How Much Does Birth Weight Matter for Child Health in Developing Countries? Estimates from Siblings and Twins

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September 2014

Abstract

200 million children globally do not meet their growth potential, and suffer the consequences in terms of future outcomes. I examine the effects of birth weight on child health and growth using information on a million children from the Demographic and Health Surveys. I account for missing data and measurement error using instrumental variables, and adopt an identification strategy based on siblings and twins. I find a consistent effect of birth weight on mortality risk, stunting, wasting, and coughing, with some evidence for fever, diarrhoea and anaemia. Bounds analysis indicates that coefficients may be substantially underestimated due to mortality selection.

JEL Classification: *I12, O15, J13*

Keywords: *Birth Weight, Early Life Conditions, Health, Development*

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1 Introduction

200 million children in developing countries are not reaching their potential, as defined by those who are adversely affected by growth restriction (Grantham-McGregor et al., 2007). In addition to the direct effect of child growth restriction on mortality itself (estimated at 2.2 million deaths in 2005), there are also substantial effects on morbidity; growth restriction was responsible for 21% of the overall global disease burden for children under 5 in 2008, as defined by Disability Adjusted Life Years (DALYs) lost (Black et al., 2008). The standard indicator used to measure growth restriction is stunting, typically defined as being below two standard deviations in terms of the WHO reference child's height for age (WHO, 2011). Stunting represents the child's potential in the absence of nutritional constraints, in-utero growth retardation, and disease environment (Headey, 2012). A third of all children in developing countries are affected by stunting, with prevalence highest in Africa, at 40% (Black et al., 2008). The failure of children to reach their potential is likely to have a perpetuating effect on poverty, given that women of short stature are more likely to give birth to smaller babies (Victora et al., 2008). Unlike the relative success in tackling infant mortality (Rajaratnam et al., 2010; Hill et al., 2012), countries with a high prevalence of stunting have made less progress in addressing this issue (Bryce et al., 2008), and economic growth does not appear to greatly improve the situation for affected children (Vollmer et al., 2014). A closely related problem is anaemia, which refers to a reduced number of red blood cells or haemoglobin. Anaemia is often caused by iron deficiency and is associated with cognitive development and productivity. Globally, 50% of all children and 30% of all non-pregnant women are affected by this condition (Balarajan et al., 2011); moreover iron deficiency is increasing in some regions (Caulfield et al., 2006). The consequence of these conditions is that hundreds of millions of children are unable to take advantage of opportunities such as expansion in education, due to poor health (Walker et al., 2007).

As well as direct effects on health, labour supply is also likely to be affected by growth restriction (i Casasnovas et al., 2005). Specifically regarding the effects of stunting, there is credible evidence of large and significant effects on education (Glewwe and Miguel, 2007). Glewwe et al. (2001) find that growth restriction affects education via both productivity and delayed enrollment by analysing a nutrition intervention programme in the Philippines, as do Alderman et al. (2001). The provision of nutritional supplements to families in Guatemala had a positive effect on grades completed for women who were affected as children, and on test scores for both men and women (Maluccio et al., 2009). A more long term follow-up of participants indicated that the intervention increased productivity in adults, which amounted to a 46% increase in average adult wages (Hoddinott et al., 2008). For recent summaries of the literature, see Dewey and Begum (2011) and Victora et al. (2008).

There is a body of research in economics on the causal impact of early life environment in higher income countries, including the effects of birth weight (Currie, 2011). Therefore, we might expect these factors to be an important determinant of outcomes such as stunting and anaemia. For developing countries, recent studies have shown a strong correlation between measures of in utero environment and child health (Christian et al., 2013; Katz et al., 2013). However, we know comparatively little about the causal impact of these inputs in this context. Relatively worse early life conditions (compared to those previously studied in the literature) may imply even stronger long run effects in lower and middle income countries (Currie and Vogl, 2013), where 27% (32 million) of total live births in 2010 were small for gestational age (Lee et al., 2013).[1] Globally, babies of low birth weight (<2,500g) account for 60-80% of neonatal mortality despite constituting only around a sixth of total births (Lawn et al., 2005).

There is also relatively little data on the optimal timing of intervention (Doyle et al., 2009). For example, it is important to know whether pregnant women should be targeted with nutritional supplements relatively more than targeting their children after birth. If it can be shown that there is no causal relationship between birth weight and child outcomes, then improving post-natal nutrition and disease environment are the relevant goals. However, if stunting can be causally attributed to birth weight, this implies that interventions which solely target child nutrition, and not maternal health and behaviour during pregnancy, will either not accomplish significant advances in helping children reach their full growth potential, or else will be an inefficient means of doing so. It may well be that there are no direct effects on certain outcomes, for example, Almond et al. (2005) find no impact of birth weight on mortality within twin pairs in the US. The relevant question is then the magnitude of these effects (particularly compared to the effects of malnutrition in childhood).

This paper adds to the literature by evaluating the effects of birth weight in a large sample of over a million children in 66 developing countries drawn from the nationally representative Demographic and Health Surveys (DHS). The few existing papers in this area tend to focus on specific countries or events. In their review, Currie and Vogl (2013) identify only one paper that deals with the effects of birth weight (Bharadwaj et al., 2013), and it focuses on educational outcomes in Chile. I also consider a new set of health outcomes (such as stunting and anaemia), which are particularly relevant for this context. In addition, I make a number of methodological contributions. Specifically, I use data on siblings and twins to determine whether estimation of the relationship between birth weight and child health is affected by omitted variable bias. I account for measurement error and missing data using instrumental variables, an issue which is likely to be particularly important in estimates obtained from comparisons within families, as fixed effect models are known to exacerbate attenuation bias. Finally, using an approach previously adopted in the treatment effects literature,

[1] Defined as being below the 10th percentile for gestational age.

I consider the role of selection bias introduced by the absence of data on children who have died.

Overall, birth weight has a meaningful and consistent effect on child health. The observed relationships are non-linear, with the optimal weight typically lying above the usual low birth weight cut off of 2,500g. I find that these effects are likely to be underestimated by mortality selection, potentially substantially. Results therefore imply that a greater policy focus on improving infant health (of which birth weight is one potential marker) is warranted in less developed countries, as this is likely to raise the health capital and life chances of the affected children, in addition to potential productivity gains and intergenerational effects which arise as a consequence.

The rest of this paper is structured as follows. Section 2 reviews the existing literature which addresses the issue of causality, and motivates the focus on stunting and child health as important outcomes of interest to policy makers. Section 4 outlines the empirical strategy and section 5 describes the results. Section 6 discusses mortality selection, while section 7 concludes.

2 Literature: What can we Learn from Twin Studies?

A summary of the economics literature on the impact of initial environment is outlined in Almond and Currie (2011a) and Almond and Currie (2011b). A number of papers have used birth weight as a marker of early life conditions, and twin comparisons as a strategy to control for omitted variable bias (Almond et al., 2005; Behrman and Rosenzweig, 2004; Black et al., 2007; Conley et al., 2003; Figlio et al., 2013; Oreopoulos et al., 2008; Royer, 2009). In general, the literature finds lasting effects on health, education and earnings, although this is not always the case for twin studies. For example, Almond et al. (2005) find a negligible effect of birth weight on within twin pair infant mortality. Estimates of effects on longer term adult outcomes are more consistently positive. It is important to note that existing twin studies have almost exclusively relied on data from high income countries such as the US. Therefore, the analysis in this paper provides the opportunity to compare existing findings to estimates in a context where early life environments are potentially more adverse than those typically examined in the literature thus far.

Some existing papers account for the endogeneity of early life health with birth weight differences in twin pairs. For example, Torche and Echevarria (2011) focus on the effects of birth weight on educational attainment in Chile using twin data, while Bharadwaj et al. (2013) examine how parental investments interact with initial endowments in the same data. Rosenzweig and Zhang (2009) address how birth weight and differential parental investment affects estimates of the effects of family size in China, and Rosenzweig and Zhang (2013) examine economic growth and gender differences, also using Chinese twins. Other papers have used natural experiments for identification. For example, Almond and Mazumder (2011) find that there are effects of being in utero during Ramadan, Linnemayr and Alderman (2011) exploit a series of early life interventions in Senegal, while Chen and Zhou (2007) and show that exposure to the Chinese famine resulted in stunting for those affected. Gørgens et al. (2011) demonstrate that selection effects are also present for Chinese famine survivors. Early life exposure to disease (Cutler et al., 2010; McEniry and Palloni, 2010) and war (Blattman and Annan, 2010) have also been shown to have important effects on later outcomes. For a full review, see Cutler and Vogl (2013) and McEniry (2013). Although there are disadvantages associated with the twin approach, the benefit of the methodology used in this paper is that I am able to directly measure early life environment (with birth weight), and consider a set of health outcomes which are especially relevant for lower and middle income countries. In addition, I do not rely on a specific event for identification.

As outlined more formally in the following section, comparing the birth weight of twins (and to a lesser extent, siblings) provides a powerful means of accounting for unobserved factors which might otherwise bias estimates of the relationship between birth weight and later outcomes. For example, it is plausible that both are co-determined by some third factor which is difficult to measure, such as parental characteristics. However, there are potential drawbacks. Twins represent a relatively small fraction of total births (generally 1%), and although ideally it would be possible to directly measure their foetal nutritional intake, this is unlikely to be feasible in practice. Recent research supports the hypothesis that differences in nutritional intake, specifically the structural arrangement of the foetuses, are a likely source of disparities in birth weight (Royer, 2009). There is medical evidence which is consistent with this view, at least among children who share the same placenta (Bajoria et al., 2001). However, the validity of this approach depends on how birth weight discordances arise. For example, if within-twin discordance occurs due to differential caloric consumption, this is of interest to policy makers as this is a mechanism which is potentially susceptible to intervention, for example via programmes to improve maternal nutrition. However, as noted by Almond et al. (2005), if size disparities arise due to other factors such as blood supply, then the implications are less clear.

Under certain conditions, where birth weight was a function of say, chance placement in the womb, then within twin pair differences could be viewed as being as good as randomly assigned,[2] approximating a natural experiment. Twins would then present the ideal opportunity to study the effects of birth weight. However, it is not clear whether these conditions are met. Factors which have been cited as determining the allocation of intra-uterine resources include implantation location of the placenta in the womb, nutritional sources at insertion point, and differential growth potential (for dizygotic twins). For identical twins who share the same placenta (monochorionic births, which occur in roughly 75% of monozygotic twins), the location of the insertion of the umbilical cord in the placenta is also likely to affect nutritional intake. It follows that there are two possible interpretations of estimates from twin studies. One is that within twin pair estimates of the effect of birth weight represent a causal parameter which is informative for policy intervention. The alternative

[2] In the sense that in a regression of child outcomes on birth weight and twin pair fixed effects, the within twin pair differences in birth weight would be independent of the within twin pair differences in the error term.

is that there are numerous sources of birth weight differences, each of which has a differential effect on later outcomes (Almond et al., 2005). A conservative interpretation of the results in this paper is therefore that birth weight represents a proxy for general in-utero environment, in which case a comparison of full sample, sibling, and twin births is informative from the perspective of determining the extent to which estimates of the effects of early life conditions on later outcomes are affected by unobserved heterogeneity at both the family and child levels.

In addition to the possibility that different sources of variation in birth weight could have heterogeneous effects, there are a number of other concerns. Firstly, the extent to which it is possible to generalise from multiple births to singletons is not clear. For example, twins have higher rates of mortality, lower birth weight, shorter gestation, and shorter birth intervals. Nevertheless, recent research indicates that twins do not experience greater morbidity or mortality risk than singletons, conditional on survival past their first year (Öberg et al., 2012). There is also a loss of efficiency associated with both sibling and twin models, as siblings and twins represent a subset of the population of births, and information on singletons is ignored in family fixed effect models. This issue is exacerbated in the DHS because detailed information is only collected on births in the previous 5 years. Secondly, data on zygosity is rarely available. Therefore, it is difficult to rule out genetic differences as a potential explanation. For example, an individual child may be small, and yet not suffer any adverse long term effects due to having met their actual growth potential. In addition, even among identical twins, information on chorionicity is difficult to obtain, and this may influence the magnitude of the effects of birth weight. A similar issue is the absence of data on gestational age in many surveys, which could potentially confound estimates of the impact of birth weight. Some cross-sectional studies have found that prematurity independently predicts child outcomes (Katz et al., 2013), while some twin studies have found that, conditional on birth weight, there is little effect of gestational age (Oreopoulos et al., 2008; Royer, 2009). An attractive feature of twin studies is that they implicitly control for gestation without having to measure it directly. However, this does not rule out the potential for the effects of birth weight to be more severe for premature births.

Finally, there may be differential investment behaviour by parents (Almond and Mazumder, 2013). For example, the less well-off twin (in terms of lower birth weight) may be provided with more health care. Alternatively, parents could conceivably direct more effort towards the better-off twin, depending on the cost of health inputs and the return to later health (Bharadwaj et al., 2013; Rosenzweig and Zhang, 2009). Although these outcomes lie on the causal pathway, from a policy perspective it may be desirable to isolate the direct biological effect from the indirect effect due to differential parental investment, particularly if these effects are malleable or heterogeneous by household type.

It is important to note that most of these concerns also apply to cross-sectional or sibling estimates of the effects of birth weight. However, twin models will provide results which are more biased (and inconsistent) than cross sectional estimates if endogenous variation accounts for a greater fraction of within twin differences in birth weight than of between family differences in birth weight (Bound and Solon, 1999). Therefore, I return to address each of these issues in the empirical section of the paper.

3 Data

The Demographic and Health Surveys (DHS) are a series of cross sections which are nationally representative of women aged 15-49 in participating countries, of which there have been 90 since 1984. As well as including a wide range of information on socio-demographic characteristics of families and households, detailed birth histories on all children born within the previous 5 years are collected. Anthropometric data (including height, weight, and, in some surveys, haemoglobin) are also taken from children who are alive at the time of interview. For a detailed description, see Corsi et al. (2012). I include all surveys from rounds 2-6 which were available at the time of analysis, and which had information on household assets and birth weight.[3] The analysis sample includes 161 country-years, 66 countries, and 1,151,556 children born between 1985 and 2011.

The main advantages of the data are the global and temporal coverage, the focus on developing countries, the sample size, the collection of anthropometric outcomes of specific policy importance, and the inclusion of siblings and twins. The disadvantages are the relatively short time frame for outcomes (up to 5 years of age), and the fact that birth weight is reported by the mother. Although previous research suggests that mother recall is reliable (Walton et al., 2000; Tate et al., 2005; O'Sullivan et al., 2000), including in developing countries (Subramanyam et al., 2010), measurement error is likely to be an important problem, particularly if it is non-random. Figure A1 in the appendix demonstrates that there is evidence of heaping in the birth weight distribution. A related issue is the fact that birth weight is missing for around 50% of children. Focusing on complete cases could provide a selected sample and biased results. For example, birth weight might only be consistently measured in medical facilities, and a substantial proportion of births occur at home (46%). There is some evidence that ignoring missing data underestimates the extent of low birth weight (Moreno and Goldman, 1990).

Descriptive statistics for other variables used in the analysis are presented in table 1. Roughly 7% of children born in the five years prior to interview have died at the time of survey. There is a wide variety of data on the demographic characteristics of the mother and family, such as education, household assets, and a birth history calendar. More detailed information on recent pregnancies (within the past five years) is also collected, including ante-natal visits, whether the

[3] For an example of the questionnaire see: http://www.measuredhs.com/Publications/Publication-Search.cfm?type=35.

mother received a tetanus shot, and the mother's fertility goals. For children, there is further data on place of birth and anthropometrics. Height (or length, if the child is less than 2 years of age) and weight are measured by interviewers using digital scales and a measuring board. In some surveys, a capillary blood sample to measure haemoglobin content is taken using a finger or heel prick. Testing is then performed using a HemoCue photometer rapid test. Children were defined as anaemic if they measured haemoglobin content of less than 10 grams per deciliter, adjusting for altitude. In addition to these objectively measured data, the mother is asked to report whether the child suffered from coughing, fever, or diarrhoea, the reference period for most surveys being the previous two weeks. Full details of the data collection procedure are available as part of the DHS manuals.[4]

For the base specification, I follow the existing literature (e.g. Finlay et al., 2011). However, despite the rich data available, there is the nevertheless still the potential for omitted variable bias. Therefore, it is important to adopt an identificiation strategy to isolate the effect of interest. In addition to providing an identification strategy, another benefit of the method I outline is that when there are a large number of potential control variables, and many plausible interactions, matching siblings and twins is a powerful means of accounting for the bias that misspecification can induce. Finally, in some cases data on certain covariates is missing, and this approach is also useful for accounting for this.

4 Empirical Strategy

Consider a simple model for estimating the effects of birth weight on risk of death (equation 1), where the mortality of child i in family j is a flexible function of birth weight – $f(BW_{ij})$, a vector of standard control variables X_{ij}, a vector of family and parental characteristics D_j, with the subscript indicating that these characteristics do not vary within siblings or twin pairs, and an error term u_{ij}. We expect factors such as maternal and paternal ability, knowledge and investments to be present in D_j. For simplicity I assume linearity of the other terms, however this assumption can be relaxed using matching on twins. This is the framework which is typically adopted in twin studies (Almond et al., 2005; Ashenfelter and Krueger, 1994; Behrman et al., 1994; Behrman and Rosenzweig, 2004; Black et al., 2007; Conley et al., 2003; Figlio et al., 2013; Oreopoulos et al., 2008; Royer, 2009).

$$\text{Mortality}_{ij} = X_{ij}\gamma + \beta^k f(BW_{ij}) + D_j\theta + u_{ij} \quad (1)$$

β^k indicates the parameters associated with $f(\bullet)$, for example in the case of a quadratic $k = 2$. As discussed above, there are two related issues which must be accounted for before the causal

[4] http://measuredhs.com/pubs/pdf/DHSM7/DHS6_Biomarker_Manual_9Jan2012.pdf.

Table 1: Descriptive Statistics

Place of Birth	No.	%	**Current Marital Status**	No.	%
Own Home	535,434	46.5	Never Married	30,722	2.7
Other Home	55,407	4.8	Married	918,208	79.7
Government Hospital	258,692	22.5	Living Together	138,945	12.1
Government Health Center	168,594	14.6	Widowed	13,746	1.2
Private Hospital or Clinic	115,959	10.1	Divorced	16,967	1.5
Other and Unknown	9,982	0.9	Not Living Together	32,931	2.9
Missing	7,488	0.7	Total	1,151,519	100
Total	1,151,556	100			
			Religion		
Birth Interval			Christian	416,227	43.2
First Birth	285,408	24.8	Muslim	321,472	33.4
1-11	10,335	0.9	Jewish	2,481	0.3
12-17	65,926	5.7	Buddhist	26,149	2.7
18-23	116,642	10.1	Hindu	123,885	12.9
24+	673,245	58.5	Sikh	3,323	0.3
Total	1,151,556	100	Traditional	18,420	1.9
			Other	21,023	2.2
Mother's Age			None	28,542	3
15-19	71,611	6.2	Unknown	1,234	0.1
20-24	291,874	25.3	Total	962,756	100
25-29	326,686	28.4			
30-34	227,915	19.8	**Multiple Birth**		
35-39	146,177	12.7	Single Birth	1,120,925	97.3
40-44	66,573	5.8	1st of Multiple	15,189	1.3
45-49	20,691	1.8	2nd of Multiple	15,234	1.3
Total	1,151,527	100	3rd of Multiple	201	0
			4th of Multiple	7	0
Mother's Education			Total	1,151,556	100
No Education	432,185	37.5			
Primary	392,742	34.1	**Partner's Education**		
Secondary	270,511	23.5	No Education	312,748	27.2
Higher Education	55,859	4.9	Primary	364,076	31.6
Don't Know/Missing	259	0	Secondary	327,543	28.4
Total	1,151,556	100	Higher Education	85,562	7.4
			Don't Know/Missing	61,627	5.4
Size of Child at Birth			Total	1,151,556	100
Very Large	80,131	7			
Larger than Average	248,706	21.6	**Region**		
Average	583,861	50.7	India	154,576	13.4
Smaller than Average	159,558	13.9	North Africa/Europe	110,577	9.6
Very Small	61,898	5.4	South East Asia	152,055	13.2
Don't Know	17,402	1.5	Latin America	157,491	13.7
Total	1,151,556	100	Sub-Saharan Africa	576,857	50.1
			Total	1,151,556	100
Wealth Index					
Poorest	281,323	24.4	**Wanted a Pregnancy**		
Poorer	242,378	21	Then	815,894	70.9
Middle	227,922	19.8	Later	190,605	16.6
Richer	211,130	18.3	No More	140,000	12.2
Richest	188,803	16.4	Don't Know/Missing	5,057	0.4
Total	1,151,556	100	Total	1,151,556	100

	Mean	Median	SD	N
Survey Year	2003	2001.93	5.83	1,151,556
Year of Birth	2000	1999.58	5.93	1,151,556
Male	1	0.51	0.50	1,151,556
Months Since Birth	27	27.72	16.97	1,151,556
Child is Dead	0	0.08	0.26	1,151,556
Birth Weight	3100	3165.35	669.04	527,050
Birth Weight (With Imputations)	3000	3122.46	773.76	1,151,513
Birth Order (Birth History)	1	1.33	0.56	1,151,556
Has Flush Toilet	0	0.23	0.42	1,103,336
Has Piped Water in House	0	0.38	0.48	1,116,302
Urban	0	0.33	0.47	1,151,556
Mother Had Tetanus Shot	1	0.70	0.46	904,880
Mother Had Ante-Natal Visit	1	0.79	0.41	912,190
Height for Age Z Score	-1.55	-1.49	1.78	746,694
Height for Weight Z Score	-0.1	-0.14	1.49	736,822
Stunting	0	0.39	0.49	746,694
Wasting	0	0.10	0.30	736,822
Fever	0	0.27	0.44	1,025,331
Cough	0	0.29	0.45	1,034,954
Diarrhea	0	0.16	0.37	1,046,605
Anemia	1	0.60	0.49	221,489
Haemoglobin	106	104.44	20.35	214,162

Source: Demographic and Household Surveys Waves 2-6

effects of in utero environment can be established. The first relates to missing data, as we only observe birth weight for a certain proportion of the sample. If measurement error is random, the addition of white noise will have the effect of biasing the estimated coefficient on birth weight towards zero (Hausman, 2001). However, suppose instead that the presence of birth weight data also reflects some unmeasured attribute of the child or family which is related to both infant health and the outcome of interest, such as a component of neighbourhood or SES. For example, children without birth weight data may live in areas without access to health care facilities. Imputation could potentially induce additional mis-measurement which could be systematically correlated with unobservables. The measurement error problem can be represented as follows, where the observed birth weight (either present in the data or imputed because of missingness) is some function of true birth weight (BW^*_{ij}) and some aspect of family environment (z_j). z_j could potentially be a subset of D_j or include additional variables.

$$\mathrm{BW}_{ij} = BW^*_{ij} + z_j + e_{ij} \ (2)$$

In addition to the data which are missing, even if birth weight is observed in the data, it is possible that measurement error is systematically associated with some background characteristic of the mother. Suppose women with less education tend to under-estimate the birth weight of their children, and that mother's education also impacts positively on their children's weight and height.

Results obtained under the assumption of missing at random would then be biased upwards in a similar manner to if there was omitted variable bias in equation (1), due to correlation between the measure of birth weight and the error term, u_{ij}. Suppose instead that this reporting bias is a function of some indicator of parental characteristics which is not observed in the data, P_j. From equations 1 and 2, we then have (under the assumption that e_{ij} is a random noise component with conditional mean 0 and is uncorrelated with u_{ij}):

$$\text{Mortality}_{ij} = X_{ij}\gamma + \beta^k f(BW^*_{ij} + z_j) + D_j\theta + P_j\tau + u_{ij} \quad (3)$$

The estimates of the effects of birth weight on mortality (β^k, the parameter(s) of interest) will be biased even when birth weight is instrumented, except when the instrument is uncorrelated with the family fixed effect. In addition, it is not possible to estimate this model as we do not observe parental behaviour. An obvious solution to this problem is to use a sibling comparison model, given that we have data on multiple children per family. However, a fixed effects model will typically exacerbate the measurement error problem unless it is highly correlated within groups (either siblings or twins). In the case of classical measurement error and a linear specification (i.e. equation (2) without z_j), it can be shown (e.g. Deaton, 1997; Griliches, 1979; Kohler et al., 2011) that:

$$\text{plim}(\beta^{FE}_{BW}) = \beta(\frac{1-\sigma^2_{e_{ij}}}{\sigma^2_{BW^*_{ij}}(1-\rho_{BW})})$$

Where ρ_{BW} is the within group correlation in birth weight. If the within group measurement error is correlated, as implied by the inclusion of z_j in (2) above, then:

$$\text{plim}(\beta^{FE}_{BW}) = \beta(1 - [(1-\rho_z)(1-\rho_{BW}).\sigma^2_{e_{ij}}.\sigma^2_{BW^*_{ij}})])$$

So that $plim(\beta^{FE}_{BW}) = \beta$ only where $\rho_z = 1$ (the within twin pair measurement error is perfectly correlated), and in general the fixed effects estimate of (3) will be more inconsistent than an OLS model as long as $\rho_z < \rho_{BW}$. For example, in the context of estimating the effects of education, plausible amounts of noise results in attenuation bias of 8% for OLS estimates, 16% for dizygotic twin models, and 32% for monozygotic twin models (Kohler et al., 2011).

However, when an alternative measure of the variable of interest is available, it is still possible to obtain consistent estimates when there is correlated measurement error by instrumenting for differences in the explanatory variable of interest with differences in the secondary measure. This approach has been previously applied to the case of estimating the returns to schooling in twins where twin reports of the other's education are used as an instrument for their own (Ashenfelter and Krueger, 1994; Behrman et al., 1994). Applying the usual fixed effects transformation to the model above (where the family means are subtracted from the individual level variables), and instrumenting for birth weight, we obtain:

$$\widetilde{Mortality}_{ij} = \widetilde{X_{ij}}\gamma + \beta^k f(\widehat{\widetilde{BW}_{ij}}) + u_{ij} \quad (4)$$

Where $\tilde{}$ donates the within family transformation such that $\widetilde{x_{ij}} = x_{ij} - {}^1/_{T_j} \sum_{t=1}^{T_j} x_{ijt}$ for all T_j members of family j. The advantage of using twin data is that it allows for initial parental investment or other endowments to vary across siblings, as each of $z_j - \widetilde{z_j}$, $D_j - \widetilde{D_j}$, and $P_j - \widetilde{P_j}$ can more reasonably be assumed to be zero. $\widehat{\widetilde{BW}_{ij}}$ is the predicted value from the equation:

$$\widehat{\widetilde{BW}_{ij}} = \widetilde{X_{ij}}\delta + \sum_{k=1}^{4} \phi_k \widetilde{\text{Size at Birth}}_{kij} + \kappa_{ij} \quad (5)$$

Where differences in birth weight are instrumented with differences in the reported Size at $\text{Birth}_{kij}, k = 1, 2, 3, 4$, ranging from "very small" to "large". The two measures (birth weight and size) are highly correlated, as confirmed by first stage partial F statistics and as illustrated in figure A2 in the appendix. The approach of augmenting the DHS birth weight data with auxiliary information on size has been previously recommended (Blanc and Wardlaw, 2005; Moreno and Goldman, 1990), although not yet implemented in the framework of instrumental variables. This method does require an exclusion restriction for consistency, which is that the measurement error in birth weight and size at birth is uncorrelated (conditional on mother or twin pair fixed effects). As it seems plausible that there would be systematic reporting differences across siblings, even conditional on having the same mother (for example, due to recall), this highlights another advantage of using data on twins, where the coefficient(s) of interest is then identified from differences in relative size within twin pairs.

In summary, the estimation procedure is as follows. I first impute the missing values using predictive mean matching, which has the advantage of returning a distribution which matches the observed bounds on birth weight (Little, 1988). The imputation model includes all covariates to be used in the regression model for the effects of birth weight, with the addition of the outcomes of interest and size at birth. Two models are run, one for mortality, and one for the other health outcomes combined. In addition, missing values are imputed separately by country. Although this approach is likely to induce measurement error, under the exclusion restriction the instrumental variables strategy should account for this problem. The existence of attenuation bias is supported by the empirical results. Additionally, the resulting estimates for the effects of birth weight in the preferred IV fixed effects model do not appear to be sensitive to the implementation of the imputation model.

Although 2,500g is the typical cut-off for low birth weight, previous research has found that the presence of a discontinuity at this value is not necessarily supported by the data, in that the optimal birth weight is likely to be substantially higher (Royer, 2009). Therefore, I begin by investigating the functional form for the effects of birth weight on the childhood outcomes using a restricted cubic spline approach. This model takes the form of a function with continuous first and second derivatives, specifically a cubic function between adjacent knots $KN_1 < KN_2 < ... < KN_K$, and a linear function for $KN_K < BW_{ij} < KN_1$ (Korn and Graubard, 1999). For 3 knots and one independent variable, this can be defined as follows:

$$\widetilde{Mortality}_{ij} = (BW_{ij} - KN_1)^3_+ - \frac{KN_3 - KN_1}{KN_3 - KN_2}(BW_{ij} - KN_2)^3_+ + \frac{KN_2 - KN_1}{KN_3 - KN_2}(BW_{ij} - KN_3)^3_+ \quad (6)$$

Where $(BW_{ij})_+ = BW_{ij}$ if $BW_{ij} > 0$, and 0 otherwise. The results of this preliminary analysis are shown in figure 2.[5] In all cases, non-linearity is apparent, with optimal weight lying above the standard 2,500g low birth weight threshold. This is in line with the findings in Almond et al. (2005) and Royer (2009), who also implement a similar analysis using linear spline functions. Figure A3 in the appendix illustrates the corresponding analysis for twins. Particularly for the twin sample, the addition of control variables increases the associated confidence intervals for heavier babies, but does not substantially alter the conclusions. Another pattern is apparent; increases in birth weight above 4,000g are generally associated with worsening outcomes (with the exception of stunting and anaemia). An alternative is a log linear specification, however this imposes diminishing returns (i.e. monotonicity), and precludes adverse effects at high birth weights. This affects relatively few babies, as 4,000g lies at the 90% percentile, but nevertheless, this analysis indicates that the effects are roughly quadratic. In section 6 I show that using a single indicator for low birth weight also suggests substantial effects.

Although in theory semi-parametric and non-parametric IV models could be adopted to adjust for measurement error, in practice the number of endogenous parameters (β^k) is limited to the number of suitable instruments. Given the functional form analysis in figure 2, I therefore adopt a more parsimonious approach specifying a second order polynomial for birth weight. Diagnostic tests confirm that this model is identified using the four categories of size at birth as instruments, as shown by the Anderson LM (1951) tests in table A12 in the appendix. Therefore, the final model is given by:

$$\widetilde{Mortality}_{ij} = \widetilde{X_{ij}}\gamma + \beta_1 \widehat{BW}_{ij} + \beta_2 \widehat{BW}_{ij}{}^2 + u_{ij} \quad (7)$$

I report the marginal effect of birth weight at 2,500g for comparability with previous literature, however improvements across the wider distribution are also likely to be of interest to policy makers.

[5] This graph shows the analysis for 3 knots, using additional knots gives similar results.

$$\frac{dMortality}{dBW} = \beta_1 + 2\beta_2 BW \ (8)$$

In the following section, I begin by presenting OLS results for the effects of birth weight where I control for the variables outlined in table 1 using the model in (7). Table A3 in the appendix presents a summary of the main outcomes for each of the samples (all births, births with birth weight data, siblings and twins). The most apparent feature of the data is that, as expected, twins are more disadvantaged on all measures. For example, mean birth weight is 2,600g for multiple births compared to 3,100g in the other samples. Mortality is 25%, compared to 7.5% in the full sample. Stunting is over 50%, compared to 30% for the full sample, and 40% for siblings.

Therefore, I determine whether this affects the generalizability of results by comparing different specifications across groups. I show estimates from models using complete data on birth weight for the OLS and IV models. For each of these cases, results are compared for all children, siblings and twins, with and without imputed values for missing birth weight data. If the effect of birth weight is found to be robust across these specifications, this would provide reasonable evidence that the effect was universal in the sense of being consistent in different populations.

An important question when estimating fixed effects models is the extent of variation in the variables of interest. Figure 1 indicates that there appears to be satisfactory variation in birth weight, with approximately 3,000 twin pairs having the same weight. The distribution is similar to that reported in Black et al. (2007) for Norway.[6] For example, their mean twin difference is 320g, compared to a mean difference of 318g in the DHS sample. The standard deviation for DHS twins is also comparable to the full DHS sample. Discordance probabilities for size at birth and the main outcomes of interest (mortality, stunting, fever, coughing, diarrhoea, and anaemia) are shown in table A2 in the appendix. For the former, the probability of twin 2 being the same size as twin 1 ranges from 64% to 79%. Ranges are similar for mortality, stunting and anaemia, but discordance probabilities are lower for fever, coughing and diarrhoea.

5 Results

5.1 Mortality

Table 2 presents results for under 5 mortality. The outcome is a binary variable indicating whether the child is alive at the time of interview. Children in the sample are up to 59 months of age. Following the previous twin literature, I use linear probability model for all specifications. Birth weight is entered as a quadratic, and the coefficient reported is the marginal effect at 2,500g. Two

[6] See figure 2 on page 420 of Black et al. (2007).

panels are presented, this first includes the full sample of children, while the second is restricted to twins. The first two columns use observations with reported birth weight. For the first panel, this amounts to data roughly 650,000 children. The third and fourth columns are based on the model where imputed birth weight (and birth weight squared) is used for missing observations. Columns 1 and 3 are the basic linear probability model. The instrumental variables fixed effects model is implemented in columns 2 and 4, where the coefficients are generated by sibling comparisons in the first panel, and twin comparisons in the second.[7] Birth weight and birth weight squared are instrumented with reported size at birth. First stage results are presented in the appendix in table A12. Tables A13 to A16 in the appendix also show the complete tables with coefficients on the other covariates.

As birth weight is measured in grams in the data, resulting estimates are multiplied by 200 so that the coefficients in table 2 indicate the effect of a 200g increase (at 2,500g). Strictly, it is more correct to think of the coefficient in terms of a 1g increase in birth weight, seeing as the derivative for the marginal effect, $\frac{dy}{dx} = \frac{dMortality}{dBW}|_{BW=2,500g}$ in (8), is theoretically only valid for a small change in x (so for example, the coefficient of -0.01 in table 2 more correctly indicates that a 1g increase in birth weight reduces the probability of mortality by $\frac{0.01}{5} = .002$ percentage points). However, Royer, (2009) gives 200g as being a plausible target for government intervention, and Almond et al. (2005) indicate that 200g is the improvement in birth weight for affected infants that could reasonably be expected from ending maternal smoking. 200g is also close to the estimated effect of participation in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) in the US (Kowaleski-Jones and Duncan, 2002). Therefore, this is likely to be a reasonable benchmark for evaluating effect sizes.

[7]There are a small number of triplets and quadruplets which are not included.

Figure 1: Twin Differences in Birth Weight

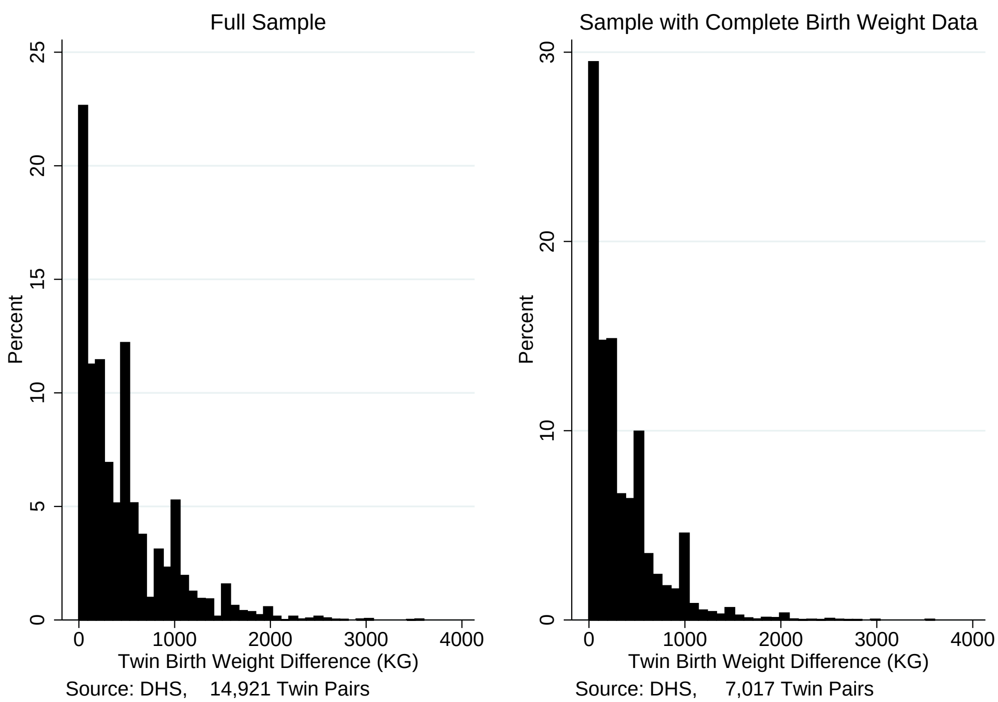
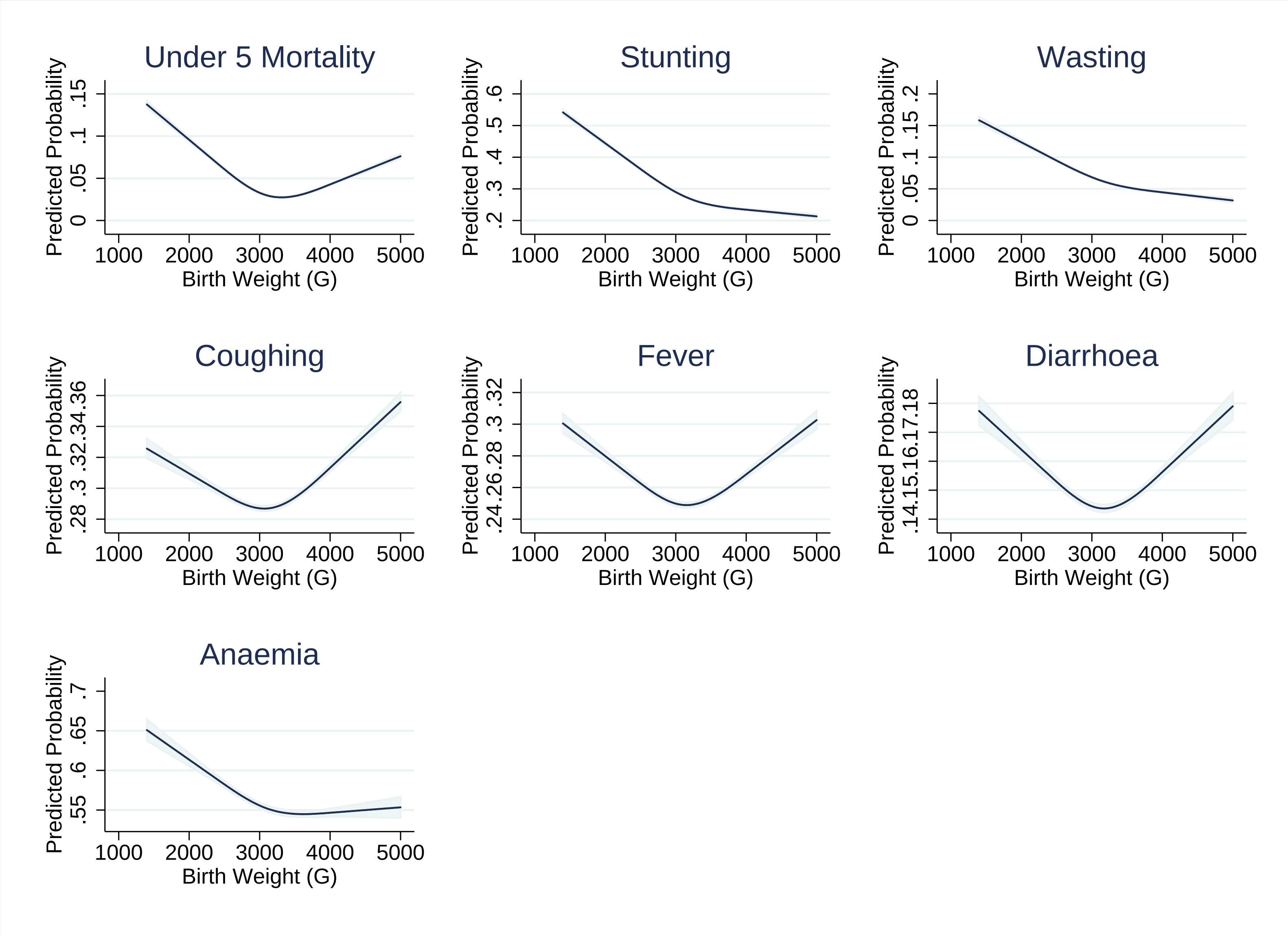


Figure 2: Restricted Cubic Spline Models for Birth Weight



Note: Graph shows the predicted probability of each outcome by birth weight using a restricted cubic spline model with 3 knots. 95% confidence intervals are shown, and adjusted for clustering at the household level. The sample uses all children with complete birth weight data.

Table 2: 200g Marginal Effect of Birth Weight on Under 5 Mortality (at 2,500g)

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.010***	-0.017***	-0.006***	-0.017***
	(0.000)	(0.001)	(0.000)	(0.001)
Imputations	No	No	Yes	Yes
Observations	527,027	263,214	1,151,490	644,047
	Twins			
Birth Weight	-0.017***	-0.008**	-0.011***	-0.008***
	(0.001)	(0.004)	(0.001)	(0.003)
Imputations	No	No	Yes	Yes
Observations	14,364	13,960	29,840	29,008

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the marginal effect of a 200g increase on the outcome at 2,500g estimated using a quadratic specification for birth weight. Columns 1 and 3 include controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Columns 2 and 4 implement the mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. The second panel uses the same specification, except restricting the sample to twins. The twin fixed effect models include controls for gender and birth order. The full table for columns 1 and 2 are presented in the appendix, as are tables showing first stage estimates. Standard errors are adjusted for clustering at the household level, as well as for 5 replications in the model which imputes missing data on birth weight and birth weight squared (Rubin, 1987).

Given that overall mortality is 7.5% in the sample, the effect size in table 2 appears to be substantial. There is a consistent and positive effect of birth weight on child mortality in all specifications, ranging from a .6 percentage point decrease in the risk of mortality per 200g increase, to a 1.7 percentage point decrease. The preferred twin IV fixed effects model on the full sample indicates an effect size of .8 percentage points.

In order to evaluate the implementation of the IV model, I conduct a number of additional analyses. Firstly, it is important to note that the first stage partial F statistics and corresponding Anderson LM (1951) tests indicate that the excluded instruments are relevant and that the model is identified. Secondly, given that there are four instruments (four categories of size at birth), and two endogenous regressors (birth weight and birth weight squared), it is feasible to conduct a test of overidentifying restrictions. For the preferred twin specification, we fail to reject the null that the instruments are valid.[8] Thirdly, the reduced form relationship between size at birth and mortality indicates a

[8] Table A12 in the appendix.

strong and negative association, including in the twin fixed effect models.[9] Finally, when I restrict the sample to countries with relatively lower rates of missingness for birth weight (<50%), I get very similar results.[10]

5.2 Child Health Outcomes

Table 3: 200g Marginal Effect of Birth Weight on Stunting (at 2,500g)

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.024***	-0.028***	-0.009***	-0.023***
	(0.000)	(0.002)	(0.000)	(0.001)
Imputations	No	No	Yes	Yes
Observations	338,540	160,804	746,662	388,988
	Twins			
Birth Weight	-0.023***	-0.020***	-0.010***	-0.023***
	(0.002)	(0.006)	(0.001)	(0.005)
Imputations	No	No	Yes	Yes
Observations	7,448	7,398	13,504	13,326

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

See note to table 2.

Table 3 presents a similar analysis for the effects of birth weight on stunting (more than 2 standard deviations below the WHO reference in terms of height for age). As with mortality, the outcome is a binary variable and I use a linear probability model. And as with mortality, the effect of birth weight is consistently negative. The cross sectional estimates for the full sample and twins are comparable. Overall, estimates indicate a reduction in the probability of stunting of between 2 and 2.3 percentage points per 200g increase in birth weight at 2,500g, depending on whether imputations are included or not. Table A5 in the appendix illustrates that effects are similar for wasting, with the corresponding results implying a 1.1 to 1.2 percentage point reduction per 200g.[11] Tables A6-A8 in the appendix present results for coughing, fever, diarrhoea and anaemia. In order

[9] Table A3 in the appendix.

[10] Table A4 in the appendix.

[11] Compared to stunting, height for weight (wasting) captures more immediate nutritional deprivation (Headey, 2012).

to address some of the potential limitations of twin studies raised in section 2, I also consider a series of robustness checks in tables A9-A10, and find little evidence of heterogeneous effects.

6 Mortality Selection

For the health outcomes considered, there is a selection problem, as we only observe the status of those children who survive. Given that we expect mortality to be higher among infants with low birth weight, and for them to have been in worse health, the assumption of missing at random is clearly not appropriate in this case, and could potentially bias estimates of the effect of birth weight. This issue has been widely studied in the treatment effects framework in labour economics, for example when wages are not observed due to absence from the job market. Considering the case of a continuous health outcome as a function of a binary treatment allows the adoption of the methodology applied in this literature. For example, when we wish to estimate the effect of birth weight on height for age, the underlying distribution is latent because we only observe the outcome for those who survive:

$$\text{Height for Age}^* = \alpha_1 + HBW\beta + \mu$$
$$\text{Survival}^* = \alpha_2 + HBW\theta + \epsilon$$
$$\text{Height for Age=I[Survival}^* \geq 0\text{]. Height for Age}^* \quad (9)$$

Where $I[.]$ is the indicator variable. Propensity to survive ($Survival^*$) is another latent variable which is also determined by birth weight. It is more intuitive to think of the treatment having a positive effect on survival, high birth weight (HBW) defined as $BW \geq 2,500g$, however the same argument applies where low birth weight is the treatment. Heckman (1979) illustrates how sample selection can be viewed in terms of omitted variable bias, and proposes a parametric correction based on the assumption of joint normality of the error terms (μ and ϵ). In practice, Heckman type selection models require an exclusion restriction for identification (Madden, 2008), and here it is problematic to conceive of a variable which would predict mortality and not underlying health.

Given the absence of a suitable selection variable, an alternative is to adopt a bounding approach. Lee (2009) uses the insight that the outcome we observe (for those who survive) for those receiving the treatment of high birth weight is a weighted average of the mean among two subgroups, the mean among those inframarginal individuals who would have survived regardless of treatment (even if they had been low birth weight), and the mean among those marginal individuals who were induced to survive by not receiving the treatment (would have died if they had been low birth weight).

$$\text{E[Height for Age } \mid \text{ High Birth Weight } = 1, \text{ Survival}^* \geq 0] =$$

$$\text{(1-p)}\ \text{E}\,[\text{ Height for Age } \mid \text{High Birth Weight } = 1, \epsilon \geq -\alpha_2]$$
$$+\ \text{(p)}\ \text{E}[\text{ Height for Age } \mid \text{High Birth Weight } = 1, -\alpha_2 - \theta \leq \epsilon < -\alpha_2]\ (10)$$

And the weights p are defined by the proportion of marginal individuals who are only in the observed sample as a result of not being low birth weight:

$$\text{p}= \frac{Pr[-\alpha_2 - \theta \leq \epsilon < -\alpha_2]}{Pr[-\alpha_2 - \theta \leq \epsilon]}\ (11)$$

The point is that if the mean for the inframarginals was observed, it would be possible to estimate the treatment effect of high birth weight (β), because the mean among this group is defined by:

$$\text{E}[\text{ Height for Age } \mid \text{High Birth Weight } = 1, \epsilon \geq -\alpha_2] =$$
$$\alpha_1 + \beta + E[\mu \mid \text{High Birth Weight } = 1, \epsilon \geq -\alpha_2]\ (12)$$

And the observed population mean for the control group is:

$$\text{E}\,[\text{ Height for Age } \mid \text{High Birth Weight } = 0,\ \text{Survival}^* \geq 0] =$$
$$\alpha_1 + E[\mu \mid \text{High Birth Weight } = 0, \epsilon \geq -\alpha_2]\ (13)$$

Under the assumption that the error terms in both equations (α_1,α_2) are jointly independent of the treatment of high birth weight, an estimate of β can be obtained by subtracting (13) from (12), the intuition being that there is no selection effect for the inframarginal group. Although (13) is not observed, an upper bound can be obtained by considering the case where the marginal group have the lowest p values of Height for Age, where p is defined by:

$$\text{p}= \frac{Pr[\text{ Survival}^* \geq 0 \mid \text{High Birth Weight } = 1] - Pr[\text{ Survival}^* \geq 0 \mid \text{High Birth Weight } = 0]}{Pr[\text{ Survival}^* \geq 0 \mid \text{High Birth Weight } = 1]}$$
$$(14)$$

And then:

$$\beta^{UB} =$$
$$E[\text{ Height for Age } \Big| \text{High Birth Weight } = 1,\ \text{Survival}^* \geq 0,\ \text{Height for Age} \geq \text{ Height for Age}_p]$$
$$\text{- E}[\ \text{Height for Age } \mid \text{High Birth Weight } = 0,\ \text{Survival}^* \geq 0]\ (15)$$

The first term in (15) is estimated by obtaining the mean Height for Age in the treatment group removing the lowest p values. It seems reasonable to focus on the upper bound here, given that it is difficult to imagine how being low birth weight could improve health. Similarly though, a lower bound could be obtained by examining the case where those in the marginal group comprise the highest p values of Height for Age. Lee (2009) shows that this approach of estimating the treatment bounds is $\sqrt{n}$ consistent and asymptotically normal. Moreover, this results holds under more general selection models, provided independence of the treatment (from selection and potential outcomes), and monotonicity of the selection effect given treatment. Random assignment would guarantee the first assumption, however this clearly does not apply to birth weight. Results should be interpreted with this limitation in mind, however, there is also no clear evidence from the analysis presented above that birth weight is endogenous.

Table 4 presents upper bounds for the effects of height for age and height for weight using this procedure and the sample with complete birth weight data. It is possible to extend the model presented above to include covariates, although the trimming procedure is then applied within cells of the control variables (which must be categorical), which means that not all covariates can be included. In this case, doing so had little effect on the estimated bounds. There is evidence of negative selection; the prevalence of low birth weight among those who are alive (and have data on health outcomes) is 11%, compared to 23% among those who are dead (and have missing data on health outcomes). The OLS model indicates that being low birth weight lowers height for age by .58 standard deviations, while the upper bound for the effect using the trimming procedure indicates that the coefficient could be as high as .82 standard deviations. Similarly for height for weight, the OLS model indicates a coefficient of -.45 deviations as the penalty for low birth weight, while the lower bound is estimated at -.68. Therefore, this provides some preliminary indication that the coefficients presented here may underestimate the adverse effects of low birth weight, potentially substantially, depending on the extent of selection induced by mortality.

7 Conclusions

This paper provides evidence on the relationship between birth weight and child outcomes in developing countries. The empirical approach accounts for missing data, measurement error, potential omitted variable bias, and mortality selection. There is clear evidence of an effect of birth weight on mortality, stunting, wasting, and coughing, and to a lesser extent for fever, diarrhoea and anaemia.

Overall, the IV results support the existence of measurement error in the raw data. Once the correction is applied using size at birth as an instrument, results are consistent with important effects of birth weight on child outcomes. This highlights the importance of adjusting for attenuation bias where birth weight is reported by the mother, a phenomenon which is known to be exacerbated in twin and sibling comparisons (Griliches, 1979). Models accounting for selection bias due to missing data on children who have died suggest that the effects of low birth weight on health outcomes are underestimated, potentially substantially.

Table 4: Treatment Effects of Low Birth Weight Under Mortality Selection

Mortality by Low Birth Weight

	Alive		Dead	
	N	%	N	%
Not LBW	301,571	89.08	16,857	76.7
LBW	36,980	10.92	5,122	23.3
Total	338,551	100	21,979	100

Selection Model Estimates

Variables	OLS Height for Age	Selection Model Height for Age	OLS Height for Weight	Selection Model Height for Weight
Low Birth Weight	-0.577***		-0.456***	
	(0.009)		(0.008)	
Upper Bound		-0.820***		-0.684***
		(0.010)		(0.009)
Observations	338,551	360,530	333,815	355,802

Bootstrap standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The top panel shows the proportion of children who are alive by low birth weight (<2,500g). Those with missing birth weight data are excluded. The bottom panel shows OLS regressions for height for age Z score and height for weight Z scores in columns 1 and 3, while upper bound estimates using the Lee (2009) procedure are displayed in columns 2 and 4. Bootstrap standard errors are shown in parentheses.

An important limitation of this approach is that although the twin literature typically appeals to differences in nutritional intake as the source of these differences (Almond et al., 2005; Black et al., 2007; Royer, 2009), it is not clear as the extent to which birth weight itself is a causal factor in later outcomes, or whether alternative sources of birth weight differences have heterogeneous effects. Timing of exposure to adversity in utero is likely to be important for later outcomes (Ekamper et al., 2013), From this perspective, it may be better to view the results presented here as indicating the effect of in utero environment, for which birth weight is likely to be a reasonable proxy. However, recent research using diagnosis of placenta previa as an instrumental variable indicates that birth weight itself may have direct effects, at least on childhood outcomes (Maruyama et al., 2013).

A summary of potential interventions targeting birth weight which could be implemented in developing countries are discussed in Bhutta et al. (2008). Birth weight is correlated with many characteristics of family background (McGovern, 2013), however randomised control trials are the ideal way of informing policy makers about the effectiveness of nutritional supplements during pregnancy, and there is some evidence to support this type of approach, although the type of supplement and context are likely to influence the outcome (Ceesay et al., 1997; Christian et al., 2003; Cogswell et al., 2003). The causal determinants of birth weight are beyond the scope of this paper, however ultimately advances may only be achieved via improvements in poverty and education. The recent focus on the importance of the education of women (Duflo, 2012), and family planning (King et al., 2007; Miller, 2010), are likely to have the added benefit of improving birth weight, thus contributing to help more children reach their full developmental potential.

Given the evidence on intergenerational effects of birth weight (Currie and Moretti, 2007; Lumey, 1992; Victora et al., 2008), any improvements in infant health are likely to have additional benefits which accrue over many years, and possibly decades. Moreover, the evidence linking health to productivity indicates potentially large economic returns to infant health (Caulfield et al., 2006). Nutrition and health are also linked to productivity (Haddad and Bouis, 1991; Hoddinott et al., 2008; Maluccio et al., 2009; Strauss, 1986; Thomas and Strauss, 1997), for example anaemia among women in Sierra Leone is estimated to cost $19 million per year (Aguayo et al., 2003). Therefore, results in this paper indicate that investments targeted at raising birth weight are likely to have a substantial long run impact on the affected individuals and societies.

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Appendix – Not Intended for Publication

Additional Figures and Tables

Figure A1: DHS Birth Weight Distribution

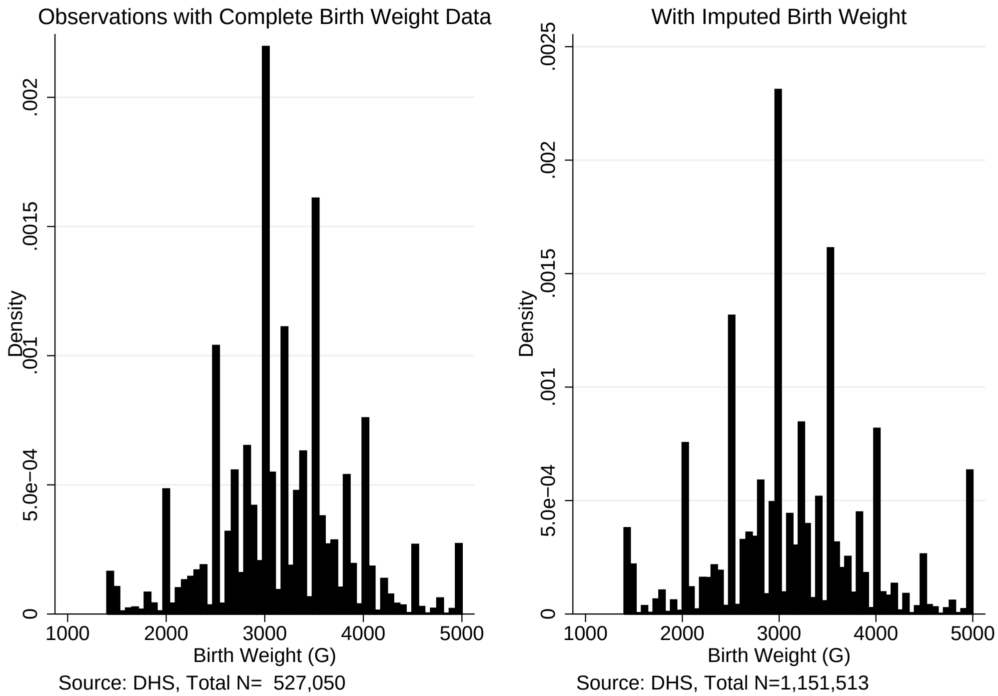
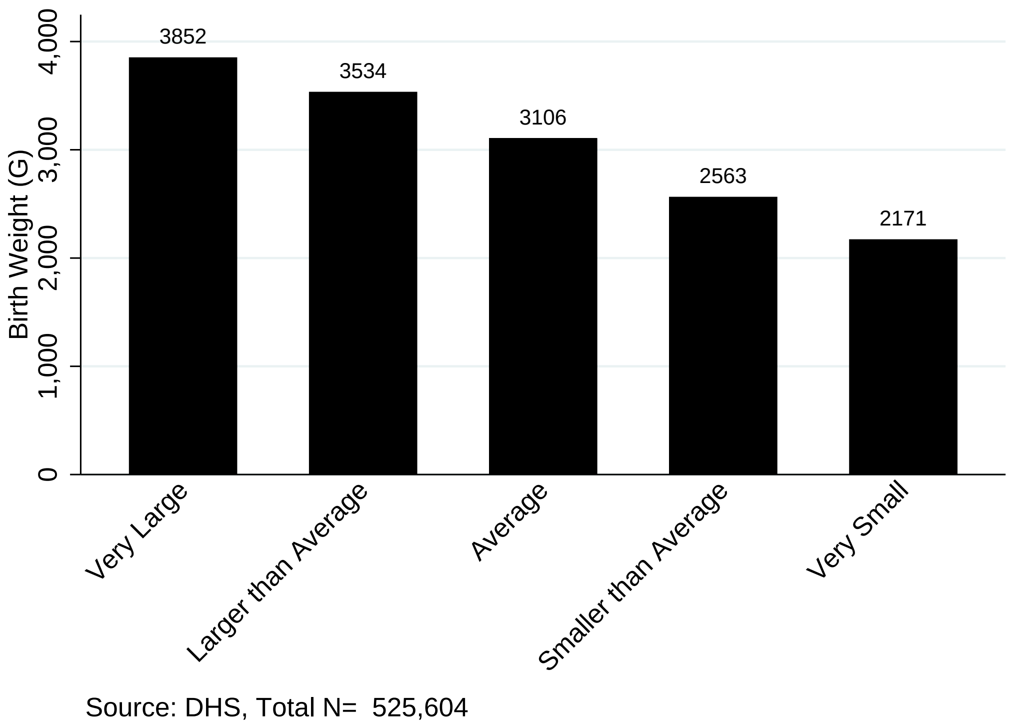
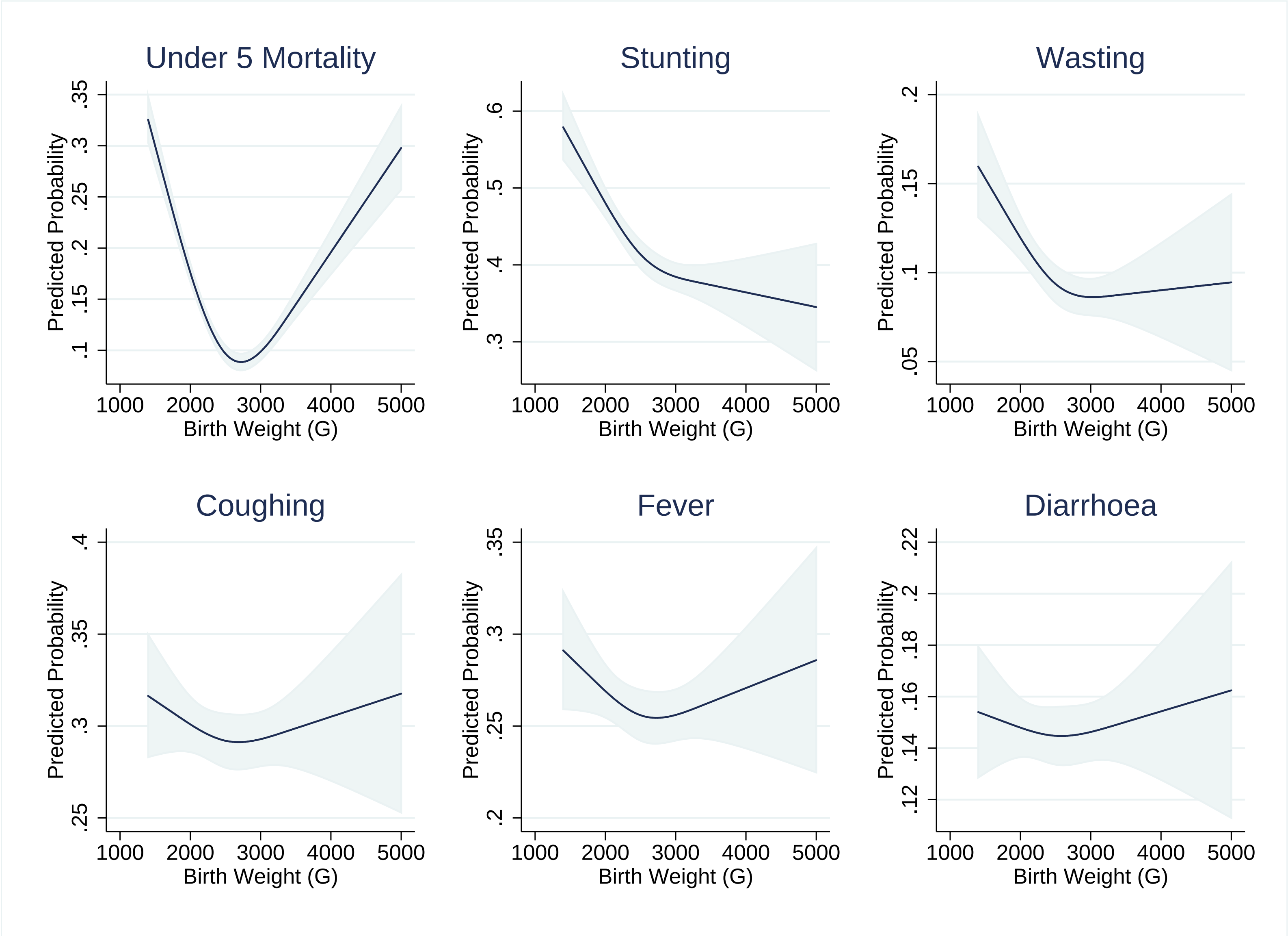


Figure A2: Birth Weight by Reported Size at Birth



Note: The sample is restricted to those with complete birth weight data.

Figure A3: Restricted Cubic Spline Models for Birth Weight (Twins)



Note: Graph shows the predicted probability of each outcome by birth weight using a restricted cubic spline model with 3 knots. 95% confidence intervals are shown, and adjusted for clustering at the household level. The sample uses all twins with complete birth weight data.

Table A1: Sample Characteristics

	Full Sample		Complete BW		Siblings		Twins	
	Mean	N	Mean	N	Mean	N	Mean	N
Birth Weight	3124.945	1,151,513	3166.457	527,050	3111.137	655,587	2581.916	30,423
	(777.844)		(667.027)		(789.785)		(770.062)	
Mortality	0.077	1,151,556	0.045	527,050	0.1	655,600	0.256	30,423
	(0.266)		(0.207)		(0.3)		(0.436)	
Smaller than Average	0.193	1,134,154	0.163	525,604	0.202	644,055	0.41	29,769
	(0.394)		(0.369)		(0.401)		(0.492)	
Stunting	0.39	746,694	0.29	338,551	0.406	393,281	0.512	16,491
	(0.488)		(0.454)		(0.491)		(0.5)	
Fever	0.268	1,025,331	0.259	480,018	0.246	533,741	0.283	22,073
	(0.443)		(0.438)		(0.431)		(0.45)	
Cough	0.288	1,034,954	0.299	486,763	0.271	538,091	0.298	22,154
	(0.453)		(0.458)		(0.445)		(0.458)	
Diarrhoea	0.162	1,046,605	0.15	494,228	0.151	544,210	0.164	22,381
	(0.368)		(0.357)		(0.358)		(0.37)	
Anaemia	0.61	221,489	0.57	104,848	0.621	116,229	0.646	5,139
	(0.488)		(0.495)		(0.485)		(0.478)	
Haemoglobin	104.076	214,162	105.925	100,063	103.585	112,137	101.524	4,841
	(20.652)		(22.149)		(19.343)		(22.407)	
Height for Age	-1.513	746,694	-1.159	338,551	-1.568	393,281	-2.021	16,491
	(1.78)		(1.665)		(1.782)		(1.776)	
Height for Weight	-0.157	736,822	0.072	333,823	-0.163	388,048	-0.333	16,328
	(1.493)		(1.419)		(1.488)		(1.529)	

Source: DHS waves 2-6.

Table A2: Discordance Probabilities for Twins

Size at Birth						
	Very Large	Larger	Average	Smaller	Very Small	Total
Very Large	63.51	11.22	13.43	6.48	5.37	100
Larger	3.48	63.03	20.45	10.11	2.92	100
Average	1.29	5.28	78.69	11.46	3.28	100
Smaller	0.92	5.13	18.12	69.62	6.21	100
Very Small	1.81	3.25	7.98	12.25	74.71	100

Mortality

	No	Yes	Total
No	85.35	14.65	100
Yes	44.51	55.49	100

Fever

	No	Yes	Total
No	91.78	8.22	100
Yes	22.59	77.41	100

Stunting

	No	Yes	Total
No	0 81.90	18.1	100
Yes	17.98	82.02	100

Diarrhoea

	No	Yes	Total
No	95.13	4.87	100
Yes	24.87	75.13	100

Cough

	No	Yes	Total
No	92.76	7.24	100
Yes	17.81	82.19	100

Anaemia

	No	Yes	Total
No	66.49	33.51	100
Yes	20.16	79.84	100

Note: Rows show the probability of twin 2 being in a particular category given the category of twin 1, while columns shown the probability of twin 1 being in a particular category given the category of twin 2.

Table A3: Reduced Form Estimates for Size at Birth and Under 5 Mortality

Variables	Under 5 Mortality OLS All Children	Under 5 Mortality OLS Sibling FE	Under 5 Mortality OLS Twins	Under 5 Mortality OLS Twin FE
Size at Birth: Very Small				
Very Large	-0.0436***	-0.0736***	-0.0651***	-0.00730
	(0.00174)	(0.00310)	(0.0158)	(0.0288)
Larger than Average	-0.0524***	-0.0819***	-0.101***	-0.0412**
	(0.00152)	(0.00258)	(0.0110)	(0.0209)
Average	-0.0551***	-0.0797***	-0.119***	-0.0490***
	(0.00146)	(0.00240)	(0.00894)	(0.0175)
Smaller than Average	-0.0371***	-0.0509***	-0.0918***	-0.0338*
	(0.00159)	(0.00259)	(0.00952)	(0.0173)
Controls	Y	Y	Y	Y
Observations	1,134,095	644,055	29,202	29,204

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the association between size at birth and under 5 mortality. Column 1 controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Column 2 implements a mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. Column 3 uses the same specification as column 1, except restricting the sample to twins. The twin fixed effect model in column 4 includes controls for gender and birth order. Standard errors are adjusted for clustering at the household level.

Table A4: Marginal Effect of Birth Weight on Under 5 Mortality for Countries with <50% Missing

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.011***	-0.018***	-0.009***	-0.017***
	(0.000)	(0.001)	(0.000)	(0.001)
Imputations	No	No	Yes	Yes
Observations	345,165	169,328	499,685	261,857
	Twins			
Birth Weight	-0.017***	-0.014***	-0.015***	-0.014***
	(0.002)	(0.005)	(0.001)	(0.004)
Imputations	No	No	Yes	Yes
Observations	8,905	8,680	12,812	12,432

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the marginal effect of a 200g increase on the outcome at 2,500g estimated using a quadratic specification for birth weight. The sample is restricted to children in countries with less than <50% missing data on birth weight. Columns 1 and 3 include controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Columns 2 and 4 implement the mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. The second panel uses the same specification, except restricting the sample to twins. The twin fixed effect models include controls for gender and birth order. The full table for columns 1 and 2 are presented in the appendix, as are tables showing first stage estimates. Standard errors are adjusted for clustering at the household level, as well as for 5 replications in the model which imputes missing data on birth weight and birth weight squared (Rubin, 1987).

Table A5: 200g Marginal Effect of Birth Weight on Wasting (at 2,500g)

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.007***	-0.008***	-0.003***	-0.012***
	(0.000)	(0.001)	(0.000)	(0.001)
Imputations	No	No	Yes	Yes
Observations	333,815	158,639	736,793	383,847
	Twins			
Birth Weight	-0.006***	-0.012***	-0.003***	-0.011***
	(0.002)	(0.004)	(0.001)	(0.004)
Imputations	No	No	Yes	Yes
Observations	7,314	7,216	13,328	13,148

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the marginal effect of a 200g increase on the outcome at 2,500g estimated using a quadratic specification for birth weight. Columns 1 and 3 include controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Columns 2 and 4 implement the mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. The second panel uses the same specification, except restricting the sample to twins. The twin fixed effect models include controls for gender and birth order. The full table for columns 1 and 2 are presented in the appendix, as are tables showing first stage estimates. Standard errors are adjusted for clustering at the household level, as well as for 5 replications in the model which imputes missing data on birth weight and birth weight squared (Rubin, 1987).

Coughing, Fever, Diarrhoea and Anaemia

Table A6 presents results for coughing. The preferred twin IV fixed effect model implies that a 200g increase in birth weight is associated with a reduction of .9 to 1 percentage points in the probability of suffering from coughing in the previous two weeks, as reported by the mother. For fever in table A6 in the appendix, results are less clear. The twin estimates indicate a smaller reduction (compared to OLS) of between .3 and .4 percentage points, but are not statistically significant distinguishable from 0. However, given the inefficiency of twin models, a more relevant comparison is a test against the full sample OLS results, and in this case we cannot reject that they are equal. The latter OLS model indicates an effect size of between .04 and .05 percentage points. The results for diarrhoea in table A7 are similar. The twin IV fixed effect models are not distinguishable from 0, but we cannot reject that they are equal to the complete case OLS estimate of .3 percentage points.

Table A9 illustrates the corresponding estimates for anaemia. Due to the lack of coverage for biomarker measures, and resulting reduced sample size, I only present sibling models, and not twins. Here, the interpretation is more ambiguous than for the previous cases. The results differ depending on whether the imputed values are used in the estimation or not, perhaps reflecting the reduced sample size. For the full sample, the magnitude of the effect of birth weight on the probability of being anaemic is .4 percentage points per 200g in the IV fixed effects model. However, the IV fixed effects coefficient ranges from -.001 to -.007. The confidence interval for this estimate includes the cross sectional estimate for both complete case and cross sectional estimates, although it is statistically different from the IV fixed effects complete case model. Overall, the evidence in favour of an effect of birth weight on these outcomes is not as strong, perhaps reflecting that these biomarkers are only available for a relatively small part of the overall sample.

Table A6: 200g Marginal Effect of Birth Weight on Fever (at 2,500g)

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.005***	-0.004***	-0.002**	-0.005***
	(0.000)	(0.001)	(0.001)	(0.001)
Imputations	No	No	Yes	Yes
Observations	480,000	223,843	1,025,285	527,226
	Twins			
Birth Weight	-0.004**	-0.004	-0.002*	-0.003
	(0.002)	(0.004)	(0.001)	(0.004)
Imputations	No	No	Yes	Yes
Observations	10,379	10,254	19,238	18,306

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the marginal effect of a 200g increase on the outcome at 2,500g estimated using a quadratic specification for birth weight. Columns 1 and 3 include controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Columns 2 and 4 implement the mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. The second panel uses the same specification, except restricting the sample to twins. The twin fixed effect models include controls for gender and birth order. The full table for columns 1 and 2 are presented in the appendix, as are tables showing first stage estimates. Standard errors are adjusted for clustering at the household level, as well as for 5 replications in the model which imputes missing data on birth weight and birth weight squared (Rubin, 1987).

Table A7: 200g Marginal Effect of Birth Weight on Diarrhoea (at 2,500g)

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.003***	-0.000	-0.001***	-0.001*
	(0.000)	(0.001)	(0.000)	(0.001)
Imputations	No	No	Yes	Yes
Observations	494,210	229,924	1,046,558	537,608
	Twins			
Birth Weight	-0.000	-0.002	-0.002*	-0.002
	(0.002)	(0.003)	(0.001)	(0.003)
Imputations	No	No	Yes	Yes
Observations	10,584	10,454	18,810	18,570

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the marginal effect of a 200g increase on the outcome at 2,500g estimated using a quadratic specification for birth weight. Columns 1 and 3 include controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Columns 2 and 4 implement the mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. The second panel uses the same specification, except restricting the sample to twins. The twin fixed effect models include controls for gender and birth order. The full table for columns 1 and 2 are presented in the appendix, as are tables showing first stage estimates. Standard errors are adjusted for clustering at the household level, as well as for 5 replications in the model which imputes missing data on birth weight and birth weight squared (Rubin, 1987).

Table A8: 200g Marginal Effect of Birth Weight on Coughing (at 2,500g)

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.023***	-0.003**	-0.006*	-0.003***
	(0.002)	(0.001)	(0.003)	(0.001)
Imputations	No	No	Yes	Yes
Observations	486,745	226,559	1,034,909	1,023,342
	Twins			
Birth Weight	-0.004*	-0.009**	-0.002	-0.010***
	(0.002)	(0.004)	(0.001)	(0.003)
Imputations	No	No	Yes	Yes
Observations	10,435	10,306	18,598	18,358

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the marginal effect of a 200g increase on the outcome at 2,500g estimated using a quadratic specification for birth weight. Columns 1 and 3 include controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Columns 2 and 4 implement the mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. The second panel uses the same specification, except restricting the sample to twins. The twin fixed effect models include controls for gender and birth order. The full table for columns 1 and 2 are presented in the appendix, as are tables showing first stage estimates. Standard errors are adjusted for clustering at the household level, as well as for 5 replications in the model which imputes missing data on birth weight and birth weight squared (Rubin, 1987).

Table A9: 200g Marginal Effect of Birth Weight on Anaemia (at 2,500g)

Variables	OLS Full Sample	Mother IV FE	OLS Full Sample	Mother IV FE
	All Children			
Birth Weight	-0.004***	-0.007**	-0.002***	-0.001
	(0.001)	(0.004)	(0.001)	(0.002)
Imputations	No	No	Yes	Yes
Observations	104,845	48,267	221,484	114,401

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model shows the marginal effect of a 200g increase on the outcome at 2,500g estimated using a quadratic specification for birth weight. Columns 1 and 3 include controls for month of birth, year of birth fixed effects, gender, birth order, order in birth history calendar, place of birth, birth interval, multiple birth, mothers age, urban/rural location, partner's education, toilet in house, water in house, marital status, survey year fixed effects, religion, maternal tetanus injection, fertility preference, ante-natal visits by the mother, country specific year of birth trends, country specific wealth index quintile, and country fixed effects. Columns 2 and 4 implement the mother fixed effects model, with controls for gender, months since birth, year of birth fixed effects, multiple birth, month of birth, place of birth, birth interval, birth history, maternal tetanus, antenatal visits, and wanted birth. The full table for columns 1 and 2 are presented in the appendix, as are tables showing first stage estimates. Standard errors are adjusted for clustering at the household level, as well as for 5 replications in the model which imputes missing data on birth weight and birth weight squared (Rubin, 1987).

Robustness Checks Among Twins

As outlined in section 2, there are a number of limitations to twin studies which potentially affect the interpretation of results from these models. For example, it is not clear as to the extent to which the observed effects have external validity for singleton births. However, for most outcomes studied in this paper, the effect of birth weight in the twin sample (without fixed effects) was broadly similar to that in singletons. In addition, there is existing evidence that twins are comparable to the wider population (Öberg et al., 2012). Another concern is that zygosity is unknown in most data sets. Therefore, it is difficult to rule out genetics effects, given that dizygotic (non-identical) twins share the same amount of genetic material as siblings (on average, 50%). However, it is still possible to determine, to a certain extent, whether these genetic endowments are affecting estimates by stratifying the sample according to sex composition. Because mixed sex pairs are dizygotic by definition, we would expect to see a difference between estimates in this group and the same sex group if genetic differences were important. Table A10 presents results for the twin sample where the model for mortality is stratified according to different attributes. I compare the linear probability model with the IV fixed effects model, and I present confidence intervals in brackets.

Following Deaton (1997), I do not weight regressions as the weighted estimator is not typically consistent, however, the first panel illustrates that adjusting for sampling weights has little effect on estimates. The second panel shows the analysis stratified according to sex composition. The fixed effect coefficient in the mixed sample is lower, however the confidence intervals are wide and overlapping in all cases, and we cannot reject that the coefficient in any particular group is different from the pooled estimate. The increased standard errors are likely a reflection of the reduction in sample size. Comparing the coefficients for males and females within mixed pairs gives similar results. The third panel allows for the effect of birth weight to vary according to birth order, as recent research on twins in the US has suggested heterogeneous effects (Choi, 2013). However, birth order is potentially an outcome of birth weight, and so it is not necessarily clear that it is appropriate to either condition on, or interact this variable with birth weight, particularly if the view is taken that birth weight is a proxy for unobserved health in utero. An additional difficulty with these data is that the indicator used is the order reported by the mother in the birth history calendar, which means it is difficult to rule out misclassification. Nevertheless, there is no evidence that the effect of birth weight in this sample differs according to who was born first. The fourth panel stratifies the sample by household wealth index in order to determine whether there is differential investment according to birth weight. For example, families with greater resources may provide greater medical care to disadvantaged children. However, there is little evidence that the effect of birth weight differs by wealth quintile. Finally, the last panel stratifies by differences in birth weight within twin pairs according to whether they are above or below the median discordance (200g).

There is some indication that the effect is larger among twins with less discordance, although, particularly for the case of the fixed effects estimates, the confidence intervals are too wide to draw concrete conclusions.

In summary, these robustness checks provide little indication that that the twin estimates are influenced by genetics or post-natal treatment, at least for the case of mortality. Table A11 presents similar results for stunting. However, these results partly reflect the fact that twin estimates are much less precise when reducing the sample size and amount of variation in birth weight when stratifying. Therefore, additional data would be helpful in further establishing whether there is heterogeneity in these effects.

Table A10: Heterogeneity in Twin Birth Weight Effects on Mortality

	OLS			FE		
	Unweighted	Weighted		Unweighted	Weighted	
All	-0.018	-0.018		-0.008	-0.008	
	(-0.020 -0.015)	(-0.021 -0.015)		(-0.016 -0.001)	(-0.018 0.001)	
N	14214	14214		13960	13960	
	Male	Female	Mixed	Male	Female	Mixed
Gender	-0.026	-0.008	-0.018	-0.015	-0.011	-0.004
	(-0.031 -0.022)	(-0.012 -0.004)	(-0.022 -0.014)	(-0.029 -0.001)	(-0.025 0.003)	(-0.016 0.008)
N	4622	4453	5139	4544	4372	5044
	First	Second				
Birth Order	-0.022	-0.014				
	(-0.025 -0.018)	(-0.017 -0.011)				
N	7070	7144				
	≤ Median	>Median		≤ Median	>Median	
Wealth Category	-0.017	-0.019		-0.01	-0.006	
	(-0.020 -0.014)	(-0.022 -0.015)		(-0.020 0.001)	(-0.017 0.005)	
N	7672	6542		7510	6428	
	≤ 200g	>200g		≤ 200g	>200g	
Birth Weight Difference	-0.021	-0.01		-0.057	-0.007	
	(-0.025 -0.018)	(-0.014 -0.007)		(-0.153 0.039)	(-0.014 0.001)	
N	7968	6246		7714	6246	

Note: The table shows the marginal effect of a 200g increase in birth weight at 2,500g on the outcome using linear probability and IV fixed effects models where size at birth is the instrument. The sample is restricted to twins with complete birth weight data. The specification is a quadratic for birth weight. In the first panel, columns 1 and 3 are unweighted, while columns 2 and 4 adjust for sampling weights. The second panel stratifies by sex composition of the twin pair. The third panel stratifies by birth order. Panel 4 stratifies by whether the household was in the median wealth category or lower, while panel 5 stratifies by whether the birth weight difference difference was 200g or less. 95% confidence intervals are shown in parentheses.

Table A11: Heterogeneity in Twin Birth Weight Effects on Stunting

	OLS			FE		
	Unweighted	Weighted		Unweighted	Weighted	
All	-0.021	-0.022		-0.02	-0.018	
	(-0.026 -0.016)	(-0.028 -0.016)		(-0.031 -0.008)	(-0.030 -0.006)	
N	7398	7398		7350	7350	
	Male	Female	Mixed	Male	Female	Mixed
Gender	-0.025	-0.017	-0.022	-0.038	-0.006	-0.022
	(-0.035 -0.016)	(-0.025 -0.008)	(-0.029 -0.014)	(-0.063 -0.014)	(-0.027 0.014)	(-0.039 -0.004)
N	2286	2377	2735	2276	2354	2720
	First	Second				
Birth Order	-0.021	-0.022				
	(-0.027 -0.015)	(-0.027 -0.016)				
N	3689	3709				
	≤ Median	>Median		≤ Median	>Median	
Wealth Category	-0.022	-0.02		-0.023	-0.018	
	(-0.029 -0.016)	(-0.026 -0.013)		(-0.038 -0.007)	(-0.036 -0.001)	
					3522	
N	3851	3547		3810	3,548	
	≤ 200g	>200g		≤ 200g	>200g	
Birth Weight Difference	-0.025	-0.018		0.018	-0.02	
	(-0.031 -0.018)	(-0.024 -0.011)		(-0.093 0.129)	(-0.032 -0.009)	
N	3928	3470		3880	3470	

Note: The table shows the marginal effect of a 200g increase in birth weight at 2,500g on the outcome using linear probability and IV fixed effects models where size at birth is the instrument. The sample is restricted to twins with complete birth weight data. The specification is a quadratic for birth weight. In the first panel, columns 1 and 3 are unweighted, while columns 2 and 4 adjust for sampling weights. The second panel stratifies by sex composition of the twin pair. The third panel stratifies by birth order. Panel 4 stratifies by whether the household was in the median wealth category or lower, while panel 5 stratifies by whether the birth weight difference difference was 200g or less. 95% confidence intervals are shown in parentheses.

First Stage and Full Regression Tables

Table A12: First Stage Estimates - Full Table

	Under 5 Mortality				Stunting				Anaemia	
	Sibling IV FE		Twin IV FE		Sibling IV FE		Twin IV FE		Sibling IV FE	
Variables	BW	BW SQ	BW	BW SQ	BW	BW SQ	BW	BW SQ	BW	BW SQ
Multiple Birth: Singleton										
First of Multiple	-0.382***	-2.140***	-0.002	0.001	-0.392***	-2.203***	-0.014*	-0.055	-0.393***	-2.306***
	(0.010)	(0.065)	(0.005)	(0.027)	(0.013)	(0.087)	(0.007)	(0.037)	(0.025)	(0.170)
Second of Multiple	-0.403***	-2.245***			-0.413***	-2.295***			-0.417***	-2.427***
	(0.010)	(0.064)			(0.013)	(0.086)			(0.025)	(0.170)
Third of Multiple	-0.485***	-2.619***			-0.438***	-2.282***			-0.254*	-1.376
	(0.042)	(0.278)			(0.062)	(0.418)			(0.106)	(0.715)
Fourth of Multiple	-0.649**	-3.567**								
	(0.203)	(1.355)								
Month of Birth: January										
February	0.007	0.045			0.004	0.026			0.033	0.217
	(0.007)	(0.048)			(0.009)	(0.061)			(0.019)	(0.127)
March	0.009	0.061			0.009	0.051			0.027	0.205
	(0.011)	(0.072)			(0.014)	(0.093)			(0.030)	(0.202)
April	0.017	0.122			0.013	0.087			0.024	0.159
	(0.015)	(0.101)			(0.019)	(0.130)			(0.043)	(0.287)
May	0.026	0.179			0.020	0.129			0.031	0.200
	(0.020)	(0.132)			(0.025)	(0.169)			(0.056)	(0.375)
June	0.039	0.275			0.031	0.201			0.051	0.321
	(0.024)	(0.163)			(0.031)	(0.208)			(0.069)	(0.466)
July	0.039	0.278			0.025	0.152			0.058	0.374
	(0.029)	(0.194)			(0.037)	(0.248)			(0.083)	(0.557)
August	0.046	0.338			0.035	0.232			0.049	0.313
	(0.034)	(0.226)			(0.043)	(0.288)			(0.096)	(0.647)
September	0.059	0.409			0.046	0.289			0.064	0.423
	(0.039)	(0.257)			(0.049)	(0.328)			(0.110)	(0.739)
October	0.065	0.458			0.045	0.277			0.061	0.373
	(0.043)	(0.289)			(0.055)	(0.369)			(0.123)	(0.830)
November	0.069	0.495			0.053	0.331			0.073	0.468
	(0.048)	(0.321)			(0.061)	(0.409)			(0.137)	(0.921)
December	0.075	0.528			0.054	0.336			0.088	0.560
	(0.053)	(0.353)			(0.067)	(0.450)			(0.151)	(1.013)
Female	-0.048***	-0.341***	-0.038***	-0.204***	-0.052***	-0.358***	-0.042***	-0.240***	-0.059***	-0.412***
	(0.002)	(0.014)	(0.008)	(0.046)	(0.003)	(0.018)	(0.011)	(0.062)	(0.005)	(0.036)
Place of Birth: Own Home										
Other Home	-0.009	-0.043			-0.019	-0.120			-0.022	-0.151
	(0.012)	(0.081)			(0.017)	(0.117)			(0.031)	(0.210)
Government Hospital	-0.010	-0.074			-0.011	-0.077			-0.018	-0.148
	(0.006)	(0.040)			(0.008)	(0.054)			(0.016)	(0.108)
Government Clinic	-0.001	-0.035			-0.005	-0.052			-0.024	-0.203
	(0.007)	(0.045)			(0.009)	(0.061)			(0.017)	(0.113)
Private Hospital or Clinic	-0.001	-0.015			-0.004	-0.030			-0.001	-0.026
	(0.007)	(0.049)			(0.010)	(0.066)			(0.019)	(0.131)
Other	0.001	0.022			0.016	0.123			0.010	0.048
	(0.017)	(0.117)			(0.022)	(0.148)			(0.047)	(0.314)
Missing	-0.047	-0.319*			-0.036	-0.276			-0.047	0.075
	(0.024)	(0.162)			(0.029)	(0.194)			(0.128)	(0.857)
Birth Interval: 1st Birth										
1-11 Months	-0.012	-0.111			-0.017	-0.103			-0.002	-0.019
	(0.010)	(0.065)			(0.014)	(0.092)			(0.031)	(0.207)
12-17 Months	0.003	-0.029			-0.002	-0.057			0.000	-0.057
	(0.005)	(0.032)			(0.006)	(0.042)			(0.013)	(0.088)
18-23 Months	0.010*	0.028			0.007	0.011			0.010	0.036
	(0.004)	(0.028)			(0.005)	(0.036)			(0.011)	(0.074)
24+ Months	0.017***	0.077***			0.015***	0.066*			0.008	0.017
	(0.003)	(0.023)			(0.004)	(0.030)			(0.009)	(0.059)
Birth History: 1st										
3	0.161	3.072			0.368	4.749				
	(0.585)	(3.911)			(0.619)	(4.144)				
4	0.155	0.790			0.148	0.796				

Table A12 – Continued on the Next Page

	Under 5 Mortality				Stunting				Anaemia	
	Sibling IV FE		Twin IV FE		Sibling IV FE		Twin IV FE		Sibling IV FE	
Variables	BW	BW SQ	BW	BW SQ	BW	BW SQ	BW	BW SQ	BW	BW SQ
	(0.610)	(4.080)			(0.589)	(3.943)				
5	0.545	3.615			0.396	2.348				
	(0.456)	(3.049)			(0.502)	(3.355)				
Birth Order: 1st										
2	-0.015*	-0.094*			-0.005	-0.047			0.027	0.176
	(0.007)	(0.048)			(0.009)	(0.061)			(0.034)	(0.229)
3	-0.032**	-0.198**			-0.009	-0.073			0.028	0.167
	(0.011)	(0.076)			(0.015)	(0.099)			(0.042)	(0.284)
4	-0.007	-0.056			0.014	0.040			0.093	0.559
	(0.020)	(0.131)			(0.026)	(0.177)			(0.068)	(0.457)
5	-0.120	-1.077*			-0.134	-0.935			0.132	0.711
	(0.064)	(0.428)			(0.086)	(0.573)			(0.180)	(1.211)
No Maternal Tetanus Shot	-0.005	-0.029			-0.003	-0.021			-0.018	-0.137
	(0.004)	(0.027)			(0.005)	(0.034)			(0.010)	(0.070)
Maternal Tetanus Missing	-0.003	-0.004			0.000	0.013			0.012	0.012
	(0.009)	(0.059)			(0.011)	(0.075)			(0.023)	(0.153)
Months Since Birth	0.008	0.054			0.005	0.032			0.005	0.034
	(0.005)	(0.032)			(0.006)	(0.041)			(0.014)	(0.092)
Wanted Birth: Before										
Later	-0.004	-0.021			-0.002	-0.014			0.007	0.046
	(0.003)	(0.022)			(0.004)	(0.028)			(0.008)	(0.057)
No More	-0.006	-0.042			-0.005	-0.043			-0.003	-0.015
	(0.005)	(0.031)			(0.006)	(0.040)			(0.013)	(0.089)
Don't Know/Missing	-0.035	-0.166			0.002	0.034			0.104	0.449
	(0.037)	(0.247)			(0.053)	(0.356)			(0.118)	(0.796)
Antenatal Visit: No										
Yes	0.012	0.049			0.022*	0.127			-0.006	-0.042
	(0.008)	(0.053)			(0.010)	(0.065)			(0.022)	(0.146)
Missing	0.003	-0.023			0.011	0.045			-0.051	-0.300
	(0.012)	(0.078)			(0.015)	(0.098)			(0.036)	(0.242)
Size at Birth: Very Small										
Very Large	1.804***	11.148***	1.462***	8.284***	1.724***	10.729***	1.441***	8.168***	1.769***	10.852***
	(0.008)	(0.051)	(0.035)	(0.202)	(0.010)	(0.070)	(0.050)	(0.286)	(0.020)	(0.136)
Large	1.383***	7.887***	1.165***	6.098***	1.316***	7.597***	1.094***	5.637***	1.319***	7.398***
	(0.006)	(0.043)	(0.023)	(0.132)	(0.009)	(0.058)	(0.033)	(0.187)	(0.018)	(0.119)
Average	0.894***	4.498***	0.774***	3.602***	0.850***	4.324***	0.729***	3.352***	0.855***	4.200***
	(0.006)	(0.041)	(0.019)	(0.111)	(0.008)	(0.055)	(0.028)	(0.163)	(0.017)	(0.115)
Small	0.384***	1.686***	0.330***	1.353***	0.358***	1.598***	0.316***	1.291***	0.350***	1.443***
	(0.007)	(0.044)	(0.018)	(0.107)	(0.009)	(0.059)	(0.028)	(0.158)	(0.018)	(0.122)
Partial F	26274	23398	992.8	885.1	13769	12402	462.7	416.9	3579	3178
Sargan	33.59		3.761		4.333		2.194		1.144	
Sargan P	0.000***		0.152		0.115		0.334		0.564	
Anderson LM	22529		804.3		12264		424.8		3496	
Anderson LM P-value	0.000***		0.000***		0.000***		0.000***		0.000***	
Observations	226482		13960		131194		7350		35950	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: First stage estimates for birth weight and birth weight squared from models using observations with complete birth weight data are shown. Where they are not part of the fixed effect, the models include controls for year of birth fixed effects, survey year fixed effects, country specific year of birth trends, country specific wealth index quintile and country fixed effects, which are not shown in the table. Standard errors in the OLS model are adjusted for clustering at the household level. The p-value from Anderson's LM test (1951), first stage partial F statistics for excluded instruments (size at birth), and Sargan's test for overidentifying restrictions is also shown.

Table A13: Effects of Birth Weight on Under 5 Mortality and Stunting - Full Table

	Under 5 Mortality			Stunting		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
Birth Weight	-0.168***	-0.275***	-0.149	-0.241***	-0.338***	-0.278
	(0.004)	(0.016)	(0.131)	(0.008)	(0.040)	(0.199)
Birth Weight Squared	0.024***	0.038***	0.021	0.024***	0.040***	0.036
	(0.001)	(0.003)	(0.024)	(0.001)	(0.006)	(0.037)
Wealth Index: Lowest						
Lower	0.010			-0.011		
	(0.007)			(0.017)		
Middle	0.009			-0.025		
	(0.007)			(0.017)		
Rich	-0.000			-0.048***		
	(0.007)			(0.017)		
Richest	-0.008			-0.100***		
	(0.006)			(0.017)		
Months Since Birth	-0.000	-0.002		0.005***	0.017***	
	(0.000)	(0.003)		(0.000)	(0.007)	
Multiple Birth: Singleton						
First of Multiple	0.060***	0.043***		0.070***	0.086***	
	(0.004)	(0.006)		(0.008)	(0.014)	
Second of Multiple	0.068***	0.146***	-0.043***	0.078***	0.047***	0.013*
	(0.004)	(0.006)	(0.005)	(0.007)	(0.014)	(0.007)
Third of Multiple	0.248***	0.289***		0.144**	0.053	
	(0.043)	(0.026)		(0.063)	(0.067)	
Fourth of Multiple	0.634***	0.538***				
	(0.210)	(0.126)				
Month of Birth: January						
February	-0.001	0.001		-0.006	-0.002	
	(0.001)	(0.004)		(0.004)	(0.010)	
March	-0.002	-0.001		-0.010***	-0.000	
	(0.001)	(0.007)		(0.004)	(0.015)	
April	-0.001	0.003		-0.005	0.009	
	(0.001)	(0.009)		(0.004)	(0.021)	
May	-0.000	0.005		-0.009**	0.031	
	(0.002)	(0.012)		(0.004)	(0.027)	
June	0.000	0.007		-0.007*	0.046	
	(0.002)	(0.015)		(0.004)	(0.033)	
July	-0.000	0.007		-0.009**	0.053	
	(0.002)	(0.018)		(0.004)	(0.040)	
August	0.001	0.007		-0.011**	0.058	
	(0.002)	(0.021)		(0.004)	(0.046)	
September	-0.004**	0.010		-0.009**	0.066	
	(0.002)	(0.024)		(0.004)	(0.053)	
October	-0.005**	0.012		-0.012**	0.085	
	(0.002)	(0.027)		(0.005)	(0.059)	
November	-0.003*	0.010		-0.014***	0.093	
	(0.002)	(0.030)		(0.005)	(0.066)	
December	-0.000	0.011		-0.018***	0.098	
	(0.002)	(0.033)		(0.005)	(0.072)	
Female	-0.009***	-0.013***	-0.024***	-0.051***	-0.047***	-0.074***
	(0.001)	(0.001)	(0.008)	(0.002)	(0.003)	(0.012)
Place of Birth: Own Home						
Other Home	-0.003	-0.007		-0.018**	-0.016	
	(0.002)	(0.008)		(0.008)	(0.019)	
Government Hospital	0.000	-0.005		-0.044***	-0.007	
	(0.001)	(0.004)		(0.003)	(0.009)	
Government Clinic	-0.003**	-0.002		-0.034***	-0.009	
	(0.001)	(0.004)		(0.004)	(0.010)	
Private Hospital or Clinic	-0.004***	-0.018***		-0.061***	-0.016	
	(0.001)	(0.005)		(0.004)	(0.011)	
Other	-0.000	-0.013		-0.038***	-0.017	
	(0.004)	(0.011)		(0.009)	(0.024)	
Missing	-0.006	0.005		-0.044***	-0.007	
	(0.005)	(0.015)		(0.010)	(0.031)	
Birth Interval: 1st						
1-11 Months	0.065***	-0.020***		0.068***	0.105***	

Table A13 – Continued on the Next Page

	Under 5 Mortality			Stunting		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
	(0.006)	(0.006)		(0.011)	(0.015)	
12-17 Months	0.030***	-0.001		0.066***	0.070***	
	(0.002)	(0.003)		(0.005)	(0.007)	
18-23 Months	0.018***	0.016***		0.054***	0.047***	
	(0.002)	(0.003)		(0.004)	(0.006)	
24+ Months	0.003*	0.003*		0.026***	0.033***	
	(0.002)	(0.002)		(0.004)	(0.005)	
Birth History: 1st						
2	-0.003*			-0.011***		
	(0.002)			(0.004)		
3	-0.002	-0.125		-0.003	-1.858***	
	(0.002)	(0.363)		(0.005)	(0.666)	
4	0.001	-0.081		0.011**	0.323	
	(0.002)	(0.379)		(0.005)	(0.634)	
5	0.002	0.007		0.036***	1.229**	
	(0.002)	(0.283)		(0.006)	(0.539)	
Birth Order: 1st						
2	0.048***	0.147***		0.024***	0.001	
	(0.002)	(0.004)		(0.004)	(0.010)	
3	0.095***	0.248***		-0.035***	-0.091***	
	(0.003)	(0.007)		(0.006)	(0.016)	
4	0.138***	0.289***		-0.072***	-0.138***	
	(0.012)	(0.012)		(0.019)	(0.028)	
5	0.196***	0.279***		-0.083	-0.183**	
	(0.059)	(0.040)		(0.086)	(0.092)	
Mother's Age: 15-19						
20-24	-0.006***			-0.017***		
	(0.001)			(0.004)		
25-29	-0.009***			-0.045***		
	(0.002)			(0.004)		
30-34	-0.009***			-0.062***		
	(0.002)			(0.004)		
35-39	-0.005***			-0.072***		
	(0.002)			(0.005)		
40-44	0.002			-0.083***		
	(0.002)			(0.006)		
45-49	0.021***			-0.096***		
	(0.004)			(0.009)		
Rural	-0.001			0.004*		
	(0.001)			(0.002)		
Mother's Education: None						
Primary	0.001			-0.030***		
	(0.001)			(0.003)		
Secondary	-0.003***			-0.064***		
	(0.001)			(0.003)		
Tertiary	-0.005***			-0.081***		
	(0.002)			(0.004)		
Don't Know/Missing	0.011			-0.099*		
	(0.028)			(0.055)		
Toilet in House: No						
Yes	-0.001			-0.023***		
	(0.001)			(0.003)		
Missing	0.003			-0.014		
	(0.003)			(0.010)		
Water in House: No						
Yes	-0.002***			0.001		
	(0.001)			(0.002)		
Missing	0.003			0.002		
	(0.004)			(0.011)		
Partner's Education: None						
Primary	-0.001			-0.009***		
	(0.001)			(0.003)		
Secondary	-0.004***			-0.033***		
	(0.001)			(0.003)		
Tertiary	-0.005***			-0.052***		
	(0.002)			(0.004)		
Don't Know/Missing	-0.001			-0.028***		

Table A13 – Continued on the Next Page

	Under 5 Mortality			Stunting		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
	(0.002)			(0.005)		
Marital Status: Never Married						
Married	-0.008***			-0.027***		
	(0.002)			(0.006)		
Living Together	-0.003			-0.022***		
	(0.002)			(0.006)		
Widowed	0.025***			-0.014		
	(0.004)			(0.010)		
Divorced	0.023***			-0.021**		
	(0.004)			(0.008)		
Not Living Together	0.012***			-0.007		
	(0.003)			(0.007)		
Religion: Christian						
Muslim	0.003*			0.002		
	(0.001)			(0.004)		
Jewish	0.011			0.004		
	(0.008)			(0.015)		
Buddhist	0.004			0.011		
	(0.004)			(0.022)		
Hindu	0.005***			-0.009		
	(0.002)			(0.008)		
Sikh	0.002			-0.098***		
	(0.007)			(0.018)		
Traditional	0.000			0.022**		
	(0.004)			(0.009)		
Other	0.001			-0.003		
	(0.003)			(0.008)		
None	-0.003			-0.005		
	(0.002)			(0.005)		
Unknown	0.003			-0.020***		
	(0.003)			(0.007)		
Maternal Tetanus Injection: Yes						
None	0.006***	0.016***		-0.005*	-0.014**	
	(0.001)	(0.002)		(0.002)	(0.005)	
Missing	0.001	0.012**		-0.014***	-0.028**	
	(0.002)	(0.005)		(0.005)	(0.012)	
Wanted Birth: Before						
Later	-0.009***	0.044***		0.004*	-0.018***	
	(0.001)	(0.002)		(0.002)	(0.004)	
No More	-0.008***	0.047***		0.010***	-0.008	
	(0.001)	(0.003)		(0.003)	(0.006)	
Don't Know/Missing	0.084***	0.107***		-0.008	-0.049	
	(0.017)	(0.023)		(0.027)	(0.057)	
Antenatal Visit: No						
Yes	-0.005***	-0.025***		-0.034***	0.008	
	(0.002)	(0.005)		(0.004)	(0.010)	
Missing	-0.005**	-0.017**		-0.040***	-0.029*	
	(0.002)	(0.007)		(0.006)	(0.016)	
Constant	8.260***	-15.319***	0.415**	14.223***	45.695***	0.922***
	(1.907)	(5.472)	(0.169)	(4.948)	(11.492)	(0.261)
Observations	527,027	263,214	14,214	338,540	160,804	7,398

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Birth weight coefficients are multipled by 1000. Models using observations with birth weight data are shown. Where they are not part of the fixed effect, the models include controls for year of birth fixed effects, survey year fixed effects, country specific year of birth trends, country specific wealth index quintile and country fixed effects, which are not shown in the table. Standard errors in the OLS model are adjusted for clustering at the household level.

Table A14: Effects of Birth Weight on Wasting and Coughing - Full Table

	Wasting			Coughing		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
Birth Weight	-0.076***	-0.084***	-0.109	-0.095***	-0.047*	-0.194
	(0.005)	(0.022)	(0.148)	(0.007)	(0.027)	(0.124)
Birth Weight Squared	0.008***	0.009**	0.009	0.014***	0.006	0.030
	(0.001)	(0.004)	(0.027)	(0.001)	(0.004)	(0.023)
Wealth Index: Lowest						
Lower	-0.016*			0.016		
	(0.010)			(0.015)		
Middle	-0.018*			-0.014		
	(0.010)			(0.015)		
Rich	-0.013			-0.002		
	(0.010)			(0.015)		
Richest	-0.031***			-0.021		
	(0.010)			(0.016)		
Months Since Birth	-0.003***	0.003		-0.006***	-0.006	
	(0.000)	(0.004)		(0.000)	(0.005)	
Multiple Birth: Singleton						
First of Multiple	0.015***	0.030***		0.048***	0.048***	
	(0.005)	(0.008)		(0.006)	(0.010)	
Second of Multiple	0.022***	0.013	0.016***	0.012**	-0.006	0.020***
	(0.005)	(0.008)	(0.005)	(0.006)	(0.010)	(0.005)
Third of Multiple	0.024	-0.003		0.015	-0.042	
	(0.043)	(0.038)		(0.053)	(0.050)	
Fourth of Multiple				0.821***	-0.128	
				(0.010)	(0.364)	
Month of Birth: January						
February	-0.002	0.005		-0.009***	-0.006	
	(0.002)	(0.006)		(0.003)	(0.007)	
March	-0.002	0.004		-0.011***	-0.016	
	(0.002)	(0.008)		(0.003)	(0.011)	
April	-0.008***	0.009		-0.017***	-0.023	
	(0.002)	(0.012)		(0.003)	(0.015)	
May	-0.004*	0.010		-0.027***	-0.024	
	(0.002)	(0.015)		(0.004)	(0.019)	
June	-0.009***	0.017		-0.033***	-0.034	
	(0.003)	(0.019)		(0.004)	(0.024)	
July	-0.011***	0.022		-0.039***	-0.043	
	(0.003)	(0.022)		(0.004)	(0.029)	
August	-0.011***	0.022		-0.040***	-0.044	
	(0.003)	(0.026)		(0.004)	(0.033)	
September	-0.014***	0.022		-0.046***	-0.060	
	(0.003)	(0.030)		(0.004)	(0.038)	
October	-0.015***	0.028		-0.050***	-0.059	
	(0.003)	(0.033)		(0.004)	(0.043)	
November	-0.017***	0.032		-0.052***	-0.057	
	(0.003)	(0.037)		(0.005)	(0.047)	
December	-0.018***	0.031		-0.060***	-0.066	
	(0.004)	(0.041)		(0.005)	(0.052)	
Female	-0.013***	-0.013***	-0.037***	-0.009***	-0.012***	-0.010
	(0.001)	(0.002)	(0.009)	(0.001)	(0.002)	(0.008)
Place of Birth: Own Home						
Other Home	0.001	0.020*		0.017***	0.013	
	(0.005)	(0.011)		(0.006)	(0.012)	
Government Hospital	-0.003	0.009*		-0.010***	-0.003	
	(0.002)	(0.005)		(0.003)	(0.006)	
Government Clinic	0.000	0.002		-0.015***	0.005	
	(0.002)	(0.006)		(0.003)	(0.007)	
Private Hospital or Clinic	-0.003	0.006		-0.012***	0.003	
	(0.002)	(0.006)		(0.003)	(0.007)	
Other	-0.003	0.000		-0.002	0.025	
	(0.005)	(0.014)		(0.008)	(0.017)	
Missing	0.001	0.013		-0.011	0.035	
	(0.005)	(0.018)		(0.010)	(0.024)	
Birth Interval: 1st						

Table A14 – Continued on the Next Page

	Wasting			Coughing		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
1-11 Months	0.002	-0.013		-0.002	-0.006	
	(0.006)	(0.008)		(0.009)	(0.010)	
12-17 Months	-0.004*	-0.016***		-0.017***	-0.021***	
	(0.003)	(0.004)		(0.004)	(0.005)	
18-23 Months	-0.000	-0.012***		-0.019***	-0.017***	
	(0.002)	(0.003)		(0.003)	(0.004)	
24+ Months	-0.005**	-0.006**		-0.009***	-0.008**	
	(0.002)	(0.003)		(0.003)	(0.003)	
Birth History: 1st						
2	0.007***			-0.005		
	(0.002)			(0.003)		
3	0.010***	-0.360		-0.004	0.385	
	(0.003)	(0.374)		(0.004)	(0.538)	
4	0.013***	0.116		-0.008*	0.596	
	(0.003)	(0.356)		(0.004)	(0.545)	
5	0.012***	0.320		-0.007	0.131	
	(0.003)	(0.302)		(0.005)	(0.425)	
Birth Order: 1st						
2	-0.004**	-0.036***		-0.059***	-0.067***	
	(0.002)	(0.006)		(0.003)	(0.007)	
3	-0.001	-0.053***		-0.103***	-0.122***	
	(0.003)	(0.009)		(0.005)	(0.011)	
4	-0.001	-0.069***		-0.125***	-0.161***	
	(0.008)	(0.016)		(0.014)	(0.020)	
5	0.001	-0.134**		-0.157**	-0.142*	
	(0.035)	(0.053)		(0.064)	(0.077)	
Mother's Age: 15-19						
20-24	-0.005*			-0.012***		
	(0.002)			(0.003)		
25-29	-0.006**			-0.018***		
	(0.003)			(0.004)		
30-34	-0.006**			-0.024***		
	(0.003)			(0.004)		
35-39	-0.004			-0.035***		
	(0.003)			(0.004)		
40-44	-0.005			-0.042***		
	(0.003)			(0.005)		
45-49	-0.007			-0.052***		
	(0.005)			(0.007)		
Rural	-0.004**			-0.004*		
	(0.001)			(0.002)		
Mother's Education: None						
Primary	-0.007***			0.016***		
	(0.002)			(0.003)		
Secondary	-0.012***			0.013***		
	(0.002)			(0.003)		
Tertiary	-0.016***			0.001		
	(0.003)			(0.004)		
Don't Know/Missing	-0.056			-0.005		
	(0.035)			(0.053)		
Toilet in House: No						
Yes	-0.005***			-0.003		
	(0.002)			(0.003)		
Missing	0.019***			-0.025***		
	(0.005)			(0.008)		
Water in House: No						
Yes	0.000			0.001		
	(0.001)			(0.002)		
Missing	-0.020***			0.018**		
	(0.006)			(0.009)		
Partner's Education: None						
Primary	-0.008***			0.016***		
	(0.002)			(0.003)		
Secondary	-0.009***			0.016***		
	(0.002)			(0.003)		
Tertiary	-0.012***			0.004		
	(0.003)			(0.004)		

Table A14 – Continued on the Next Page

	Wasting			Coughing		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
Don't Know/Missing	-0.012***			0.017***		
	(0.003)			(0.005)		
Marital Status: Never Married						
Married	-0.009***			0.032***		
	(0.003)			(0.006)		
Living Together	-0.007**			0.048***		
	(0.003)			(0.006)		
Widowed	-0.002			0.020**		
	(0.006)			(0.009)		
Divorced	-0.005			0.035***		
	(0.005)			(0.008)		
Not Living Together	-0.004			0.051***		
	(0.004)			(0.007)		
Religion: Christian						
Muslim	0.011***			-0.015***		
	(0.002)			(0.003)		
Jewish	-0.003			0.040**		
	(0.006)			(0.016)		
Buddhist	-0.023			-0.028**		
	(0.015)			(0.013)		
Hindu	0.021***			-0.040***		
	(0.006)			(0.006)		
Sikh	-0.074***			-0.030		
	(0.010)			(0.022)		
Traditional	-0.001			0.020**		
	(0.006)			(0.008)		
Other	0.000			0.018***		
	(0.004)			(0.007)		
None	0.003			0.005		
	(0.003)			(0.006)		
Unknown	0.002			-0.060***		
	(0.004)			(0.007)		
Maternal Tetanus Injection: Yes						
None	0.000	0.006*		-0.005**	-0.009**	
	(0.001)	(0.003)		(0.002)	(0.004)	
Missing	-0.000	0.009		0.005	-0.013	
	(0.003)	(0.007)		(0.004)	(0.009)	
Wanted Birth: Before						
Later	-0.003***	-0.003		0.048***	0.000	
	(0.001)	(0.003)		(0.002)	(0.003)	
No More	-0.003**	0.002		0.041***	0.010**	
	(0.001)	(0.004)		(0.002)	(0.005)	
Don't Know/Missing	0.013	-0.026		0.015	0.025	
	(0.015)	(0.033)		(0.024)	(0.040)	
Antenatal Visit: No						
Yes	-0.007***	-0.005		0.009**	0.009	
	(0.002)	(0.006)		(0.004)	(0.008)	
Missing	-0.012***	-0.010		0.004	0.004	
	(0.003)	(0.009)		(0.006)	(0.011)	
Constant	0.630	-6.527	0.325*	-22.197***	-2.312	0.579***
	(2.876)	(6.562)	(0.194)	(4.792)	(8.643)	(0.164)
Observations	333,815	158,639	7,262	486,745	226,559	10,367

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Birth weight coefficients are multipled by 1000. Models using observations with birth weight data are shown. Where they are not part of the fixed effect, the models include controls for year of birth fixed effects, survey year fixed effects, country specific year of birth trends, country specific wealth index quintile and country fixed effects, which are not shown in the table. Standard errors in the OLS model are adjusted for clustering at the household level.

Table A15: Effects of Birth Weight on Fever and Diarrhoea- Full Table

	Fever			Diarrhoea		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
Birth Weight	-0.099***	-0.051*	-0.000	-0.064***	0.000	-0.000
	(0.007)	(0.028)	(0.000)	(0.005)	(0.025)	(0.000)
Birth Weight Squared	0.015***	0.007	0.000	0.010***	-0.000	0.000
	(0.001)	(0.004)	(0.000)	(0.001)	(0.004)	(0.000)
Wealth Index: Lowest						
Lower	0.016			-0.006		
	(0.017)			(0.011)		
Middle	-0.007			-0.020*		
	(0.016)			(0.011)		
Rich	-0.013			-0.020*		
	(0.017)			(0.011)		
Richest	-0.096***			-0.043***		
	(0.017)			(0.011)		
Months Since Birth	-0.004***	-0.016***		-0.004***	-0.000	
	(0.000)	(0.005)		(0.000)	(0.004)	
Multiple Birth: Singleton						
First of Multiple	0.045***	0.056***		0.025***	0.047***	
	(0.006)	(0.010)		(0.005)	(0.009)	
Second of Multiple	-0.003	-0.014	0.017***	-0.005	-0.013	0.006
	(0.006)	(0.010)	(0.005)	(0.005)	(0.009)	(0.004)
Third of Multiple	-0.044	-0.092*		-0.034	-0.053	
	(0.048)	(0.051)		(0.036)	(0.045)	
Fourth of Multiple	0.797***	-0.170		-0.257***	-0.440	
	(0.009)	(0.372)		(0.008)	(0.332)	
Month of Birth: January						
February	-0.012***	-0.016**		-0.007***	0.008	
	(0.003)	(0.007)		(0.003)	(0.006)	
March	-0.007**	-0.033***		-0.008***	0.001	
	(0.003)	(0.011)		(0.003)	(0.010)	
April	-0.015***	-0.051***		-0.011***	0.000	
	(0.003)	(0.015)		(0.003)	(0.014)	
May	-0.021***	-0.069***		-0.015***	-0.003	
	(0.003)	(0.020)		(0.003)	(0.018)	
June	-0.025***	-0.084***		-0.017***	-0.002	
	(0.004)	(0.024)		(0.003)	(0.022)	
July	-0.032***	-0.109***		-0.022***	0.000	
	(0.004)	(0.029)		(0.003)	(0.026)	
August	-0.032***	-0.121***		-0.023***	-0.003	
	(0.004)	(0.034)		(0.003)	(0.030)	
September	-0.031***	-0.144***		-0.023***	0.007	
	(0.004)	(0.039)		(0.003)	(0.035)	
October	-0.035***	-0.147***		-0.023***	0.009	
	(0.004)	(0.043)		(0.003)	(0.039)	
November	-0.037***	-0.164***		-0.022***	0.014	
	(0.004)	(0.048)		(0.003)	(0.043)	
December	-0.037***	-0.178***		-0.027***	0.013	
	(0.005)	(0.053)		(0.004)	(0.047)	
Female	-0.010***	-0.015***	-0.013	-0.011***	-0.013***	-0.012*
	(0.001)	(0.002)	(0.008)	(0.001)	(0.002)	(0.007)
Place of Birth: Own Home						
Other Home	0.012**	0.016		-0.009**	-0.014	
	(0.005)	(0.012)		(0.004)	(0.011)	
Government Hospital	-0.011***	0.006		-0.015***	-0.001	
	(0.003)	(0.006)		(0.002)	(0.005)	
Government Clinic	-0.008***	0.007		-0.011***	0.002	
	(0.003)	(0.007)		(0.002)	(0.006)	
Private Hospital or Clinic	-0.017***	0.011		-0.021***	-0.007	
	(0.003)	(0.007)		(0.002)	(0.006)	
Other	-0.004	0.019		-0.009	-0.004	
	(0.008)	(0.017)		(0.007)	(0.015)	
Missing	-0.018*	0.027		-0.032***	-0.025	
	(0.010)	(0.025)		(0.007)	(0.022)	
Birth Interval: 1st						

Table A15 – Continued on the Next Page

	Fever			Diarrhoea		
Variables	OLS Full Sample	Sibling IV FE	IV FE Twins	OLS Full Sample	Sibling IV FE	IV FE Twins
1-11 Months	-0.014*	-0.020*		-0.000	-0.008	
	(0.008)	(0.010)		(0.007)	(0.009)	
12-17 Months	-0.026***	-0.024***		-0.000	-0.010**	
	(0.004)	(0.005)		(0.003)	(0.004)	
18-23 Months	-0.023***	-0.015***		-0.001	-0.005	
	(0.003)	(0.004)		(0.003)	(0.004)	
24+ Months	-0.014***	-0.007**		-0.001	-0.001	
	(0.003)	(0.003)		(0.002)	(0.003)	
Birth History: 1st						
2	0.004			-0.004*		
	(0.003)			(0.002)		
3	0.013***	0.994*		-0.003	-0.019	
	(0.004)	(0.549)		(0.003)	(0.490)	
4	0.015***	0.623		-0.001	0.592	
	(0.004)	(0.557)		(0.003)	(0.497)	
5	0.024***	-0.464		0.001	0.514	
	(0.004)	(0.434)		(0.004)	(0.387)	
Birth Order: 1st						
2	-0.069***	-0.074***		-0.038***	-0.062***	
	(0.003)	(0.007)		(0.002)	(0.006)	
3	-0.111***	-0.129***		-0.053***	-0.092***	
	(0.005)	(0.011)		(0.003)	(0.010)	
4	-0.143***	-0.185***		-0.045***	-0.118***	
	(0.013)	(0.021)		(0.009)	(0.018)	
5	-0.172***	-0.200***		-0.001	-0.136**	
	(0.050)	(0.076)		(0.053)	(0.068)	
Mother's Age: 15-19						
20-24	0.000			-0.020***		
	(0.003)			(0.003)		
25-29	-0.006*			-0.034***		
	(0.003)			(0.003)		
30-34	-0.010**			-0.044***		
	(0.004)			(0.003)		
35-39	-0.016***			-0.055***		
	(0.004)			(0.004)		
40-44	-0.026***			-0.056***		
	(0.005)			(0.004)		
45-49	-0.035***			-0.068***		
	(0.007)			(0.006)		
Rural	0.005***			-0.007***		
	(0.002)			(0.002)		
Mother's Education: None						
Primary	0.009***			-0.000		
	(0.002)			(0.002)		
Secondary	-0.002			-0.014***		
	(0.003)			(0.002)		
Tertiary	-0.016***			-0.028***		
	(0.004)			(0.003)		
Don't Know/Missing	-0.007			-0.046**		
	(0.051)			(0.023)		
Toilet in House: No						
Yes	-0.002			-0.005***		
	(0.002)			(0.002)		
Missing	-0.016*			-0.017***		
	(0.009)			(0.006)		
Water in House: No						
Yes	-0.008***			0.001		
	(0.002)			(0.001)		
Missing	0.007			0.016**		
	(0.009)			(0.007)		
Partner's Education: None						
Primary	0.011***			0.004*		
	(0.003)			(0.002)		
Secondary	0.008***			-0.001		
	(0.003)			(0.002)		
Tertiary	0.001			-0.008***		
	(0.004)			(0.003)		

Table A15 – Continued on the Next Page

Variables	Fever: OLS Full Sample	Fever: Sibling IV FE	Fever: IV FE Twins	Diarrhoea: OLS Full Sample	Diarrhoea: Sibling IV FE	Diarrhoea: IV FE Twins
Don't Know/Missing	0.009*			0.010***		
	(0.005)			(0.004)		
Marital Status: Never Married						
Married	0.025***			0.027***		
	(0.006)			(0.005)		
Living Together	0.036***			0.039***		
	(0.006)			(0.005)		
Widowed	0.037***			0.035***		
	(0.009)			(0.007)		
Divorced	0.033***			0.046***		
	(0.008)			(0.006)		
Not Living Together	0.047***			0.042***		
	(0.006)			(0.005)		
Religion: Christian						
Muslim	-0.001			0.004		
	(0.003)			(0.002)		
Jewish	0.025			0.000		
	(0.015)			(0.012)		
Buddhist	-0.026*			-0.005		
	(0.014)			(0.010)		
Hindu	-0.049***			-0.017***		
	(0.006)			(0.005)		
Sikh	-0.031			-0.005		
	(0.020)			(0.016)		
Traditional	0.022**			0.007		
	(0.009)			(0.006)		
Other	-0.002			0.011**		
	(0.007)			(0.005)		
None	0.002			-0.002		
	(0.005)			(0.004)		
Unknown	-0.039***			-0.003		
	(0.006)			(0.005)		
Maternal Tetanus Injection: Yes						
None	-0.012***	-0.012***		-0.004**	-0.010***	
	(0.002)	(0.004)		(0.002)	(0.003)	
Missing	-0.015***	-0.029***		-0.004	-0.002	
	(0.004)	(0.009)		(0.003)	(0.008)	
Wanted Birth: Before						
Later	0.037***	0.001		0.022***	0.001	
	(0.002)	(0.003)		(0.001)	(0.003)	
No More	0.034***	0.007		0.027***	0.009**	
	(0.002)	(0.005)		(0.002)	(0.004)	
Don't Know/Missing	-0.013	-0.015		-0.000	-0.019	
	(0.024)	(0.040)		(0.018)	(0.035)	
Antenatal Visit: No						
Yes	0.005	0.001		-0.013***	-0.007	
	(0.004)	(0.008)		(0.003)	(0.007)	
Missing	0.006	-0.003		-0.017***	-0.024**	
	(0.006)	(0.012)		(0.004)	(0.010)	
Constant	11.981**	-2.225	0.533***	23.662***	-3.587	0.358**
	(4.674)	(8.796)	(0.179)	(3.509)	(7.843)	(0.142)
Observations	480,000	223,843	10,313	494,210	229,924	10,516

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Birth weight coefficients are multipled by 1000. Models using observations with birth weight data are shown. Where they are not part of the fixed effect, the models include controls for year of birth fixed effects, survey year fixed effects, country specific year of birth trends, country specific wealth index quintile and country fixed effects, which are not shown in the table. Standard errors in the OLS model are adjusted for clustering at the household level.

Table A16: Effects of Birth Weight on Anaemia - Full Table

	Anaemia	
Variables	OLS Full Sample	Sibling IV FE
Birth Weight	-0.076***	-0.120
	(0.014)	(0.078)
Birth Weight Squared	0.011***	0.017
	(0.002)	(0.012)
Wealth Index: Lowest		
Lower	-0.006	
	(0.024)	
Middle	-0.006	
	(0.024)	
Rich	-0.045*	
	(0.025)	
Richest	-0.147***	
	(0.028)	
Months Since Birth	-0.009***	0.019
	(0.001)	(0.016)
Multiple Birth: Singleton		
First of Multiple	0.023*	0.094***
	(0.013)	(0.030)
Second of Multiple	0.025**	0.025
	(0.012)	(0.030)
Third of Multiple	0.230**	0.142
	(0.091)	(0.124)
Fourth of Multiple		
Month of Birth: January		
February	0.003	0.026
	(0.007)	(0.022)
March	-0.002	0.057
	(0.007)	(0.035)
April	-0.001	0.066
	(0.008)	(0.050)
May	-0.003	0.104
	(0.008)	(0.065)
June	-0.002	0.115
	(0.008)	(0.081)
July	-0.014	0.126
	(0.009)	(0.097)
August	-0.020**	0.175
	(0.010)	(0.112)
September	-0.031***	0.186
	(0.010)	(0.128)
October	-0.023**	0.226
	(0.011)	(0.144)
November	-0.024**	0.221
	(0.012)	(0.160)
December	-0.023*	0.242
	(0.013)	(0.176)
Female	-0.028***	-0.025***
	(0.003)	(0.006)
Place of Birth: Own Home		
Other Home	-0.010	-0.059
	(0.012)	(0.036)
Government Hospital	-0.015***	-0.015
	(0.006)	(0.019)
Government Clinic	-0.007	-0.015
	(0.006)	(0.020)
Private Hospital or Clinic	-0.019***	-0.016
	(0.007)	(0.023)
Other	-0.030	-0.035
	(0.019)	(0.055)
Missing	0.009	0.243
	(0.039)	(0.149)
Birth Interval: 1st		

Table A16 – Continued on the Next Page

	Anaemia	
Variables	OLS Full Sample	Sibling IV FE
1-11 Months	0.042**	0.010
	(0.020)	(0.036)
12-17 Months	0.037***	0.023
	(0.009)	(0.015)
18-23 Months	0.037***	0.025*
	(0.008)	(0.013)
24+ Months	0.020***	0.030***
	(0.007)	(0.010)
Birth History: 1st		
2	0.000	
	(0.007)	
3	0.004	
	(0.009)	
4	0.012	
	(0.010)	
5	0.024**	
	(0.010)	
Birth Order: 1st		
2	0.015	-0.129***
	(0.014)	(0.040)
3	0.008	-0.172***
	(0.017)	(0.049)
4	0.039	-0.097
	(0.046)	(0.079)
5	0.157	-0.262
	(0.148)	(0.210)
Mother's Age: 15-19		
20-24	-0.025***	
	(0.007)	
25-29	-0.043***	
	(0.008)	
30-34	-0.055***	
	(0.009)	
35-39	-0.060***	
	(0.010)	
40-44	-0.075***	
	(0.011)	
45-49	-0.104***	
	(0.016)	
Rural	0.002	
	(0.005)	
Mother's Education: None		
Primary	-0.022***	
	(0.005)	
Secondary	-0.031***	
	(0.006)	
Tertiary	-0.059***	
	(0.009)	
Don't Know/Missing		
Toilet in House: No		
Yes	-0.001	
	(0.006)	
Missing	0.012	
	(0.019)	
Water in House: No		
Yes	0.001	
	(0.004)	
Missing	-0.043**	
	(0.020)	
Partner's Education: None		
Primary	-0.016***	
	(0.005)	
Secondary	-0.024***	
	(0.006)	
Tertiary	-0.045***	
	(0.008)	

Table A16 – Continued on the Next Page

	Anaemia	
Variables	OLS Full Sample	Sibling IV FE
Don't Know/Missing	-0.023**	
	(0.010)	
Marital Status: Never Married		
Married	-0.031**	
	(0.013)	
Living Together	-0.012	
	(0.013)	
Widowed	-0.031*	
	(0.018)	
Divorced	0.003	
	(0.018)	
Not Living Together	0.002	
	(0.015)	
Religion: Christian		
Muslim	0.044***	
	(0.007)	
Jewish	0.059**	
	(0.027)	
Buddhist	0.034	
	(0.024)	
Hindu	0.073***	
	(0.011)	
Sikh	0.126***	
	(0.027)	
Traditional	0.025*	
	(0.013)	
Other	0.006	
	(0.014)	
None	0.004	
	(0.013)	
Unknown	-0.065***	
	(0.021)	
Maternal Tetanus Injection: Yes		
None	0.001	-0.020
	(0.005)	(0.012)
Missing	-0.008	-0.018
	(0.009)	(0.027)
Wanted Birth: Before		
Later	-0.003	-0.002
	(0.004)	(0.010)
No More	0.014**	-0.000
	(0.005)	(0.015)
Don't Know/Missing	-0.050	-0.039
	(0.073)	(0.138)
Antenatal Visit: No		
Yes	-0.011	-0.022
	(0.010)	(0.025)
Missing	-0.016	0.043
	(0.015)	(0.042)
Constant	134.044***	18.161
	(12.892)	(20.211)
Observations	104,845	48,267

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Birth weight coefficients are multipled by 1000. Models using observations with birth weight data are shown. Where they are not part of the fixed effect, the models include controls for year of birth fixed effects, survey year fixed effects, country specific year of birth trends, country specific wealth index quintile and country fixed effects, which are not shown in the table. Standard errors in the OLS model are adjusted for clustering at the household level.