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Health Endowments and Parental Investments in Infancy and Early Childhood

ASHLESHA DATAR
M. REBECCA KILBURN
DAVID S. LOUGHRAN

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Ashlesha Datar, M. Rebecca Kilburn, David S. Loughran

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Abstract

This paper tests whether parents reinforce or compensate for child endowments. We employ birth weight as a proxy for endowments and estimate how the difference in birth weight across siblings impacts specific parental investments, including breastfeeding initiation and duration, well-baby visits, immunizations, preschool attendance, and kindergarten entry age. We also examine whether parental investment in a child is impacted by her siblings' endowments. Our results indicate that heavier birth weight children receive higher levels of most parental investment than their lower birth weight siblings suggesting that parental investments in infancy and early childhood reinforce differences in endowments. In one exception, we find weak evidence that lower birth weight children enter kindergarten slightly later than their normal birth weight siblings, which could be interpreted as a compensating parental investment. Presence of a low birth weight sibling in the household increases the likelihood of investments such as well-baby visits and immunizations.

1. Introduction

Two of the seminal models of how parents allocate resources to children with different initial endowments generate opposite predictions. 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 marginal returns to investing are higher for better endowed children than they are for lower 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 lower endowment 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 effectiveness of policies aimed at affecting initial endowments, 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 (Griliches, 1979; Rosenzweig and Schultz, 1982; Rosenzweig and Wolpin, 1988; Pitt, Rosenzweig & Hassan, 1990; Behrman, Rosenzweig and Taubman, 1994). 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 are contingent on functional form and other identifying assumptions.

In this paper, we employ data from the National Longitudinal Survey of Youth – Child (NSLY-C) file to assess whether parents compensate for or reinforce endowments using an observable proxy for initial endowment—birth weight—and direct measures of parental investments, including breastfeeding initiation and duration, well-baby visits, immunizations, preschool attendance, and kindergarten entry age. Specifically, we examine the correlation between sibling differences in birth weight and differences in parental investments those siblings receive.

The principal innovation in our approach is in the direct measurement of both endowments and parental investments. 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 a measure of endowments parents themselves might employ. Likewise, we directly measure how parental investment varies with endowments rather than how some consequence of parental investment (e.g., educational attainment) varies with endowments. Our approach also has the advantage that it does not rely on having a sample of twins.[2] Resource allocation decisions in families with twins may be quite different than in families with non-twin 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,

[1] Aylward, et al. (1989); MacDorman and Atkinson (1999); McCormick (1985); Paneth (1995); Brooks, et al. (2001); Lee and Barratt (1993); Strauss (2000); Richards, et al. (2001); Matte, et al. (2001); McCormick, et al. (1990); Corman and Chiakind (1993); Currie and Hyson (1999); Conley and Bennett (2000); Boardman, et al (2002); Behrman and Rosenzweig (2004); Almond, Chay and Lee (2005); Black, Devereux and Salvanes (2005).
[2] Examples of studies using twins data include Behrman, Rosenzweig and Taubman (1994) and Behrman, Pollak and Taubman (1982).

as measured by the number of LBW siblings present in the household at the time of investment, might impact investments that parents make in a given child.

The results we present in this paper have direct implications for the large empirical literature that seeks to estimate the impact of birth weight on later outcomes. Even with controls for variation in genetic endowments, the correlation between birth weight and later outcomes might not constitute a causal estimate if parental investment responds to birth weight. For example, the correlation between birth weight and later outcomes could understate the true causal impact of birth weight if parents compensate for low birth weight with high levels of parental investment. The opposite would be true if parents reinforce differences in birth weight.

Our results indicate that heavier birth weight children generally receive higher levels of parental investment at very young ages than do their lower birth weight siblings, suggesting that parental investments in infancy and early childhood generally reinforce differences in endowments. In one exception, we find weak evidence that lower birth weight children enter kindergarten slightly later than their normal birth weight siblings, which may be interpreted as compensating behavior. With regards 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 of investments such as well-baby visits and immunizations. Our findings are robust to the addition of an extensive set of controls for variation in family circumstance and mother's health at the time of each sibling's birth, and prenatal investments in each sibling.

2. Intra-household Allocation Decisions

Economic models of intrahousehold resource allocations 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). In particular, two special cases of these models have received attention. The wealth model, proposed by Becker and Tomes (1976), assumes that parents are concerned with maximizing the total wealth (earnings plus wealth) of each child. To achieve that objective, parents invest in the human capital of each child until the marginal rate of return to education is driven down to the rate of return on financial assets. Subsequently, parents allocate additional resources in the form of transfers (gifts and bequests) so as 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 depends on the properties of the earnings function. If a greater endowment implies a greater (smaller) marginal return to investment, then parents adopt a reinforcing (compensating) strategy.

In contrast, the separable earnings-transfer (SET) model (Behrman, Pollak and Taubman, 1982) assumes that parents care about the distribution of earnings across their children and not just the distribution of total wealth. In this model, parents' strategy depends both on their aversion to inequality as well as the properties of the earnings function. The SET model predicts that if equity concerns were strong enough and if the marginal returns to investment were greater for children with greater genetic endowments then parents would adopt a compensating strategy. Since neither theory 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 U.S. data 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 by estimating the effect of a one-standard deviation difference in IQ on schooling attainments for siblings, dizygotic (DZ) twins and monozygotic (MZ) twins.[3] He found that the effects of IQ were significantly lower within sibling pairs compared to across individuals. Furthermore, the IQ effects were smaller the more alike the siblings, in terms of age, gender and genetics, suggesting that families compensate for genetic endowments, measured by IQ. Behrman, Pollak and Taubman (1982) used adult, male twin data from the U.S. to estimate parental preference parameters from a SET model, assuming specific functional forms for utility and earnings. Their estimates suggest that parents may slightly reinforce endowment differences across their children but that they have substantial concern about the distribution of their children's earnings.[4] More recently, Behrman, Rosenzweig and Taubman (1994) estimated a variance components model of returns to schooling using data on MZ and DZ twins from Minnesota. They found that schooling attainment was higher for the twin who had a greater endowment, which they concluded suggests reinforcing behavior. However, the study did not examine other investments besides schooling allocation decisions, which are likely to be made in part by the individuals themselves. Moreover, schooling attainment is as much an outcome as it is a measure of parental investment.

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[3] MZ twins share identical genetic endowments and pre-natal environments and so differences in birth weight between identical twins are thought to be attributable solely to differences in nutrition each twin receives in-utero. DZ twins share the same in-utero environment, but have different genetic endowments.

[4] Behrman, Pollak and Taubman (1995) point out that one of the concerns with this study is that even though the theoretical model assumes that parents provide the resources for the education of their children, the empirical analysis was based on a sample of veterans, many of whom were eligible for GI bill benefits. The availability of GI Bill benefits would make the data look more as if it they were drawn from a world where the marginal returns to education equaled the market rate of interest since individuals eligible for GI Bill benefits would presumably decide whether to obtain more or less schooling based on their own benefits and costs. Thus, the failure to take account of the GI Bill may bias these results in favor of the wealth model.

Therefore, it is unclear whether the results reflect parents' reinforcing strategy or merely capture the returns to innate ability.

Outside the United States, a number of studies have examined how intrahousehold resource allocations respond to changes in economic conditions and genetic endowments within the context of developing countries. These studies generally find evidence of reinforcing behavior in parental investments. Rosenzweig and Schultz (1982) found that differences in male-female child survival rates in rural India could be explained by differences in expected earnings opportunities. Since males traditionally have higher returns in the labor market they receive a larger share of family resources and have a greater propensity to survive compared to females. They infer from these results that parental investment reinforces endowments, in this case being male. Rosenzweig and Wolpin (1988), using data on Colombian households, also found that boys were more likely to be vaccinated than were girls. Two other studies examined the effects of health inputs on subsequent outcomes and argue that the effects of health inputs may be biased due to intrafamily heterogeneity resulting from parental response to endowment differences across siblings. These studies constructed a measure of child-specific health endowment using residuals from the health production function and found that siblings with greater health endowments receive more calories (Pitt, Rosenzweig & Hassan, 1990), are more likely to be followed by a closely spaced birth, and are more likely to be breastfed (Rosenzweig & Wolpin, 1988). However, estimates from studies using this "residual approach" depend critically on there being no omitted endogenous inputs in the health production function. Moreover, the estimating procedures rely on assumptions about the orthogonality between local area variables, such as prices and landholdings, and unobserved family endowments.

Most recently, Ayalew (2005) examined how parental investments such as school enrollment and provision of supplementary food responded to differences in health and education endowments of their children using panel data on households from Ethiopia. The study used the residual approach discussed above to construct a measure of health endowment but used scores from an ability test as the measure of educational endowment. Estimates from household fixed effect models showed that inherently healthy children were more likely to be sent to school but were less likely to receive supplementary food. These findings suggest that parents follow a reinforcing strategy in educational investments but follow a compensating strategy in allocating health inputs. It remains to be seen whether these investment patterns are observed in developed countries.

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 health 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 number of LBW siblings present in the household at the time of investment, on investments that parents make in a given child.

3. Empirical Approach

Our empirical approach is based on an underlying economic model, similar in spirit to that employed by Behrman, Rosenzweig and Taubman (1994), where health, educational, and other types of parental investments (*I*) made at a particular point in time in child *i* belonging to family *f*, depend upon the child's own health endowment (e_i), the health endowments of other siblings present in the family at the time of investment in child *i* (e_{-if}), and other child and family characteristics at that time (X_{if}) that influence parental investments (e.g. tastes, resources). These investments may take the form of monetary or time investments from the parents. A linear econometric specification of this model might take the form:

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

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 post-natal) 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 equation (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. Prior studies have mostly focused on estimating β_1 and, for several reasons, have not been able to estimate β_2. First, their treatment of endowments as unobservable to the researcher did not allow for estimation of β_2. Second, studies that used twins data could not estimate β_2 due to lack of variation in e_{-if} across the twins.[5] Our approach of using an

[5] Since twins are born at the same time the endowments of other siblings present in the household at the time of investment (e_{-if}) cannot vary across the twins.

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 since 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 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, omission of e_{-if} in an estimation of equation (1) would bias the estimate of β_1.

In order to examine within-family differences in parental investments, the model in equation (1) may be rewritten as follows:

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

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. The parameter β_1 now has a within-family interpretation and measures whether parents invest more or less in children with higher endowments compared to children with lower endowments. A positive sign on β_1 would indicate that parental investments are reinforcing (i.e. parents make relatively more investments in children with higher endowments). A negative sign on β_1 would indicate that parental investments are compensating (i.e. parents make relatively more investments 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}). e_{-if} can vary across siblings as a result of the arrival in the household of other siblings with different endowments. Presence of other less-endowed siblings in the household might increase or decrease the level of parental investment in a child. A positive (negative) sign on β_2 would indicate that parents make more investments in children who have siblings with higher (lower) endowments present in the household at the time of the investment. For example, if a LBW sibling who demands expensive and unavoidable therapies (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. We call this behavior a "learning effect." This may encourage them to invest more in child A relative to child B, other things 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 due to parents' desire to either reinforce or compensate for endowment differences. In the context of the example described above, child A might receive more (less) investment than child B if parents seek to reinforce (compensate) endowment differences.

We implement the model in equation (2) by using the child's own birth weight (BW_i) and the number of low birth weight (LBW) (i.e., $\leq 2500g$) siblings present in the household at time the time of the investment ($NLBW_{-i}$), excluding sibling i, to capture the own-endowment and siblings' endowment, respectively. Equation (2) may now be rewritten as follows:

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

We choose to use $NLBW_{-i}$ as our measure of siblings' endowment rather than, for example, the mean birth weight of all other siblings in the family, in order to deal with the fact that some investments are made in some 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 investment in that child at that point in time is unaffected by the possibility that that child's future siblings could be LBW.

We estimate the model in Equation (3) using mother fixed-effects (MFE). We include a number of child-specific controls, including family income, mother's education, marital status, mother's age at birth, and a proxy for mother's health, all measured at the time of birth. 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 as well as indicators for the child's gender and birth order.   This extensive set of child-specific covariates is intended to control for child-specific factors potentially correlated with both parental investment and birth weight (φ_i). We admit, however, that the model could omit some child-specific factors that could bias our estimates of β_1 and β_2.

We estimate the MFE models using three alternate specifications for own-birth weight – (1) continuous birth weight, (2) indicator for LBW, and (3) indicators for three birth weight categories (2500-3299 grams, 3300-3699 grams, and ≥3700 grams). This later specification will allow us to examine whether investment responses to birth weight differences are only 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 investment from the parents compared to the sibling with a lower birth weight, conditioning on normal birth weight siblings.

Although our hypotheses about the investment-endowment relationship are motivated by the theoretical models of Becker and Tomes (1976) and Behrman, Pollak and Taubman (1982), it is possible that compensating or reinforcing parental investment could arise for other reasons. For example, investing more in a LBW child compared to her normal birth weight sibling may be the result of unavoidable factors such as the need to make life-saving medical expenditures. On the other hand, despite wanting to invest more in the LBW child parents may be unable to do so simply due to the child's inability to receive the investment. For example, LBW babies who need to be placed in an incubator for a period of time may be too fragile to be breastfed. Comparing estimates from models with a LBW indicator to estimates from those with indicators for normal birth weight 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 normal birth weight siblings then there is less likelihood that it is the result of these remedial (or compulsory) interventions.

4. 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.[6] 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 1986-2000 with birth weight information available for at least one child. Next, we only keep children for whom there is information on at least one of the parental investments examined in the paper. 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

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

particular parental investment being examined and again restrict our sample to families with at least two children with information on that parental investment.[7]

We exploit four key features of the NLSY-C for the purposes of this paper. 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, and 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 deliberately focus on investments in early childhood since investments observed during this period are more obviously under the control of the parents in contrast to many later investments, such as schooling, which are under the control of both parents and child. Our analyses consider the following investments:

(1) Initiation and duration (weeks) of breastfeeding

(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

(4) Whether the child attended preschool (including Head Start)

(5) Kindergarten entrance age (KEA) in months, and whether the child was held back from entering kindergarten even after he or she was eligible

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[7] Our largest samples are for breastfeeding, well-baby visits, and preschool attendance. Data on immunizations were only collected in the first three waves of the NLSY resulting in smaller sample sizes for these regressions. Samples for kindergarten entrance age and kindergarten delay are smaller because we must impute kindergarten entrance age and, in the case of kindergarten delay, we drop observations for which we could not obtain data on state kindergarten entry age laws when the child was 4 or 5 years old.

Health investments during a child's first year such as breastfeeding, well-baby visits and immunizations are all highly recommended by the American Academy of Pediatrics (AAP, 2000) and are also included as objectives in the Department of Health and Human Services' (DHHS) Healthy People 2010 (DHHS, 2000). Attending high quality preschool programs, including Head Start has also been shown to have positive and lasting effects on outcomes in childhood and adulthood, especially for children from disadvantaged backgrounds (Karoly et al, 2006; Currie and Thomas, 1995). 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. Finally, a higher age at kindergarten entry has been shown to have a positive effect on children's academic achievement at school entry and on their test score gains in the early school years (Datar, 2006). It has become increasingly common for parents to delay kindergarten entry for their children (beyond the minimum age permitted in their state) with the thought that an extra year's maturity will help them succeed in school (Zill, Loomis & West, 1997).

While most of the parental investment variables are directly reported in the NLSY-C, KEA is not and must be imputed using information on age, grade and grade repetitions. The imputation of KEA is described in Appendix A. 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 her own health endowment and use the number of LBW siblings present in the household at the time of investment as our measure of sibling endowment.[8] As mentioned earlier, birth weight information for each child in the NLSY-

[8] Birth weight information was missing for 9 percent of our sample. We replaced missing birth weight data using 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, birth order dummies, mother's height, mother's age at child's birth,

C is reported by the mother retrospectively. 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. A recent study in the United Kingdom which compared mother-reported birth weight with registration data on birth weight for the same children found that 82 percent of mothers reported their baby's weight within 30 grams (~one ounce) of the registration weight and 92 percent 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 the Tate et al (2005) study was about 9 months.

The mean (standard deviation) birth weight in our sample is 3,300 (579) grams (Table 1). Just over one percent of our sample was born at <1,500 grams, about 8 percent between 1,501-2,499 grams, 39 percent between 2,500-3,300 grams, 21 percent between 3,300-3,600 grams, and 31 percent at >3,600 grams.[9]

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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 non-missing birth weight, we obtain nearly identical estimates.

[9] The rate of LBW in the United States during our sample period ranged from 6.7-7.6 percent during our sample period (CDC, 2002)

There exists a substantial amount of within-family variation in birth weight and parental investments in our data. The top panel in Table 2 reports the percent of total variance explained by within-family variation for birth weight and parental investments defined as continuous variables. The simple one-way analysis of variance estimates that the family-effect accounts for about 51 percent 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 percent less than the standard deviation of birth weight in the overall sample (579 grams). There is considerable variation within families in the weeks of breastfeeding and in kindergarten entry age. For dichotomous variables, the bottom panel in Table 2 reports the percentage of families whose children differ along that specific dimension. About 15 percent of the families in this sample had at least one child who was LBW and at least one child of normal birth weight. Significant within-family variation also exists in parental investments. For example, 37 percent 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 percent.

5. Results

Table 3 reports estimates of the effect of own-endowment and sibling-endowment on parental investments from MFE models that use a continuous specification for birth weight. Estimates in the top panel are from models that include only a limited set of covariates—child's gender and dummies for birth order. The estimates indicate that higher birth weight siblings are significantly more likely to be breastfed, taken for well-baby visits in the first year, and receive the full course of DPT/oral polio vaccines than are their lower birth weight siblings. However, there are no significant differences between heavier and lighter born siblings in the duration of breastfeeding, preschool attendance or KEA. The presence of LBW siblings in the household increases the likelihood of a child being taken for a well-baby visit and receiving vaccinations.

Estimates in the bottom panel of Table 3 are derived from models that employ additional covariates intended to control for differences in family circumstances at birth and prenatal investments between siblings. 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 model with limited covariates. The magnitude of the own-endowment effects is small but significant—being 1 kg heavier than your sibling is associated with a 3 percentage point increase in the likelihood of being breastfed relative to your lighter sibling and a similar increase in the relative likelihood of being taken for a well-baby visit and receiving DPT/oral polio vaccines in the first year of life. The sibling-birth weight effect is statistically significant only for well-baby visits and vaccinations. Presence of LBW siblings in the household at the time of birth increases the likelihood of a well-baby visit by almost 4 percentage points and of vaccination by 2.8 percentage points.

In Table 4, we report estimates from models that include two alternate specifications for birth weight. The top panel presents estimates from models that include a LBW indicator as the measure of own-endowment, and the bottom panel presents estimates that include categorical birth weight indicators. We find that LBW children are significantly less likely to be breastfed, receive DPT/oral polio vaccines, and attend a preschool program compared to their normal birth weight siblings. The magnitude of these effects is sizeable, ranging from just under 4 percentage points to a 6 percentage point reduction in the likelihood of receiving these investments. These patterns suggest that parents reinforce differences in health endowments of their children by making more health investments in their normal birth weight children compared to their LBW children. KEA is weakly associated with LBW, although parents' investment strategy appears to be compensating here in contrast to their strategy for investments in infancy. LBW children enter kindergarten about a month older, on average, compared to their normal birth weight siblings. Estimates from models that used delayed entry instead of age of entry as the dependent variable indicate a similar association although the estimates are not statistically significant.[10] These findings indicate that parents slightly compensate for the low endowment of their children by sending them to school at an older age. Finally, the LBW sibling effect is statistically significant only for well-baby visits and suggests that presence of LBW siblings in the household increases the likelihood of receiving these investments.

The lower panel in Table 4 reports estimates from models that include indicators for various categories of the child's own birth weight: (1) 2500-3299 grams, (2) 3300-3699 grams, and (3) greater than or equal to 3700 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 normal birth weight siblings. In other words, even after conditioning on normal birth weight siblings, does the sibling with a relatively higher birth weight receive more or less investment from the parents compared to the sibling with a lower birth weight. Testing for differences in coefficients on the birth weight categories answers this question. We find that a child weighing at least 3700 grams is significantly more likely to be breastfed than a child weighing 2500-3299 grams, even though both siblings are in the normal birth weight range. One of the implications of this finding is that the reduced likelihood of a LBW child being breastfed compared to his or her normal birth weight sibling is not simply attributable to medical reasons that could prevent a LBW child from being breastfed

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10 As discussed in Appendix A, our measure of KEA may suffer from measurement error, which could bias these estimates downward.

(e.g. being in an incubator or intensive care unit, or inability to suckle).[11] Similar reinforcing patterns are seen even when comparisons are made within normal birth weight siblings.

We find similar patterns for other investments such as well-baby visits and vaccinations, however the differences in coefficients across the birth weight categories were statistically significant for well-baby visits only. Birth weight differences were not associated with significant differences in other investments such as preschool attendance and KEA among normal birth weight siblings.

While the MFE models eliminate all unobserved family factors that are shared by all siblings and also control for maternal health and observed family circumstances around the time of birth, there may be other child-specific unobserved factors that vary across siblings but remain in the error term. In Table 5 we present results from a robustness check that examines whether our results hold even when we restrict our sample to siblings born at most 2 years apart. The idea here is that unobserved measures of family circumstance are likely to remain relatively constant across closely spaced births but may vary significantly across children born many years apart. The sample sizes for this analysis shrink significantly, though, 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 LBW siblings reported in the previous tables generally remain sizeable even in this restricted sample.

6. Conclusions

In this paper we examined whether parental investment responds to health endowments. We tested whether parents reinforce initial differences in health endowments of their children, as

[11] The benefits of breastfeeding premature and LBW infants are well documented in the medical literature (see Smith et al, 2003). Therefore, it is unlikely that medical safety may be a reason for differences in breastfeeding between LBW and normal birth weight siblings.

measured by their birth weight, by making more health and educational investments in better-endowed children or whether they 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 since these outcomes are easily observed by parents, have been shown to be correlated with short and long-term outcomes, and are revealed at birth—a time *before* most deliberate investments in children are made. Therefore, our measure of endowments is plausibly the same as a measure of endowments parents themselves might employ. We examined child-specific investments in infancy and early childhood such as breastfeeding initiation and duration, immunization, well-baby visits, preschool attendance, and KEA.

Consistent with most prior literature, our results suggest that parents generally engage in reinforcing behavior in response to differences in their children's health endowments. Most notably, better-endowed children, as measured by higher birth weight, were significantly more likely to be breastfed, taken for well-baby visits, receive the full dose of DPT/oral polio vaccines, and attend a preschool program compared to their less-endowed siblings. These results hold even after we include controls for maternal health and family circumstance around the time of birth, or restrict our sample to closely-spaced births. In one exception, we found weak evidence that lower birth weight children entered kindergarten slightly later than their normal birth weight siblings, which may be interpreted as compensating behavior. With regards to the effect of other siblings' endowments, we find that 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 may be the result of either a learning effect or parents' desire to reinforce endowment differences.

Why might parental investments reinforce endowments? One explanation comes from economic theory, which suggests that if the returns to early childhood investments are greater for better-endowed children then they are likely to receive more of these investments compared to their less-endowed siblings (e.g., Becker and Tomes, 1976). Similar arguments have also been proposed by evolutionary biologists who argue that parents tend to divide resources equally unless the marginal benefit from one unit is larger for one child compared to the other (Trivers, 1972). An alternative explanation comes from psychology. Researchers have documented that parental favoritism is common and is correlated with child characteristics including birth order and gender (Harris and Howard, 1984). If parental affection is positively correlated with the child's endowments, such as birth weight, then one might expect them to make reinforcing investments (Kim, 2005).

Regardless of what drives this reinforcing behavior, our results suggest that parental response to children's health endowments may widen disparities in children's outcomes within families. On the other hand, public programs that succeed in raising birth weight might leverage even greater long-term gains if parents respond to higher birth weight by increasing their own investments in their children.

Appendix A: Computation of Kindergarten Entry Age (KEA) and Delay

In principle, *KEA* can be identified with information on current age, grade attended, and number of grades repeated according to the following identity:

$$KEA_i = Age_{it} - (Grade_{it} + Repeats_{it}) \times 12 \quad (1)$$

where KEA_i and Age_{it} are measured in months, Age_{it} is measured when the child first enters $Grade_{it}$ (and we assume the school year starts in the same month every year), and $Repeats_{it}$ is the number of repeated grades between when the child first entered kindergarten and when the child entered $Grade_{it}$.[12] We encountered a number of difficulties in computing *KEA*, however, in the NLSY-C. First, many mothers are interviewed during the summer months, and while these mothers are instructed to report the grade their child just completed rather than the grade their child is about to enter, it is clear from the data that there is substantial misreporting of grade in those months. Second, and more importantly, grade repetition in the NLSY-C is poorly reported. The survey first asked mothers to report which grades, if any, their child repeated in 1994. Prior to 1994, mothers were asked only if their child had ever repeated a grade. For many children, then, we simply do not know which grades they repeated. Additionally, grade repetition reports collected in 1994, 1996, 1998, and 2000 are internally inconsistent for a substantial fraction of children. For example, a mother who reports in 1994 that her child repeated first grade, may not report so when asked in 1996, or a mother who reports in 1992 that her child ever repeated a grade, may not indicate which grade this child repeated when asked in 1994.[13] Despite these measurement issues, we impute *KEA* according to Equation (1) for the

[12] To be precise, skipped grades should also be included in Equation (1). The NLSY does not report skipped grades and so we abstract from that issue here.
[13] An additional complication is that the grade repetition questions were asked for children in a select age range and that age range has changed over the course of the survey.

entire sample. To do this, we first impute *Repeats* for 565 children with missing data on which grade the child repeated.[14] We then apply the identity in Equation (1) assuming the school year starts on September 1. The resulting distribution of kindergarten entrance ages compares favorably to the distribution of kindergarten entrances ages directly reported in the Early Childhood Longitudinal Study-Kindergarten Class (ECLS-K), a nationally representative sample of children in kindergarten.

An awkward aspect of calculating *KEA* in this manner is that, for 46 percent of the sample, *KEA* varies over time for the same child, which, obviously, is not possible. Rather than arbitrarily taking one estimate of *KEA* per child, we allow *KEA* to vary and include a control for whether *KEA* varies at the child-level. We also include in all of our regressions a control for whether *Repeats* was imputed and, to partially account for potential mismeasurement of $Grade_{ijt}$, controls for interview month ($Month_{ijt}$).

We also construct a variable indicating whether the child was held out of school for an additional year using information on the actual age they entered kindergarten and the earliest age at which they were eligible to enter kindergarten based on the entry age laws in their state. We obtain data on kindergarten entrance age policies for the years 1975-2000 from the Education Commission of the States, the Council of Chief State Education Officers, or from individual state education offices. We can match state-level cut-off months to children in the year they turned age five for 80 percent of the sample. For another eight percent of the sample, we can match state-level cut-off months to children in the years they turn four or six. Eleven percent of our sample lives in a state in which local school boards determine age-eligibility for kindergarten and

[14] We impute grade repeated with a Poisson model using age, grade, PIAT scores, gender, race, birth order, number of siblings, mother's age at birth, mother's AFQT, poverty status at birth, marital status, and region dummies as explanatory variables. The sample is restricted as in Table 1, but uses only the last observation for each child. Imputed values for grade repeated are rounded to the nearest integer.

we could not determine the state of residence for one percent of our sample. For the analysis of KEA delay decisions we drop observations with missing KEA policy data.

Tables

Table 1: Descriptive Statistics

Variable	Mean	Std. Dev.	Obs
Early Childhood Parental Investments			
Ever breastfed	0.45	0.50	9413
No. of weeks breastfed, conditional on breastfeeding	19.92	20.53	4200
Received well-baby care in the first year	0.92	0.28	8803
Received complete dose of DPT/oral Polio vaccine	0.94	0.24	7441
Attended a preschool program	0.55	0.50	9678
Kindergarten entry age (months)	64.26	7.29	6665
Delayed entry into kindergarten	0.09	0.29	5740
Child Characteristics			
Birth weight (100g)	32.98	5.79	10000
Birth weight <2500g	0.09	0.27	10000
Birth weight: 2500-3299g	0.39	0.47	10000
Birth weight: 3300-3599g	0.21	0.39	10000
Birth weight >=3600g	0.31	0.44	10000
Female	0.49	0.50	10000
First born	0.37	0.48	10000
White	0.52	0.50	10000
Black	0.28	0.45	10000
Hispanic	0.20	0.40	10000
Maternal and Family Characteristics			
Mother married at child’s birth	0.67	0.47	10000
Mother's education at child’s birth			
Less than high school	0.28	0.45	10000
High school	0.72	0.45	10000
Annual family income at child’s birth (thousands of dollars)	28.91	72.33	10000
Mother’s age at child’s birth	24.83	5.52	10000
Mother had health condition that limits work	0.03	0.17	10000
Time of first prenatal care use			
Never	0.01	0.12	10000
In first trimester	0.72	0.45	10000
In second trimester	0.13	0.33	10000
In third trimester	0.14	0.34	10000
Frequency of alcohol use during pregnancy			
Never	0.72	0.45	10000
Less than once a month	0.14	0.34	10000
Once a month or more	0.14	0.35	10000
Frequency of cigarette use during pregnancy			
Never	0.74	0.44	10000
Less than one pack a day	0.18	0.39	10000
One or more packs a day	0.08	0.27	10000

Table 2: Within Family Variation in Birth Weight and Parental Investments

Continuous Variables	Percent of total variance explained by within-family variation
Birth weight in 100s of grams	51.4
No. of weeks breastfed, conditional on initiation	50.4
Kindergarten entry age in months	38.7

Dichotomous Variables	Percent of families with within-variation
Low birth weight (<2500g)	15.1
Ever breastfed	22.5
Received well-baby care in the first year	15.0
Received complete dose of DPT/oral Polio vaccine	9.8
Attended a preschool program	36.9
Delayed entry into kindergarten	15.6

Table 3: Mother Fixed-Effect Estimates of the Effects of Own- and Sibling- Birth Weight on Early Childhood Parental Investments

	Ever Breastfed	Weeks Breastfed [a]	Well baby visit in first year	Full dose of DPT/oral polio vaccines	Attended any preschool program	Kindergarten entry age in months	Whether parents delayed kindergarten entry
Model with limited covariates							
Continuous birth weight (in 100s of grams)	0.004***	0.065	0.003***	0.003***	0.001	-0.023	-0.001
	[0.001]	[0.094]	[0.001]	[0.001]	[0.001]	[0.024]	[0.001]
No. of LBW siblings present at the time of investment	0.014	-0.604	0.040***	0.028**	0.019	-0.04	-0.001
	[0.015]	[1.916]	[0.014]	[0.013]	[0.020]	[0.411]	[0.020]
Model with full covariates							
Continuous birth weight (in 100s of grams)	0.003***	0.073	0.003***	0.003***	0.001	-0.027	-0.001
	[0.001]	[0.095]	[0.001]	[0.001]	[0.001]	[0.024]	[0.001]
No. of LBW siblings present at the time of investment	0.015	-0.832	0.039***	0.028**	0.015	0.086	0.005
	[0.015]	[1.919]	[0.014]	[0.013]	[0.020]	[0.415]	[0.020]
Observations	9206	3539	8325	6859	9573	6082	5139
Number of Mothers	3439	1418	3160	2674	3539	2332	1987

Notes: Estimates in Panel A are from mother fixed-effect models that include a limited set of covariates, including child's sex, birth order, indicator for whether birth weight was imputed, and indicator for whether the sibling endowment variable was constructed using imputed birth weight. Estimates in Panel B are from mother-fixed effect models that add mother's health, education, marital status, and age at birth, prenatal investments and family income as additional covariates. Figures in brackets are standard errors. * Significant at 10%, ** Significant at 5%, *** Significant at 1%. [a] Models for "weeks breastfed" were estimated using families where all siblings were breastfed.

Table 4: Mother Fixed-Effect Estimates From Alternate Birth Weight Specifications

	Ever Breastfed	Weeks Breastfed [a]	Well baby visit in first year	Full dose of DPT/oral polio vaccines	Attended any preschool program	Kindergarten entry age in months	Whether parents delayed kindergarten entry
Low birth weight indicator							
Low birth weight indicator	-0.038**	-1.187	-0.015	-0.050***	-0.060**	1.044*	0.041
	[0.018]	[2.248]	[0.017]	[0.015]	[0.029]	[0.606]	[0.030]
No. of LBW siblings present at the time of investment	0.014	-1.063	0.045***	0.02	-0.021	0.613	0.023
	[0.017]	[2.254]	[0.016]	[0.015]	[0.026]	[0.564]	[0.027]
Birth weight categories							
BW: 2500g-3299g [b]	0.03	1.198	0.018	0.046***	0.056*	-1.124*	-0.035
	[0.018]	[2.254]	[0.017]	[0.015]	[0.029]	[0.606]	[0.030]
BW: 3300g-3699g	0.046**	1.44	0.019	0.052***	0.052*	-1.228*	-0.051
	[0.020]	[2.360]	[0.018]	[0.017]	[0.031]	[0.636]	[0.031]
BW>=3700g	0.056***[&]	2.009	0.041**[&]	0.055***	0.056*	-0.889	-0.038
	[0.020]	[2.359]	[0.019]	[0.017]	[0.031]	[0.639]	[0.031]
No. of LBW siblings present at the time of investment	0.015	-1.194	0.042***	0.02	-0.017	0.653	0.021
	[0.017]	[2.246]	[0.016]	[0.015]	[0.026]	[0.560]	[0.027]
Observations	9206	3539	8325	6859	9573	6082	5139
Number of Mothers	3439	1418	3160	2674	3539	2332	1987

Notes: All estimates are from mother fixed-effect models that include the child's sex and birth order, mother's health, education, marital status, prenatal investments, family income, indicator for whether birth weight was imputed, and indicator for whether the sibling endowment variable was constructed using imputed birth weight as additional covariates. Figures in brackets are standard errors. * Significant at 10%, ** Significant at 5%, *** Significant at 1%. [&] Significantly different from coefficient for birth weight between 2500-3299g at the 5% level. [a] Models for "weeks breastfed" were estimated using families where all siblings were breastfed. [b] Omitted category is birth weight <2500g.

Table 5: Birth Weight Effects Using Siblings Born <=2 Years Apart

	Ever Breastfed	Weeks Breastfed [a]	Well baby visit in first year	Full dose of DPT/oral polio vaccines	Attended any preschool program	Kindergarten entry age in months	Whether parents delayed kindergarten entry
Model 1: Continuous birth weight							
Continuous birth weight (in 100s of grams)	0.003**	0.011	0.001	0.003**	0.003	-0.03	0.001
	[0.001]	[0.143]	[0.002]	[0.001]	[0.002]	[0.043]	[0.002]
No. of LBW siblings present at the time of investment	0.032	-0.093	0.084***	0.051**	-0.01	-0.096	0.002
	[0.024]	[2.755]	[0.026]	[0.021]	[0.029]	[0.678]	[0.035]
Model 2: Low birth weight indicator							
Low birth weight indicator	-0.033	-4.799	0.006	-0.063***	-0.013	0.297	0.06
	[0.027]	[2.940]	[0.029]	[0.023]	[0.049]	[1.220]	[0.061]
No. of LBW siblings present at the time of investment	0.034	-2.377	0.091***	0.037	-0.001	-0.115	0.051
	[0.025]	[3.019]	[0.028]	[0.023]	[0.042]	[1.102]	[0.055]
Model 3: Birth weight categories							
BW: 2500g-3299g [b]	0.021	5.265*	0.003	0.064***	0.004	0.01	-0.047
	[0.028]	[2.994]	[0.029]	[0.024]	[0.049]	[1.218]	[0.061]
BW: 3300g-3699g	0.049	3.768	0.002	0.053**	0.031	-0.891	-0.067
	[0.031]	[3.194]	[0.032]	[0.026]	[0.051]	[1.246]	[0.063]
BW>=3700g	0.055*	4.151	0.01	0.065**	0.039	-0.154	-0.04
	[0.031]	[3.206]	[0.033]	[0.027]	[0.052]	[1.256]	[0.063]
No. of LBW siblings present at the time of investment	0.034	-2.463	0.087***	0.038*	-0.001	-0.199	0.043
	[0.025]	[3.023]	[0.028]	[0.023]	[0.042]	[1.090]	[0.054]
Observations	3321	1239	2898	2761	3547	2179	1848
Number of Mothers	1370	546	1220	1150	1439	911	772

Notes: All estimates are from mother fixed-effect models that include the full set of covariates. Figures in brackets are standard errors. * Significant at 10%, ** Significant at 5%, *** Significant at 1%. [a] Models for “weeks breastfed” were estimated using families where all siblings were breastfed. [b] Omitted category is birth weight <2500g.

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