2008).

<sup>47</sup>The more recent birth certificate data referenced above include an indicator for the use of artificial surfactant. We do not see a visible discontinuity in this variable, again potentially due to the small sample of births. These data also include an indicator for steroid medication administered to mothers prior to birth to help their newborns’ lungs produce more surfactant and mature more quickly; although ideally we would use this indicator as the basis for a placebo test (since the medication is provided prior to the measurement of birth weight), the fact that we did not observe a visible discontinuity on this variable may again be due to the small sample of births for which we observe this variable.

with VLBW status in each period, even though average mortality just above the threshold declines from 8.13% in the first period to 3.78% in the last period. The estimates suggest an 18%, 12%, 2%, and 18% lower mortality rate compared to the mean rate for infants born within 3 ounces above the threshold. In general, these trends over time are not consistent with a “surfactant” story or any other clear medical technology story of which we are aware.

The five-state discharge data also allow us to consider changes in our first stage over time. We again divide the sample into four periods, in part to coincide with the time periods studied in the nationwide data: 1991-1994, 1995-1998, 1999-2002, and 2003-2006. Here, hospital charges appear to be greater for those born just below the threshold in all time periods, with a smaller difference in the 2003-2006 period.<sup>48</sup> In general, this suggests that both our first stage and reduced form results are relatively unchanged over time.

## B. Hospital types

We also consider potential heterogeneity in outcomes and treatment across hospitals, which our regression discontinuity design allows us an opportunity to rigorously address. In contexts without a regression discontinuity, comparisons of births in higher quality hospitals to births in lower quality hospitals could be biased: on one hand, a positive correlation could arise if healthier mothers choose to give births at better hospitals; on the other hand, a negative correlation could arise if riskier mothers choose to give birth at better hospitals, knowing that their infant will need more care than an average newborn. However, as discussed above, because birth weight should not be able to be predicted in advance of birth with the accuracy needed to move a birth from just above to just below our 1500 gram threshold of interest, selection should not be *differential* across our discontinuity - implying we will have internally valid estimates of how the effect of being born in a given type of hospital changes across our cutoffs of interest.

One natural grouping of hospitals, given our population under study, is by the level of neonatal care available in an infant’s hospital of birth. For our California data, classifications of neonatal care availability by hospital by year are available over our time period based on the work of Phibbs *et al.* (2007).<sup>49</sup> In the sample of newborns within our bandwidth, 10% of births

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<sup>48</sup>Part of this difference arises because the states in our sample change with time because of data availability (see Appendix Table A3) - the last period does not include California, for example.

<sup>49</sup>We are grateful to Ciaran Phibbs for sharing this data with us. Phibbs *et al.* (2007) used the same California data we study to identify the quality level of NICUs (Levels 1 to 3D) by hospital by year, based on NICU

occur at hospitals with no NICU, 1.2% at hospitals with a Level 0 NICU, 0.04% at hospitals with a Level 1 NICU, 11.3% at hospitals with a Level 2 NICU, 10.9% at hospitals with a Level 3A NICU, 28.7% at hospitals with a Level 3B NICU, 21.6% at hospitals with a Level 3C NICU, and 16.2% at hospitals with a Level 3D NICU. Because of the low number of births we observe in Level 0 or Level 1 NICUs, we create a combined category for births in Level 0, 1, and 2 NICU hospitals.

While we can examine our reduced form estimates by NICU quality level (no NICU, and levels 0, 1, 2, 3A, 3B, 3C, and 3D), it is worth noting that we expect to lack sufficient sample size within these NICU quality level sub-samples to give clean estimates of these effects for our one-year mortality outcome.<sup>50</sup> Regression estimates which interact our regression discontinuity variable as well as our linear birth weight trends with indicators for the NICU quality level available in a newborn's hospital of birth generally do not give statistically significant estimates for our one-year mortality outcome, with the exception of Level 0/1/2 NICU hospitals - for which we estimate a negative, statistically significant coefficient.<sup>51</sup> Qualitatively, the mortality coefficients are negative for non-NICU hospitals as well as Level 0/1/2 and Level 3D NICU hospitals (although the magnitude of the coefficient for Level 3D NICU hospitals is orders of magnitude smaller than the other two coefficients), whereas the mortality coefficients for other hospitals are positive (again, not statistically significant).

Using charges as a first stage outcome in the same regression framework, we estimate economically and statistically significant positive coefficients for non-NICU hospitals as well as Level 0/1/2 and Level 3B hospitals; coefficients for the other hospitals do not produce statistically significant coefficients.<sup>52</sup>

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quality definitions from the American Academy of Pediatrics (definitions primarily based on whether hospitals offer specific types of procedures, such as specific types of ventilation and surgery).

<sup>50</sup>In the California data, we estimate a treatment coefficient of -0.0027 (s.e.=0.0068, p=0.6909).

<sup>51</sup>Specifically, we estimate the following: for non-NICU hospitals, -0.0304 (s.e.=0.0223, p=0.1735); for Level 0/1/2 NICU hospitals, -0.0436 (s.e.=0.0186, p=0.0193); for Level 3A NICU hospitals, 0.0032 (s.e.=0.0209, p=0.8773); for Level 3B NICU hospitals, 0.0141 (s.e.=0.0116, p=0.2244); for Level 3C NICU hospitals, 0.0076 (s.e.=0.0149, p=0.6099); and for Level 3D hospitals, -0.0017 (s.e.=0.0177, p=0.9224).

<sup>52</sup>Specifically, we estimate the following: for non-NICU hospitals, 15000 (s.e.=7800, p=0.0534); for Level 0/1/2 NICU hospitals, 19000 (s.e.=5300, p=0.0003); for Level 3A NICU hospitals, 10000 (s.e.=9600, p=0.2967); for Level 3B NICU hospitals, 15000 (s.e.=7100, p=0.0400); for Level 3C NICU hospitals, 453 (s.e.=11000, p=0.9678); and for Level 3D hospitals, -12000 (s.e.=9900, p=0.2306).



## VIII. ESTIMATING RETURNS TO MEDICAL SPENDING

In this section, we combine our first stage and reduced form estimates to derive two-sample estimates of the marginal returns to medical spending for newborns near 1500 grams (sub-section A). For comparability to the existing literature, we also present a Cutler & Meara (2000)-style time series estimate of the returns to large changes in spending over time for newborns in our bandwidth (sub-section B).

### A. Two-sample estimates of marginal returns to medical spending

As discussed in Section IV, we can combine our results to produce two-sample estimates of the effect of treatments on health outcomes around the VLBW threshold. To do so, we need to invoke the exclusion restriction that the VLBW designation can only affect mortality through treatments captured by our treatment measure. As discussed above, this assumption is more plausible for our summary treatment measures (charges, costs, and length of stay) than it is for our mechanism variables. We focus on costs because it is intuitive to summarize treatment in terms of dollars.

Because we examine health outcomes and summary treatments in different data sources, we require extra assumptions to combine our estimates. To be as conservative as possible in these assumptions, we can restrict our combined estimate to California, the only population for which we observe one-year mortality and costs in the same data set. (As discussed above, we do not observe one-year mortality in the other states in our 5-state sample.) In the California data, in a specification without covariates, we estimate that costs increase by \$2900 (se = 1770) as birth weight approaches the VLBW threshold from above, but we do not estimate a statistically significant change in mortality across the threshold. Thus, we do not have enough statistical power to estimate the effect of higher treatment levels on mortality using our regression discontinuity design in this sample.<sup>53</sup>

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<sup>53</sup>However, we can estimate the OLS effect of charges on mortality within our bandwidth. This estimate indicates that a ten thousand dollar increase in charges is associated with a .00080 percentage point (se = .00029) decline in mortality. (For direct comparability to our regression discontinuity estimates, we also estimate an OLS specification that includes gram trends interacted with the cutoff, and the point estimate is very similar: -.00780 (se = .0029).) Compared to mean mortality of 4.7 percent in the three ounces above 1500 grams, this estimate implies a 17% reduction in mortality. In general, we expect OLS estimates of the incremental returns to treatment to be too small because babies that are unobservably sicker have higher charges and higher mortality rates. However, by restricting our OLS estimate to such a small bandwidth, we appear to eliminate much of this bias: the OLS estimate on the California sample is very similar to the two-sample RD estimates discussed in the

For another conservative combination of estimates with more power, we can combine mortality and cost estimates based only on the states in the 5-state sample. Because we only observe one-year mortality for these states in the national data, we obtain the one-year mortality estimate on the national data, restricted so that it contains only those newborns in the 5-state sample in available years. We standardize covariates across the two samples, so that if we had the exact same babies in the two samples, our two-sample estimate would be identical to a one-sample estimate on the complete data.<sup>54</sup> Coefficients are shown in the last column of Table 5, where \$4,550 in additional costs are associated with a 0.73 percentage point reduction in mortality.

If we are willing to assume that costs differences in the 5-state sample in the available years (1991-2006) are broadly representative of what we would observe in the full national sample in available years (1983-2002), we can compare our main results: a difference of \$3,812 in costs and a one-year mortality reduction of 0.73 percentage points as birth weight approaches the VLBW threshold from above.

Equivalently, we can compute a measure of dollars per newborn life saved. In the numerator, our hospital costs estimate in the full 5-state sample implies incremental costs of \$3,812 for each VLBW baby. In the denominator, our mortality estimate in the full national sample implies that 0.73 percentage point reduction in mortality among VLBW newborns. These estimates imply that the cost per newborn life saved is \$522,191 ( $\$3,812/.0073$ ). In the 5-state sample over the years that overlap with the nationwide data, we attain a slightly larger estimate of costs per newborn life saved of \$623,699 ( $\$4,553/.0073$ ).

We can compare these estimates of the cost per newborn life saved to a variety of potential benchmarks. If we take the very conservative view that the mortality effects that we estimate do not persist beyond one year, it is relevant to compare our costs estimates to estimates of the value of one year of life from the literature, which are generally around \$100,000 (see Cutler (2004)). Based on this comparison, the interventions that we observe do not appear to be cost-effective. On the other hand, it is generally agreed that one-year mortality effects likely persist

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next paragraph.

<sup>54</sup>Specifically, we restrict the national data to the 5 states in the years 1991 and 1995-2002. Also, for comparability with the 5-state sample, we restrict the national sample to contain only in-hospital births. Because we do not have individual-level identifiers, we cannot restrict the national sample to contain the exact same babies as the 5-state sample, but the agreement is very good. The restricted national sample contains 23,698 infants, and the 5-state sample contains 21,479.

well beyond one year. In this case, we can compare our cost estimates to estimates of the value of an entire life. If our one-year mortality effects persist and but life span and quality of life are decreased, it is relevant to compare our estimates to quality-adjusted value of newborn life calibrations, such as those from Cutler & Meara (2000). Specifically, in their analysis, Cutler & Meara (2000) calibrate the value of life in 1990 for babies born at 1,000 to 2,499 grams (based on life expectancy and expected quality of life, including disabilities associated with being low birth weight) to be approximately \$2,700,000, implying our estimated expenditures are cost effective. If we take the even less conservative view that our one-year mortality effects persist and that the newborns who are saved do not experience decreases in life span or quality of life, the relevant benchmark is approximately \$3 to \$7 million dollars (Cutler, 2004). Comparison with this benchmark suggests that the treatments that we observe are very cost-effective.

#### B. Comparison to time-series estimates of returns to medical spending

As an alternative benchmark, we can also compare our marginal return estimate to the type of return estimate calculated by Cutler & Meara (2000). The spirit of the Cutler-Meara calculation is to assume that within-birth weight changes in survival over time are primarily due to improvements in medical technologies, and to thus value medical improvements by looking at changes over time in within-birth weight expenditures and health outcomes. For comparability to our marginal returns calculation, we undertake this calculation in our California data as a “long difference” in costs (in 2006 dollars) and one-year mortality from 1991 to 2002. Within our bandwidth, we estimate a \$30,000 increase in costs and a 0.0295 decline in one-year mortality over this period, which implies a return under the Cutler-Meara assumptions of \$1 million dollars. By this metric, our marginal return estimates appear to be similar or slightly smaller than time-series returns to large changes in spending for newborns in our bandwidth.

## IX. ALTERNATIVE THRESHOLDS

In this section we discuss alternative birth weight thresholds (sub-section A) as well as gestational age-based thresholds (sub-section B).

## A. Alternative birth weight thresholds

The main limitation to our analysis is that these returns are estimated at a particular point in the birth weight distribution. To the extent that we find large returns to treatment for newborns just below the 1500 gram threshold, the evidence suggests that the threshold should at least be moved to a higher birth weight where cost of saving a statistical life are closer to the value of statistical life estimates.

We can also examine other points in the birth weight distribution where differences in treatment may be expected. It should be emphasized that the presence of discontinuities at other thresholds does not invalidate our main findings at 1500 grams. Rather, discontinuities at other thresholds could allow us to estimate and trace out marginal returns for wider portions of the overall birth weight distribution.

Discussions with physicians and readings of the medical literature suggest that some other cutoffs may also be relevant. The ICD-9 codes, for example, list separate diagnosis codes (V21.30-V21.35) that depend on birth weight in grams: 0-500, 500-999, 1000-1499, 1500-1999, 2000-2500, *etc.* Cloherty & Stark (1998) report separate recommendations for care for “extremely low-birth weight infants (ELBW)” with birth weight lower than 1000 grams. Empirically, Figure 2 suggests potential increases in mortality in the one-ounce bins that incorporate the 1600 gram threshold. In addition, there may be other salient thresholds in gestational age that physicians use to determine care.

To investigate other potential thresholds, we estimate differences in the reduced-form and charges for each 100 gram interval between 1000 and 3000 grams. We use local linear regression estimates because they are less sensitive to observations far from the thresholds, and our pilot bandwidth of 3 ounces for comparability. The largest change in charges relative to the mean is found at 1500 grams (11.4% compared to the mean), with the exception of the ELBW (1000 gram) threshold, which shows a 12.6% increase just prior to the threshold compared to a mean of \$209,000. When the data were plotted, however, the discontinuity at 1500 reflects curvature and noise in the data rather than a discontinuous change.

Other notable cutoffs include: 1600, where the reduced-form suggests a mortality change, but the treatment difference has the opposite sign (-3%) but not statistically significant.; 2000, which shows a 4% increase, again not statistically significant; and -18% at 2500 grams compared

to a mean of \$7200. This result is statistically significant, but is not found when log costs are considered, and a visual inspection of the data does not suggest a meaningful discontinuity at the threshold.

In terms of mortality, with much larger sample sizes in the nationwide data, we are able to find statistically significant differences at different points in the distribution. In general, differences of 15% compared to the mean are not uncommon, but visual evidence suggests that discontinuities are not found. Rather, linear trends on either side of a given threshold are inadequate at points in the birth weight distribution other than 1500 grams, which can lead to spurious results at these other points.

## B. Gestational age and SGA analyses

As motivated by the discussion in Section IIB, we examine heterogeneity in outcomes and treatment by gestational age across the 37-week threshold. In graphical analyses using the nationwide sample, measures of average mortality by gestational week appear smooth across the 37-week threshold. Similarly, in graphical analyses using the California data, which report gestation in days, measures of average mortality, charges, and length of stay by gestational day appear smooth across the threshold. Corresponding regression results yield statistically significant coefficients of the expected sign, but, we do not emphasize them here given the lack of a visibly discernable discontinuity in the graphical analysis.<sup>55</sup>

Even though we do not observe meaningful discontinuities in outcomes or treatment at 37 gestational weeks, there is still reason to investigate the *interaction* between birth weight and gestational age through the “small for gestational age” (SGA) classification: newborns below the 10th percentile of birth weight for a given gestational age. From conversations with physicians, we have reason to believe that doctors use SGA charts such as a common one established by Fenton (2003). On this chart, 2500 grams is almost exactly the 10th percentile of birth weight for a gestational age of 37 weeks. If physicians treat based on SGA cutoffs, we expect discontinuities in outcomes and treatment at 2500 grams to be most pronounced exactly at 37 weeks and less

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<sup>55</sup>Specifically, the coefficient on an indicator variable for “below 37 gestational weeks” is -0.00070 (robust s.e.=0.0001277) in a specification that includes linear trends, run on an estimation sample of 21,562,532 observations within a 3 week bandwidth around 37 weeks. Mean mortality above the threshold is 0.0032. To address the concern that discontinuities could be obscured in cases where gestational age can be manipulated, we also estimate a specification which includes only vaginal births that are not induced or stimulated and find similar results.

pronounced at other values of gestational weeks, although we are agnostic about the pattern of decline. In regression results (not shown) in which we fully interact the low birth weight indicator and linear trends with dummies for each value of gestational week, we indeed find that the effect of the low birth weight designation is largest in magnitude at 37 weeks, and it declines as gestational weeks move away from 37 in both directions. Several of the interacted low birth weight indicators are statistically different from zero. Overall, this evidence is consistent with treatment based on SGA around 2500 grams.

We also examine a potential SGA treatment threshold around the 1500 gram discontinuity. In the Fenton (2003) chart, 1500 grams is considered SGA for babies with between 32 and 33 gestational weeks. In regression results (not shown), we see that discontinuities in mortality around 1500 grams are most pronounced at 29 weeks and decrease on either side of 29 weeks, which is not clearly consistent with treatment based on the Fenton (2003) definition of SGA around 1500 grams.

## X. CONCLUSION

In the universe of all births in the US over 20 years, we estimate that newborns weighing just below 1500 grams have substantially lower mortality rates than newborns that weigh just over 1500 grams, despite a general decline in health associated with lower birth weight. Specifically, one-year mortality falls by approximately one percentage point as birth weight crosses 1500 grams from above, which is large relative to mean one-year mortality of 5.5% just above 1500 grams. Robustness tests suggest some variation around this point estimate, although a reduction in mortality for newborns just below the threshold is generally found.

It appears that infants categorized as “very low birth weight” have a lower mortality rate because they receive additional treatment. Using all births from 5 large states that report treatment measures and birth weight—states that have a similar mortality discontinuity as the nationwide sample—we find that treatment differences are on the order of \$9,500 in hospital charges - or \$4,000 when these charges are converted into costs. While these costs may not represent social costs for such care—the nurses, physicians, and capital expenditures may not be affected by the births of a small number of very low birth weight infants - they represent our best summary measurement of the difference in treatment that VLBW classification affords.

Taken together, our estimates suggest that the cost of saving a statistical life for newborns near 1500 grams is on the order of \$500,000 to \$650,000 in 2006 dollars, suggesting that greater levels of spending for at-risk infants near 1500 grams would be expected to yield benefits that outweigh their costs.



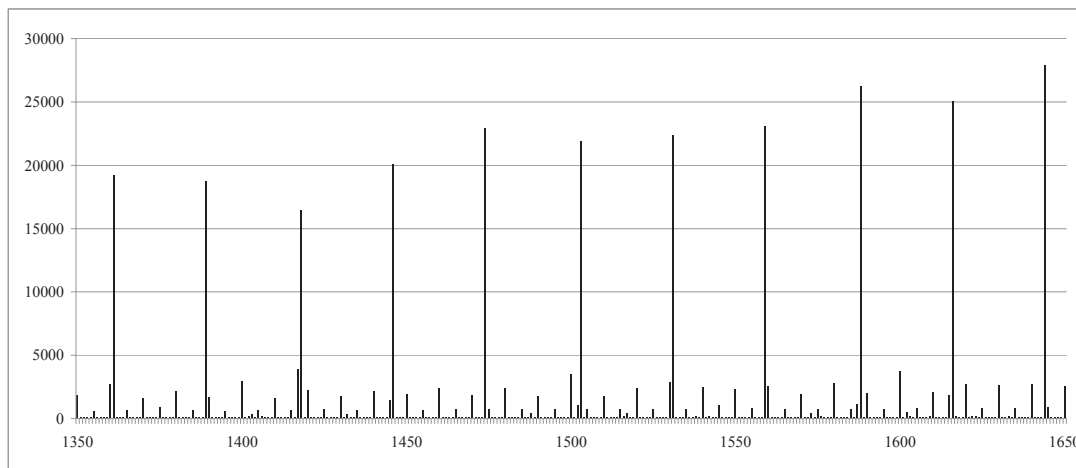
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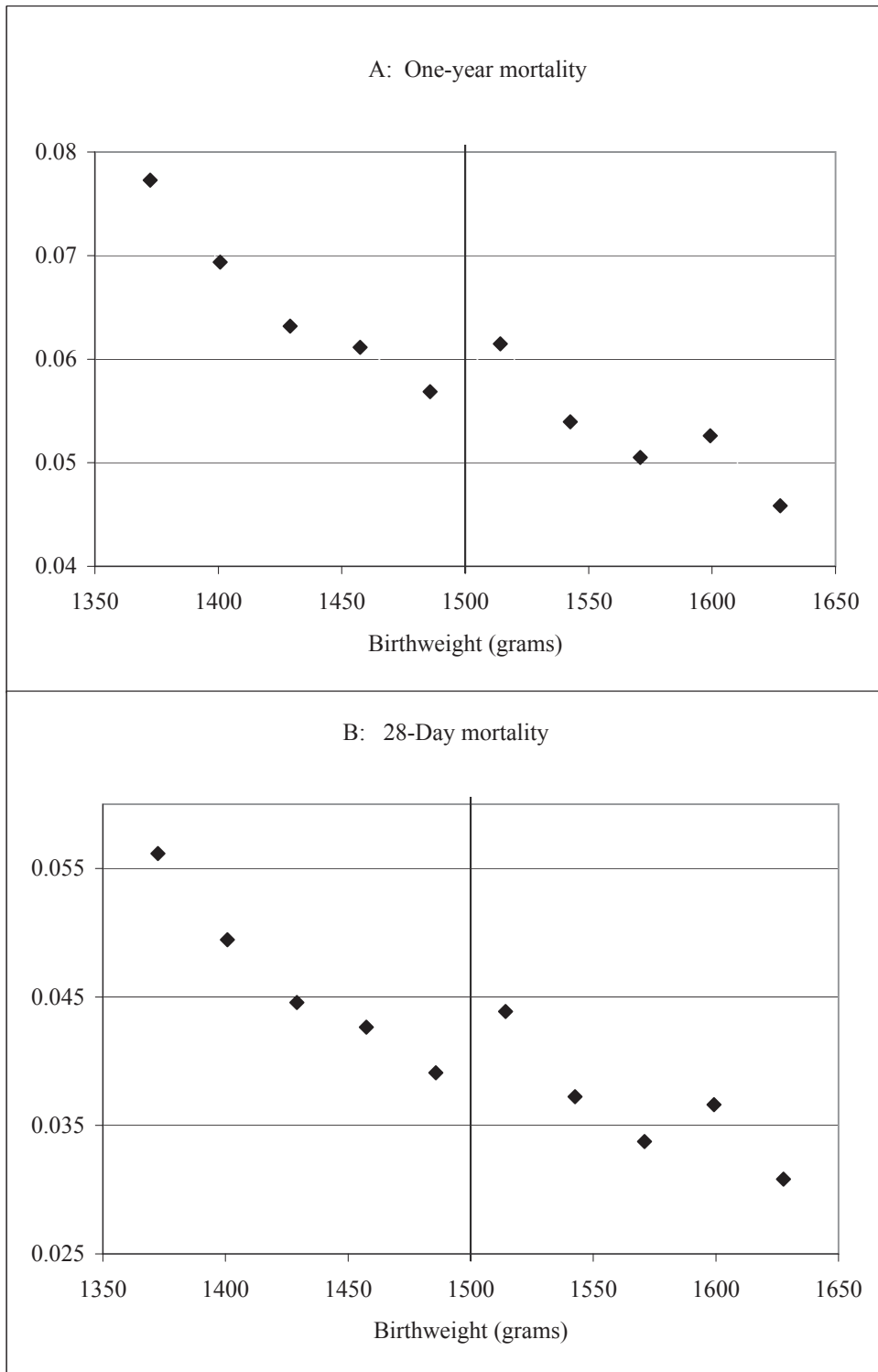
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Figure 1: Frequency of births by gram: Population of US births between 1400-1600 grams



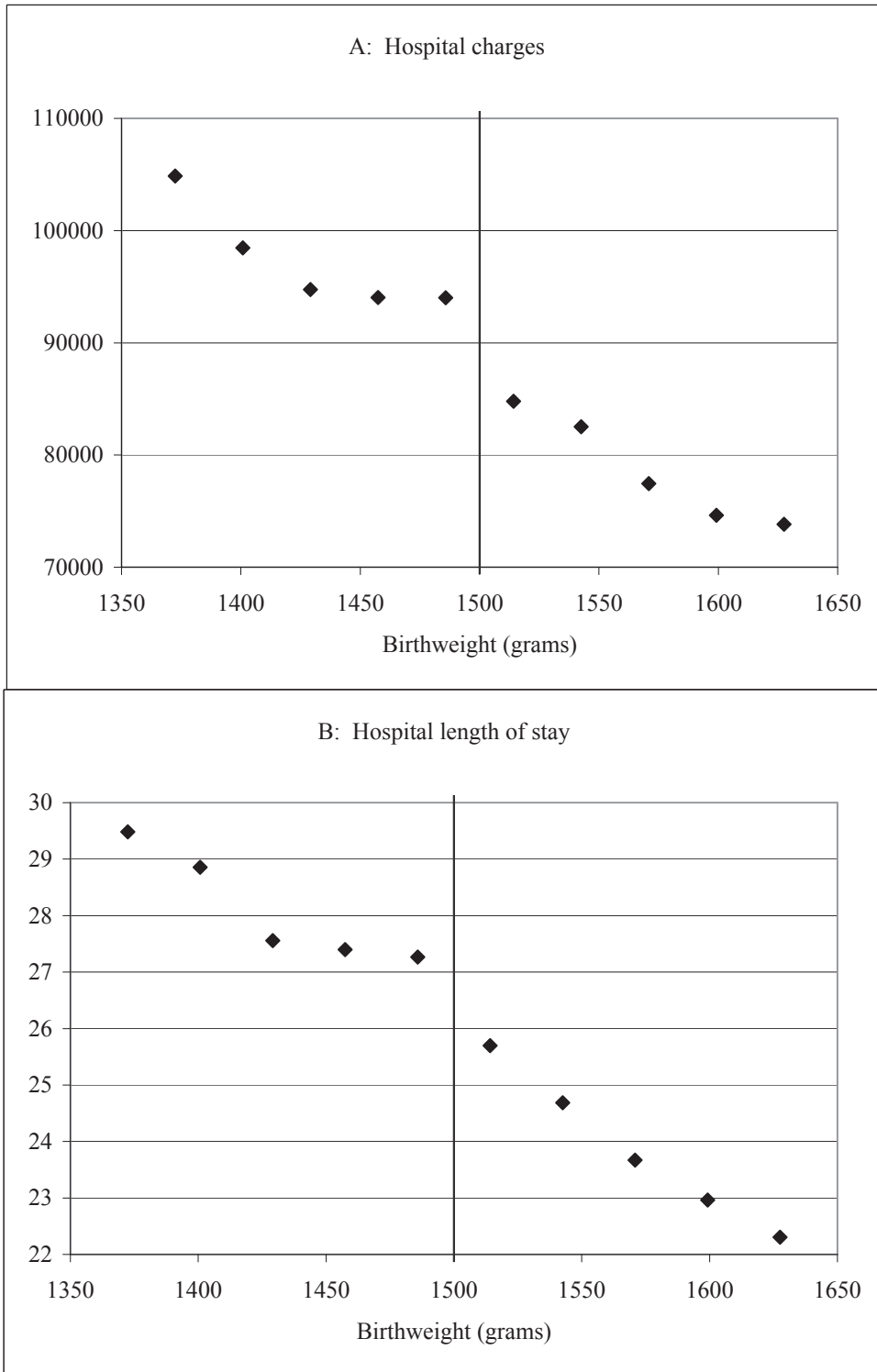
*Notes:* NCHS birth cohort linked birth/infant death files, 1983-1991 and 1995-2003, as described in the text.

Figure 2: One-year and 28-day mortality around 1500 grams



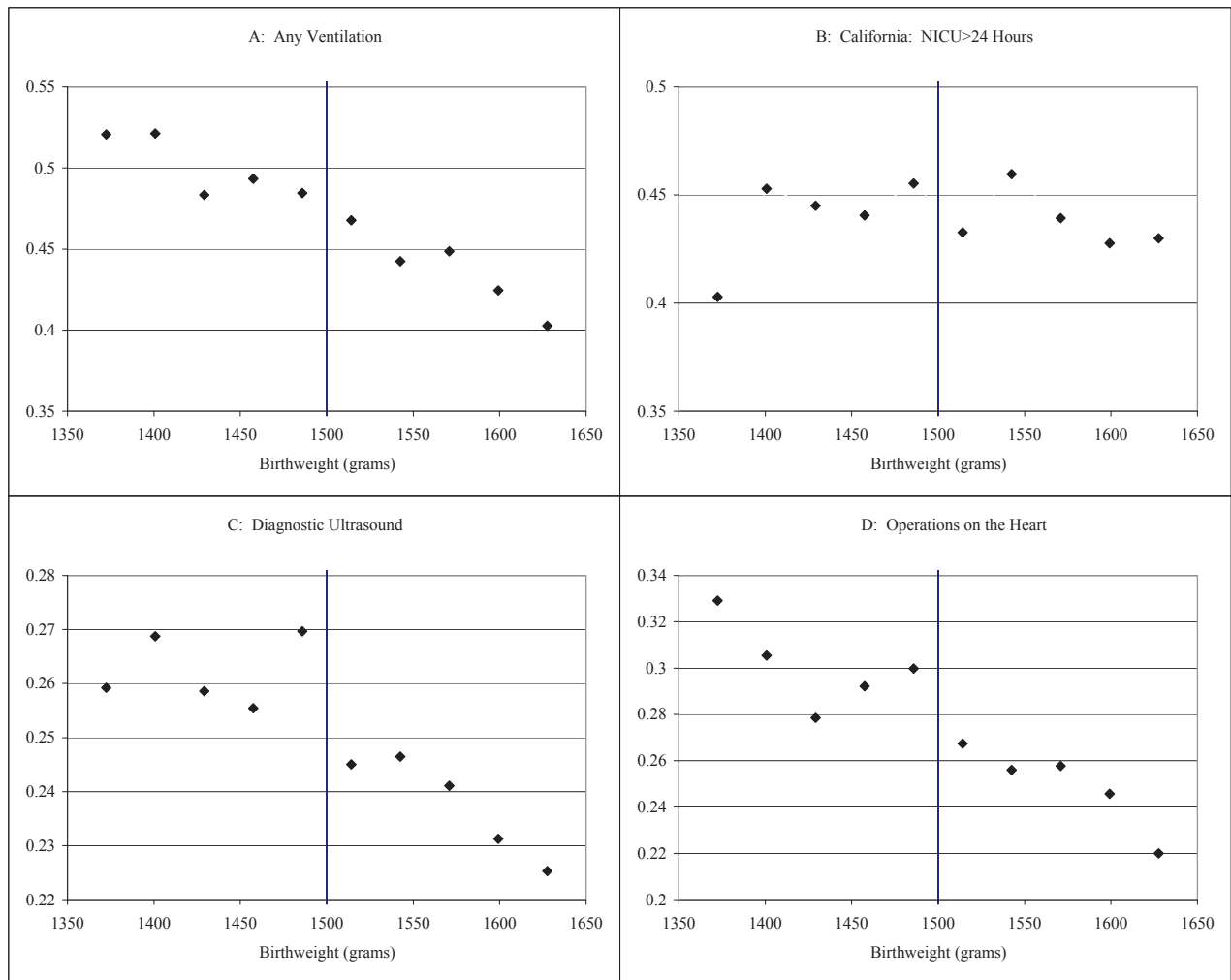
Notes: NCHS birth cohort linked birth/infant death files, 1983-1991 and 1995-2003, as described in the text. Points represent means in one-ounce bins radiating from 1500 grams.

Figure 3: Summary treatment measures around 1500 grams



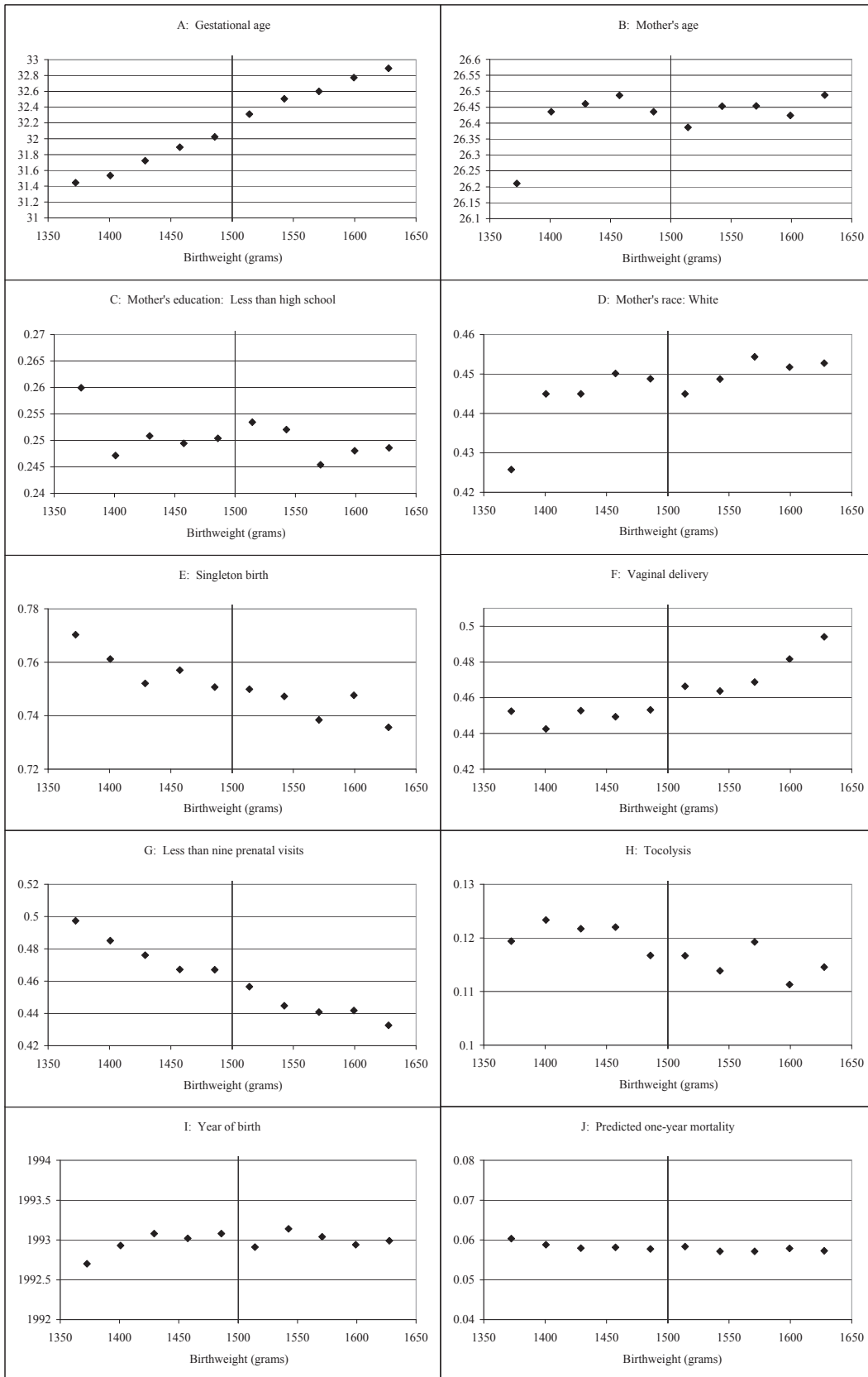
Notes: Data are all births in the 5-state sample (AZ,CA,MD,NY, and NJ), as described in the text. Charges are in 2006 dollars. Points represent means in one-ounce bins radiating from 1500 grams.

Figure 4: Specific treatment measures around 1500 grams



*Notes:* Data are all births in the 5-state sample (AZ,CA,MD,NY, and NJ), as described in the text. Charges are in 2006 dollars. Points represent means in one-ounce bins radiating from 1500 grams.

Figure 5: Covariates around 1500 grams



Notes: NCHS birth cohort linked birth/infant death files, 1983-1991 and 1995-2003. Points represent means in one-ounce bins radiating from 1500 grams.



Table 1: Infant mortality by very low birth weight status, National data, 1983-2002 (available years)

	Dependent variable: one-year mortality				28-day mortality				
	Model:	local linear	OLS	OLS	OLS	local linear	OLS	OLS	OLS
Birthweight < 1500g		-0.0121 (0.0023)**	-0.0095 (0.0022)**	-0.0076 (0.0022)**	-0.0073 (0.0022)**	-0.0107 (0.0019)**	-0.0088 (0.0018)**	-0.0074 (0.0018)**	-0.0073 (0.0018)**
Birthweight < 1500g * Grams from cutoff			-0.0001 (0.0000)**	-0.0001 (0.0000)**	-0.0001 (0.00004)**		-0.0001 (0.0000)**	-0.0001 (0.0000)**	-0.0001 (0.00003)**
Birthweight >= 1500g * Grams from cutoff			-0.0002 (0.0000)**	-0.0002 (0.0000)**	-0.0002 (0.00003)**		-0.0002 (0.0000)**	-0.0002 (0.0000)**	-0.0002 (0.00002)**
Year controls			No	Yes	Yes		No	Yes	Yes
Main controls			No	No	Yes		No	No	Yes
Mean of dependent variable above cutoff:		0.0553				0.0383			
<hr/>									
	Dependent variable: 7-day mortality				24-hour mortality				
	Model:	local linear	OLS	OLS	OLS	local linear	OLS	OLS	OLS
Birthweight < 1500g		-0.0068 (0.0017)**	-0.0060 (0.0016)**	-0.0049 (0.0016)**	-0.0048 (0.0016)**	-0.0068 (0.0017)**	-0.0043 (0.0013)**	-0.0036 (0.0013)**	-0.0035 (0.0013)**
Birthweight < 1500g * Grams from cutoff			-0.0001 (0.0000)**	-0.0001 (0.0000)**	-0.0001 (0.00002)**		-0.0001 (0.0000)**	-0.0001 (0.0000)	-0.00004 (0.00002)
Birthweight >= 1500g * Grams from cutoff			-0.0001 (0.0000)**	-0.0001 (0.0000)**	-0.0001 (0.00002)**		-0.0001 (0.0000)**	-0.0001 (0.0000)**	-0.00009 (0.00002)**
Year controls			No	Yes	Yes		No	Yes	Yes
Main controls			No	No	Yes		No	No	Yes
Mean of dependent variable above cutoff:		0.0301				0.0191			
Observations		202071							

Notes: Local linear regressions use a bandwidth of 3 ounces (85 grams). OLS models estimated on a sample within 3 ounces above and below the VLBW threshold. "Main controls" include the newborn characteristics that are available for all of the years in the nationwide data: an indicator that the mother was born outside the state where the infant was born, indicators for mother's age, education, father's age, the newborn's sex, gestational age indicators, race, and plurality, as well as state indicators. \* significant at 5% /; \*\* significant at 1%. Robust standard errors in parentheses.

Table 2: Summary treatment measures by very low birth weight status, 5-state sample, 1991-2006

Dependent variable:	hospital charges				length of stay				
	Model:	local linear	OLS	OLS	OLS	local linear	OLS	OLS	OLS
Birthweight < 1500g		9450 (2710)**	9021.92 (2,448)**	8205.34 (2,416)**	9065.17 (2,297)**	1.97 (0.451)**	1.78 (0.417)**	1.76 (0.417)**	1.46 (0.411)**
Birthweight < 1500g * Grams from cutoff			-17.30 (37.00)	-31.76 (36.47)	6.17 (34.63)		-0.0010 (0.0065)	-0.0014 (0.0065)	-0.0058 (0.0064)
Birthweight >= 1500g * Grams from cutoff			-73.30 (30.18)*	-86.84 (29.78)**	-79.51 (28.23)**		-0.0231 (0.0052)**	-0.0238 (0.0053)**	-0.0260 (0.0052)**
Year controls			No	Yes	Yes		No	Yes	Yes
Main controls			No	No	Yes		No	No	Yes
Mean of dependent variable above cutoff:		81566				24.68			
Observations		28928				30935			

*Notes:* Local linear regressions use a bandwidth of 3 ounces (85 grams). OLS models estimated on a sample within 3 ounces above and below the VLBW threshold. Five states include AZ, CA, MD, NY, and NJ (various years). "Main controls" include the newborn characteristics that are available for all of the years in the nationwide data: an indicator that the mother was born outside the state where the infant was born, indicators for mother's age, education, father's age, the newborn's sex, gestational age indicators, race, and plurality, as well as state indicators. We are missing data on charges for some observations which are births at hospitals which do not charge for inpatient care but rather charge based on monthly capitated payment schedules (such as Kaiser hospitals in California). \* significant at 5%; \*\* significant at 1%. Robust standard errors in parentheses.

Table 3: Specific treatment measures by very low birth weight status: Five-state sample, 1991-2006

Dependent variable:	Ventilation (various methods)			California: >24 hours in NICU			
	Model:	local linear	OLS	OLS	local linear	OLS	OLS
Birthweight < 1500g		0.0357 (0.0125)**	0.0380 (0.0115)**	0.0274 (0.0112)*	0.0372 (0.0170)*	0.0282 (0.0157)	0.0265 (0.0156)
Birthweight < 1500g * Grams from cutoff			0.0001 (0.0002)	0.00001 (0.0002)		0.0003 (0.0002)	0.0003 (0.0002)
Birthweight >= 1500g * Grams from cutoff			-0.0001 (0.0002)	-0.0001 (0.0001)		0.0003 (0.0002)	0.0003 (0.0002)
Year controls			No	Yes		No	Yes
Main controls			No	Yes		No	Yes
Mean of dependent variable above cutoff:		0.511			0.444		
Observations		30935			16528		

Dependent variable:	Diagnostic ultrasound of infant			Cardiac operations			
	Model:	local linear	OLS	OLS	local linear	OLS	OLS
Birthweight < 1500g		0.0196 (0.0109)	0.0166 (0.0101)	0.0297 (0.0095)**	0.0147 (0.0112)	0.0155 (0.0104)	0.0236 (0.0100)*
Birthweight < 1500g * Grams from cutoff			0.00004 (0.00015)	0.00012 (0.00014)		-0.0000 (0.0002)	0.0001 (0.0001)
Birthweight >= 1500g * Grams from cutoff			-0.00006 (0.00013)	0.00018 (0.00012)		-0.0003 (0.0001)*	-0.0002 (0.0001)
Year controls			No	Yes		No	Yes
Main controls			No	Yes		No	Yes
Mean of dependent variable above cutoff:		0.244			0.260		
Observations		30935			30935		

*Notes:* Local linear regressions use a bandwidth of 3 ounces (85 grams). OLS models estimated on a sample within 3 ounces above and below the VLBW threshold. Five states include AZ, CA, MD, NY, and NJ (various years). "Main controls" include the newborn characteristics that are available for all of the years in the nationwide data: an indicator that the mother was born outside the state where the infant was born, indicators for mother's age, education, father's age, the newborn's sex, gestational age indicators, race, and plurality, as well as state indicators. The dependent variable in the NICU models is only available in our California data, and equals one if the infant spent more than 24 hours in a NICU or died in the NICU at less than 24 hours. \* significant at 5%; \*\* significant at 1%. Robust standard errors in parentheses.

Table 4: Specification and robustness checks

	Dependent variable:	hospital costs		log(hospital costs)	log (hospital charges)	median regression hospital charges	log(length of stay)
		Birthweight < 1500g	4206 (1068)**	3812 (1033)**	0.263 (0.106)**	0.282 (0.037)**	9415 (1593)**
Sample		5-State	5-State	5-State	5-State	5-State	5-State
Controls		No	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable above cutoff:		39628	39628	9.91	10.58	81566	2.78
Observations		28769	28769	28769	28769	28928	30935

	Dependent variable:	hospital transfer	missing or zero charges	hospital charges including transfers	hospital costs including transfers	log(charges) including transfers	log(length of stay) including transfers
		Birthweight < 1500g	-0.011 (0.0067)	0.0023 (0.0018)	7297 (4313)	2872 (1776)	0.223 (0.045)**
Sample		5-State	5-State	California	California	California	California
Controls		Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable above cutoff:		0.100	0.0052	109421	45141	11.0	2.99
Observations		30935	25895	14560	14560	14560	16528

*Notes:* All models are OLS, estimated on a sample within 3 ounces above and below VLBW threshold. All models include the gram-trend variables and our "main controls," which include the newborn characteristics that are available for all of the years in the nationwide data: an indicator that the mother was born outside the state where the infant was born, indicators for mother's age, education, father's age, the newborn's sex, gestational age indicators, race, and plurality, as well as state and year indicators. Charges are in \$2006. We are missing data on charges for some observations which are births at hospitals which do not charge for inpatient care but rather charge based on monthly capitated payment schedules (such as Kaiser hospitals in California). In logs, we also lose observations with zero charges, which occur for births at hospitals which do not charge for inpatient care (such as Shriners hospitals in California). Five states include AZ, CA, MD, NY, and NJ (various years). We do not have data on missing charges for NJ. \* significant at 5%; \*\* significant at 1%. Robust standard errors in parentheses.

Table 5: Results by year and for overlap of NCHS/five-state data

A. NCHS Nationwide Data					In-hospital births only	
Dependent variable: 1-year mortality	Years:				Five states, all NCHS years	Five states, years in NCHS and multi-state data
	1983-1987	1988-1991	1995-1998	1999-2002	1983-2002 (available years)	1991; 1995-2002
Birthweight < 1500g	-0.0144 (0.0051)**	-0.0077 (0.0048)	-0.00081 (0.0038)	-0.0069 (0.0035)*	-0.0122 (0.0042)**	-0.0073 (0.0051)
Mean of dependent variable above cutoff:	0.0813	0.0622	0.0410	0.0378	0.054	0.039
Observations	50947	47545	49989	53590	49839	23698

B. Multi-State Sample					In-hospital births only	
Dependent variable: hospital charges	hospital charges	hospital charges	hospital charges	hospital charges	hospital charges	hospital costs
	Years:				Years in NCHS and multi-state data	
	1991-1994	1995-1998	1999-2002	2003-2006	1991; 1995-2002	1991; 1995-2002
Birthweight < 1500g	12055 (4,538)**	3515 (3,167)	16985 (4,930)**	582 (6,151)	10108 (2738)**	4553 (1242)**
Mean of dependent variable above cutoff:	69566	71392	93717	96124	80721	39946
Observations	5018	10711	9504	3695	21479	21479

*Notes:* All models are OLS, estimated on a sample within 3 ounces above and below VLBW threshold. All models include the gram-trend variables and our "main controls," which include the newborn characteristics that are available for all of the years in the nationwide data: an indicator that the mother was born outside the state where the infant was born, indicators for mother's age, education, father's age, the newborn's sex, gestational age indicators, race, and plurality, as well as state and year indicators. Although in theory the births included in the NCHS birth records in the state-years available in our multi-state sample should be the same as the births included in the multi-state sample, in practice the samples are slightly different (as evidence from the difference in sample size), largely due to 300-400 fewer births in the discharge data in each year from 2000-2002. \* significant at 5%; \*\* significant at 1%. Robust standard errors in parentheses.

Table 6: Bandwidth sensitivity

## A. NCHS Nationwide Data

Dependent variable:	1-year Mortality				
	Bandwidth	30	60	90	120
Birthweight < 1500g	-0.0267 (0.00382)**	-0.0162 (0.00269)**	-0.0114 (0.0022)**	-0.00911 (0.00190)**	-0.00865 (0.00170)**
Mean of dependent variable above cutoff:	0.0607	0.0562	0.0545	0.0532	0.0515
Observations	72937	163415	233880	304630	376400

Dependent variable:	28-Day Mortality				
	Bandwidth	30	60	90	120
Birthweight < 1500g	-0.0228 (0.00322)**	-0.0146 (0.00227)**	-0.0101 (0.00185)**	-0.00828 (0.00160)**	-0.00773 (0.00143)**
Mean of dependent variable above cutoff:	0.0431	0.0390	0.0377	0.0367	0.0352
Observations	72937	163415	233880	304630	376400

## B. 5-State Sample

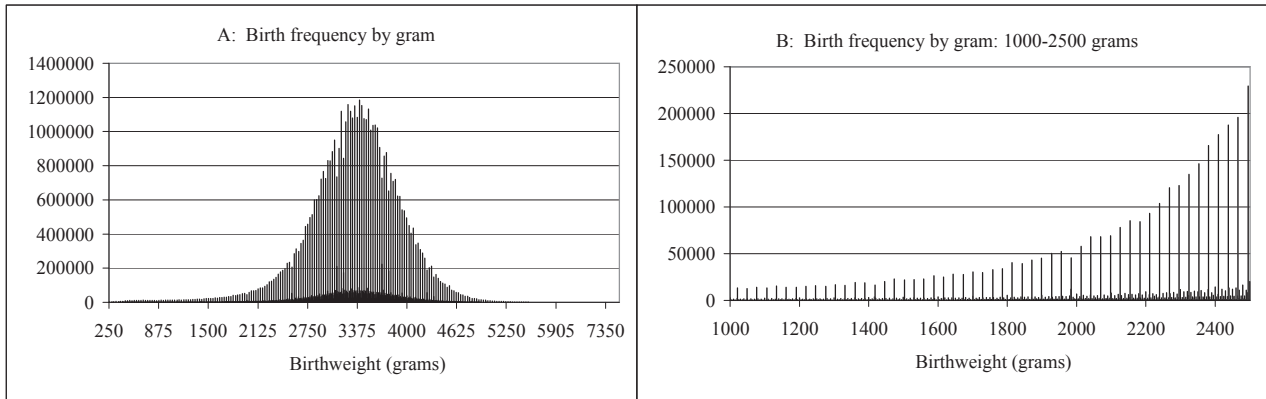
Dependent variable:	Hospital Charges				
	Bandwidth	30	60	90	120
Birthweight < 1500g	7670 (4300)*	8380 (3210)**	9290 (2630)**	8070 (2270)**	6490 (2030)**
Mean of dependent variable above cutoff:	83890	81527	80235	79092	77158
Observations	10533	21404	31990	42012	52471

Dependent variable:	Hospital Costs				
	Bandwidth	30	60	90	120
Birthweight < 1500g	4460 (1880)*	3620 (1390)**	3970 (1140)**	3410 (985)**	2580 (881)**
Mean of dependent variable above cutoff:	41063	39321	38572	38028	37094
Observations	10533	21404	31990	42012	52471

Dependent variable:	Length of Stay				
	Bandwidth	30	60	90	120
Birthweight < 1500g	2.38 (0.743)**	1.84 (0.536)**	1.91 (0.439)**	1.53 (0.379)**	1.14 (0.340)**
Mean of dependent variable above cutoff:	25.7	24.8	24.3	24.0	23.5
Observations	11254	22877	34183	44868	56067

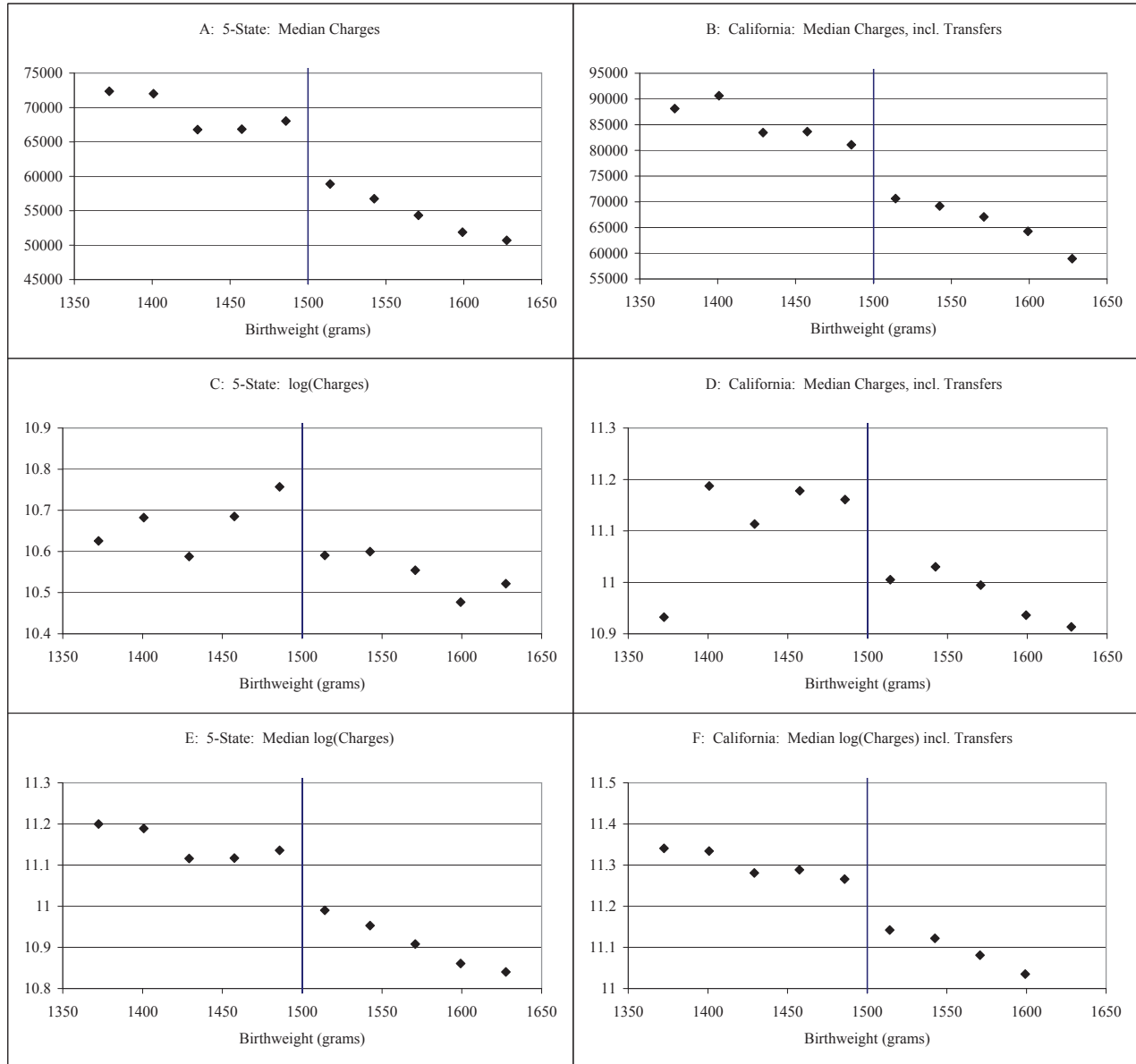
*Notes:* All models are OLS, estimated on a sample within 3 ounces above and below VLBW threshold. All models include the gram-trend variables and our "main controls," which include the newborn characteristics that are available for all of the years in the nationwide data: an indicator that the mother was born outside the state where the infant was born, indicators for mother's age, education, father's age, the newborn's sex, gestational age indicators, race, and plurality, as well as state and year indicators. \* significant at 5%; \*\* significant at 1%. Robust standard errors in parentheses.

Figure A1: Birth frequencies for wider bandwidths



*Notes:* NCHS birth cohort linked birth/infant death files, 1983-1991 and 1995-2003, as described in the text.

Figure A2: Alternative summary treatment measures around 1500 grams



*Notes:* Data are all births in the 5-state sample (AZ,CA,MD,NY, and NJ), as described in the text. Charges are in 2006 dollars. Points represent means in one-ounce bins radiating from 1500 grams.



Table A1: Coefficients on selected covariates

Sample: NCHS Nationwide Data		Multi-State Sample
Dependent variable: 1-year mortality		hospital charges
Birthweight < 1500g	-0.0073 (0.0022)**	Birthweight < 1500g 9065 (2,297)**
Birthweight < 1500g * Grams from cutoff	-0.00011 (0.0000)**	Birthweight < 1500g * Grams from cutoff 6.175 -34.631
Birthweight >= 1500g * Grams from cutoff	-0.00018 (0.0000)**	Birthweight >= 1500g * Grams from cutoff -79.513 (28.23)**
Prenatal Visits: 9-14	-0.0041 (0.0012)**	=1 if newborn is male and not missing 11611 (1,145)**
Prenatal Visits: >=15	-0.0028 (0.0017)	Pre-term birth 22958 (1,688)**
Mother born outside state	-0.0011 (0.0011)	Mother's Race/Ethnicity: African American 1827 -1481
First birth	0.0193 (0.0012)**	Mother's Race/Ethnicity: Other 4533 (1,600)**
Mother's Age: 31-35 (compared to <16)	-0.0132 (0.0049)**	Twin birth 3405 (1,346)*
Mother's Age: 36-40	-0.0122 (0.0051)*	Multiple (non-twin) birth 11835 (2,354)**
Mother's Age: 41+	-0.0011 (0.0065)	Cesarean Section 2770 (1,199)*
Mother's Education: High School	0.0001 (0.0015)	Arizona (compared to NJ) -1653 -2484
Mother's Education: Some College	-0.0029 (0.0017)	California 101580 (1,805)**
Mother's Education: College+	0.0032 (0.0019)	New Jersey 87235 (1,608)**
Mother's Education: missing	0.0176 (0.0028)**	New York 60591 (1,500)**
Father's Age: 31-35 (compared to <16)	-0.0013 (0.0187)	Year = 1991 (compared to 2003) -92968 (4,690)**
Father's Age: 36-40	-0.0016 (0.0188)	Year = 2006 3937 -4694
Father's Age: 41+	-0.0044 (0.0188)	Constant 31557 (4,237)**
Father's Age: missing	0.0021 (0.0187)	
Male	0.0144 (0.0010)**	
Gestational Age: 37 weeks (compared to <31)	0.0252 (0.0038)**	
Gestational Age: 40 weeks	0.0121 (0.0053)*	
Gestational Age: 41 weeks	0.0118 (0.0069)	
Mother's Race/Ethnicity: African American	-0.0189 (0.0014)**	
Mother's Race/Ethnicity: Hispanic	-0.0034 (0.0019)	
Singleton birth	0.0446 (0.0018)**	
Twin birth	0.0114 (0.0018)**	
Year = 2002 (compared to 1984)	-0.0356 (0.0035)**	
Constant	0.0470 (0.0243)	
Mean of dependent variable above cutoff:	0.0553	81566.3098
Observations	202071	28928

Notes: All models are OLS, estimated on a sample within 3 ounces above and below VLBW threshold. Charges are in \$2006. Five states include AZ, CA, MD, NY, and NJ (various years)  
 \* significant at 5%; \*\* significant at 1%. Robust standard errors in parentheses.

Table A2: Selected covariate comparison (controlling for trends in birth weight)

	adjusted mean below threshold	raw mean above threshold	p-value
Fewer than 9 Prenatal visits	0.478	0.443	(0.000)**
First birth	0.4145	0.4145	(0.860)
Mother's Age	26.41	26.44	(0.122)
Mother's Education: <High School	0.2515	0.2495	(0.158)
Mother's Education: High School	0.3385	0.3365	(0.195)
Mother's Education: Some College	0.1738	0.1728	(0.574)
Mother's Education: College+	0.1466	0.1506	(0.001)**
Mother's Education: Missing	0.0896	0.0906	(0.465)
Mother born outside state	0.4116	0.4076	(0.217)
Mother's Race: White	0.4426	0.4506	(0.000)**
Mother's Race: African American	0.2504	0.2524	(0.524)
Mother's Race: Hispanic	0.1315	0.1275	(0.058)
<hr/>			
Father's Age	29.85	29.86	(0.646)
Missing Father's Age	0.245	0.242	(0.076)
Father's Education: <High School	0.1276	0.1266	(0.459)
Father's Education: High School	0.2636	0.2656	(0.371)
Father's Education: Some College	0.1019	0.0999	(0.287)
Father's Education: College+	0.1066	0.1086	(0.243)
Father's Education: Missing	0.4003	0.3993	(0.698)
<hr/>			
Male	0.4983	0.5003	(0.574)
Gestational Age	31.73	32.62	(0.000)**
Singleton Birth	0.7357	0.7437	(0.006)**
Twin Birth	0.2074	0.2214	(0.000)**
Multiple (non-twin) Birth	0.0349	0.0349	(0.969)
<hr/>			
Vaginal Birth	0.4504	0.4754	(0.000)**
Obstetric Procedures: Amnioscentesis	0.0480	0.0510	(0.004)**
Obstetric Procedures: Induction	0.0874	0.1004	(0.000)**
Obstetric Procedures: Stimulation	0.0604	0.0664	(0.000)**
Obstetric Procedures: Tocolysis	0.1210	0.1150	(0.000)**
Obstetric Procedures: Ultrasound	0.6454	0.6484	(0.091)
Obstetric Procedures: Other	0.0624	0.0634	(0.151)
<hr/>			
Year of Birth	1992.97	1993.00	(0.110)
Predicted 1-year Mortality	0.0586	0.0576	(0.000)**

Notes: Sample is NCHS national data. For most covariates, the number of observations is 341,140. Delivery method is available for 229,843 births; obstretic procedures are available for 229,175 births. \* significant at 5%; \*\* significant at 1%.

Table A3: 5-State sample: Data summary

Year	AZ	CA	MD	NJ	NY	Total
1991	0	122,384	0	0	0	122,384
1992	0	122,615	0	0	0	122,615
1993	0	121,489	0	0	0	121,489
1994	0	119,767	0	0	0	119,767
1995	0	116,287	15,941	26,904	64,221	223,353
1996	0	112,473	15,789	27,078	60,699	216,039
1997	0	110,757	15,699	26,833	60,002	213,291
1998	0	109,321	15,945	27,346	60,090	212,702
1999	0	107,266	16,317	28,005	59,101	210,689
2000	0	110,879	16,880	27,467	59,100	214,326
2001	12,958	113,076	17,032	27,725	0	170,791
2002	15,435	112,783	17,066	27,699	0	172,983
2003	17,072	0	17,781	28,378	0	63,231
2004	23,199	0	17,975	28,826	0	70,000
2005	24,276	0	18,801	28,689	0	71,766
2006	26,363	0	20,029	29,764	0	76,156
Total	119,303	1,379,097	205,255	334,714	363,213	2,401,582

*Notes:* Table displays years for which each of our state datasets are available, and the relevant sample sizes.

Table A4: One-year mortality results by cause of death

Dependent variable:	<u>one-year mortality, by cause</u>										
	all cause	infectious and parasitic diseases	neoplasms	endocrine, nutritional, metabolic, immunity, blood disorders	nervous system, sense organ disorders	respiratory system disorders	digestive system disorders	congenital anomalies	perinatal conditions	symptoms, signs, ill-defined conditions	other
Model:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Birthweight < 1500g	-0.0095 (0.0022)**	-0.0031 (0.0055)	0.0009 (0.0024)	0.0029 (0.0031)	-0.0131 (0.0050)**	0.0017 (0.0070)	-0.0006 (0.0050)	0.0091 (0.0191)	-0.0294 (0.0188)	0.0173 (0.0108)	0.0144 (0.0093)
Mean of dependent variable above cutoff:	0.0553	0.0225	0.0038	0.0055	0.0177	0.0339	0.0161	0.4024	0.3567	0.0831	0.0582
Observations	202071	11090	11090	11090	11090	11090	11090	11090	11090	11090	11090

Dependent variable:	<u>one-year mortality, by cause</u>				
	"external" cause	respiratory distress syndrome (RDS)	sudden infant death syndrome (SIDS)	jaundice	meningitis
Model:	OLS	OLS	OLS	OLS	OLS
Birthweight < 1500g	0.0045 (0.0048)	-0.0054 (0.0109)	0.0148 (0.0095)	-0.0034 (0.0018)	-0.0016 (0.0030)
Mean of dependent variable above cutoff:	.0141	.0878	.0608	.0022	0.0052
Observations	11090	11090	11090	11090	11090

*Notes:* The ten cause of death classifications (other than all cause mortality) in the first row were constructed to be categories which could be defined consistently over time, across a change in cause of death coding which occurs partway through our sample; these broad categories partition non-missing causes of death. The second row extracts some individual causes of death from these broad categories. We exclude observations with missing information on the timing or cause of death. OLS models estimated on a sample within 3 ounces above and below the VLBW threshold. All models include the gram-trend variables. \* significant at 5%; \*\* significant at 1%. Robust standard errors in parentheses.