ENDOWMENTS AND PARENTAL INVESTMENTS IN INFANCY AND EARLY CHILDHOOD*

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This article tests whether parents reinforce or compensate for child endowments. We estimate how the difference in birth weight across siblings impacts specific parental investments: breast-feeding, well-baby visits, immunizations, and preschool attendance. Our results indicate that normal-birth-weight children are 5%–11% more likely to receive early childhood parental investments than their low-birth-weight siblings. Moreover, the presence of additional low-birth-weight siblings in the household increases the likelihood of investments such as well-baby visits and immunizations for normal-birth-weight children. These results suggest that parental investments in early childhood tend to reinforce endowment differences.

Economists have a long-standing interest in the question of how parents allocate resources to children with different endowments, where endowments are broadly defined as genetically inherited characteristics that are predetermined prior to the human capital accumulation process and are rewarded directly or indirectly (through their interaction with human capital investments) in labor and marriage markets (Behrman 1997). The economic theory of intrahousehold resource allocation suggests that parental investment could compensate for or reinforce initial endowments. On the one hand, parents concerned solely with maximizing the aggregate welfare of their children might reinforce initial endowments by investing relatively more in their better-endowed children, assuming that marginal returns to investing are higher for better-endowed children than they are for lesser-endowed children (Becker and Tomes 1976). On the other hand, equity concerns might drive parents to compensate for low initial endowments by investing relatively more in their lesser-endowed children under the same assumptions about the marginal returns to investment (Behrman, Pollak, and Taubman 1982). Whether parents compensate for or reinforce initial endowments has implications for the intergenerational transmission of human capital, the long-term consequences of policies that seek to improve initial endowments, the distribution of human capital and income, and the econometric estimation of the impact of endowments on subsequent short- and long-term educational, health, and labor market outcomes.

A sizable empirical literature has sought to determine whether parental investments compensate for or reinforce endowments (Behrman, Rosenzweig, and Taubman 1994; Griliches 1979; Pitt, Rosenzweig, and Hassan 1990; Rosenzweig and Schultz 1982; Rosenzweig and Wolpin 1988). The econometric approach of this literature has generally assumed that endowments are observable to parents but unobservable to researchers. Consequently, the literature has relied on indirect tests of whether parents compensate for or reinforce parental investment, the validity of which is contingent on functional form and other identifying assumptions.

In this article, we employ data from the National Longitudinal Survey of Youth–Child (NLSY-C) file to assess whether parents compensate for or reinforce endowments,

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using birth weight as an observable proxy for initial endowments and using breast-feeding initiation, well-baby visits, immunizations, and preschool attendance as direct measures of early childhood parental investments. Specifically, we examine the correlation between sibling differences in birth weight and differences in the parental investments those siblings receive.

The principal innovation in our approach is in the direct measurement of both endowments and parental investments. For the purposes of this article, we define endowments as basic differences in mental and physical capacities between children at birth resulting from genetic and prenatal influences outside the control of the mother. Birth weight is directly observed by parents, and it is reasonable to assume that parents might perceive birth weight as an indicator of underlying endowments. Birth weight is strongly correlated with outcomes in infancy, childhood, and adulthood and is a target of many public health and welfare programs.[1] Consequently, our measure of endowments is plausibly the same as one that parents themselves might employ. We directly examine how endowments are related to parental investments in early childhood, a period when investments have been shown to have high returns (Heckman 2007).

Another advantage of our approach is that it does not rely on having a sample of twins.[2] Although data on twins have significant advantages for estimating birth-weight effects on child and adult outcomes due to their ability to control for shared family, environmental, and (in the case of identical twins) genetic factors, resource allocation decisions in families with twins may be quite different than in families with nontwin siblings. For example, significantly varying contemporaneous parental investment between twins seems less feasible than varying parental investment across siblings of different ages.

Finally, our approach allows us to examine how the endowments of other siblings, as measured by the number of low-birth-weight (LBW; < 2,500 grams) siblings present in the household at the time of investment, might impact investments that parents make in a given child.

Our results indicate that normal-birth-weight (NBW; ≥ 2,500 grams) children are 5%–11% more likely to receive important parental investments at very young ages than their LBW siblings, suggesting that parental investments in infancy and early childhood generally reinforce differences in endowments. With regard to the effect of siblings' endowments on parental investments, we find some evidence that the presence of LBW siblings in the household at the time of investment is associated with an increased likelihood that NBW siblings receive investments such as well-baby visits and immunizations. Our findings are robust to the addition of an extensive set of controls that capture differences in parental and household resources at the time of birth. In particular, differences at the time of birth between siblings in household income, mother's marital status, education and health, and prenatal investments do not explain our findings. Our findings prevail across several robustness checks; thus, unobserved sibling-specific heterogeneity is unlikely to be biasing our results.

These results suggest that estimates of the effect of birth weight on later outcomes will encompass not only the pure biological effect of birth weight but the effect of such investments as well. This is important because policies that succeed in increasing birth weight might improve long-run outcomes both because birth weight itself matters for later

1. Most studies have provided correlational evidence on the relationship between birth weight and outcomes in childhood and adulthood (e.g., Aylward et al. 1989; Boardman et al. 2002; Brooks et al. 2001; Corman and Chaikind 1993; Currie and Hyson 1999; Lee and Barratt 1993; MacDorman and Atkinson 1999; Matte et al. 2001; McCormick 1985; McCormick, Gortmaker, and Sobol 1990; Paneth 1995; Richards et al. 2001; Strauss 2000). More recently, studies based on siblings (Conley and Bennett 2000) and twins (Almond, Chay, and Lee 2005; Behrman and Rosenzweig 2004; Black, Devereux, and Salvanes 2007) have provided quasi-experimental evidence. See Black et al. (2007) for an overview of the literature.

2. Examples of studies using data on twins are Behrman et al. (1982) and Behrman et al. (1994).

outcomes and because higher birth weight induces more parental investment. This, in turn, means that cost-benefit analyses of policies that seek to increase birth weight must account not only for the direct cost of such policies but also for the cost of parental investment these policies indirectly induce.

INTRAHOUSEHOLD ALLOCATION DECISIONS

Economic models of intrahousehold resource allocation that have dominated the literature consist of consensus parental preferences models in which parents allocate given resources across children so as to maximize a single utility function, subject to appropriate constraints (Behrman 1997). We examine two special cases of these models, which are discussed in detail elsewhere (Becker 1991; Behrman, Pollak, and Taubman 1995; Mulligan 1997). The wealth model, proposed by Becker and Tomes (1976), assumes that parents are concerned with maximizing the total wealth (earnings plus transfers) of each child and that all children receive equal weight in the parents' utility function ("equal concern"). To achieve this objective, parents invest in the human capital of each child until the marginal rate of return to human capital is driven down to the rate of return on financial assets. Subsequently, parents allocate additional resources in the form of transfers (gifts and bequests) in order to offset fully any inequalities in their children's earnings, ultimately equalizing the distribution of wealth across all children. The model's prediction regarding parents' investment strategy, therefore, depends on the properties of the earnings function. If a greater endowment implies a greater (or smaller) marginal return to investment, then parents adopt a reinforcing (or compensating) strategy.[3]

In contrast, the separable earnings-transfer (SET) model (Behrman et al. 1982) assumes that parents care about the distribution of earnings as well as transfers across their children, not just the distribution of total wealth. In this model, parents' strategy depends on four factors: (1) the degree of equal concern across children,[4] (2) preferences for equity versus productivity, (3) distribution of endowments among children, and (4) properties of the earnings function. As such, parents' investment strategy could be neutral, reinforcing, or compensating. For example, the SET model predicts that if the marginal returns to investment were greater for children with greater endowments, parents may adopt a compensating or reinforcing strategy, depending on whether equity or productivity concerns were dominant. Since neither the wealth model nor the SET model makes unambiguous predictions regarding parents' investment strategies, the question of whether parental investments are compensating, reinforcing, or neutral is ultimately an empirical one.

Empirical studies using data from the United States have focused on examining how parental investments in children's human capital are related to endowment differences across children. Griliches (1979) examined whether there is reinforcement or compensation in investments in children and found that the effects of IQ were significantly lower within sibling pairs than across individuals. Furthermore, the more alike the siblings in terms of age, gender, and genetics, the smaller the IQ effects; this finding suggests that families compensate for genetic endowments, as measured by IQ. Behrman et al. (1982) used twins data for U.S. adult males to estimate parental preference parameters from a SET model. Based on data on schooling and earnings of the twins and functional form assumptions to account for endowment differences, their estimates suggest that

3. This characterization of the Becker and Tomes wealth model is accurate only if parents invest enough resources in their children's human capital to drive the marginal rate of return to those investments down to the market rate of interest. However, parents may not do so due to either capital market constraints or other factors; if this were the case, the model does not necessarily predict investment in human capital until the market rate of return is obtained, and wealth of the children generally is not equalized even if the parents have equal concern (Behrman et al. 1995).

4. Unequal concern across children might be based on a variety of characteristics, such as sex, birth order, beauty, cuteness, brightness, energy, and health.

parents may slightly reinforce endowment differences among their children but that they also have substantial concern about the distribution of their children's earnings. Behrman et al. (1994) estimated a model of returns to schooling using data on twins from Minnesota, using variance components of individual-specific earnings as endowments and body mass index in adulthood as a proxy for health endowments. They found that schooling attainment was higher for the twin with the greater endowment, which, they concluded, suggests reinforcing behavior. This study did not examine investments other than schooling attainment and, in particular, did not examine preschool allocations that are more proximate to endowments at birth.

Outside the United States, a number of studies have examined how intrahousehold resource allocations respond to changes in economic conditions and endowments within the context of developing countries. These studies have generally found evidence of reinforcing behavior in parental investments (Ayalew 2005; Pitt et al. 1990; Rosenzweig and Schultz 1982; Rosenzweig and Wolpin 1988).

In summary, the empirical literature has generally treated endowments as characteristics that are assumed to be observable to parents but unobservable to the researcher. As a result, estimates of the relationship between these unobserved child endowments and parental investments relied on either functional form assumptions about parents' utility or on the absence of omitted variables in health production functions. This study differs from prior work by using birth weight as a proxy for endowments and conducting a direct test of whether variation in birth weight across siblings generates systematic differences in intrahousehold allocations of investments in early childhood. Using birth weight as an observed proxy for endowments also allows us to examine the effect of siblings' endowments, as measured by the birth weight of older siblings present in the household at the time of investment, on investments that parents make in a given child.

EMPIRICAL APPROACH

Our empirical approach is based on an underlying economic model—similar in spirit to that employed by Behrman et al. (1994)—in which health, educational, and other types of postnatal parental investments (I) made at a particular point in time in child i belonging to family f depend on the child's own endowment (e_i), the endowments of other siblings present in the family at the time of investment in child i (e_{-if}), and other time-varying child and family characteristics (X_{if}) that influence parental investments (e.g., income and mother's marital status). These parental investments may take the form of money or time. A linear econometric specification of this model might take the following form:

$$I_{if} = e_{if}\beta_1 + g(e_{-if})\beta_2 + X_{if}\Phi + \gamma_f + \varphi_i + \varepsilon_{if}, \qquad (1)$$

where $g(.)$ is some function of the endowments of siblings present in the household at the time of investment in child i; γ_f represents unobserved endowments and environmental influences (pre- and postnatal) common to all siblings in a family; φ_i represents unobserved child-specific factors capturing the child's individual endowment and other unobserved determinants of investments that vary across siblings within a family; and ε_{if} is an idiosyncratic error term.

The parameters β_1 and β_2 in Eq. (1) are the key parameters of interest in this study, and they capture the effect of own endowment and the endowments of other siblings on own investment, respectively. The parameter β_1 measures whether parents invest more or invest less in children with higher endowments compared with children with lower endowments. A positive sign on β_1 would indicate that parental investments are reinforcing (i.e., that parents invest relatively more in children with higher endowments). A negative sign on β_1 would indicate that parental investments are compensating (i.e., that parents invest relatively more in children with lower endowments).

The parameter β_2 measures the effect of within-family differences in the endowments of other siblings present in the household at the time of the investment (e_{-if}). The presence of other, less-endowed siblings in the household might increase or decrease the level of parental investment in a child. A positive sign on β_2 would indicate that parents invest more in children who have siblings with higher endowments present in the household at the time of the investment; a negative sign would indicate the opposite. For example, if a LBW sibling who demands expensive treatment (either by law or by social convention) was present at the time of child A's birth but not at the time of child B's birth, then parents may have fewer resources to devote to child A. Alternatively, the realization of a low-endowment child might raise the concern parents have for all their children, thereby raising levels of all subsequent investments. This may encourage them to invest more in child A relative to child B, other things being equal. In addition to the potential mechanisms described above, the endowments of other siblings in the household at the time of investment may also affect parental investments because of parents' desire to either reinforce or compensate for endowment differences. In the context of the example described above, child A might receive more investment than child B if parents seek to reinforce endowment differences, or less investment than child B if they seek to compensate for endowment differences.

Most prior studies focused on estimating β_1 and, for several reasons, were not able to estimate β_2. First, their treatment of endowments as unobservable to the researcher did not allow for the estimation of β_2. Second, studies that used data on twins could not estimate β_2 because of lack of variation in e_{-if} across the twins.[5] Our approach of using an observable proxy for endowments allows us to estimate the effect of e_{-if}. The effect of other siblings' endowments on investments in child i is of interest because it is likely to impact the amount of investment parents make in child i. In addition, the endowments of siblings within a family are likely to be correlated such that $\text{Corr}(e_{if}, e_{-if}) \neq 0$. In other words, the endowments of siblings born prior to child i are likely to be correlated with the endowments of child i. As a result, the omission of e_{-if} in an estimation of Eq. (1) would bias the estimate of β_1.

Taking within-family differences, as shown in Eq. (2) below, can eliminate bias due to unobserved family-specific heterogeneity (γ_f).[6]

$$\Delta I_i = \Delta e_i \beta_1 + \Delta g(e_{-i})\beta_2 + \Delta X_i \Phi + \Delta \varphi_i + \Delta \varepsilon_i, \tag{2}$$

where Δ represents a within-family difference. All variables are now of the form $\Delta I_i = I_{if} - \bar{I}_f$, where $\bar{I}_f$ is the within-family mean of I_i. We refer to this specification as the mother fixed-effect (MFE) specification.

We estimate the model in Eq. (2) using the child's own birth weight (BW_i) and the number of low-birth-weight (LBW; i.e., $\leq$ 2,500g) siblings present in the household at the time of the investment ($NLBW_{-i}$), excluding sibling i, to capture own endowment and siblings' endowment, respectively.[7] Eq. (2) may now be rewritten as follows:

$$\Delta I_i = \Delta BW_i \beta_1 + \Delta g(NLBW_{-i})\beta_2 + \Delta X_i \Phi + \Delta \varphi_i + \Delta \varepsilon_i. \tag{3}$$

5. Endowments of other siblings present in the household at the time of investment (e_{-if}) do not vary across twins in a twin pair. Studies of twins, however, define sibling endowment differently. Since their samples include only twin pairs, the sibling endowment variable is defined as the twin's endowment and, therefore, these models are unable to estimate the effect of other, nontwin siblings' endowments. In twins studies, sibling endowment varies within fraternal twin pairs because they are not genetically identical and remains the same in identical twin pairs because they are genetically identical.

6. One of the limitations of estimating a within-family or mother fixed-effect (MFE) model is that we are unable to examine parental responses to children's endowments among families with only one child (25% of mothers). Consequently, estimates from such a model may be generalizable only to families with two or more children.

7. In alternate models, we used other measures of sibling endowment, such as the mean and variance of birth weights of all siblings older than the focal child. This did not change our results.

Estimates of β_1 and β_2 from Eq. (3) might still be biased due to sibling-specific unobserved heterogeneity ($\Delta\varphi_i$) that remains in the error term. In other words, birth-weight differences across siblings may be endogenous. This is because birth weight captures not only exogenous endowments but also choices that mothers make that can influence birth weight (e.g., smoking during pregnancy). We address this concern by including a number of time-varying sibling-specific controls, all measured at the time of the child's birth. These include family income, mother's education, mother's marital status, mother's age at birth, and a proxy for mother's health—whether the mother had a health condition that limits work. We also control for prenatal investments in child *i*, such as month of first prenatal care use and frequency of alcohol and cigarette consumption during pregnancy, since these are choices that are correlated with birth weight and, plausibly, postnatal investments. Finally, we also include indicators for the child's gender and first-born, and state and birth-year fixed effects to control for differences in access to and price of health care across states and over time. This extensive set of controls captures variation across siblings in parents' capacity to invest and, perhaps, motivation to make prenatal and postnatal investments.[8]

We use the number of LBW siblings present in the family at the time of investment in child *i* to measure siblings' endowment rather than, for example, the total number of LBW siblings ever born in the family, in order to account for investments made in children before their siblings are born. Our model assumes that parents condition parental investment in child *i* on the birth weight of all siblings that are alive at the time of investment in child *i* rather than the birth weight of all siblings ever born, including those born after time *t*. Thus, $NLBW_{-i} = 0$ for children whose siblings have yet to be born, which reflects our assumption that investments in a child at that point in time are unaffected by the possibility that the child's future siblings could be LBW.

The model in Eq. (3) is estimated using three alternate specifications for own birth weight: (1) continuous birth weight, (2) an indicator for LBW, and (3) indicators for three birth-weight categories (2,500–3,299 grams, 3,300–3,699 grams, and ≥ 3,700 grams). This latter specification will allow us to examine whether investment responses to birth-weight differences are restricted to certain parts of the birth-weight distribution. For example, using indicators for birth-weight categories as measures of own endowment would allow us to test whether the sibling with a relatively higher birth weight receives more or less parental investment than the sibling with a lower birth weight, conditioning on NBW siblings.

For purposes of comparison, we also report estimates from ordinary least squares (OLS) regressions that control for child's race and number of siblings in the household in addition to the covariates listed above. Standard errors are adjusted in all models by clustering at the mother level. Results from linear models are reported in the article, although estimates from logit fixed-effects models (conditional logit) showed the same pattern of results.

Note that compensating or reinforcing parental investment need not solely result from deliberate actions of parents. The ability of parents to engage in compensating or reinforcing investments can be influenced by circumstances outside their control. For example, a LBW child in the hospital may be more likely to receive parental investments than the NBW sibling because these investments are critical for her recovery. On the other hand, despite wanting to invest more in the LBW child, parents may be unable to do so simply because of the child's inability to receive the investment.[9] For example, LBW babies who need to

8. We do not control for selective child mortality because child mortality rates in the United States are only about 6 per 1,000 live births. We also do not control for differences in pregnancy or labor complications across siblings. These might be correlated with birth weight and might also alter parental concern for the child. Robustness checks reported later in the article potentially address this concern.

9. It is unlikely that medical advice or treatment may be a reason for differences in breast-feeding and immunization between LBW and NBW siblings, since the benefits and safety of these investments in premature and LBW infants are well-documented in the medical literature (Saari and Committee on Infectious Diseases 2003; Smith et al. 2003)

be placed in an incubator for a period of time may be too fragile to be breast-fed. Comparing estimates from models with a LBW indicator to estimates from those with indicators for NBW categories, as described above, may shed some light on whether such remedial interventions are at work. In other words, if the effect of birth weight on investments is seen even among NBW siblings, then there is less likelihood that it is the result of these remedial (or compulsory) interventions. Later in the article, we show results from models that investigate this issue in more detail.

DATA AND MEASUREMENT ISSUES

We use data from the NLSY-C, which contains detailed information about the children born to female respondents of the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 began in 1979 with a sample of 12,686 young adults between the ages of 14 and 21. NLSY79 respondents were surveyed annually between 1979 and 1994 and biennially thereafter.[10] The children of the female NLSY79 respondents have been surveyed biennially since 1986. As of the 2000 survey wave, the NLSY-C had collected data on 11,205 children born to 6,283 mothers. We first restrict our sample to mothers with at least two children surveyed between 1986 and 2000 with birth-weight information available for at least one child. We further restrict the sample to children for whom there is information on at least one of the parental investments examined in the article. This reduces the sample to 10,000 children born to 3,660 mothers. The exact sample sizes in our regressions drop further when we exclude observations with missing values for the particular parental investment being examined and again restrict our sample to families with at least two children with information on that parental investment.[11]

We exploit four key features of the NLSY-C for the purposes of this article. First, the NLSY-C collects data on all children born to NLSY79 mothers, which allows us to examine intrafamily resource allocation decisions. Second, the NLSY-C collects data on birth weight for all surveyed children. The third key feature of the NLSY-C is that it collects information on a number of health and educational investments that parents make in their children starting in infancy and early childhood. Finally, the availability of information regarding maternal and family characteristics, as well as prenatal investments at the time of each sibling's birth, is a unique feature of these data and allows us to control for such differences across siblings.

Our measures of postnatal investments include the health and educational investments that parents make in their children's early years. We focus on investments in early childhood because they are proximate to endowments at birth and are widely recognized as critical determinants of outcomes over the life course (Engle et al. 2007; Heckman 2006). Our analyses consider the following investments: (1) whether the child was breast-fed;[12] (2) whether the child was taken for a well-baby visit in the first year after birth; (3) whether the child received all doses of DPT and oral polio vaccines; and (4) whether the child attended preschool (including Head Start).

The American Academy of Pediatrics (AAP 2000) highly recommends that all parents make numerous health investments during a child's first year, including breast-feeding,

10. Two subsamples—the military subsample and the poor, non-Hispanic white subsample—were dropped from the survey in 1984 and 1990, respectively.

11. Our largest samples are for breast-feeding, well-baby visits, and preschool attendance. Data on immunizations were collected only in the first three waves of the NLSY, resulting in smaller sample sizes for these regressions.

12. In analyses not shown here, we also estimated MFE models for the number of weeks the child was breast-fed, using only the sample of mothers who breast-fed. The effect of birth weight on weeks breast-fed was small and statistically insignificant. However, this result is likely to be biased due to sample selection; therefore, we do not include these results in the article. An alternate approach would be to model weeks breast-fed using the full sample of mothers. When we estimated this model, the coefficient on birth weight was positive and significant. However, the error term in this model is likely to be misspecified due to the large number of zeroes in the dependent variable.

well-baby visits, and immunizations, and these are also included as objectives in the U.S. Department of Health and Human Services' (DHHS) *Healthy People 2010* (U.S. DHHS 2000). These activities all require considerable time commitments and may also require monetary payments or insurance coverage. Pediatricians may schedule as many as six well-baby visits during a child's first year. These visits deliver preventive services for healthy children and are likely to include assessments of growth and development, screening for disabilities such as hearing problems, immunizations, and guidance for parents on issues like nutrition and injury prevention. Less than half of children in our sample were ever breast-fed; but more than 90% had at least one well-baby visit in the first year, and the same percentage received complete doses of DPT and polio vaccines (Table 1).

Attending high-quality preschool programs, including Head Start, has been shown to have positive and lasting effects on outcomes in childhood and adulthood, especially for children from disadvantaged backgrounds (Currie and Thomas 1995; Karoly, Kilburn, and Cannon 2005). Our data do not contain information on the quality of the preschool program, and so we are unable to differentiate participation in high-quality versus other programs. Slightly more than half of the children in our sample attended preschool (Table 1). The means and standard deviations of the parental investment variables and other explanatory variables in our model are reported in Table 1.

We use the child's birth weight to proxy for his or her own endowments and use the number of LBW siblings present in the household at the time of investment as our measure of sibling endowment.[13] As mentioned earlier, mothers retrospectively reported birth-weight information for each child in the NLSY-C. Specifically, mothers were asked to report the birth weight of children in the survey wave closest to the child's birth, which minimizes bias due to lengthy recall periods. The first survey wave of the NLSY-C was in 1986, and many NLSY-C children were born before 1986. Thus, the median number of months between the child's birth and the mother's report of that child's birth weight (the "recall" period) in our sample is almost two years. For children born before 1986, the median recall period is 52 months, and for children born in 1986 or later, the median recall period is 13 months.[14]

Alternate indicators of a child's endowments at birth that are available in our data include birth length and gestational age (or prematurity). However, birth weight is highly correlated with gestational age and birth length, implying that little additional information may be gained by examining gestational age or birth length separately from birth weight. Nevertheless, we estimated our models using three different measures of initial endowment: (1) continuous birth length (inches), (2) gestational age (in weeks), and (3) a small-for-gestational-age indicator (birth weight less than the 10th percentile for a given

13. Birth-weight information was missing for 9% of our sample. We replaced missing birth-weight data with imputed birth weight based on linear regression models that included child's sex and race, mother's marital status at birth, education, income, square of income, dummy variables for first-born, mother's height, mother's age at child's birth, month of first prenatal care use, alcohol and cigarette use during pregnancy, and child's year of birth as predictors. This new birth-weight variable was used to capture own endowment. In order to construct the sibling-endowment variable, we first estimated a logit model to predict the probability of LBW for children missing birth-weight information based on the same set of covariates used to predict continuous birth weight. The new LBW variable equaled 1 or 0 for those children having birth-weight data and equaled the predicted probability of being LBW for those children missing birth-weight data. Having LBW information on each sibling in the family allowed us to construct our measure of sibling endowment by simply adding the new LBW variable for siblings present in the household at the time of the investment. We include an indicator for whether birth weight was imputed and an indicator for whether imputed data were used to construct the sibling endowment variable in all our regression models. When replicating our analyses using only observations with nonmissing birth weight, we obtained nearly identical estimates.

14. A recent study in the United Kingdom that compared mother-reported birth weight with registration data on birth weight for the same children found that 82% of mothers reported their baby's weight within 30 grams (about 1 ounce) of the registration weight, and 92% reported their baby's weight within 100 grams (Tate et al. 2005). Additionally, measurement error was found to be mean zero. The recall period in that study was about 9 months.

Table 1. Descriptive Statistics

Variable	Mean	SD	Number of Observations
Early Childhood Parental Investments			
Ever breast-fed	0.45	0.50	9,413
Received well-baby care in the first year	0.92	0.28	8,803
Received complete dose of DPT and oral polio vaccine	0.94	0.24	7,441
Attended a preschool program	0.55	0.50	9,678
Child Characteristics			
Birth weight (100g)	32.98	5.79	10,000
Birth weight < 2,500g	0.09	0.27	10,000
Birth weight = 2,500–3,299g	0.39	0.47	10,000
Birth weight = 3,300–3,599g	0.21	0.39	10,000
Birth weight ≥ 3,600g	0.31	0.44	10,000
Female	0.49	0.50	10,000
First-born	0.37	0.48	10,000
White	0.52	0.50	10,000
Black	0.28	0.45	10,000
Hispanic	0.20	0.40	10,000
Maternal and Family Characteristics			
Mother married at child's birth	0.67	0.47	10,000
Mother's education at child's birth			
Less than high school	0.28	0.45	10,000
High school diploma or more	0.72	0.45	10,000
Annual family income at child's birth (thousands of dollars)	28.91	72.33	10,000
Mother's age at child's birth	24.83	5.52	10,000
Mother had health condition that limits work	0.03	0.17	10,000
Time of first prenatal care use			
Never	0.01	0.12	10,000
In first trimester	0.72	0.45	10,000
In second trimester	0.13	0.33	10,000
In third trimester	0.14	0.34	10,000
Frequency of alcohol use during pregnancy			
Never	0.72	0.45	10,000
Less than once per month	0.14	0.34	10,000
Once per month or more	0.14	0.35	10,000
Frequency of cigarette use during pregnancy			
Never	0.74	0.44	10,000
Less than one pack per day	0.18	0.39	10,000
One or more packs per day	0.08	0.27	10,000

gestation); we found the same pattern of results. These results are not reported here but are available from the authors on request. Each of these birth outcomes, including birth weight, is merely a proxy for the broad construct we call "child endowment" and might therefore suffer from measurement error. We would expect that this measurement error (if

Table 2. Within-Family Variation in Birth Weight and Parental Investments

Variable	% of Total Variance Explained by Within-Family Variation	% of Families With Within-Family Variation
Continuous Variable		
Birth weight in 100s of grams	51.4	
Dichotomous Variable		
Low birth weight (< 2,500g)		15.1
Ever breast-fed		22.5
Received well-baby care in the first year		15.0
Received complete dose of DPT and oral polio vaccine		9.8
Attended a preschool program		36.9

random) would bias estimates of how endowments affect parental investment downward. This downward bias would be even more pronounced in fixed-effects models.

The mean birth weight in our sample is 3,300 grams, with a standard deviation of 579 grams (Table 1). Just over 1% of our sample was born at less than 1,500 grams; about 8%, between 1,501 and 2,499 grams; 39%, between 2,500 and 3,299 grams; 21%, between 3,300 and 3,599 grams; and 31%, at 3,600 or more grams.[15]

There exists substantial within-family variation in birth weight and parental investments in our data. The top panel in Table 2 reports the percentage of total variance explained by within-family variation for birth weight and parental investments defined as continuous variables. As estimated by the simple one-way analysis of variance, the family effect accounts for about 51% of the overall variance in birth weight in our sibling sample. Moreover, the standard deviation of birth weight within families (498 grams) is only 14% less than the standard deviation of birth weight in the overall sample (579 grams). There is considerable variation within families in terms of the number of weeks of breast-feeding. For dichotomous variables, the bottom panel in Table 2 reports the percentage of families whose children differ along that specific dimension. About 15% of the families in this sample had at least one child who was LBW and at least one child of NBW. Significant within-family variation also exists in parental investments. For example, 37% of families have at least one child who attended a preschool program and at least one child who did not. For most other dichotomous investments, the percentage of families in which some children received the investment and other children did not ranged between 10% and 22%.

RESULTS

Table 3 reports estimates of the effect of own endowment and sibling endowment on parental investments using a continuous specification for birth weight. Estimates in panel A are from OLS regressions that include the full set of covariates. OLS estimates show a strong reinforcing pattern of parental investments: higher-birth-weight children are significantly more likely to receive early childhood parental investments than their lower-birth-weight siblings. For example, an increase of 1 standard deviation in birth weight (580 grams) increases the likelihood of being breast-fed by 2.3 percentage points. The presence of other LBW siblings in the household has a small positive effect on the likelihood of receiving full DPT and polio immunization, which is again evidence of reinforcement. Estimates in panel B are from MFE models that include a limited set of covariates, such as child's

15. The rate of LBW in the United States during our sample period ranged from 6.7% to 7.6% (Centers for Disease Control and Prevention 2002).

Table 3. Ordinary Least Squares and Mother Fixed-Effects Estimates of Birth-Weight Effects on Early Childhood Parental Investments

Regression Specification	Ever Breast-fed	Well-Baby Visit in First Year	Full Dose of DPT and Oral Polio Vaccines	Attended Any Preschool Program
A. Ordinary Least Squares				
Continuous birth weight (in 100s of grams)	0.004**	0.002**	0.002**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Number of LBW siblings present at the time of investment	–0.022	0.013	0.018†	–0.002
	(0.016)	(0.011)	(0.010)	(0.015)
B. Mother Fixed-Effects Model With Limited Covariates				
Continuous birth weight (in 100s of grams)	0.004**	0.003**	0.003**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Number of LBW siblings present at the time of investment	0.014	0.040**	0.028*	0.02
	(0.016)	(0.018)	(0.015)	(0.024)
C. Mother Fixed-Effects Model With Full Set of Covariates				
Continuous birth weight (in 100s of grams)	0.003**	0.003**	0.003**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Number of LBW siblings present at the time of investment	0.01	0.043**	0.026†	0.005
	(0.016)	(0.018)	(0.016)	(0.024)
Number of Observations	9,206	8,325	6,859	9,573
Number of Mothers	3,439	3,160	2,674	3,539

Notes: LBW = low birth weight. Limited covariates are child's sex, birth order, an indicator for whether birth weight was imputed, an indicator for whether the sibling endowment variable was constructed using imputed birth weight, and dummy variables for birth year and state. The full set of covariates contains the limited set of covariates plus mother's health, education, marital status, and age at birth; prenatal investments; and family income at the child's birth. Estimates in panel A are from ordinary least squares regressions that control for the full set of covariates, child's race, and number of siblings in the household. Estimates in panel B are from mother fixed-effects models that include the limited set of covariates. Estimates in panel C are from mother fixed-effects models that include the full set of covariates. Figures in parentheses are robust standard errors.

†$p \le .10$; *$p \le .05$; **$p \le .01$

gender and dummy variables for first-born. Controlling for time-invariant family-specific unobserved heterogeneity does not change the estimated effects of birth weight significantly. Some differences compared with the OLS estimates are that birth-weight effects on preschool attendance are found to be insignificant, although the presence of other LBW siblings in the household increases the likelihood of a child being taken for a well-baby visit.

Estimates in panel C of Table 3 are derived from MFE models that include additional covariates intended to control for sibling-specific differences in parental resources available for investing in children. These include mother's health, education, marital status, and age at child's birth; family income at child's birth; and prenatal investments. Estimates from this model are remarkably similar to those from the OLS model and the MFE model with limited covariates. Because observed sibling-specific heterogeneity does not affect our results, it is less likely that unobserved sibling-specific heterogeneity will bias our results.[16]

16. In analyses not shown here, we examined the extent of within-family variation in key predictors of birth weight, such as prenatal investments, and found evidence of significant variation. There is within-family variation in whether the mother went for a prenatal care visit in the first trimester, whether the mother smoked during pregnancy, and whether the mother drank alcohol during pregnancy for 46%, 35%, and 24% of the mothers, respectively. We estimated MFE models to examine what factors predicted within-family variation in prenatal investments. We found that within-family variation in prenatal investments is largely a function of mother's age, first-born, and marital status. The largest effects are seen for mother's age, suggesting that as mothers get older, they are more likely to

Table 4. Mother Fixed-Effects Estimates From Alternate Birth-Weight Specifications

Regression Specification	Ever Breast-fed	Well-Baby Visit in First Year	Full Dose of DPT and Oral Polio Vaccines	Attended Any Preschool Program
A. LBW Indicator				
LBW indicator	–0.040*	–0.015	–0.051**	–0.064†
	(0.019)	(0.019)	(0.017)	(0.034)
Number of LBW siblings present at the time of investment	0.008	0.049*	0.015	–0.032
	(0.017)	(0.020)	(0.019)	(0.033)
B. Birth-Weight Categories (ref. = birth weight < 2,500g)				
Birth weight = 2,500g–3,299g	0.033†	0.018	0.048**	0.060†
	(0.020)	(0.019)	(0.018)	(0.033)
Birth weight = 3,300g–3,699g	0.051*	0.025	0.053**	0.064†
	(0.021)	(0.021)	(0.018)	(0.035)
Birth weight ≥ 3,700g	0.051*	0.041†‡	0.052**	0.043
	(0.022)	(0.022)	(0.019)	(0.036)
Number of LBW siblings present at the time of investment	0.008	0.046*	0.016	–0.029
	(0.017)	(0.020)	(0.019)	(0.033)
Number of Observations	9,206	8,325	6,859	9,573
Number of Mothers	3,439	3,160	2,674	3,539

Notes: LBW = low birth weight. All estimates are from mother fixed-effects models that include the child's sex and birth order; mother's health, education, and marital status; prenatal investments; family income at the child's birth; an indicator for whether birth weight was imputed; an indicator for whether the sibling endowment variable was constructed using imputed birth weight as additional covariates; and dummy variables for birth year and state. Figures in parentheses are robust standard errors.

†$p \leq .10$; *$p \leq .05$; **$p \leq .01$

‡Significantly different from coefficient for birth weight between 2,500g–3,299g at $p \leq .05$.

The magnitude of the own-endowment effects reported in panel C is small but significant—being 1 kilogram heavier than a sibling is associated with a 3-percentage-point increase in the likelihood of being breast-fed relative to the lighter sibling and a similar increase in the relative likelihood of being taken for a well-baby visit and receiving DPT and oral polio vaccines in the first year of life. The sibling birth-weight effect is statistically significant only for well-baby visits and vaccinations. The presence of other LBW siblings in the household at the time of birth increases the likelihood of a well-baby visit by 4.3 percentage points and of full DPT and polio vaccination by 2.6 percentage points.

In Table 4, we report estimates from MFE models that include two alternate specifications for birth weight. First, panel A presents estimates from models that include a LBW indicator as our measure of own endowment. LBW children are significantly less likely to be breast-fed, receive DPT and oral polio vaccines, and attend a preschool program than their NBW siblings. The magnitude of these effects is sizable: the likelihood of receiving

make prenatal investments during pregnancy. These results suggest that factors that are important predictors of within-family variation in prenatal investments are already controlled for in our postnatal parental investment models. Nevertheless, we estimated alternate models for postnatal parental investments that included flexible functional forms for mother's age at birth (e.g., dummy variables for each age, and quadratic and cubic polynomials in age) and child's year of birth (dummy variables for each year, and quadratic and cubic polynomials). Our results did not change. Finally, we reestimated our postnatal investment models on the subsample of mothers whose prenatal investments did not vary across their children (46% of mothers) and found the same reinforcing pattern of results.

these investments is reduced from 4 to just over 6 percentage points. In percentage terms, the largest effects are seen for preschool attendance. LBW children are 11% less likely to attend preschool than their NBW siblings. The corresponding effects for breast-feeding and DPT and polio immunization are 8.8% and 5.3%, respectively. These patterns suggest that parents reinforce differences in children's endowments by making more health investments in their NBW children than in their LBW children. Finally, the LBW sibling effect is statistically significant only for well-baby visits; the presence of other LBW siblings in the household increases the likelihood of receiving these investments.

Panel B in Table 4 reports estimates from models that include indicators for various categories of the child's own birth weight: (1) 2,500–3,299 grams, (2) 3,300–3,699 grams, and (3) ≥ 3,700 grams. The objective of this analysis was to examine whether we observe reinforcing or compensating parental investment throughout the birth-weight distribution or just when comparing LBW children to their NBW siblings. In other words, among NBW siblings, does the sibling with a relatively higher birth weight receive more or less investment from the parents than the sibling with a lower birth weight? Testing for differences in coefficients on the birth-weight categories answers this question. We find that within the NBW range, children who are heavier at birth are more likely to receive health investments than their siblings who are relatively lighter at birth, as indicated by higher coefficients on the higher-birth-weight categories. However, these differences are not statistically significant at conventional levels. Higher birth weight does not confer any advantage in terms of preschool attendance among NBW siblings.

ROBUSTNESS CHECKS

In this section, we present results from robustness checks that address two potential concerns with the results presented above. The first concern is regarding sibling-specific heterogeneity that is not captured by MFE or observed sibling-specific covariates. In particular, one may be concerned that changes in parental ability, knowledge, or perhaps motivation to invest are not well-captured by our observed covariates. In Table 5, we report MFE estimates from the subsample that includes only siblings born no more than two years apart. Although unobserved measures of parental resources may vary significantly across children born many years apart, they are likely to exhibit less variation across closely spaced births.[17] The sample size for this analysis shrinks significantly, contributing to a decline in the statistical significance of some of the estimates. However, point estimates in Table 5 show that the effects of own birth weight and the number of other LBW siblings reported in the previous tables generally remain sizable even in this restricted sample.

The second concern is regarding whether birth-weight effects on parental investments seen in Table 4 are driven primarily by the child's medical need or ability to receive the investment or by a deliberate parental decision. For instance, LBW children may be less able to breast-feed simply because they may be placed in the neonatal intensive care unit for the initial few days after birth, when breast-feeding tends to be established. In Table 6, we report MFE estimates from the subsample that excludes children with very low birth weight, since these children are arguably the most likely to be either unfit to receive an investment or need it on medical grounds. We find that although the effects on breast-feeding decrease somewhat, the effects on well-baby visits, immunizations, and preschool

17. In our sample, the median change in family income for children born no more than two years apart was about 12%; about 10% of the mothers had a change in marital status (married versus other); and 3% of the mothers changed between "no high school," "high school diploma," or "at least some college." The corresponding numbers for children born more than two years apart are 44%, 20%, and 13%, respectively. Therefore, changes in key variables reflecting family circumstances are much less significant over a short period than over a longer period. Moreover, in our regressions, the coefficients on family income, maternal education, and marital status were not significant for any of the investments, which suggests that changes in these observed family circumstance variables did not influence differential postnatal parental investments across siblings.

Table 5. Mother Fixed-Effects Estimates Using Siblings Born at Least Two Years Apart

Regression Specification	Ever Breast-fed	Well-Baby Visit in First Year	Full Dose of DPT and Oral Polio Vaccines	Attended Any Preschool Program
A. Continuous Birth Weight				
Continuous birth weight (in 100s of grams)	0.003*	0.001	0.003*	0.003
	(0.002)	(0.002)	(0.001)	(0.003)
Number of LBW siblings present at the time of investment	0.021	0.089**	0.04	–0.017
	(0.024)	(0.034)	(0.026)	(0.040)
B. LBW Indicator				
LBW indicator	–0.039	0.005	–0.062*	–0.031
	(0.029)	(0.033)	(0.028)	(0.076)
Number of LBW siblings present at the time of investment	0.018	0.094*	0.026	–0.021
	(0.027)	(0.037)	(0.029)	(0.073)
C. Birth-Weight Categories (ref. = < 2,500g)				
Birth weight = 2,500g–3,299g	0.032	0.007	0.062*	0.024
	(0.030)	(0.035)	(0.029)	(0.074)
Birth weight = 3,300g–3,699g	0.061†	–0.005	0.051†	0.059
	(0.031)	(0.036)	(0.028)	(0.075)
Birth weight ≥ 3,700g	0.039	0.016	0.067*	0.043
	(0.034)	(0.037)	(0.031)	(0.078)
Number of LBW siblings present at the time of investment	0.019	0.090*	0.027	–0.022
	(0.027)	(0.037)	(0.029)	(0.071)
Number of Observations	3,321	2,898	2,761	3,547
Number of Mothers	1,370	1,220	1,150	1,439

Notes: LBW = low birth weight. All estimates are from mother fixed-effects models that include the full set of covariates. Figures in parentheses are robust standard errors.

†$p \le .10$; *$p \le .05$; **$p \le .01$

attendance remain generally stable. This suggests that although some of the effect of birth weight on breast-feeding may not be a deliberate parental decision, the estimated effects on other early childhood investments appear to be largely the result of deliberate parental decisions to reinforce birth-weight differences.

DISCUSSION AND CONCLUSIONS

In this article, we examined whether parental investment responds to children's endowments. We tested whether parents reinforce initial differences in their children's endowments, as measured by their birth weight, by making more health and educational investments in better-endowed children or instead compensate for initial differences in endowments by making relatively more of such investments in their less-endowed children. We used birth-weight differences across siblings as our measure of endowment differences because birth weight is easily observed by parents, is a well-known correlate of short- and long-term outcomes by both the medical community and parents themselves, and is revealed at birth. Therefore, our measure of endowments is plausibly the same as a measure of endowments that parents themselves might employ. We examined child-specific investments in infancy and early childhood: breast-feeding, immunizations, well-baby visits, and preschool attendance.

Table 6. Birth-Weight Estimates From the Sample That Excludes Children With Very Low Birth Weight

Regression Specification	Ever Breast-fed	Well-Baby Visit in First Year	Full Dose of DPT and Oral Polio Vaccines	Attended Any Preschool Program
A. Continuous Birth Weight				
Continuous birth weight (in 100s of grams)	0.003**	0.002*	0.002†	0
	(0.001)	(0.001)	(0.001)	(0.001)
Number of LBW siblings present at the time of investment	0.011	0.038*	0.018	0.002
	(0.016)	(0.019)	(0.015)	(0.024)
B. LBW indicator				
LBW indicator	–0.028	–0.011	–0.043*	–0.065†
	(0.021)	(0.020)	(0.018)	(0.034)
Number of LBW siblings present at the time of investment	0.009	0.042*	0.003	–0.042
	(0.019)	(0.021)	(0.018)	(0.033)
C. Birth-Weight Categories (ref. = birth weight < 2,500g)				
Birth weight = 2,500g–3,299g[a]	0.022	0.015	0.041*	0.061†
	(0.021)	(0.020)	(0.018)	(0.034)
Birth weight = 3,300g–3,699g	0.037†	0.022	0.045*	0.063†
	(0.022)	(0.022)	(0.018)	(0.035)
Birth weight ≥ 3,700g	0.038	0.038†	0.043*	0.042
	(0.023)	(0.022)	(0.020)	(0.037)
Number of LBW siblings present at the time of investment	0.01	0.039†	0.003	–0.039
	(0.019)	(0.021)	(0.018)	(0.033)
Number of Observations	9,056	8,200	6,737	9,424
Number of Mothers	3,396	3,123	2,634	3,496

Notes: LBW = low birth weight. All estimates are from mother fixed-effects models that include the full set of covariates. Figures in parentheses are robust standard errors.

†$p \leq .10$; *$p \leq .05$; **$p \leq .01$

Consistent with most prior literature, our results suggest that parents generally engage in reinforcing behavior in response to differences in their children's endowments. Most notably, better-endowed children, as measured by higher birth weight, were significantly more likely than their lower-birth-weight siblings to be breast-fed, to be taken for well-baby visits, to receive the full dose of DPT and oral polio vaccines, and to attend a preschool. These results hold even after we include controls for sibling-specific heterogeneity in parental resources. Economic models of intrahousehold resource allocation suggest that such behavior might be the result of unequal concern across children[18] and/or parental preference toward efficiency as opposed to equity.

The reinforcing effects were most pronounced for comparisons of LBW children with their NBW siblings: NBW children were 5%–11% more likely to receive early childhood parental investments. However, there was no differential advantage of being heavier at birth among siblings who were NBW. This is not surprising if we believe that parents do not perceive birth weight to be a signal for the child's endowment after she crosses the NBW threshold. Consequently, parents may be less likely to make differential investments

18. One recent study found that parental affection is greater for the child with a higher IQ relative to his or her sibling (Kim 2005).

across their NBW children but may invest differently across their LBW and NBW children. Another possible explanation for this finding is that medical reasons might limit the ability of children with very low birth weight to receive certain investments, although we found limited evidence for this hypothesis.

With regard to the effect of other siblings' endowments, the presence of LBW siblings in the household at the time of investment significantly increases the likelihood of certain investments, such as well-baby visits and immunizations. This pattern is consistent with at least two explanations. First, parents might be more likely to make health investments in subsequent children if previous children were born with LBW. This is because many LBW children receive health or developmental interventions, a large component of which is often parental education on how to promote child development (Hebbeler et al. 2007). Hence, we would expect that families with a previous LBW child would, on average, have more information about actions they could take to improve child outcomes than families with no LBW children. Second, if parents have a strong preference for efficiency rather than equity, and the returns to health investments increase with the initial endowment, then parents might choose to reinforce endowment differences.

In models not shown here, we also examined whether the effects of birth weight on parental investments in infancy and early childhood vary by socioeconomic characteristics, such as maternal education and family income. Socioeconomic factors might influence parental preferences for equity versus efficiency as well as parents' ability to make compensatory investments. However, we found no clear and statistically significant patterns in the endowment-investment relationship by maternal education and family income.

Our findings have important implications for studies that examine the short- and long-term effects of birth weight. Our results show that parents respond to birth-weight differences of their children by making more investments in their NBW children relative to their LBW children. The investments we examined are early-life health and developmental investments that have well-known relationships with child and adult outcomes and are among the most widely recommended objectives of public health campaigns (e.g., U.S. DHHS 2000; and World Health Organization 2008).

Our findings are important for assessing the welfare implications of policies that target birth weight. Policies that succeed in increasing birth weight might improve long-run outcomes not only because birth weight itself matters for later outcomes but also because higher birth weight induces more parental investment. The positive association between birth weight and parental investments, in turn, implies that cost-benefit analyses of policies that seek to increase birth weight must account for both the direct costs and benefits of such policies and the costs and benefits of parental investments that these policies indirectly induce.

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