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Abstract

It is now widely recognized that birth endowments can have long-lasting effects on later-life outcomes. An intriguing question is how parents respond to shifts in child endowments. Some of the estimates in literature may be affected by small samples and unobservable mother-specific factors, limiting the power of policy implications. We exploit variation within twins to estimate the effect of birth weight on health investments in children. Using data from 68 developing countries, we find that lower birth weight babies receive less health care investments in infancy. These effects are larger for countries with higher infant mortality rates, lower life expectancy, and poorer sanitation facilities. Collectively, the findings suggest that parental behaviors contribute to amplify the baseline effects of birth endowments on long-run outcomes.

Keywords: twins; birth weight; parental investments

JEL Codes: D1, I1, J1

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1. Introduction

It is now widely recognized that poor environmental conditions *in utero* can have long-lasting effects on later-life outcomes. Children born with poor endowments exhibit lower earnings, worse socioeconomic status, and reduced cognitive abilities (Black et al., 2007; Figlio et al., 2014; Oreopoulos et al., 2008). Yet an important question remains. How parent's investments respond to shifts in child endowments? Learning on this behavioral response provides a window into mechanisms by which parents contribute to lessen or exacerbate the biological effects of *in utero* shocks. Becker and Tomes (1976) provided theoretical insights on the sign of this relationship, arguing that parents are likely to devote more investments in their better-endowed children in order to maximize returns to investing. In a different vein, Behrman et al. (1982) emphasized the role of parents' aversion to sibling-inequality and point out that parents invest disproportionately in the less-endowed child to ameliorate inequalities. More generally, Yi et al. (2014) state that parents adopt compensating and reinforcing investments along different dimensions of human capital.

Despite the long theoretical debate, there is still a relatively small body of empirical work about the impact of child endowments, commonly proxied by birth weight, on parents' investments. A number of recent studies finds a positive relation between these variables (Aizer and Cunha, 2012; Datar et al., 2010), while others document the opposite (Bharadwaj et al., 2013a; Del Bono et al., 2012). However, the majority of these empirical analyses cannot establish causation. A major empirical challenge in conducting such a test is that birth endowments are endogenous and determined by family background characteristics that may be determinants of postnatal investments. For example, health at birth is influenced by maternal behaviors, such as smoking, alcohol use and restricted nutrition, which differ between more-educated and less-educated mothers. In addition, birth endowments may be affected by other important features, including parent's knowledge or awareness about health care, all of which are difficult to control for but impact subsequent investments. Many previous studies have adopted sibling-fixed effects estimators, thereby controlling any time-invariant family qualities. Yet this approach would be inadequate if birth endowments and postnatal investments are correlated with time-varying characteristics of the mother. For example, a very stressful event during pregnancy (e.g, parental job loss) may directly affect both birth endowments and postnatal investments, creating a correlation between these variables even in the absence of a causal link. In general, it is difficult to solve the endogeneity problem using cross-sectional or sibling comparisons.

In this paper, we revisit this discussion with an investigation of the effect of child endowments on health investments using a within-twin identification strategy for a large set of developing countries. As in prior literature, we proxied birth endowments by birth weight, an indicator that is easily observed by parents and that has been linked to poor health and impaired cognitive development. The differences in birth weight within twins provide us with an empirical strategy that limits the scope for omitted variables bias. Since twins share the same pregnancy, it is impossible for parents to treat differently their twins during the prenatal period. Rather, the differences in birth weight are largely driven by idiosyncratic factors, such as different nutritional sources at different umbilical cord insertion points within the placenta (Phillips, 1993; Zhang et al., 2001). Importantly for identification, there are striking differences in birth weight within twins. In our sample, about of 20 percent of variation in birth weight is explained by unobserved individual differences within twins. The reasons why one of them up with a higher birth weight and the other one did not can be considered as random. Thus, any differences we observe in terms of parents' investments can be plausibly attributed to birth endowments.

While a few studies have used the “twins” approach, they suffer from some key limitations. First, the sample of twins used in most of previous studies are relatively small. An example is Rosenzweig and Zhang (2009) who use a sample of 1,000 Chinese twins, finding perhaps unsurprisingly imprecise estimates of the effect of birth weight on parental investments. Second, these studies have typically used poor measures of investments. An early study by Behrman et al. (1994) uses completed years of schooling as a measure of parental investment. Empirically this is problematic in view of evidence indicating that child endowments can affect directly this outcome independently of parental inputs. These limitations have impeded the development of clear stylized facts.

Our study overcomes each of these limitations. First, we use a large dataset, comprising approximately 17,000 twins, which enables us to observe both birth weight and health investments and adds a strong statistical power to the analysis. This sample is extracted from 200 comparable Demographic and Health Surveys (DHS) conducted in 68 countries where detailed information about parental inputs are consistently recorded for all children under five. As far as we know, this is the largest dataset on twins with information on both birth weight and parental investments. Second, we measure more directly post-natal investments by using vaccination for specific diseases as our primary outcome of interest. Necessary vaccinations as polio and measles have been shown to be effective in preventing ill health and mortality, so they are important health inputs in developing countries where access to appropriate medical treatment is limited. Our paper presents arguably the best evidence to date on the causal effects of birth weight on health investments.

The results indicate that lower birth weight babies are less likely to receive necessary vaccines. This is in marked contrast to the findings of earlier twin studies, which generally find no significant effects of birth weight. In addition, the results suggest significant nonlinearities across the birth weight distribution, with stronger effects among children in the low end of the birth weight distribution. Taking advantage of the great diversity of countries in the data, we also assess whether these effects vary heterogeneously by the country's level of development. We show that families in poorer countries are more likely to engage in differential health investments than families in richer countries. The effects of birth weight are particularly stronger for countries with higher infant mortality rates, lower life expectancy, and poorer sanitation facilities. In these countries, the effects generally increase by 17 to 80 percent. We then evaluate how much of the differences in investments can explain the mortality effects of birth weight. A back-of-the envelope calculation suggests that the differences in investments would explain at least 2 percent of the infant mortality effects of very low birth weight. While this effect may seem small, we would remind our readers of the fact that this result is driven only by the subset of health inputs examined here. Certainly, there are other critical health investments that we do not directly observe and that are likely to move with child capabilities in a similar way. Hence, this estimate should be viewed as a lower bound on the importance of health investments.

These findings have implications for our understanding of why initial capabilities matter so much for later life outcomes. Previous studies document positive effects of health investments on several measures of human capital.[1] This, combined with our results, suggests that the relationships between birth weight and adult socioeconomic status reflect partially different resource allocations that parents make among their better and worse endowed children. It implies of course that the effects of birth weight on long-term outcomes are unlikely to be only the result

[1] See, for example, Almond et al. (2010) and Bharadwaj et al. (2013b). In addition, see Nores and Barnett (2010) for an inventory of the effects of early childhood interventions conducted in developing countries on several domains, including cognition and health.

of biological mechanisms. The family responses to child endowments should be taken into account to understand the distributional effects of early childhood policies.

Within our empirical framework, the most natural concern is that of selective mortality. Indeed, a selection issue arises because children experiencing mortality simply are not included in the analysis. Any selection bias that results from using this select group most likely will bias our estimates of the effect of birth weight toward zero, so our estimates should be taken to be lower bounds. We present evidence consistent with this. Unlike other studies, we have information on investments even for children who had died before the interview, allowing us to investigate the potential impacts of such bias. Additional to including deceased children to the analyses, we also addressed this issue by imputing the missing information under best and worst possible scenarios. We find that selection issue potentially produces large biases. The effects of birth weight on health investments could be approximately 70 percent larger than our baseline estimates suggest.

Our study is related to the recent contribution by Figlio et al. (2014). As part of a larger analysis, they compare school attendance among twins and observe that twins who attend higher quality schools tend to have heavier birth weights than those attending lower quality schools. While this is an interesting finding, it cannot be interpreted as evidence conclusive of reinforcing behaviors because many schools partially select their students based on academic ability. We also build on the recent work by Adhvaryu and Nyshadham (2014), who assess parental responses to shifts in cognitive endowments induced by an iodine supplementation program. They document positive effects on health investments and find evidence of sibling spillovers. Our paper is also related to the literature examining variations in environmental conditions *in utero* to infer parental responses. These studies generally rely on uncommon and severe historical events, such as influenza epidemic, and changes in local environment caused by accidents. Examples include Almond et al. (2009), Parman (2013), and Venkataramani (2012). They tend to find evidence in favor of the interpretation that parents adopt reinforcing responses. These studies are important and have undoubtedly advanced our knowledge of the parental responses to child endowments, but there are still concerns on that we can generalize from them. In addition, studies in this area rarely use direct measures of parental investments and have used rather indirect strategies to deduce parental behaviors. An example is Almond et al. (2009) who find that the effect of the prenatal radioactive fallout on cognitive ability was concentrated among low education parents and interpret this as an indication that parents adopt reinforcing strategies. This interpretation is provocative but requires corroboration.

The rest of the paper is structured as follows. In section 2, we provide background literature information on the relationship between birth endowments and parental investments. In section 3, we describe our data and empirical approach. In section 4, we present our findings, including robustness checks. Section 5 concludes.

2. Background

2.1. Birth weight and initial endowments

In this study, we define child endowments as an initial stock of health and cognitive capacities that are determined at birth. Consistent with previous work, we use birth weight as an overall measure of this initial stock of capacities. Medical literature has established a strong link between low birth weight and impaired development of the brain. Children born with very low birth weight are more likely to suffer from attention deficit, dyspraxia, and impaired learning (Marlow et al., 1993, 1989). Abernethy et al. (2002) provide evidence that these learning disabilities among lower birth weight children stem from differences in global brain growth and the development of brain structures related to memory. A vast medical literature has also associated low birth weight to health problems such as cerebral palsy, deafness, epilepsy, blindness, asthma, and lung disease (Brooks et al., 2001; Kaelber and Pugh, 1969; Lucas et al., 1998; Matte et al., 2001; Nelson and Grether, 1997; Paneth, 1995; Richards et al., 2001). Most of these health and cognitive problems tend to persist over time and thereby have the potential to explain low levels of human capital.[2]

Taken together, this brief survey of medical studies indicates that birth weight is a reliable measure of initial endowments. Importantly for the purposes of this paper, birth weight is an objective measure that is easily observed by parents. Some health problems in lower birth weight babies are visible within a few days after birth. In fact, once a baby is born with very low birth weight, doctors generally warn parents about potential health risks associated to this condition. It has also been widely documented the ability of mothers to identify birth endowments of their infant children from very early ages (Adhvaryu and Nyshadham, 2014). For instance, an early study by Brazelton (1984) demonstrates a wide variability in maternal reports on observable behaviors in their infants even at one week after birth. These behaviors include the ability of babies to relate situations to themselves, which is a feature attributable to the cognitive abilities of babies (Meltzoff and Moore, 1997, 1983).

2.2. Previous estimates of the effects of child endowments on parental investments

Much of the growing literature about the effects of birth weight on parental investments has used sibling-fixed effects to mitigate concerns regarding omitted variable bias. Datar et al. (2010) is one of the first studies to use this approach. They employ U.S data and find that low birth weight

[2] See, for example, Botting et al. (1997) and Powls et al. (1995).

children are less likely to be breast-fed, receive vaccinations and attend a preschool program relative to normal birth weight children. Subsequent studies have investigated the effect of birth weight on time investments, such as reading, playing, and doing hobbies (Hsin, 2012), and investments based on the quality of the mother-child interaction (Aizer and Cunha, 2012). These studies tend to find positive effects of birth weight on parents' investment. Most notably, Hsin (2012) finds that that less-educated mothers are more likely to reinforce initial endowments compared to most-educated mothers, suggesting that socioeconomic status plays an important role in determining parental responses. Differently from these studies, Del Bono et al. (2012) use a structural dynamic model of family resource allocations and find compensatory behaviors in breastfeeding. While the use of variation within siblings eliminates the bias attributable to time-invariant omitted factors, this approach will be biased if there are sibling-specific unobserved events correlated with both birth weight and postnatal investments. Therefore, causality should be viewed with caution.

As solution to omitted variable bias, a few studies have used variation within twins. A pioneering work in this area is Behrman et al. (1982) who use data on 1,021 twins and try to infer parental responses from a wage equation. They conclude that parents adopt compensatory responses. An important limitation is that they do not use any direct measure of initial endowments or parental investments. In a later paper, Behrman et al. (1994) use educational attainment as a measure of parental investment. Using a sample of 900 twins, these authors find that initial endowments is positively associated with years of schooling, interpreting this as an evidence that parents adopt reinforcing behaviors. Nevertheless, years of schooling are an outcome, not an input, and parental control over it is limited. Completed years of education is the result of parental inputs, but also of other factors such as child's perseverance, which may differ among children for biological or other reasons.

In a study about the effects of family-size on human capital investments, Rosenzweig and Zhang (2009) incorporate the effects of birth weight on schooling expenditures as part of their analysis. They are among the first studies to directly measure both child endowments and parental investments among twins. In a sample of 1,169 twins, these authors find a slightly significant effect of birth weight on schooling expenditures. A parallel study by Royer (2009) finds insignificant estimates of the effects of birth weight on breastfeeding and the number of days in a hospital. In addition, Currie and Almond (2011) use the same sample and generally document insignificant effects on a wider set of investment measures, including amount of praise and affection offered, and disciplinary practices. Bharadwaj et al. (2013) use a relatively larger sample (about 5,000 twins) and find insignificant effects of birth weight on cognition investments such as reading or educational encouragement. They also use data on siblings and find evidence that parents participate in compensatory behaviors. Bharadwaj et al. (2013) argue that the inputs examined have a high degree of public goods within the household and that it could explain why birth weight have no significant effects among twins.

Overall, these studies provide mixed results, ranging from no effects to reinforcing responses. A complication with these studies is that they are based on relatively small sample sizes and this may lead to imprecise estimates. For example, Rosenzweig and Zhang (2009) also estimate the effect of birth weight separately for exempt and non-exempt from the one-child-per-family policy and find insignificant impacts in both subsamples (which is in contrast to their results in the combined sample). In addition, most of previous studies using the twin strategy rely on indirect measures of parental investments. As Royer (2009) emphasizes, the number of days in a hospital could be hardly a reliable measure of parents' investment, as it may simply reflect medical decisions made by health professionals rather than parents. We attempt to overcome both empirical challenges. First, we use data on 17,000 twins from a wide range of countries. The advantage of using this relatively large sample is that a larger sample size implies greater statistical power than these previous studies. Second, we measure directly health investments by using vaccination coverage. Our study is the first to examine the effects of birth weight on these inputs using within-twin variation.

3. Data and empirical strategy

3.1.Data

The source of data for this study comes from Demographic and Health Surveys (DHS).[3] The DHS are nationally representative surveys of women ages 15 to 49. While these surveys have been implemented in more than 80 countries since 1984, they are comparable across countries and years. The main strength of this dataset is that it contains detailed information about birth histories and child healthcare for all children under three, four or five years (depending on the wave). For this paper, we use all surveys from rounds II, III, IV, V and VI with available information on birth weight and vaccination history. We exclude data from the round I because it does not contain data on birth weight. Countries where information about birth weight was never collected are also excluded. In total, the analysis includes 200 waves from 68 countries. We pool these files into a single file. We identify twins based on whether they were declared as twins by their mother. In total, there are 30,425 twins. Triplets are also included in the sample. Available measures of vaccination reported consistently across the different rounds of the DHS include

[3] DHS data are available for download by registering at: http://www.measuredhs.com/.

BCG, DPT, polio and measles.[4] Our main variables of interest are *number of immunizations, at least one immunization*, and *completely immunized*. We exclude twins with missing information on these variables. This restriction results in dropping about 2 percent of the sample. Appendix Table A1 displays a list of specific surveys and their sample sizes.

Although the DHS are a rich source of data, they also have limitations. In particular, at the interview, mothers were asked to provide information on birth weight and the use of this retrospective information may rise some concerns. Recall bias may be important if mothers are less likely to accurately remember birth weight for births that are distant. Furthermore, many mothers in developing countries do not give birth in hospitals, so birth weight information for these births are subject to considerable recall errors. More importantly, the measurement error might be systematic if parents selectively think of their "better" child as the one having higher birth weight. We further discuss below these issues and perform some robustness to address the potential impact of the measurement error on our results.

We exclude twin pairs where at least one birth had no information on birth weight. Our final dataset consists of 17,779 twins who were born between 1985 and 2014. While our analyses focuses on twins, we also present some results for singletons. There are 308,522 singleton children with at least one sibling and with information about both birth weight and health investments. Table 1 presents basic descriptive statistics splitting the sample between twins and singletons. Both groups of births are similar respect to mother's characteristics, such as education, age and marital status. In terms of birth weight, there are substantial differences between twins and singletons. Birth weight is higher for singletons, with the average at 3,218 grams compared to 2,523 grams for twins. This is a difference of more than 27 percent. The

[4] BCG protects against tuberculosis and DPT protects against diphtheria, pertussis and tetanus.

incidence of low birth weight (defined as birth weight less than 2,500 grams) is five times higher in twins. Figure 1 makes clearer the differences in birth weight between twins and singletons.

The basis of our identification strategy relies on the fact that twin pairs differ in birth weight, and sometimes the difference is substantial. As already hinted at in the Introduction, medical literature indicates that one major reason for intra-pair variation in birth weight is differences in placental cord insertion, which leads to different nutritional intakes and blood perfusion (Zhang et al., 2001).[5] Since parental control over these factors is limited, the birth weight within a given twin pair is as good as randomly assigned. Figure 2 shows the distribution of the twin birth weight-difference. The mean birth weight difference is 330 grams, or 13 percent of the average twin's birth weight. This figure is remarkably similar to the 320 grams found for Norway (Black et al., 2007), the 290 grams for U.S (Almond et al., 2005) and the 284 grams recently reported for Florida (Figlio et al., 2014). Our data also reveal that 59.1 percent of twin pairs exhibit a birth weight difference higher than 200 grams, and 17.3 percent have a birth weight difference higher than 600 grams. Furthermore, about of 20 percent of variation in birth weight among twins cannot be explained by unobservable mother-specific factors. Therefore, it is apparent that there is a fair amount of variation for identification.

We make use of additional information to test some forms of heterogeneity. We collect country level data on the percentage of households with access to improved sanitation, total health expenditure as percentage of GDP, infant mortality rate, life expectancy at birth, and real GDP per capita (in 2005 US dollars).[6] The information on these data is provided by the World

[5] See Breathnach and Malone (2012) for a survey on the factors causing twin birth weight-differences.

[6] Total health expenditure are computed as the sum of public and private health expenditure. It refers to the provision of health services (preventive and curative), family planning activities, nutrition activities, and emergency aid designated for health but does not include provision of water and sanitation.

Bank, which constructs an annual panel comprising 247 countries from 1960 to 2014, though not all variables are available for all countries in all periods.[7] We take the average of each of these variables over time, ignoring missing years.[8] Finally, we gather data on sex ratio at birth from The World Factbook.[9]

3.2.Empirical strategy

Our strategy to estimate the effect of birth weight on parental investments is to use the sample of twins and to include family-fixed effects. Since twins share the same mother, the inclusion of family-fixed effects controls for family background, prenatal investments, and any unobservable family-specific factor which may affect both birth endowments and parental investments. Therefore, the impact of birth endowments is identified using idiosyncratic differences in birth weight within twin pairs. We use the following specification:

$$y_{ij} = \alpha + \beta LN(birth\ weight)_{ij} + \delta' X_{ij} + \eta_j + \xi_{ij} \qquad (1)$$

where *y* represents our measures of parental investments of the twin *i* who was born from the mother *j*. *X* is a vector that contains additional child-specific controls such sex, and birth order. The inclusion of the mother-fixed, η_j, will absorb all the above-mentioned unobservable mother-specific factors that affect birth weight and parental investments. ξ_{ij} is an idiosyncratic error term assumed orthogonal to birth weight. The primary parameter of interest is β, which represents the impact of birth weight on parental investments. Positive values for this parameter are interpreted as reinforcing responses while that negative ones are interpreted as compensatory behaviors.

[7] Available online at http://data.worldbank.org/indicator
[8] The results are qualitatively similar if we use the median values of these variables.
[9] Available online at https://www.cia.gov/library/publications/the-world-factbook/fields/2018.html

4. Results

4.1. Baseline results

Table 2 reports estimates from equation (1). Each coefficient is from a separate regression. We have multiplied the coefficients and standard errors by 1,000 to make them easier to read. Table 2 also presents OLS and sibling-fixed effects. OLS regressions control for year and month-of-birth, mother's education, mother's age, mother's marital status, country, urban residence, year and month-of-survey, child's sex and age, and gender-specific birth order. With the sibling-fixed effects, we control for child's sex and age, and gender-specific birth order (all time-invariant variables are differenced out).

For at least one immunization, the OLS coefficient of 7.224 implies that a 10 percent increase in birth weight increases the likelihood of receiving ate least one immunization by 0.07 percentage points. This estimate is slightly larger when twin-fixed effects are included. Conversely, we find dramatic differences for the other outcomes. Panel A shows that the estimated coefficient is negative in the OLS specification, but it switches to positive when are incorporated twin-fixed effects. A similar pattern is found for the probability of being completely immunized. In general, the OLS results provide little evidence of a systematic relationship between birth endowments and parental investments. The variability in the sign across outcomes suggests that omitted variables may play an important role. In contrast, the results from the twin-fixed effects strategy are more conclusive. They suggest that birth endowments have a positive and significant effect on health investments, with all coefficients significant at the five percent level. Relative to the mean, the largest estimates from the twin approach imply that a 10 percent increase in birth weight would increase the likelihood of being completely immunized by 0.35 percent.

The results from our twin sample are somewhat similar to those reported using the sibling sample. For example, the coefficient of 104 in the sibling-fixed effects specification is not statistically indistinguishable from the 96 point estimate obtained using the twin sample. The coefficients from the sibling-fixed effects are more precisely estimated. This is not surprising given that the singleton sample is substantially larger. These similarities across the point estimates suggest that the omitted variable bias might be small and that the results from the twin sample may have external validity to the rest of population. However, we cannot rule out the possibility that the causal effects of birth endowments differ between twins and singletons, but the sibling fixed effects results are biased so that they are similar to those from the twins. The differences between the cross-sectional results between twins and singletons suggest that this may be indeed the case. Therefore, some caution is required with respect to generalizability.

In Table 3, we take a closer look at of the results. Now, we estimate the effects of birth weight separately for each of the vaccinations. The magnitudes vary depending on the vaccination but we find that heavier babies are more likely to be vaccinated than lighter babies are. In Appendix Table A2, we run equation (1) disaggregating these vaccinations by individual doses. Specifically, we estimate the effects of birth weight for each of the three polio and DPT doses separately. The results clearly show that birth weight remains a strong determinant of receipt of individual vaccinations, replicating qualitatively our main findings.

4.2.Heterogeneity

The ample variation in socioeconomic characteristics across the 68 countries examined here allow us to explore heterogeneity in the effects of birth weight. It is naturally interesting to understand whether the effects of birth weight vary by the country's level of development. If the elasticity of substitution between consumption and child investments is higher for poorer families, then reinforcing responses would be more pronounced in less-developed countries.[10] Furthermore, since credit constraints are more severe for families in developing countries, the inability to smooth consumption may lead families to concentrate their resources on the better-endowed children in the developing world. Alternatively, if child endowments and health investments are complementary in the production function for child quality, then one might expect larger effects in richer countries because the investments parents make have higher returns.[11]

We next investigate these hypotheses in Table 4 by estimating our twin-fixed effects estimator for children from countries in the bottom tertile of measures of economic development. If these hypotheses are valid, we would expect see different impacts in magnitude on these subsamples. Panel A replicates our baseline estimates. In Panel B, we run our specification for countries in the low end of the per capita GDP distribution. The coefficient for number of immunizations is positive and significant, but decreases somewhat in magnitude relative to the baseline. The same is true for at least one immunization and in fact the estimated effect is no longer statistically significant. We find a larger effect on the likelihood of being completely immunized. Overall, there is inconclusive evidence that birth weight have heterogeneous impacts

[10] See Almond and Mazumder (2013) for a more detailed discussion.

[11] For a detailed discussion on the role of complementarities in the models of human capital, see Cunha and Heckman (2007), and Conti and Heckman (2010).

on parental investments for families from lower GDP. Likely this is because the GDP is not the best measure of economic development.

In the next set of panels, we look at alternative measures of economic development. Specifically, we estimate the model for countries with lower levels of improved sanitation, life expectancy, health expenditure, and infant survival rates. Qualitatively, the results for these regressions replicate the patterns found before. The estimated coefficients tend to be higher for these countries, particularly so for number of immunizations and the probability of receiving all immunizations. The effects of birth weight on these inputs increase by 17 to 80 percent relative to the baseline estimates, suggesting that families in poorer countries are more likely to adopt reinforcing behaviors than are families in richer countries. As a final exercise, we use as an aggregate measure the first principal component from a principal components analysis on these measures of economic development, including per capita GDP. This aggregate measure captures 70 percent of the total variance of the variables. Again, we find larger impacts for countries in the low end of the country's level of development. It could be argued that these differential effects of birth weight across countries are driven by differential mortality selection into the sample. This is highly unlikely given the discussion in section 4.4.2. As discussed below, any selection bias from selective mortality most likely lead to underestimates of the effects of birth weight. Since infant mortality rates are higher in poorer countries, it is reasonable to believe that the bias from selective mortality goes precisely in the opposite direction of the differential impacts observed in Table 4.

Another question of interest is whether the effects of birth weight are different in countries with son preferences, which implies of course to look differential impacts by gender. One could imagine that families in countries with high son preference may have different rates of differential investment than families in countries with low son preference. In particular, parents in countries with boy preference may believe that better-endowed boys have much higher returns to investing than poorer-endowed girls (or boys). Therefore, we would like to test whether there are heterogeneous impacts by gender and the extent to which such differential impacts are higher for countries with son preferences. To explore this question systematically, we define a country as having son preference if the sex ratio at birth is higher than the range considered normal. The sex ratio in United States and United Kingdom is 105, so we use them as reference since they are presumed to be free of son preference.

We then estimate a "triple-differences" model by regressing health investments on birth weight, a dummy for gender interacted with birth weight, a dummy for son preference status interacted with birth weight, and an interaction between birth weight, gender and son preference condition. Table 5 shows the results. Columns (1) replicates our baseline estimates. The effects of birth weight tend to be higher in countries with son preferences, but these differences are never statistically significant at the conventional levels. The interaction between birth weight and the dummy for boy indicates that there is no significant differential impacts by gender. Finally, the coefficients on the triple interaction is positive for number of immunizations and probability of receiving at least one immunization. However, the estimated coefficients are tightly bounds around zero and insignificants. We conclude that there is no evidence of differential impacts by son preference or gender.

4.3.Parental responses and mother's education

Previous studies have discussed a role for mother's education in determining parental responses to child endowments, arguing that low-education parents are more likely to adopt reinforcing behaviors. This is consistent with the evidence presented in Table 4 that the effects of birth weight are generally higher in poorer countries. If low-education parents are credit constrained or have a high elasticity of substitution between consumption and investments, then they may be more likely to reinforce for differences in endowments (Almond and Mazumder, 2013). However, previous studies showing evidence in favor of these interpretations rely on sibling-fixed effects estimators and their results may be confounded if sibling unobserved factors vary by socioeconomic status.[12] The question is of particular interest in view of literature supporting the provocative results that the effects birth endowments on later life outcomes differ by parent's education (Almond et al., 2009; Figlio et al., 2014). Learning whether parental responses to child endowments is different between less educated and better educated mothers would shed light on the mechanism behind the differential impacts of child endowments on later life outcomes.

Motived by this discussion, we have examined whether birth weight has heterogeneous impacts by mother's education. We estimate our preferred twin fixed effects specification for children whose mothers have 12 years of schooling or less (Table 6). Qualitatively, the results replicate the patterns found before. We find that the coefficients tend to be higher for this subsample relative the baseline estimates. Although these differences are statistically significant only in a few cases, the overall patterns appear generally consistent across columns in Table 6. It seems to be the case that less educated parents adopt larger reinforcing responses. These results complement the evidence in Table 4. Furthermore, they suggest that the differentials impacts of child endowments on cognitive abilities documented in Almond et al. (2009), for example, may

[12] As Almond and Mazumder (2013) note, low and high education parents may have the same causal responses to child endowments, but the observed correlation between endowments and investments is higher for less educated parents due to unobserved sources of stress such as financial difficulties that affect both endowments and parental responses.

be explained because poor families do concentrate their resources on the better-endowed children.

4.4. Additional robustness checks

This section performs a number of additional analyses that addresses important issues with our empirical approach. These issues include measurement error and selection. We also explore alternative specifications and investigate whether birth weight affects breastfeeding.

4.4.1. Measurement error

One important caveat to our analysis is that birth weight is based on maternal reports. Some mothers might misreport birth weight information and this could introduce a relevant measurement error. Recall errors are likely more important for mothers who do not give birth in hospitals and for distant births whose mothers are less likely to remember detailed information about birth histories. In the presence of a random measurement error, our estimates of the effects of birth weight would be attenuated. Naturally, mothers who give births in hospitals are different in ways that could affect postnatal investments so that the measurement error would be systematically correlated with birth weight. The use of twin-fixed effects will capture any such mother-specific differences. Still, one could be concerned if mothers selectively think of their better child as the one having higher birth weight. This is a reasonable hypothesis but note that any positive correlation between the measurement error and birth weight will tend to bias our estimates toward zero.[13] If so, our results showing significant impacts become even more telling.

[13] Suppose that we want to estimate $y = \alpha + \beta x^{*} + \varepsilon$. However, x^{*} is measured with error so that we only observe *x*, which is the true variable plus some noise (i.e, $x = x^{*} + v$) . If the measurement error, v, is positively correlated

We conducts some robustness checks. In Panel B of Table 7, we restrict the sample to children who were born in health facilities. The results of this exercise report coefficients very similar to the baseline in Panel A. For example, the coefficient for at least one immunization is almost identical to the baseline estimate (i.e, 10.9 versus 11.8). In Panel C, we restrict our sample to more recent births whose mothers might be more likely to remember information about birth histories. We then estimate our specification for twins under 24 months of age. This reduces substantially the baseline sample, approximately by 50 percent. In this smaller sample, we find generally larger point estimates and even statistically significant effects, which is consistent with the notion that recent births have better information on birth weight records. As a final check, we limit the sample to waves of the DHS with relatively lower missing-data rates. This is important because some waves have high rates of missing data on birth weight. In Appendix Table A3, we repeat the baseline analysis, but exclude waves of the DHS with more than 50-90 percent of missing data on birth weight. As shown, our baseline estimates are very robust in these different subsamples. For example, the effect of birth weight on number of immunizations is 96.641 (s.e.= 37.701) without restricting the sample, and 99.592 (s.e.= 42.889) when the sample is restricted to waves with missing-data rates lower than 50 percent.

4.4.2. Selective mortality

While the use of data on twins rather than siblings it is empirically compelling, a major concern is that of selective mortality. As such, our analysis does not include infants who died and previous studies show that lower birth weight is associated with increased rates of infant mortality (Almond et al., 2005; Black et al., 2007; Oreopoulos et al., 2008). To address this issue,

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with the true value of the variable of interest, x^*, then the probability limit of β_{OLS} as $N \rightarrow \infty$ is $plim(\beta_{OLS}) = \beta \frac{\sigma^2_{x^*}+\sigma_{x^*v}}{\sigma^2_{x^*}+\sigma^2_v+2\sigma_{x^*v}}$. As σ_{x^*v} is positive by assumption, the estimate of β will be smaller than the true coefficient.

we exploit the feature of the DHS data that mothers are asked to report on investments and birth weight even for children who had died before the interview. Therefore, we can examine the impact of mortality on our estimates by simply including these deceased twins in our estimation sample. When we included these children in the analysis, we find even stronger impacts of birth weight (Appendix Table A4). In general, point estimates are three times as large as our baseline estimates and are significant at the one percent level.

As a further check, we calculate bounds by imputing the missing information for infants who did not survive before the investments was possible. Although it is impossible to know what the effects of birth weight would have been on parental inputs of the dead children, we adopt a simple approach to compute bounds: lower bounds assume that all dead children would have received investments and upper bounds assume exactly the opposite. We find the upper bounds quite similar to those estimates that simply include all deceased children in the sample (Column (2)). Although these bounds are not very tight, they exclude zero. If one assumes that the most reliable scenario is that deceased children were treated worse than surviving children were, then the upper bounds would imply that our baseline estimates could underestimate the impact of birth weight by approximately 70 percent.

4.4.3. Alternative specifications

In this section, we assess the robustness of our results to different versions of our basic specifications. First, we estimates our baseline specification but using birth weight in grams rather than the log of birth weight as the key independent variable. Second, we classify birth weight into categories and use dummy variables to estimate possible nonlinear effects of birth endowments. While using the natural log of birth weight does allow for nonlinear effects, the use of dummy variables helps uncover more detailed relationships between child endowments and parental investments. We group children by whether their birth weight was 1,499 grams or less, 1500 to 2000 grams, 2001 to 2500 grams, 2501 to 3000 grams, 3001 to 3500 grams, and 3500 grams or more.

We regress our outcome measures on these alternate variants of log of birth weight. The results are reported in Table 8. Panel A reports results for birth weight in grams, and Panel B reports results using dummy indicators for birth weight categories. All estimated coefficients on birth weight are positive and significant. For example, the coefficient of 0.008 indicates that a one standard deviation increase in birth weight (694 grams) would increase the likelihood of being completely immunized by approximately 0.55 percentage points. The results on birth weight categories also support our findings that child endowments have a positive effect on parental investments. These results further suggest significant nonlinearities in the relationships. In particular, infants born with a birth weight below 1,500 grams are about 2.1 percentage points less likely to be completely immunized, but for infants born between 2000 and 2500 grams there is no discernable effect. In general, our conclusions are qualitatively similar across these alternative specifications.

4.4.4. Results for breastfeeding

Appendix Table A5 examines the effect of birth weight on breastfeeding, an important source of nutrition for infants. We use different definitions of breastfeeding duration, such as ever breastfed, breastfed for more than six month, and months of breastfeeding.[14] We first estimate twin-fixed models in Panel A. Irrespective of how breastfeeding duration is defined, there is very

[14] The minimum length of breastfeeding recommended by the World Health Organization is six months.

little evidence that birth weights affect breastfeeding. However, if we estimate sibling-fixed effects models, we find that birth weight is positively related to breastfeeding duration, with all coefficients statistically significant at the five percent level. A 10 percent increase in birth weight is associated with an increase of 6.5 in the number of months breastfed. Relative to the mean of 25, this effect is substantial at 26 percent. The differences in the results between twins and siblings is perhaps not surprising given that it may be more difficult to implement favoritism among twins in terms of breastfeeding. We therefore believe that birth weight has, at best, small impacts on this input among twins.

4.5. *Impact on infant mortality*

One way to place our estimates in perspective is to assess how much of the differences in parental investments can explain the higher neonatal mortality rates among lower birth weight babies. We use estimates from the literature of the effects of immunizations on infant mortality (Aaby et al., 2010, 2005; Moulton et al., 2005). The within-twin difference in the mortality rates between very low birth weight children versus heavier children (>3,500 gr) is 33 percentage points.[15] A back of the envelope calculation suggest that the differences in health investments are responsible for at least 2 percent of the increased mortality rate among lower birth weight children.[16] This is a lower-bound estimate that does not take into account other health behaviors that we do not directly observe and that are likely to move with child endowments in a similar way.

[15] We use estimates from Oreopoulos et al. (2008) of the effect of birth weight on infant mortality.

[16] This calculation is performed as follows. First, we calculate the difference between the probability of death conditional on not receiving a give vaccine and the probability of death conditional on receiving it. Second, we multiply this difference by the within-twin difference in the fraction of very low birth weight children versus heavier children (>3,500 gr) receiving the vaccine. Finally, we sum these differences overall all vaccinations and divide the total by the within-twin difference in mortality rates between very low birth weight children versus heavier children.

5. Conclusions

Many previous studies have provided a variety of evidence that poor endowments at birth can have adverse consequences on human capital accumulation. The extent to which these effects can be totally attributed to biological mechanisms remains controversial. It has been stated that, in part, such effects reflect parental responses to variations in child endowments. Prior literature has provided conflicting evidence about the sign and magnitude of these behavioral responses. These estimates, however, may be affected by small samples and omitted variables, limiting the power of policy implications. We have examined the effect of birth endowments, proxied by birth weight, on health investments using a within-twin strategy for a large set of developing countries. In comparing twins, we account for omitted variables determining prenatal and postnatal investments. In contrast to previous studies, we find that health investments do respond to variations in child endowments. Indeed, lower birth weight babies are less likely to receive health investments. These behaviors are responsible for at least 2 percent of the higher mortality rates among lower birth weight children.

It is useful to point out that our analysis does not address several questions. We have examined vaccinations and other types of health investments are certainly important. Clearly, mothers who seek vaccinations are likely to make other types of health care as well. If one knew the causal impacts of birth endowments on others health investments, we would be able to explain a greater portion of the higher mortality rates among lower birth weight children. This should be addressed in future studies. Another important issue is that our study do not explicitly deal with the issue of multidimensional capabilities of initial endowments. Recently, it has been argued that parental responses may differ across dimensions of human capital and birth weight has been related to different dimensions including health and cognitive development. Our estimates do not allow us to disentangle what source of variation in birth weight has more or less influence on parental investment. This is important in view that parental response to a cognitive shock could be different from a health shock, as hypothesized by Yi et al. (2015). We emphasize that our findings should be interpreted as a combined influence of these different dimensions. The magnitude of this reduced form estimate is still important, as birth weight is an objective measure that can be used as a direct target of policy. Social programs that seek to reduce the incidence of low birth weight may have positive externalities on health investments in infancy and cost-benefit analyses of these interventions should account for them. Finally, future research should also investigate the impacts of birth endowments on other human capital investments, including cognitive and non-cognitive investments. Reliable estimates of these parameters are crucial to understand the role of household behavior in determining the long-run effects of prenatal conditions.

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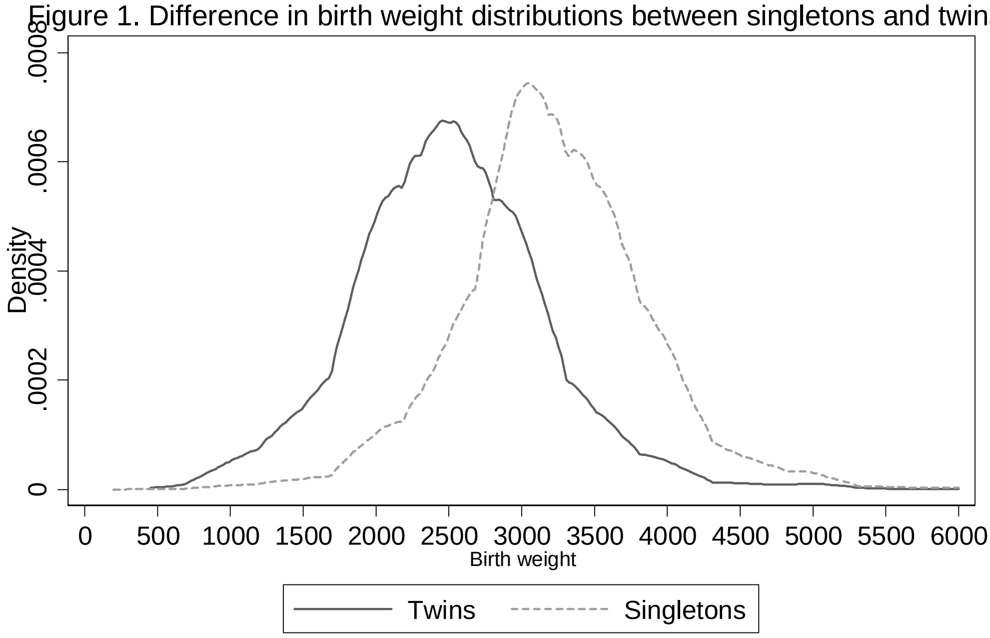
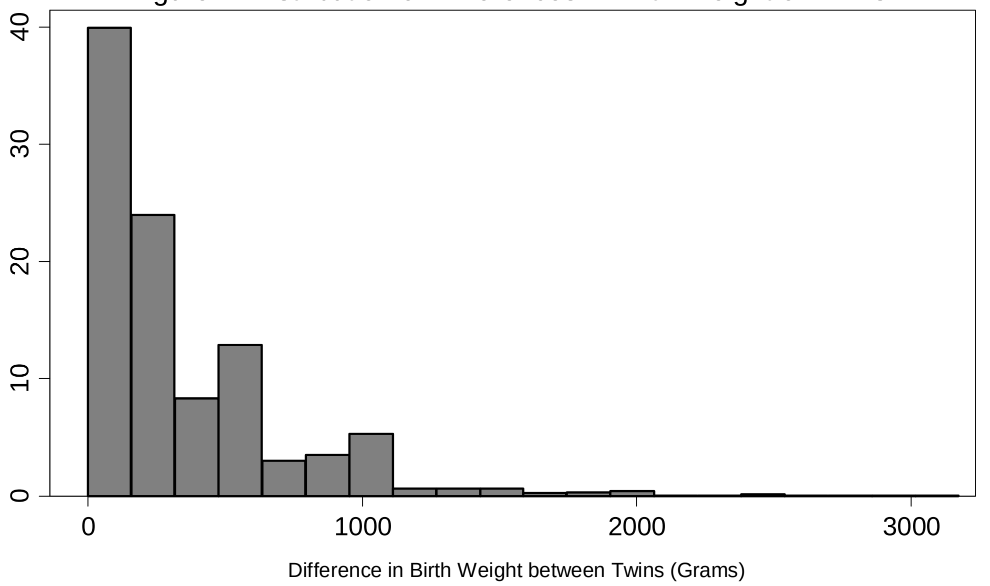
Figure 1. Difference in birth weight distributions between singletons and twins



Notes. Figure 1 plots kernel density distributions of infant birth weight for twins (solid line) and singletons (dashed line) in our sample.

Figure 2. Distribution of Differences in Birth Weight of Twins



Notes. Each bar represents the percentage of twins whose birth weight difference falls within the specified range. The mean birth weight difference among twins in our sample is 330 grams.

Table 1. Summary statistics

	Twins sample		Singleton sample	
	Mean	SD	Mean	SD
Child's characteristics:				
Infant birth weight (grams)	2,523.19	694.91	3,218.35	651.65
Fraction low birth weight (<2,500 grams)	0.45	0.50	0.09	0.28
Birth order	4.04	2.36	3.14	2.10
Fraction male	0.49	0.50	0.51	0.50
Mother's characteristics				
Age	30.43	6.21	28.33	5.88
Fraction married	0.73	0.44	0.70	0.46
Years of schooling	6.45	4.91	6.89	4.92
Fraction of mothers liven in urban areas	0.49	0.50	0.51	0.50
Health investments				
Fraction with BCG vaccine	0.92	0.27	0.90	0.30
Number of polio doses (max =3)	2.45	0.98	2.42	0.99
Number of DPT doses (max =3)	2.44	1.04	2.45	1.02
Fraction with measles vaccine	0.70	0.46	0.71	0.46
Number of immunizations (max=8)	6.50	2.33	6.47	2.32
Fraction with at least one immunization	0.96	0.20	0.96	0.20
Fraction completely immunized	0.54	0.50	0.52	0.50

Notes. There are 17,779 twins and 308,522 singletons.

Table 2. The effect of birth weight on parental investments

	Singleton sample		Twin sample	
	OLS	Family F. E.	OLS	Family F. E.
	(1)	(2)	(3)	(4)
		Panel A: Number of immunizations		
LN(Birth weight)	2.746	104.891	-110.804	96.641
	[17.957]	[22.239]***	[58.203]*	[37.701]**
		Panel B: At least one immunization		
LN(Birth weight)	2.732	5.867	7.224	10.992
	[1.932]	[2.642]**	[6.056]	[5.270]**
		Panel C: Completely immunized		
LN(Birth weight)	-6.118	9.183	-30.178	19.276
	[4.143]	[5.520]*	[15.044]**	[7.725]**

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. OLS regressions control for year and month-of-birth dummies, indicators for mother's years of education, dummies for mother's age, country dummies, mother's marital status (married), urban residence, year and month-of-survey dummies, child's sex, dummies for child's age (in months) and gender-specific birth order dummies. Sibling-fixed effects regressions control for child's sex, dummies for child's age (in months), and gender-specific birth order dummies. Twin-fixed effects regressions control for child's sex and gender-specific birth order dummies. There are 17,779 twins and 308,522 singletons.

Table 3. Effect of birth weight on specific vaccinations

	Vaccinations			
	BCG	No. of polio doses	No. of DPT doses	Measles
	(1)	(2)	(3)	(4)
	Panel A: Twin sample with family fixed effects			
LN(Birth weight)	11.283	41.884	33.760	11.687
	[5.796]*	[14.910]***	[16.806]**	[6.925]*
Mean of dependent variable				
N	17,775	17,763	17,764	17,717
	Panel B: Singleton sample with family fixed effects			
LN(Birth weight)	8.234	40.132	47.647	9.904
	[3.407]**	[10.148]***	[10.332]***	[4.578]**
Mean of dependent variable				
N	308,463	308,136	308,150	307,301

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. Sibling-fixed effects regressions control for child's sex and age, and gender-specific birth order dummies. Twin-fixed effects regressions control for child's sex and gender-specific birth order dummies.

Table 4. Twin-fixed effects estimates of the effect birth weight on parental investments (Heterogeneity by economic development)

	No. of immunizations (1)	At least one immunization (2)	Completely immunized (3)
	Panel A: Baseline estimates		
LN(Birth weight)	96.641	10.992	19.276
	[37.701]**	[5.270]**	[7.725]**
N	17,779	17,779	17,779
	Panel B: Bottom tertile of the per capita GDP distribution		
LN(Birth weight)	94.137	6.857	24.713
	[41.022]**	[5.038]	[9.682]**
N	11,623	11,623	11,623
	Panel C: Bottom tertile of the infant survival distribution		
LN(Birth weight)	138.427	13.862	26.491
	[45.172]***	[6.628]**	[8.517]***
N	11,819	11,819	11,819
	Panel D: Bottom tertile of the life expectancy distribution		
LN(Birth weight)	132.226	11.971	33.775
	[43.518]***	[6.364]*	[8.418]***
N	11,608	11,608	11,608
	Panel E: Bottom tertile of the life the health expenditure distribution		
LN(Birth weight)	147.826	18.857	26.907
	[52.603]***	[7.992]**	[10.887]**
N	8,182	8,182	8,182
	Panel F: Bottom tertile of the percentage improved sanitation distribution		
LN(Birth weight)	142.131	12.232	33.432
	[45.355]***	[6.519]*	[8.663]***
N	11,532	11,532	11,532
	Panel G: Bottom tertile of the economic development index distribution		
LN(Birth weight)	125.756	11.284	32.854
	[44.595]***	[6.541]*	[8.642]***
N	11,153	11,153	11,153

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. All regressions use twin-fixed effects and control for child's sex and gender-specific birth order dummies. Countries in the low end of the infant survival distribution refer to countries in the top tertile of the infant mortality distribution. The economic development index refers to the first principal component from a principal components analysis on per capita GDP, infant mortality rate, improved sanitation, life expectancy at birth, and health expenditure as percentage of the GDP. This aggregate measure captures 70 percent of the total variance of the variables.

Table 5. Twin-fixed effects estimates of the effect birth weight on parental investments (Heterogeneity by gender preferences)

	(1)	(2)	(3)	(4)
	Panel A: No. of immunizations			
LN(Birth weight)	96.641	72.235	102.200	79.294
	[37.701]**	[38.491]*	[40.115]**	[41.453]*
LN(Birth weight) *x* Son preference		211.195		206.474
		[147.854]		[145.547]
LN(Birth weight) *x* Male			-11.301	-13.213
			[45.020]	[44.975]
LN(Birth weight) *x* Son preference *x* Male				2.584
				[3.258]
	Panel B: At least one immunization			
LN(Birth weight)	10.992	7.281	14.201	10.725
	[5.270]**	[5.384]	[5.585]**	[5.771]*
LN(Birth weight) *x* Son preference		32.112		31.395
		[20.894]		[20.537]
LN(Birth weight) *x* Male			-6.523	-6.826
			[5.892]	[5.884]
LN(Birth weight) *x* Son preference *x* Male				0.521
				[0.485]
	Panel C: Completely immunized			
LN(Birth weight)	19.276	19.447	20.205	20.331
	[7.725]**	[8.301]**	[9.499]**	[9.875]**
LN(Birth weight) *x* Son preference		-1.482		-1.181
		[23.283]		[23.595]
LN(Birth weight) *x* Male			-1.888	-1.869
			[10.440]	[10.451]
LN(Birth weight) *x* Son preference *x* Male				-0.092
				[0.775]

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. All regressions use twin-fixed effects and control for child's sex and gender-specific birth order dummies. A country is defined as having son preference if the sex ratio at birth is higher than 105.

Table 6. Twin-fixed effects estimates of the effect birth weight on parental investments (Heterogeneity by mother's education)

	No. of immunizations	At least one immunization	Completely immunized
	(1)	(2)	(3)
		Panel A: Baseline estimates	
LN(Birth weight)	96.641	10.992	19.276
	[37.701]**	[5.270]**	[7.725]**
N	17,779	17,779	17,779
		Panel B: Results for low-education parents	
LN(Birth weight)	105.891	12.483	24.133
	[43.507]**	[6.157]**	[8.121]***
N	14,627	14,627	14,627

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. All regressions use twin-fixed effects and control for child's sex and gender-specific birth order dummies. Results from Panel B are based on a sample restricted for twins whose mothers have 12 years of schooling or less.

Table 7. Twin-fixed effects estimates of the effect birth weight on parental investments (births in health facilities and children under 24 months of age)

	No. of immunizations	At least one immunization	Completely immunized
	(1)	(2)	(3)
		Panel A: Baseline estimates	
LN(Birth weight)	96.641	10.992	19.276
	[37.701]**	[5.270]**	[7.725]**
N	17,779	17,779	17,779
		Panel B: Births in health facilities	
LN(Birth weight)	81.923	11.829	13.000
	[38.742]**	[5.359]**	[7.820]*
N	16,340	16,340	16,340
		Panel C: Children under 24 months of age	
LN(Birth weight)	130.773	17.395	20.003
	[51.477]**	[8.064]**	[10.926]*
N	8,533	8,533	8,533

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. All regressions use twin-fixed effects and control for child's sex and gender-specific birth order dummies.

Table 8. Twin-fixed effects estimates of the effect birth weight on parental investments (Alternative specifications)

	No. of immunizations		At least one immunization		Completely immunized	
	(1)	(2)	(3)	(4)	(5)	(6)
Birth weight (in grams)	0.037		0.004		0.008	
	[0.016]**		[0.002]*		[0.003]**	
Birth weight <1500		-134.464		-14.673		-25.917
		[64.415]**		[8.343]*		[12.435]**
Birth weight 1500-2000		-96.323		-7.826		-21.547
		[41.622]**		[5.115]		[8.979]**
Birth weight 2000-2500		-39.646		-2.855		-11.814
		[33.520]		[3.975]		[7.513]
Birth weight 2500-3000		-33.192		-1.725		-8.957
		[34.500]		[3.927]		[7.195]
Birth weight 3000-3500		-5.221		-0.35		-4.401
		[32.025]		[3.631]		[6.904]

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. All regressions use twin-fixed effects and control for child's sex and gender-specific birth order dummies. There are 17,779 twins.

APPENDIX

Table A1. Sample size by country

Countries	Years of Survey	Sample size of twins
Albania	2008/2009	33
Armenia	2000, 2005, 2010	78
Azerbaijan	2006	21
Burkina Faso	1992/1993, 1998/1999, 2003, 2010	372
Benin	1996, 2001, 2006, 2011, 2012	902
Bolivia	1993/1994, 1998, 2003/2004, 2008	224
Brazil	1991, 1996	76
Burundi	2010/2011	94
Congo Democratic Republic	2007, 2013/2014	429
Central African Republic	1994/1995	20
Congo (Brazzaville)	2005, 2011/2012	351
Cote d'Ivoire	1994, 1998/1999, 2011/2012	234
Cameroon	1991, 1998, 2004, 2011	549
Colombia	1990, 1995, 2000, 2004/2005, 2009/2010	487
Dominican Republic	1991, 1996, 1999, 2002, 2007, 2013	651
Egypt	1992, 1995, 2000, 2005, 2008, 2014	922
Ethiopia	1992, 1997, 2003	45
Gabon	2000/2001, 2012	258
Ghana	1993, 1998/1999, 2003, 2008, 2014	266
Gambia	2013	128
Guinea	1999, 2005, 2012	264
Guatemala	1995, 1998/1999	88
Guyana	2009	36
Honduras	2005/2006, 2011/2012	217
Haiti	1994, 2000, 2005/2006, 2012	88
India	1992/1993, 1998/1999, 2005/2006	527
Indonesia	1991, 1994, 1997, 2002/2003, 2007, 2012	804
Jordan	1990, 1997, 2002, 2007, 2012	1015
Kenya	1993, 1998, 2003, 2008/2009, 2014	387
Cambodia	2000, 2005/2006, 2010/2011, 2014	245
Kazakhstan	1995, 1999	18
Comoros	1996, 2012	78
Kyrgyz Republic	1997, 2012	80
Liberia	2006/2007, 2013	72
Lesotho	2004/2005, 2009/2010	94
Morocco	1992, 2003/2004	96
Moldova	2005	16

Table A1. Sample size by country (continued)

Countries	Years of Survey	Sample size of twins
Madagascar	1992, 1997, 2003/2004, 2008/2009	194
Mali	1995/1996, 2001, 2006, 2012/2013	243
Maldives	2009	48
Malawi	1992, 2000, 2004/2005, 2010	693
Mozambique	1997, 2003, 2011	379
Nicaragua	1997/1998, 2001	142
Nigeria	1990, 2003, 2008, 2013	360
Niger	1992, 1998, 2006, 2012	228
Namibia	1992, 2000, 2006/2007, 2013	264
Nepal	2006/2011	24
Peru	1991/1992, 1996, 2000, 2003/2004/2005/2006, 2007/2008, 2009, 2010, 2011, 2012	1254
Philippines	1993, 1998, 2003, 2008, 2013	306
Pakistan	1990/1991, 2006, 2012/2013	92
Paraguay	1990	52
Rwanda	1992, 2000, 2005, 2010/2011	282
Sierra Leone	2008, 2013	237
Senegal	1992/1993, 2005, 2010/2011, 2012/2013, 2014	855
Sao Tome and Principe	2008	56
Swaziland	2006/2007	42
Chad	1996/1997, 2004	34
Togo	1998, 2013/2014	193
Tajikistan	2012	66
Timor-Leste	2009/2010	50
Turkey	1998, 2003/2004	90
Tanzania	1991/1992, 1996, 1999, 2004/2005, 2009/2010	445
Uzbekistan	1996	10
Vietnam	1997, 2002	26
Yemen	1991/1992, 2013	45
South Africa	1998	68
Zambia	1992, 1996, 2001/2002, 2007, 2013/2014	485
Zimbabwe	1994, 1999, 2005/2006, 2010/2011	251

Table A2. Results by individual dose (DPT and polio)

	Vaccinations					
	Polio 1	Polio 2	Polio 3	DPT 1	DPT 2	DPT 3
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Twin sample with family fixed effects					
LN(Birth weight)	14.687	12.703	12.761	8.635	14.533	11.907
	[5.981]**	[6.380]**	[5.932]**	[6.010]	[6.929]**	[6.526]*
Mean of dependent variable	0.90	0.83	0.71	0.88	0.82	0.73
N	17,763	17,745	17,745	17,763	17,744	17,740
	Panel B: Singleton sample with family fixed effects					
LN(Birth weight)	7.396	16.334	15.504	9.958	15.635	20.991
	[3.405]**	[4.224]***	[5.197]***	[3.597]***	[4.175]***	[4.809]***
Mean of dependent variable	0.90	0.82	0.69	0.88	0.82	0.73
N	308,136	307,756	307,755	308,144	307,817	307,745

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. Sibling-fixed effects regressions control for child’s sex and age, and gender-specific birth order dummies. Twin-fixed effects regressions control for child’s sex and gender-specific birth order dummies.

Table A3. Twin-fixed effects estimates of the effect of birth weight on parental investments (Excluding surveys with higher rates of missing data on birth weight)

	No. of immunizations	At least one immunization	Completely immunized
	(1)	(2)	(3)
	Panel A: Baseline results		
LN(Birth weight)	96.641	10.992	19.276
	[37.701]**	[5.270]**	[7.725]**
N	17,779	17,779	17,779
	Panel B: Excluding surveys with missing rates higher than 90 percent		
LN(Birth weight)	96.780	11.014	19.298
	[37.736]**	[5.275]**	[7.731]**
N	17,765	17,765	17,765
	Panel C: Excluding surveys with missing rates higher than 80 percent		
LN(Birth weight)	97.604	11.125	19.389
	[38.166]**	[5.334]**	[7.819]**
N	17611	17611	17611
	Panel D: Excluding surveys with missing rates higher than 70 percent		
LN(Birth weight)	105.420	11.763	19.838
	[39.824]***	[5.588]**	[8.151]**
N	16,732	16,732	16,732
	Panel E: Excluding surveys with missing rates higher than 60 percent		
LN(Birth weight)	97.809	11.145	18.424
	[40.811]**	[5.775]*	[8.446]**
N	15,939	15,939	15,939
	Panel F: Excluding surveys with missing rates higher than 50 percent		
LN(Birth weight)	99.592	11.718	15.956
	[42.889]**	[6.072]*	[8.715]*
N	15,241	15,241	15,241

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. All regressions use twin-fixed effects and control for child's sex and gender-specific birth order dummies.

Table A4. Twin-fixed effects estimates of the effect birth weight on parental investments (Selective mortality)

	Baseline estimates	Including deceased infants	*Bounds to account for mortality*	
			Lower bound	Upper bound
	(1)	(2)	(3)	(4)
		Panel A Number of immunizations		
LN(Birth Weight)	96.641	266.532	56.659	273.676
	[37.701]**	[68.701]***	[43.984]	[75.169]***
N	17,779	18,315	18,315	18,315
		Panel B: At least one immunization		
LN(Birth Weight)	10.992	34.634	11.616	38.743
	[5.270]**	[9.202]***	[5.181]**	[10.640]***
N	17,779	18,315	18,315	18,315
		Panel C: Completely immunized		
LN(Birth Weight)	19.276	36.659	5.141	32.268
	[7.725]**	[10.292]***	[9.505]	[10.366]***
N	17,779	18,315	18,315	18,315

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. All regressions use twin-fixed effects and control for child's sex and gender-specific birth order dummies.

Table A5. Twin-fixed effects estimates of the effect birth weight on breastfeeding

	Ever Breastfed	Breastfed for more than six months	No. of months Breastfed
	(1)	(2)	(3)
	Panel A: Twin sample with family fixed effects		
LN(Birth weight)	1.023	2.378	-1919.436
	[4.412]	[11.452]	[1352.362]
Mean of dependent variable	0.97	0.80	32.29
N	16,069	16,069	16,069
	Panel B: Singleton sample with family fixed effects		
LN(Birth weight)	2.930	13.892	574.271
	[1.271]**	[3.557]***	[251.683]**
Mean of dependent variable	0.98	0.81	25.47
N	294,702	294,702	294,702

Notes. Robust standard errors in brackets (***p < 0.01, **p < 0.05, *p < 0.1) are clustered at the mother level. All coefficients and standard errors are multiplied by 1,000 to make them easier to read. Sibling-fixed effects regressions control for child's sex, dummies for child's age (in months), and gender-specific birth order dummies. Twin-fixed effects regressions control for child's sex and gender-specific birth order dummies.