The Effects of Negative Economic Shocks at Birth on Child Health in Sub-Saharan Africa[†]

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Abstract

In this study, I estimate the effects that mothers' experience of negative economic shocks during pregnancy or shortly after childbirth has on children's subjective and objective health measures in Malawi. Using data from the Malawi Longitudinal Study on Families and Health (MLSFH), I find that children whose mothers were hit by such economic shocks were about 7 percentage points less likely to be reported to be in excellent health and 8 percentage points less likely to be reported to be in much better health compared to children of the same age and sex in the same village by their mothers. They were also about 300 grams lighter and 0.3 centimeters shorter than others, although the latter estimate is relatively imprecise and not statistically significant at conventional significance levels. These results are robust to various econometric specifications and sample selection rules. In addition, I propose a simple model to account for the fact that economic shocks are self-reported and show that my results are likely to continue to hold under reasonable assumptions about the rates of false positive and false negative reports of these economic shocks.

Keywords: *Economic shocks, Child health, Malawi, Sub-Saharan Africa*
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1 Introduction

There has been a strong and long-standing interest in social sciences in the production function of infant health, as it has been shown to be critical for the development of health and human

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capital more broadly throughout the entire life-cycle (Aizer and Currie 2014, Almond *et al.* 2017, Barker 1990, Bhalotra and Rawlings 2013, Case and Paxson 2008*a*,*b*, 2009, 2010, Case *et al.* 2005, Currie 2011, Currie and Almond 2011, Currie and Moretti 2007, Currie and Vogl 2013, Currie *et al.* 2018, Grossman 1972, Heckman 2006, Nandi *et al.* 2017, Rosenzweig and Schultz 1982)[1]. As a result, understanding the determinants of infant health, particularly at the very beginning of life, is crucial for any theories and policies addressing the development of human capital throughout the life course.

Studies have shown that mothers' socio-demographic characteristics, such as their education, income, employment, as well as their health care use and health behaviors such as cigarette smoking and alcohol consumption, are all important inputs that enter into the infant health production function (Corman *et al.* 2017). A growing and more recent literature in economics has focused on the importance of in utero stress experienced by mothers, which has been shown to have important negative effects on infant health (Corman *et al.* 2017, Currie *et al.* 2018).

Among all of these different forms of exposure to stress, malnutrition in utero and during very early life due to irregular food intakes and lack of nutrients is particularly detrimental to children's health and development (Almond and Mazumder 2011, Almond *et al.* 2015, Barker 1995, Lavy *et al.* 2016, Nandi *et al.* 2017, Neelsen and Stratmann 2011, Schultz-Nielsen *et al.* 2016). Indeed, it is now well-documented that nutritional deprivation in formative years can have permanent effects on body-size in adulthood (Barker 1998, Coly *et al.* 2006, De Onis and Branca 2016, Glewwe *et al.* 2001, Martorell 1999), risks of chronic diseases (Huxley *et al.* 2000, Whincup *et al.* 2008), cognitive development (Hoddinott *et al.* 2013) and socio-economic outcomes (Currie and Vogl 2013, Hoddinott *et al.* 2013, Martorell *et al.* 2009, WHO 1995).

Malnutrition can be triggered by many factors, among which lack of disposable income is perhaps the most important (Sen 1982). Economic shocks occurring in utero or early in life can be particularly damaging for infants whose mothers live in vulnerable environments with very limited resources. Indeed, mothers can find themselves trapped in critical situations in which the only way they can cope with the consequences of economic shocks is to adjust their diet or the ones of their newborns. This is especially true in regions of extreme poverty with non-existent or weak public safety nets. In Sub-Sahara African countries, for example, any shocks that affect the economic situation of pregnant women or mothers can have devastating effects on the health of their children if they are forced to reduce their infant's food quality and food intakes or cease

[1] See Prinz *et al.* (2018) for a review.

breastfeeding earlier than recommended (Joanna Briggs et al. 2012)[2].

While evidence of the importance of economic shocks during pregnancy or at birth on infant health is well known in developed countries (Banerjee *et al.* 2010, Carlson 2015, Rohde *et al.* 2017, Van den Berg *et al.* 2006), corresponding evidence in developing countries is more scarce (Currie and Vogl 2013).

Using a month-long blackout in Zanzibar[3] as a negative transitory income shock, Burlando (2014) finds that children exposed in utero to the electric power outage were about 150 grams lighter at birth compared to those who were not exposed to the shock. Maccini and Yang (2009) find that women who were exposed to positive income shock (measured in terms of unexpected positive rainfall shock) during the year of their birth in Indonesia were in better self-rated health and were taller than others, by about half a centimeter, when they were adults. Bozzoli and Quintana-Domeque (2014) document a decrease in birth weight of about 30 grams for children born from mothers who were subject to macroeconomic fluctuation following the Argentinian economic crisis early in 2000s. Similarly, Paxson and Schady (2005) find that the economic crisis in Peru resulted in an increase of about 2.5 percentage point in infant mortality rate for children born during the crisis of the late 1980s.

While evidence suggests that negative (positive) economic shocks during pregnancy or shortly after birth negatively (positively) affect infant health, the nature of the shocks in some of these studies in developing countries raises concerns about the generalizability of their findings (Maccini and Yang 2009). While lack of and excess rainfall are likely to be the most common type of shocks in rural regions in developing countries (Adhvaryu *et al.* 2018, Dinkelman 2013), other shocks like severe economic crises and blackouts, although interesting in their own, might happen relatively infrequently and be very specific to the local situation. The effects of these particular shocks could therefore raise concern about generalizability because of the very specific subpopulations these effects are estimated for, calling into question the relevance of the findings.

In this study, I estimate the effects that negative economic shocks experienced by mothers while pregnant or shortly after giving birth have on subjective and objective health measures for children in Malawi. Malawi is a Sub-Saharan country located in East central Africa and is one of the world's poorest countries. With about 70% of its population living below the international

[2]Economic shocks could for instance increase the opportunity cost of breastfeeding through their effects on labor demand (Thai *et al.* 2012).

[3]The blackout was due to an accidental break in the undersea cable that connects Zanzibar with the electricity generators on mainland Tanzania.

poverty line in 2016 ($1.90 per person per day) (International Monetary Fund 2017), Malawi is a country where poverty is deep and wide. Poverty is particularly high in rural areas where about 85% of the population lives (Orr *et al.* 2001), most of them in small farms. As a rural country with a mostly agricultural economy, poverty is closely linked to malnutrition, food insecurity and famine (Devereux 1999, Orr *et al.* 2001)[4]. Children are often the collateral victims of these economic shocks in Malawi, one reason being that dietary adjustments are the principal coping strategies in cases of economic difficulties (Devereux 1999)[5]. Although child malnutrition is on the decline, the prevalence of stunting among children under five in Malawi is still at 37% (International Monetary Fund 2017), one of the highest rate in the world (De Onis and Branca 2016).

I investigate the consequences of negative economic shocks at birth on two sets of health outcomes. The first set of outcomes represents measures of subjective health of children as reported by their mother. The second set of outcome variables represents anthropometric measures of these children (weight and height) which are directly associated with malnutrition. Anthropometric outcomes such as weight and height are widely used as health indicators for assessing the adequacy of nutrition and growth in infancy (Currie and Vogl 2013, Fishman *et al.* 2004, Thomas and Strauss 1992, Thomas *et al.* 1990, WHO 1995) and have been shown to be related to infant survival (Chen *et al.* 1980, Fishman *et al.* 2004), skill development and productivity later in life (Cravioto and Arrieta 1986).

Using data from the Malawian Longitudinal Study of Family and Health (MLSFH), I find that children whose mothers experience economic shocks during the year of their birth are about 7 percentage points less likely to be in excellent health and 8 percentage points less likely to be in much better health as compared to children of the same age and sex in the same village. I show that in addition to having statistically significant effects on these subjective health measures, negative economic shocks also have substantial effects on objective health measures. I find that children who are born during the year when their mothers experience economic shocks were about 300 grams lighter and 0.3 centimeters shorter than others. These effects are large and are robust to the inclusion of various economic specifications and sample selections. I show that mothers who are able to draw financial and in-kind help from their social network are

[4]In 2013, 84% of individuals living in rural households experienced food insecurity for at least one month per year (International Monetary Fund 2017).

[5]In a survey of 104 household conducted in Zamba district in the South of Malawi, Devereux (1999) found that 74% of rural households reported eating only one meal per day in the hungry season –usually from December to April–.

able to buffer the negative effects of these shocks on children health while mothers with limited access to such help are not able to do so. Overall, these results suggest that mothers have difficulty maintaining their own and their children's nutritional intake when hit by economic shocks, hindering the normal development of their infants.

2 Data source and sample selection

The analysis is based on the Malawian Longitudinal Study of Family and Health (MLSFH), a panel survey collected of rural households in Malawi almost every two years since 1998. Originally established to study the influence of social network on fertility behaviors and HIV risk perceptions, the scope of the MLSFH has since then greatly expanded and provides now very detailed information on demographic and socio-economic characteristics, family structure, social network and social capital, intergenerational relations as well as health conditions of about 4,000 people living in three rural regions of Malawi (Kohler *et al.* 2014): Rumphi in the north, Mchinji in the centre and Balaka in the south. While not designed to be representative of rural Malawi, the sample characteristics of the MLSFH has been found to match those of the Demography and Health Survey (DHS), which is representative of the rural population in Malawi (Anglewicz *et al.* 2009).

My study uses the fifth wave of data collection of 2008. Among the now ten waves of data that have been collected to date, wave 5 is the only one that includes anthropometric measures of children, which I use to derive objective measures of child health status.

The results for subjective health measures are based on a sample of 1784 children who were born between 2003 and 2008, which I will refer to as the "subjective health sample". Only a subset of them participated in the anthropometric measurements module. The sample from which I derive my results on objective health measures is therefore smaller, consisting of 789 children, which I will call the "anthropometric sample".

2.1 Outcome variables

2.1.1 Subjective measures of health

From the family roster of the MLSFH survey wave 5, I derive six binary outcome variables from three questions asking mothers to evaluate the health of their children.

The first question asks mothers whether their child has been ill in the past 12 months and if yes, for how long. Possible answers to the question were "no", "yes, for less than a month", "yes, for 1 to 3 months", "yes, for 3 to 6 months", "yes, for 6 months or longer" and "don't know". I derived two binary variables from this question, one that takes the value 1 if the child has been ill over the past 12 months and 0 otherwise, and another that takes the value 1 if the child has been ill for at least 1 month and 0 otherwise. Panel A of Table 1 shows that 59% of the children in my sample were ill at some point during the year preceding the interview and 6% of them were ill for at least 1 month.

The next two binary variables are derived from the second question, which asks mothers to rate the health of their child in general. Based on a Likert scale measure ranging from "excellent" to "very poor", I derive a binary variable that takes the value 1 if they considered the health of their child as being very good or better and 0 otherwise, and another indicator that takes the value 1 if the health of the child was considered as being excellent and 0 otherwise. Panel A of Table 1 shows that a large share of the 1784 children in my sample were reported to be at least in very good health (75%) and in excellent health (38%), respectively.

My last two outcome variables are derived from a third question in which the mothers are asked to compare the health of their child to other children of the same age and sex in the village. The first variable derived from this question takes the value 1 if the mothers considered their child to be in better health as compared to other children of the same age and sex in the village and 0 otherwise, and the second takes the value 1 if they considered the health of their child to be much better than other children of the same age and sex and 0 otherwise. Table 1 shows that the subjective assessment of the mothers regarding the health of their children is very high, with 65% of the mothers saying that their child is in better health and 34% of them saying that they are in much better health than others.

Columns 5 and 7 of Table 1 show that on average, children who have experienced a shock at birth have lower subjective health measures as compared to those who have not experienced these shocks. While purely associative, these differences are already suggestive regarding the possible negative effects of economic shocks at birth on children's health.

Because all the binary outcome variables derived above are subjective, I complement my analysis by also using anthropometric measures of the children in my sample to estimate the effects of negative economic shocks at birth on objective child health measures.

Table 1: Descriptive statistics of the subjective health sample (*Panel A*) and anthropometric sample (*Panel B*)

	All sample				Shock at birth		No shock at birth	
	Mean	Std	Min	Max	Mean	Std	Mean	Std
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Subjective health sample								
Ill over the past 12 months	.59	.49	0	1	.63	.48	.57	.49
Ill for more than 1 month	.06	.24	0	1	.08	.27	.06	.23
Very good health	.75	.43	0	1	.73	.44	.76	.43
Excellent health	.38	.49	0	1	.34	.47	.40	.49
Better health than others	.65	.48	0	1	.62	.48	.66	.47
Much better health than others	.34	.47	0	1	.30	.46	.35	.48
Economic shock at birth	.27	.44	0	1	1		0	
Female	.50	.50	0	1	.51	.50	.50	.50
Age	2.60	1.67	0	5	1.87	1.36	2.87	1.70
Age of the mother at birth	27.09	7.31	11	50	27.14	7.21	27.07	7.35
Obs.	1784							
B. Anthropometric sample								
Weight (in kg)	11.41	3.46	2	23	9.81	3.02	12.08	3.42
Height (in cm)	81.58	13.59	43	145	74.96	11.75	84.39	13.34
Economic shock at birth	.30	.46	0	1	1	0	0	0
Female	.53	.50	0	1	.53	.50	.53	.50
Age	2.44	1.47	0	5	1.63	1.17	2.78	1.45
Age of the mother at birth	27.10	7.51	15	50	27.55	7.72	26.91	7.41
Obs.	789							

Note: Sample derived from the MLSFH data wave 5. Descriptive statistics of children of the respondents born between 2003 and 2008.

2.1.2 Objective measures of health - Anthropometrics

From the anthropometric module of wave 5, I derive several objective outcome variables to determine the health status of the children in the study. First, I determine the effects of negative economic shocks on weight in kilograms (kg) and height (length) in centimeters (cm)[6].

On the one hand, child weight has been shown to be correlated with infant prospects for survival (Rosenzweig and Schultz 1982, Susser *et al.* 1972) as well as with the prevalence of several infectious diseases such as pneumonia, diarrhoea and malaria (Fishman *et al.* 2004). Being underweight as a child is also linked to impaired cognitive development, intellectual deficits and poor school performance and is associated with increased risk of chronic diseases later in life, functional impairment and reduced work capacity (Fishman *et al.* 2004). Height, on the other hand, is a good proxy for exposure to disease and deprivation typically experienced within the first three years of life (Beach *et al.* 2018, Currie and Vogl 2013, Parman 2015, Thomas *et al.* 1990, WHO 1995). In general, abnormal anthropometric measurements can have

[6]Note that these measures were collected by trained interviewers following strict protocols and were not self-reported. I use height to refer to both length, measured in recumbent position, and stature, measured in standing position. In all regressions in which height is the outcome variable, I control for whether the height of the child was measured in a recumbent or standing position.

significant short- and long-term health consequences such as an increase in incidence and severity of morbidity, mortality, poor psychological and intellectual development (WHO 1995) and are strong indicators of malnutrition.

Panel B of Table 1 shows that on average, children in my sample weight about 11kg (first row) and are about 82cm tall (second row). Again, comparing these measures between children who experienced economic shocks at birth and those who did not (Column 5 and 7), one can see a large difference between the two groups, which may potentially suggest important effects of these economic shocks on child health. It is worth mentioning here that the substantial difference in these two groups are mainly due to age difference. Indeed, as shown in the fifth row of Panel B of Table 1, children who experienced a shock at birth are on average 1.6 years old whereas those who did not are 2.8 years old. These differences in age in both samples are due to the structure of the questionnaire. That is, respondents are more likely to report shocks that were experienced in recent years and, given that I am matching these shocks to children born between 2003 and 2008, children with economic shocks at birth will be by construction younger than others. In the analysis that follows, I will include age in year dummies in all my models to ensure that I am estimating the effects of economic shocks on child health for a given age.

2.2 Economic shocks

The 2008 questionnaire includes an economic shock module in which respondents are asked whether their households have faced any negative economic shocks over the last five years and if so, during which years the shocks occurred, and their impact on the community in which they live[7].

More specifically, the question is: "Over the past five years, was your household severely affected negatively by any of the following unexpected events or crises?", where the proposed unexpected shocks were listed as follows: "Death or serious illness of an adult member or someone who provides support for yourself or your family", "Poor crop yields, loss of crops due to disease or pests, or loss of livestock due to theft or disease, or loss of coupon", "Loss of source of income- such as loss of employment, business failure, someone who had been assisting the household stopped their support", "Big change in price of grain (either increase or decrease)", "Breakup of household, such as a divorce", "Damage to house due to fire, flood, or other unexpected event" or

[7]Note that questions related to economic shocks were asked after the questions about children's health. This should minimize the potential concern of ex-post rationalization that could influence mothers' evaluation of the health of their children.

Table 2: Descriptive statistics of the negative economics shocks in both samples

	Subjective health sample		Anthropometric sample	
	Count	%	Count	%
	(1)	(2)	(3)	(4)
Obs.	1784		789	
Economic shock at birth	473	.27	235	.30
Idiosycratic shocks	189	.40	99	.42
Common shocks	329	.70	163	.69
Death or serious illness	125	.26	65	.28
Poor crop yields or loss due to disease/pests	197	.42	105	.45
Loss of source of income	74	.16	38	.16
Big change in price of grain	146	.31	79	.34
Breakup of household	32	.07	17	.07
Damage to house due to fire, flood etc	22	.05	15	.06
Other shocks	2	.00	2	.01

Note: Sample derived from the MLSFH data wave 5. Idiosyncratic shocks are shocks affecting the respondent's household only. Common shocks are shocks affecting other households as well. The first two columns represent the count and % of shocks in my subjective health sample and the last two represent the same statistics for my anthropometric sample. It is worth noting that children can experience several shocks during their year of birth.

any shock respondents could specify. Moreover, for the three most important shocks that they have experienced over the past five years, respondents are asked when (calendar year) these shocks occurred and whether these shocks affected "only the household", "other households as well", "most households in the community" or "all households in the community".

In my analysis, each unit of observation is a child. Given that I know the year of birth of each child and the year when economic shocks occur, I can match these self-reported shocks to the year of birth of each child. More specifically, my self-reported economic shock variable takes the value 1 if the mother experienced a negative economic shock the year when her child was born and 0 otherwise. The limitation of this analysis is that these economic shocks are self-reported. As will be detailed in Section 6, errors in self-reported shocks are likely to bias my estimated effects. I discuss this issue at length below, where I propose two different ways to assess the robustness of my findings to economic shock misreports.

Table 2 describes the shocks that children in my two samples have been exposed to in utero or during the year of birth. Out of the 1784 children in my subjective health sample, 473 were born in a year when their mother experienced a negative economic shock (about 27%), which shows how prevalent and widespread economic instability is in these low income and rural regions of Malawi. About 70% of these shocks were "common shocks" in the sense that they affected not only the household of the respondent but also other households, and 40% of these shocks were "idiosyncratic" in the sense that they impacted the household of the respondent only. When breaking down these shocks by categories, "Poor crop yields or loss due to disease/pests" and "Big change in price of grain" were the two most common shocks, representing about 42% and 31% of the shocks, respectively. When looking at my anthropometric sample (Columns 3 and 4), one can see that the rate of occurrence of these shocks and the percentage of these shocks are very similar to the subjective health sample.

2.3 Control variables

Some characteristics of the mothers, if not controlled for, could result in omitted variable bias in my attempt to estimate the effects of economic shocks on child health. Indeed, any variables that are not controlled for and are correlated with both my assumed exogenous and self-reported economic shock and my dependent variables would jeopardize the causal interpretation of my estimates. For this reason, I control for a wide set of mother characteristics. For instance, wealth of the respondents is likely to be correlated with the probability of experiencing (and reporting) a negative economic shock and with child health. For this reason I include as independent variable a continuous wealth index based on a set of 20 dwelling characteristics and ownership of household durable assets constructed using first principal component analysis (Chin 2010, Filmer and Pritchett 1998, Hyder *et al.* 2015, Vyas and Kumaranayake 2006)[8]. In addition to wealth, my analysis also controls for various factors that could influence both self-reported negative economic shock and child health such as the region where the mother lives, the ethnicity and the level of education of the mother, which proxies for unobserved family background characteristics (Behrman and Wolfe 1987, Thomas *et al.* 1990), the value of the crops that the household has produced during the last growing season, the total household expenditure on various items (clothes, fabric, shoes, medical expenses, fertilizer, seeds, hired labor, agricultural tools and equipment and expenses related to funerals, all at the household level) and the amount the household has spent for its children over the three months prior to the interview[9]. The sex and age of the child, the age of the mother at the time of the child birth and the child's birth order are also controlled for.

Table 1 shows that about half of the children in my subjective and objective samples are

[8] Wealth measures based on household asset ownership are usually used to control for stable household wealth characteristics (Behrman and Knowles 1999, Thomas and Strauss 1992). Because wealth can potentially be directly related to the (previous) experience of economic shocks, I use as robustness check past wealth measures instead of the current one (in 2008) to control for initial wealth levels that could mitigate the damaging effects of negative shocks. I show that my results are robust to various specifications of wealth measure variables.

[9] Household expenditure is usually considered as a better measure of long-run resources availability than total income, especially in rural communities where income is variable (Thomas and Strauss 1992, Thomas *et al.* 1990).

girls and the average age of the children at the interview is about 2.5 years old. As shown in this table, the average age of the mothers at child birth in the two samples is very similar and equal to 27.1 years old.

3 Method

Multivariable linear regressions are conducted to estimate the effects of negative economic shocks experienced by mothers during the year they gave birth on the various subjective and objective measures of child health[10]. More specifically, I estimate the following model:

$$H_i = \alpha_0 + \alpha_1 S_i^* + X_i' \alpha_2 + \nu_i \tag{1}$$

where H_i is the subjective or objective health measures of child i in 2008, S_i^* is a dummy variable that takes the value 1 if the mother of i has experienced a negative economic shock during the year of i's birth and X_i' is a set of control variables. Because of the size of my sample, I sequentially add more controls in my specification and investigate the stability of my estimates. In my benchmark specification, X_i' includes a set of child age dummies, the age of the mother at child birth as well as the sex of the child (set 1). Set 2 includes set 1 as well as mothers' socio-economic characteristics that are relatively stable over time and are less likely to be affected by economic shocks. More specifically, set 2 adds the marital status of the mother, her level of education (dummies for none, primary and secondary level of education), the component analysis-based continuous wealth score as well as the birth order of the child[11]. Set 3, in addition to the controls in set 2, includes variables that can possibly be affected by economic shocks and mediate the relationship between these shocks and child health. Set 3 includes the total value of the household crop production over the last growing season (in deciles), the total household expenditure and the total household expenditure on children in the three months prior to the interview, both also in deciles. In addition to these variables, all my regressions include ethnicity and region dummies to control for any systematic differences in these three regions. The samples I use to derive my results on subjective (objective) measures of health consist of

[10] I also provide estimates derived from Logit and Probit models when the outcome variables under consideration are binary.

[11] Note that I use the most up-to-date information available at the year of birth to define these variables. In other words, information collected in wave 5 (2008), wave 4 (2006) and wave 3 (2004) was used to define these variables for children born in 2007-2008, 2005-2006 and 2003-2004, respectively. If information was missing in some waves, I use the most recent information available.

1784 (789) children from 1153 (589) different mothers[12]. In all the results below, standard errors are clustered at the mother level.

4 Results

4.1 Subjective outcomes

Table 3 summarizes the effects of negative economic shocks on the subjective health measures of the children in my sample. Columns 1, 2 and 3 represent these effects when the set of control variables 1, 2 and 3 are used, respectively. The first two rows show that experiencing an economic shock in utero or early in life increases the probability of being ill later in life, although these effects are small and relatively imprecisely estimated. Rows 3 and 4 show that economic shocks reduce the probability of being in very good and excellent health by about 4 percentage points and 7 percentage points, respectively. The effects on the probability of being on excellent health is statistically significant at the 95%-level whereas the effects on very good health fail to be significant at conventional levels. The negative effects of economic shocks can also be seen when mothers are asked to evaluate the health of their child as compared to children of the same age and sex in the village. Children born in the year when an economic shock occurs are less likely to be in better and much better health than their counterparts. The latter effect is large and highly significant: children who experienced a shock in utero or in the year of birth are 8% points less likely to be in much better health than others. When looking at these effects separately for boys and girls (Columns 4 and 5), one can see that the effects are rather similar, although they are more pronounced and precisely estimated for boys. This finding is consistent with the literature about the fragility of boys in the beginning of life (Kraemer 2000). Corresponding results using Logit and Probit models instead of linear probability models are presented in Tables A.1 and A.2 of the Online Appendix A. The marginal effects presented in these two tables are very close to those obtained in Table 3.

The above provides first preliminary evidence of the negative effects of economic shocks experienced in utero or at birth on child health later in life. That being said, these results are based on subjective measures and might therefore be biased because of reporting heterogeneity. Exploiting my relatively large subjective health sample and the fact that I observe several

[12] Note that I dropped 4 outliers in my objective measure sample: these four children were reported to be 103, 99, 48 and 33 centimeters tall at age 0, 0, 3 and 4, respectively. Including them in my analysis however does not significantly affect the results of my estimates.

Table 3: Effects of negative economic shocks at birth on subjective health outcomes - linear probability model

Probability of being:	Set 1 (1)	Set 2 (2)	Set 3 (3)	Boy (4)	Girl (5)
ill in the last 12 months	.010	.021	.019	.005	.034
	(.028)	(.032)	(.032)	(.045)	(.045)
ill for more than 1 month	.016	.016	.015	.019	.012
	(.014)	(.016)	(.016)	(.024)	(.023)
in very good health	-.016	-.037	-.036	-.034	-.048
	(.026)	(.030)	(.030)	(.040)	(.043)
in excellent health	-.033	-.069**	-.073**	-.097**	-.058
	(.027)	(.030)	(.030)	(.044)	(.044)
in better health	-.029	-.059*	-.054*	-.066	-.045
	(.028)	(.032)	(.032)	(.044)	(.047)
in much better health	-.033	-.081***	-.080***	-.114***	-.060
	(.027)	(.029)	(.029)	(.041)	(.043)
Obs.	1784	1384	1382	700	682

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Effects of negative economic shocks during the year of birth on subjective health outcomes. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively. Columns 4 and 5 include the same controls as Column 3. Estimates of the other coefficients are available upon request.

children per mother in some cases, I can add mother fixed effects in the analysis to control for unobserved mother characteristics that are constant across births such as potential time-invariant effects, including reporting heterogeneity. Table B.1 in the Online Appendix B shows that my results are robust to the inclusion of mother fixed effects to capture unobserved attributes of mothers and/or households that could be correlated with both child health and economic shocks[13].

The results so far present some important evidence of the negative effects that economic shocks experienced at birth have on subjective child health outcomes. I now focus my analysis on objective measures of child health.

[13] The same appendix presents the fixed-effect logit analog to the previous linear fixed-effect analysis. Results in Table B.2 are again very consistent to the ones obtained in the linear fixed-effect analysis.

4.2 Objective health measures

4.2.1 Main results

Table 4 shows the effects of economic shocks experienced in utero or during the year of birth on children's weight. Again, Columns 1, 2 and 3 include the set of controls 1, 2 and 3, respectively. One can see in the first row that these shocks have a strong and statistically significant effect on the weight of the children in my sample. Experiencing such a shock decreases children's weight by about 330 grams. These effects are particularly robust to the inclusion of additional control variables in Columns 2 and 3. Looking at the other control variables included in my models, one can see that, not surprisingly, weight increases with age and girls are lighter than boys. The age of the mother at birth increases the weight of the children by about 30 grams, which is consistent with previous studies (Bakker *et al.* 2011, MacLeod and Kiely 1988). Again, when estimating the models by sex (Columns 4 and 5), one can see that the economic shocks have more damaging effects on boys than on girls (549 grams lighter for boys compared to 123 grams for girls). This suggests the absence of gender bias towards male (Maccini and Yang 2009) and is consistent with the fact that boys are more prone to being underweight, stunted and suffering from wasting early in life (De Onis *et al.* 1997).

Table 5 presents the same set of estimates but looks at height instead of weight. One can see that economic shocks experienced in utero or during the year of birth reduced the height of the children by about 0.3 centimeters (depending on the set of controls) but these effects fail to be significant. As was the case in the subjective measures of health and weight, boys appear to be more affected by these shocks, although in the case of height, the effect is not precisely estimated.

These estimates control for wealth level in 2008. Because wealth level could potentially be affected by economic shocks, I assess the robustness of my results in defining wealth level as the level prior to the birth of the child. Table C.1 in the Online Appendix C shows that using household wealth level in 2004, which is prior to most of the births in my sample, does not affect my estimates substantively.

Moreover, the analysis above focuses on the average effects of economic shocks in utero or at birth on children's weight and height. It could however be interesting to know where in the weight and height distributions these effects are taking place. To do so, I compute weight-for-age and height-for-age z-scores and estimate the effects of economic shocks on the probability of being d

Table 4: Effects of negative economic shocks at birth on weight

	(1)	(2)	(3)	Male (4)	Female (5)
Economic shock at birth	-0.336**	-0.325*	-0.353**	-0.549**	-0.123
	(0.164)	(0.173)	(0.175)	(0.253)	(0.207)
Age of child = 1	3.103***	3.107***	3.140***	3.421***	2.655***
	(0.242)	(0.255)	(0.257)	(0.390)	(0.295)
Age of child = 2	4.510***	4.589***	4.608***	4.792***	4.154***
	(0.243)	(0.259)	(0.263)	(0.402)	(0.296)
Age of child = 3	6.736***	6.819***	6.868***	6.735***	6.679***
	(0.262)	(0.291)	(0.292)	(0.428)	(0.322)
Age of child = 4	8.521***	8.631***	8.619***	8.818***	8.159***
	(0.232)	(0.268)	(0.272)	(0.387)	(0.290)
Age of child = 5	9.426***	9.582***	9.664***	8.763***	9.833***
	(0.355)	(0.463)	(0.459)	(0.562)	(0.465)
Age of mother at birth	0.030***	0.031*	0.030*	0.044***	0.023*
	(0.010)	(0.018)	(0.018)	(0.014)	(0.012)
Female	-0.501***	-0.590***	-0.593***		
	(0.144)	(0.157)	(0.156)		
Mother married at birth		0.046	0.047		
		(0.350)	(0.352)		
Primary level of education		-0.133	-0.167		
		(0.219)	(0.216)		
Secondary level of education		-0.274	-0.258		
		(0.443)	(0.454)		
Wealth score		-0.064	-0.061		
		(0.054)	(0.058)		
Birth order		-0.013	-0.047		
		(0.064)	(0.065)		
Total value of crop production (10 quantiles)			-0.011		
			(0.037)		
Total expenditure of HH (10 quantiles)			-0.127*		
			(0.066)		
Total expenditure on children (10 quantiles)			0.179**		
			(0.072)		
Constant	5.330***	5.905***	5.779***	4.639***	5.154***
	(0.509)	(0.705)	(0.725)	(0.828)	(0.591)
Observations	789	639	639	372	417

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Effects of negative economic shocks during the year of birth on weight. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively. Columns 4 and 5 include the same controls as Column 1. The reference category is a boy of age 0 from the central region of Malawi who did not experience any economic shock at birth.

standard deviations away from the sex- and age-specific median, with $d = \{-2, -1, 1, 2\}$, as well as the probability of being lower than the median ($d = 0$). Results in the Online Appendix D show that these effects appear to be quite homogeneous along the weight and height distributions and that potentially all children are affected by these shocks.

In sum, children whose mothers experience a negative economic shock during pregnancy or the year of childbirth are significantly less likely to be in excellent health and to be in much better health than similar children in the village who are of the same age and sex. When it comes to objective measures of health, these children were about 300 grams lighter than others and about 0.3 centimeters shorter. I now further investigate the robustness of these findings and

Table 5: Effects of negative economic shocks at birth on height

	(1)	(2)	(3)	Male (4)	Female (5)
Economic shock at birth	-0.705	-0.355	-0.356	-1.257	-0.152
	(0.539)	(0.583)	(0.588)	(0.799)	(0.740)
Age of child = 1	13.395***	13.155***	13.171***	14.010***	12.567***
	(0.794)	(0.822)	(0.825)	(1.155)	(1.180)
Age of child = 2	20.862***	21.278***	21.292***	20.505***	20.956***
	(0.995)	(1.022)	(1.025)	(1.386)	(1.446)
Age of child = 3	28.177***	28.234***	28.262***	27.191***	28.811***
	(1.166)	(1.251)	(1.253)	(1.593)	(1.747)
Age of child = 4	34.980***	35.648***	35.664***	35.130***	34.672***
	(1.144)	(1.244)	(1.254)	(1.507)	(1.760)
Age of child = 5	41.801***	41.711***	41.763***	38.857***	43.891***
	(1.441)	(1.499)	(1.509)	(1.735)	(2.215)
Age of mother at birth	0.050*	0.069	0.069	0.086**	0.025
	(0.030)	(0.058)	(0.057)	(0.042)	(0.040)
Female	-1.503***	-1.666***	-1.663***		
	(0.414)	(0.440)	(0.441)		
Mother married at birth		0.304	0.332		
		(1.201)	(1.222)		
Primary level of education		-0.389	-0.426		
		(0.757)	(0.765)		
Secondary level of education		0.059	0.032		
		(1.265)	(1.270)		
Wealth score		0.202	0.200		
		(0.158)	(0.175)		
Birth order		-0.107	-0.129		
		(0.202)	(0.208)		
Total value of crop production (10 quantiles)			-0.028		
			(0.113)		
Total expenditure of HH (10 quantiles)			-0.015		
			(0.182)		
Total expenditure on children (10 quantiles)			0.074		
			(0.188)		
Constant	56.358***	57.602***	57.455***	57.569***	54.092***
	(1.541)	(2.401)	(2.521)	(2.358)	(2.090)
Observations	789	639	639	372	417

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Effects of negative economic shocks during the year of birth on height. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively. Columns 4 and 5 include the same controls as Column 1. The reference category is a boy of age 0 from the central region of Malawi who did not experience any economic shock at birth.

contextualize them.

4.2.2 Social transfers as informal safety net

There is a growing literature that demonstrates that the negative effects of in utero or early exposure to stress and adversity can be mitigated by parental compensating or reinforcing investments (Adhvaryu *et al.* 2018, Adhvaryu and Nyshadham 2016, Almond and Mazumder 2013, Bharadwaj *et al.* 2018, Sievertsen and Wüst 2017). In Malawi, potential mitigation effects would be most likely to come from informal safety nets (Devereux 1999, Orr *et al.* 2001)[14]. Because

[14]It has been found that subsistence oriented agrarian societies have complex web of support networks that help its more vulnerable members to protect themselves against risks and shocks (Scott 1977). Devereux (1999) shows that transfer can contribute to as much as 14 percentage of total income in household in rural Malawi. He

of the relatively poor public service and lack of financial resources, individuals in economic and financial difficulties in Sub-Saharan countries like Malawi often rely on informal safety nets, drawing on support from extended family, friends and other people in the community (Devereux 1999, Ellis *et al.* 2003, Orr *et al.* 2001).In my context, mothers who experience a negative economic shock during pregnancy or shortly after giving birth could seek out for help among persons in her village or community. To investigate whether informal transfers can attenuate the effects of negative economic shocks on child health, I categorize mothers whose informal financial and in-kind transfers, measured in the number of persons they have received transfers from at the time of the interview, are under the median value in one group and the rest in an other. I then regress child health on my economic shock variable using these two different samples and compare the coefficients.

Table 6 show that mothers who received financial and in-kind transfers from their social network were able to buffer the negative effects of economic shocks on both the weight and height of their children. On the other hand, children who experienced an economic shock at birth and whose mothers received transfers from no or relatively few people where about 600 grams lighter and 1.8 centimeters shorter than others. These effects are very large and show how important informal safety nets can be in settings with relatively poor public service and weak social welfare system[15].

4.2.3 Additional robustness checks

It has been shown that the exact period in early life during which a negative economic shock occurs matter for child development, with the year of birth being the period that is particularly critical for child development (Maccini and Yang 2009). To test this hypothesis, I follow Maccini and Yang (2009) and assess whether experiencing a negative economic shock one year before or one year after the year of birth leads to the same effects as experiencing a similar shock during the year of birth. By including these three dummy variables for the occurrence of shocks in the same regression, I can rule out the possibility that the negative effects estimated thus far are due to serially correlated shocks that happened prior or after the year of birth.

Table F.1 in the Online Appendix F shows the results of my estimations. Results for weight

suggests that the value of transfers can be much higher if one includes in kind transfers such as food, fertilizers, clothes and unremunerated labour and childcare.

[15]Using the same proxies for informal safety net but measured in 2006 instead of 2008 to partially deal with potential endogeneity issues shows similar patterns. More specifically, having a network from which mothers have received help buffers the negative effects of economic shocks (see Table E.1 in the Online Appendix E).

Table 6: Effects of negative economic shocks on objective measures of health, splitting the sample by whether the mothers have received more or less transfers than the median value

Financial and in-kind transfers received	Weight		Height	
	Below	Above	Below	Above
	(1)	(2)	(3)	(4)
Set of controls 1	-0.569**	-0.167	-1.628**	-0.040
	(0.254)	(0.219)	(0.796)	(0.740)
Set of controls 2	-0.585**	-0.197	-1.851**	0.642
	(0.286)	(0.221)	(0.894)	(0.789)
Set of controls 3	-0.600**	-0.216	-1.978**	0.692
	(0.294)	(0.219)	(0.888)	(0.782)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. I split my sample by whether respondents have received informal transfers, be they financial or in-kind, from fewer (Columns 1 and 3) or more (Columns 2 and 4) persons than the median value in my sample.

show that, while experiencing a negative economic shocks irrespective of when it occurs does have a negative effect on weight, it is only for those shocks that occur during the year of birth that have large and statistically significant effects on weight. Results for height show again that only economic shocks experienced during the year of birth affect height negatively. Again, these effects are not precisely estimated and it is hard to see any conclusive evidence in these results in the case of height.

Additionally, one concern in my analysis is that negative economic shocks during the year of birth could have an effect on child mortality. If that is the case, then my results above could potentially underestimate the true effects of these shocks on child health, as there would be a selection into life, leaving only "healthy" and "strong" children in my sample. Fortunately, the MLSFH allows me to test this hypothesis by tracing back the death of all the children of the female respondents that were born between 2003 and 2008. I can therefore investigate whether negative economic shocks at birth increased the probability of children dying by 2008. For this analysis, my sample consists of 1939 children that were born between 2003 and 2008, out of which 131 died by the time of the interview in 2008 (mortality rate of about 6.8%). Results in Table G.1 of the Online Appendix G show that negative economic shocks at birth do not have any effects on mortality, irrespective of whether shocks have affected only the household of the respondent (idiosyncratic shocks) or other households as well (common shocks)[16]. My results therefore seem not to be biased due to mortality selection[17].

[16] These effects are estimated separately for each type of shocks.

[17] The under-5 mortality rate (U5MR) prevailing in Malawi in 2008 is equal to 9.8% (The World Bank 2008).

The results above represent the effects of negative economics shocks experienced in utero or during the year of birth on child health, assuming that these self-reported economic shocks are exogenous and correctly reported. I now scrutinize these two assumptions a little further.

5 The effects of community level shocks

Assuming that the economic shocks are correctly reported, estimating their causal effects on child health relies on the assumption that these shocks are exogenous, that is, uncorrelated with the error term of the statistical model. This assumption may not hold if unobserved (and uncontrolled for) characteristics of the mothers have an effect on the probability of experiencing negative economic shocks and on child health. Among the economic shocks considered in the economic shock module of the MLSFH, "death or serious illness of an household member", "loss of source of income", "breakup of household" and "damage to house due to fire, flood etc" are perhaps the ones that are most susceptible to being endogenous because they may be affected by the mother's or household's unobserved behaviors and characteristics. On the other hand, in addition to being the most common sources of economic shocks, "Poor crop yields, or loss due to disease/pests"[18] and "big change in price of grain"[19] are plausibly more exogenous. These shocks, sometimes called covariate shocks in the literature (Pradhan and Mukherjee 2018), have the potential to affect not only the household of the respondents but the community as a whole. To strengthen the causal interpretation of my results, I therefore use to occurrence of these two exogenous shocks to create a new negative economic shock dummy, which I call "covariate shock", that takes the value 1 if one or both of these shocks were experienced by the mothers at childbirth and 0 otherwise.

Moreover, one of the strengths of the economic shock module in the MLSFH is that it also

It is therefore possible that infant deaths are underreported in my sample and that the estimated effects of economic shocks on mortality reported in Table G.1 are biased towards 0.

[18] In the questionnaire, the shock is described as "Poor crop yields, loss of crops due to disease or pests, or loss of livestock due to theft or disease, or loss of coupon". Droughts, pests and diseases are the most damaging factors affecting crop production in Malawi (Giertz *et al.* 2015). While some of the underlying reasons for this negative shock could potentially be due to individual behaviors, such as agricultural practices and mitigation activities, it is however likely that the occurrences of this shock is independent to individuals characteristics in my context. Lack of rainfall and presence of pests and diseases, themselves exacerbated by adverse weather events, are likely to be beyond individual's control. In addition, the use of pesticides and storage tools among smallholder farmers in Malawi is low because of their prices and unavailability in local markets (Maonga and Maharjan 2004). Farmers therefore have to rely on traditional methods that, although reliable, are very limited as compared to more modern techniques.

[19] The vast majority of the people living in rural Malawi owns small amount of farmland (Maonga and Maharjan 2004) and are therefore unlikely to have any influence on market prices. Volatility in the price of maize, the most important staple crop in Malawi, results from environmental factors, unpredictable domestic market interventions and export policies (Giertz *et al.* 2015).

asks respondents to report whether the shocks have also affected other households in the community. Specifically, respondents are asked whether the shocks they report have affected their "own household only", "other households as well", "most households in the community" or "all households in the community". I can thus also exploit this information and restrict my two most plausibly exogenous shocks to only those that have affected both the respondent's households and other households in the community. This reinforces the credibility of the exogeneity assumption of the shocks I am using in my analysis.

Table 7 presents the results of regressing weight (first panel) and height (second panel) on the occurrence of covariate shocks at birth by differentiating the degree of the effects that these shocks have on the local community. The estimates in this table are derived using set of controls 1 in the econometric specification[20]. Columns 1 shows the effects of the covariate shock dummy that combines both poor crop yields and big changes in price of grain and Column 2 presents the results when these two shocks are considered separately but in the same regression. The results in these two columns include shocks that have affected only the household of the respondents. One can see in Column 1 that when I restrict my analysis to covariate shocks, the effect on height is larger (about 1 centimeter decrease) and the effect on weight is roughly similar. The two coefficients estimated fail to be significant at conventional levels. When considering these two shocks separately but in the same regression in Column 2, one can see that big changes in price of grain have the most detrimental effects on weight and height, with a reduction of about 400 grams on weight and 1 centimeter on height. The effect on weight is significant at 90% confidence. Columns 3 and 4 show the results of similar estimations but when shocks that have affected only the household of the respondents are excluded in the analysis. One can see that considering only shocks that have affected both the respondents' household and other households results in a strong effect on height, with a decrease of about 1.4 centimeters. This effect is statistically significant at 95%. The effect on weight is in the ballpark of what I obtain in my benchmark analysis, although it fails to be precisely estimated. When these shocks are considered separately (Column 4), I again find that big changes in price of grain matter the most, with a significant effect of about 520 grams on weight. Finally, I restrict my analysis to shocks that have affected at least most households in the community in Columns 5 and 6. Again, the idea is that these shocks are unlikely to be driven by mothers or households behaviors

[20]Corresponding analyses using the two other sets of control variables are included in the Online Appendix H Tables H.1 and H.2.

Table 7: Effects of covariate shocks on objective health outcomes for various levels of negative economic shocks

	(1)	(2)	(3)	(4)	(5)	(6)
1. Weight						
Covariate shocks	-.257		-.216		-.305	
	(.191)		(.221)		(.257)	
Poor crop yields,loss of crops due to disease or pests		.107		.218		-.169
		(.225)		(.271)		(.355)
Big change in price of grain		-.445*		-.520*		-.246
		(.245)		(.275)		(.299)
2. Height						
Covariate shocks	-.959		-1.421**		-1.357*	
	(.605)		(.672)		(.772)	
Poor crop yields, loss of crops due to disease or pests		-.377		-.797		-1.363
		(.673)		(.774)		(1.126)
Big change in price of grain		-1.018		-1.086		-0.713
		(0.808)		(0.901)		(0.916)
Including shocks affecting only HH	y	y				
Excluding shocks affecting only HH			y	y		
Including only shocks affecting most or all HH in community only					y	y
Obs.	789	789	789	789	789	789

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. These regressions include set of controls 1. Covariate shock is a dummy variable that combines shocks due to poor crop yields/disease/pest and those due to big changes in price of grain. Columns 1 and 2 include shocks affecting all households, including those that have affected only the household of the respondents. Columns 3 and 4 exclude shocks that have affected only the household of the respondents. Columns 5 and 6 take into account only shocks that have affected most or all households in the community.

and can therefore be considered as exogenous. The results I derive from these estimations in Columns 5 and 6 are close to the ones in Columns 3 and 4. More specifically, I again find that these covariate shocks, when considered together, have a large and somewhat precisely estimated effect on height of about 1.4 centimeters. The effect on weight is in line with my benchmark results although it fails once again to be statistically significant.

In sum, when I restrict my analysis to shocks that are more plausibly exogenous and that have affected other households in the community as well, I obtain large and statistically significant negative effects of these shocks on height of about 1.4 centimeters. The effect on weight is a bit smaller and less precisely estimated but still roughly similar to the effects estimated in my benchmark analysis. These results suggest that when the community as a whole is hit by a shock, mothers cannot rely on informal safety nets to cope with economic difficulties.

6 Self-reported shock bias

One of the concerns in my analysis is that economic shocks are self-reported. In other contexts, it has been found that misreporting may be systematically related to observed and unobserved characteristics of individuals (Meyer and Mittag 2018, Meyer *et al.* 2018). It is therefore possible that my shock variable may suffer from reporting errors due to false positive and false negative reported shocks. One issue for instance is that respondents are asked to recall economic shocks that have occurred as far as five years prior to the interview. Although unexpected events or crises are not easily forgotten, those who recall having experienced negative economic shocks over the last previous five years might be very different from those who do not[21]. Other reasons often given to explain misreporting are social desirability and essential survey condition or survey design such as the survey mode and method (Meyer *et al.* 2018). Some respondents might therefore report having experienced a negative economic shock in a given year even if it did not occur –case of false positive– while others might not report a shock even if it did occur –case of false negative. For these reasons, errors in self-reported shocks might bias my estimates.

As noted in Section 3, the structural equation I am interested in estimating is the following:

$$H_i = \alpha_1 S_i^* + X_i' \alpha_2 + \nu_i \tag{2}$$

where S_i^* represents the true shock dummy variable experienced by the respondents and $\nu_i \perp X_i, S_i^*$. A concern might be that the shock I observe in my data, S_i, does not correspond to the vector of real shocks S_i^*. This difference may stem from both observed and unobserved characteristics of the individuals. In this section, I assess whether my estimates are likely to be robust to such misreporting. I show that, unlike in the case of classical measurement error in which attenuation bias can be expected, endogenous misreporting may lead to attenuation or expansion bias, and potentially generate estimates that have the opposite sign of the true effect, a result that is discussed in greater detail by Kreider (2010), Kreider *et al.* (2012) and Nguimkeu *et al.* (2017).

Assuming that the occurrence of real economic shocks is exogenous, I can suppose that S_i^* follows a Bernoulli distribution with parameter p, $S^* \sim \text{Bern}(p)$.

$$S_i^* = \begin{cases} 1 & \text{if} \quad p \\ 0 & \text{if} \quad 1 - p \end{cases} \tag{3}$$

I assume that the researcher observes the reported shock S_i with $S_i = d_{i,S^*} + S_i^*$ where I define

[21] Evidence is rather inconclusive in that regard as it has been found that longer recall period does not necessarily lead to more errors (Meyer *et al.* 2018). Bound *et al.* (2001) suggest that it is the complexity of a given experience over time rather than the passage of time that is related to misreporting, with salient and frequent events more easily remembered than irregular events.

d_{i,S^*} as:

$$d_{i,S^*} = \mathbb{1}(y_i^* \geqslant n \cap S_i^* = 0) - \mathbb{1}(y_i^* \leqslant m \cap S_i^* = 1) \tag{4}$$

with $\mathbb{1}(.)$ is the indicator function. Essentially, d_{i,S^*} is a function that introduces misreporting in my model. The continuous latent variable y^* represents the ability/willingness of the respondents to correctly report the economic shocks they experienced in a given year. More specifically, false positive cases arise ($S_i^* = 0$ and $d_{i,S^*} = 1$ such that $S_i = 1$) when y^* is larger or equal to a certain cutoff n, with $n > 0$, which represents the threshold that determines the proportion of false positive reports in my sample. Similarly, a false negative report, which is characterized by $S_i^* = 1$ and $d_{i,S^*} = -1$ such that $S_i = 0$, occurs when $y^* \leqslant m$, with $m < 0$, m representing the cutoff that determines the rate of false negative reports.

I define y_i^* with a linear function as:

$$y_i^* = w_i'\gamma + u_i \tag{5}$$

with $u_i \sim \mathrm{N}(0,1)$ and w_i a vector of observable individual reporting characteristics that determines respondents' likelihood to falsely report economic shocks.

To go back to my original equation, the researcher estimates:

$$H_i = \alpha_1 S_i + X_i'\alpha_2 + \epsilon_i \tag{6}$$

where I plugged in $S_i^* = S_i - d_{i,S^*}$. This means that $\epsilon_i = \nu_i + (S_i^* - S_i)\alpha_1 = \nu_i - \alpha_1 d_{i,S^*}$. Clearly, the OLS estimator is biased if $E(\epsilon_i|X,S) = E(\nu_i - \alpha_1 d_{i,S^*}|X,S) \neq 0$.

In the case where there is no misreporting, then the OLS estimator will be unbiased. This can be seen by setting $S = S^*$ such that $d_{i,S^*} = 0$ and thus $E(\nu_i - \alpha_1 d_{i,S^*}|X,S) = E(\nu_i|X,S) = E(\nu_i|X,S^*) = 0$ by assumption.

If there is misreporting and it is exogenous in the sense that the factors that explain misreporting are not correlated with the unobservable variable in the structural equation, that is $corr(y_i^*, \nu_i) = 0$ with $E(\nu_i|X,S^*) = 0$, then $E(\nu_i - d_{i,S^*}\alpha_1|X,S) = E(\nu_i|X,S) - \alpha_1 E(d_{i,S^*}|X,S) = -\alpha_1 E(d_{i,S^*}|X,S)$. The measurement error in the independent variable is part of the error term, which creates a bias. Like in the classical measurement error, one can see that in case of exogenous misreporting, the bias will attenuate the OLS estimator. Indeed, given that d_{i,S^*} and S

are positively correlated, and that the true α_1 is negative by assumption, then there is $\hat{\alpha}_1 > \alpha_1$, that is, there exists an attenuation bias in this case.

Misreporting is endogenous when $corr(y_i^*, \nu_i) \neq 0$, that is when the reporting characteristics of individuals, observable or not, are correlated with variables that are uncontrolled for in Eq. 1 and that explain both H_i and S_i. Indeed, when $corr(y_i^*, \nu_i) \neq 0$, then $E(\nu_i | S_i) \neq 0$ because of y_i^*. In that case, the estimates that results from regressing H_i on S_i will be biased. I show in the Online Appendix I that the asymptotic bias in case of endogenous misreporting can be written as:

$$plim(\hat{\alpha} - \alpha) = \frac{\Gamma - \alpha\Lambda}{\Theta} \tag{7}$$

where the direction of the bias is determined by the sign of the numerator. As shown in Nguimkeu *et al.* (2017) and detailed in the Online Appendix, both attenuation bias and expansion bias can occur. There can even be cases where the OLS estimates can have the wrong sign.

I provide two ways to empirically address the issue of misreporting. The first approach, which is a more heuristic one, relies on restricting my sample to mothers with similar reporting characteristics. The second approach exploits the structure of the model above and attempts to identify the respondents who are the most likely to falsely report negative economic shocks by trying to predict y_i^*.

The first technique I put in place to address this issue of self-reported shocks is to include in my sample respondents with the same unobserved "reporting" characteristics. Intuitively, it is possible that mothers who report no shocks at all between 2003 and 2008 are different from those who report 7 shocks (the maximum number) in the same period, not only in terms of observed characteristics x_i, but also in terms of uncontrolled reporting characteristics y_i^*. To control for these reporting characteristics, I follow Currie *et al.* (2018) and restrict my study sample to mothers who report a given number of shocks between 2003 and 2008, thereby including in sample only mothers with the same reporting patterns.

More specifically, I define as B the set of observations that are included in my analysis. So far, B was composed of all the mothers in MLSFH who gave birth between 2003 and 2008, irrespective of their number of shocks reported, that is:

$$B_1 = \{i : \mathbb{1}[0 \leqslant |s_{m_i}| \leqslant 7] = 1\} \tag{8}$$

where B_1 is the set of observations included in my analysis and s_{m_i} the number of shocks reported by the mother m of child i between 2003 and 2008. In my benchmark sample, I included in B_1 mothers who reported 0 to 7 shocks. The idea is now to sequentially restrict my study sample to mothers with similar number of shocks reported, such that their reporting style becomes more and more similar as the restriction becomes more binding. I therefore define others B_i with $i = \{2, 3, 4, 5\}$ as

$$B_2 = \{i : \mathbb{1}[1 \leqslant |s_{m_i}| \leqslant 7] = 1\} \tag{9}$$

$$B_3 = \{i : \mathbb{1}[1 \leqslant |s_{m_i}| \leqslant 4] = 1\} \tag{10}$$

$$B_4 = \{i : \mathbb{1}[2 \leqslant |s_{m_i}| \leqslant 7] = 1\} \tag{11}$$

$$B_5 = \{i : \mathbb{1}[2 \leqslant |s_{m_i}| \leqslant 4] = 1\} \tag{12}$$

B_2 restricts my study sample to mothers who reported over the period 2003-2008 between 1 and 7 shocks whereas B_3 restricts it to mothers who reported between 1 and 4 shocks. Compared to B_2, B_4 increases the lower bound to at least 2 and B_5 restricts the analysis to mothers who reported between 2 and 4 economic shocks from 2003 to 2008. My analysis thus relies on the assumption that mothers who experience similar number of reported shocks between 2003 and 2008 but not during the years they have given birth serve as an appropriate control group to mothers who did experience negative economic shocks the year of childbirth[22]. In terms of the model above, these restrictions drop observations with extreme values of y_i^* so that $\mathbb{1}(w_i'\gamma + u_i \geqslant n)$ and $\mathbb{1}(w_i'\gamma + u_i \leqslant m)$ never occur; that is, I restrict my set of mothers to those who have reporting characteristics $m \leqslant y_i^* \leqslant n$, whatever the value of S^* is, which guarantees that $S = S^*$.

Table 8 reports the results of this analysis using set of controls 1[23]. Column 1 shows the results including all $i \in \{B_1\}$, which are the results in my benchmark specification in Columns 1 of Tables 4 and 5. Column 2 includes $i \in \{B_2\}$. Discarding in my analysis the 31 children whose mothers have not reported any shocks between 2003 and 2008 does not affect my estimates. Further restricting my sample to mothers in B_3 leads to an increase of the detrimental effects of

[22] One possible limitation of this analysis is that mothers who did not experience a negative economic shock at childbirth actually experienced that extra shock after childbirth and not before it. And because economic shocks after birth are more likely to have negative impacts on child health than shocks occurring prior to the year of birth, the results I get from this analysis are likely to underestimate the true difference in child health between children who did and those who did not experience a shock at birth.

[23] Results are similar when using set of controls 2 and 3. Table J.1 in the Online Appendix J presents these results.

Table 8: Effects of negative economic shocks on objective measures of health using various set of mothers

	B_1 (1)	B_2 (2)	B_3 (3)	B_4 (4)	B_5 (5)
Weight	-.336**	-.323**	-.357**	-.307*	-.352*
	(.164)	(.164)	(.167)	(.184)	(.187)
Height	-.705	-.788	-.916*	-.842	-.997*
	(.539)	(.536)	(.540)	(.593)	(.598)
Obs.	789	758	725	621	588

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The results are derived using the set of controls 1. B_1 corresponds to my benchmark sample. B_2 restricts my sample to mothers who experienced at least one shock between 2003 and 2008. B_3 includes mothers who have experienced at least one shock but less than 5 and B_4 restricts my analysis to mothers who experienced at least 2 shocks. B_5 includes only mothers who reported between 2 and 4 negative shocks between 2003 and 2008.

economic shocks on weight and height from about 330 to 360 grams and 0.7 to 0.9 centimeters. Unlike previous results, the latter effect is now statistically significant at 90% confidence. Finally, when I restrict my sample to children whose mothers have reported at least 2 shocks to a maximum of 7 shocks (B_4, Column 4) and 4 shocks (B_5, Column 5), one can see that the estimates barely change. This analysis suggests that my results are robust to different reporting patterns and that self-report bias might be relatively modest in my setting.

The strategy above relies on the assumption that by restricting my sample to mothers who report similar numbers of shocks between 2003 and 2008, I am effectively able to ensure that they have identical reporting styles. This presumably allows to control for reporting patterns and therefore isolate the effects of negative economic shocks on children who experienced a shock at birth relative to those who did not. Another approach to assess whether my results are robust to misreporting is to explicitly model misreporting by allowing observed and unobserved characteristics of the mothers to explain true and false (positive and negative) reports[24]. The model below attempts to give some insights on the effect of endogenous misreporting and its consequences on the OLS estimator.

[24]The model that follows is a mix between Nguimkeu *et al.* (2017), who allow false negative reports in their model (one sided model) and Kreider (2010) who estimates conservative lower bounds of the OLS estimates by allowing small fractions of self-reports to be in error.

More specifically, the way I proceed to correct for endogenous misreporting bias is to change the reporting status of mothers with "unusual" reporting patterns, in the sense that $w_i'\gamma + u_i \leqslant m$ and $w_i'\gamma + u_i \geqslant n$[25]. This is similar in spirit to Kreider (2010) who identified conservative bounds estimates by changing the reporting status of respondents of the same particular type by hypothetically assuming the knowledge of their misreporting.

Given the assumptions of the model, I can write the probability of reporting or not reporting a shock as:

$$P(S_i = 1) = P(S_i^* = 1 \cap w_i'\gamma + u_i > m) + P(S_i^* = 0 \cap w_i'\gamma + u_i \geqslant n) \tag{13}$$

$$P(S_i = 0) = P(S_i^* = 0 \cap w_i'\gamma + u_i < n) + P(S_i^* = 1 \cap w_i'\gamma + u_i \leqslant m) \tag{14}$$

Because I assume that $S_i^* \perp u_i$ and $S^* \sim \text{Bern}\,(p)$, I can represent the above expressions as:

$$P(S_i = 1) = p(1 - \Phi(m - w_i'\gamma)) + (1-p)(1 - \Phi(n - w_i'\gamma)) = P_i(p, \gamma : m, n) \tag{15}$$

$$P(S_i = 0) = (1-p)\Phi(n - w_i'\gamma) + p\Phi(m - w_i'\gamma) = 1 - P_i(p, \gamma : m, n) \tag{16}$$

where I assume that u_i follows a standard normal distribution, i.e., $\Phi(.)$ is the cumulative distribution function of the standard normal distribution.

The likelihood function of this model is therefore:

$$l_n(p, \gamma : m, n) = \prod_{i=1}^{N} P_i(p, \gamma : m, n)^{S_i} (1 - P_i(p, \gamma : m, n))^{1-S_i} \tag{17}$$

from which I can derive the log-likelihood function:

$$L_n(p, \gamma : m, n) = ln(l_n(p, \gamma : m, n)) = \sum_{i=1}^{N} S_i \, lnP_i(p, \gamma : m, n) + (1 - S_i) \, ln(1 - P_i(p, \gamma : m, n)) \tag{18}$$

From this expression, I can estimate the probability of truly experiencing an economic shock p, that I denote by $\hat{p}$. $\hat{p}$ represents the value that maximizes the likelihood of observing the vector S while allowing for a proportion of false negative (m) and false positive (n) reports in the reported negative economic shocks. I choose values of m and n so as to allow for 0, 1, 2, 5 and 10% of false positive reports and 0, 2, 5, 10 and 20% of false negative reports, as false negative reporting

[25]One way to assess the robustness of my findings to exogenous misreporting would be to randomly select observations in my sample and change their shock status.

is usually more likely than false positive report (Meyer *et al.* 2018, Nguimkeu *et al.* 2017)[26]. The variables in the vector w are variables for which the extreme values predict the respondent's untruthful reporting. The upper tail of the y^* distribution should reflect individuals who falsely report the occurrence of negative economic shocks when none have occurred and the lower tail represents individuals with false negative reports, that is individuals who did experience economic shocks but report that they did not.

The first variable I include in w is the difference between the average number of shocks per interview reported by the respondent's interviewer and the average number of shocks reported by all the other interviewers. The idea is that interviewers have an effect on the reporting pattern of the respondents and any deviation from the average might be due to false positive or false negative reports. For instance, an interviewer whose respondents report on average a higher number of shocks per interview than other interviewers is more likely to have some of his or her respondents report shocks that did not occur (false positive). On the other hand, a very low average rate of reported shocks per interview for a given interviewer compared to others is correlated with the probability of the interviewer's respondents falsely reporting the nonoccurrence of a shock (false negative)[27].

The second variable I include in the vector w is the number of "Don't know" and "Can't remember" responses the respondents have used to answer the questions from all the modules in the survey. The rationale behind this variable is that individuals with many of such answers are more likely to not report a shock that did occur (false negative) than respondents with fewer "Don't know" and "Can't remember" answers. On the other hand, respondents with few of such answers are more likely to report shocks even though they did not occur (false positive).

The same idea applies to my third variable that exploits a question at the very end of the survey that asks the interviewers to evaluate the degree of cooperation of the respondent during the interview as compared to other respondents, on a scale of 1 ("Bad") to 4 ("Very good"). Respondents with a high level of cooperation are assumed to be more likely to report false positive shocks while the opposite is true for those with a low degree of cooperation. I standardize these three variables to put them on the same scale and to guarantee that $\hat{y}^*$ follows a distribution that is close to $\mathrm{N}(0, 1)$.

[26]Note that in the case of no false negative reports, I have $m = -\infty$ such that $P(S_i = 0) = (1-p)\Phi(n - w_i'\gamma)$. In the case of no false positive reports, $n = +\infty$ and $P(S_i = 1) = p(1 - \Phi(m - w_i'\gamma))$. The case where there is no false positive nor false negative reports corresponds to the benchmark case.

[27]It is worth noting that interviewers were randomly allocated to respondents.

The strategy to account for misreporting in my estimation is to change the shock status of those who are the most likely to false negatively or false positively report a shock. To do so, after estimating $\hat{p}$ from the maximum likelihood function above, I compute the number of individuals for which I have to change the shock status, $R_{i,j}$ with $i = \{0, 2, 5, 10, 20\}$ corresponding to the rate of false negative and $j = \{0, 1, 2, 5, 10\}$ the rate of false positive reports, such that the new vector S, that I denote by S', has a rate of shocks equal to $\hat{p}$ with:

$$\hat{p} = \frac{N_{S'=1}}{N} = \frac{N_{S^*=1}}{N} \tag{19}$$

Obviously, $R_{0,0} = 0$. For cases where there is no mix of false positive and false negative reports, i.e., $R_{0,.}$ and $R_{.,0}$, I compute R as:

$$R = |p - \hat{p}| \times N \tag{20}$$

with N as the number of observations in my sample and $p = \frac{N_{S=1}}{N}$ as the proportion of individuals who self-reported experiencing a negative shock. I then change the shock status of the $R_{.,0}$ individuals at the left tail of the $\hat{y}^* = w_i'\hat{\gamma}$ distribution if $S = 0$ (false negative), and change the shock status of the $R_{0,.}$ individuals at the right tail of the $\hat{y}^* = w_i'\hat{\gamma}$ distribution if $S = 1$ (false positive), starting in both cases from the most extreme values.

In cases where both false positive and false negative reports are present, i.e., when $R_{i,j}$ with $i = \{2, 5, 10, 20\}$ and $j = \{1, 2, 5, 10\}$, I compute $R_{i,j}$ as:

$$R_{i,j} = max(R_{i,0}, R_{0,j}) + R'_{i,j} \tag{21}$$

with $R'_{i,j} = |p - \hat{p}| \times N$ for every pair of i and j, $[2, 5, 10, 20] \times [1, 2, 5, 10]$. The reason I take the $max(.)$ is because the false positive and false negative reports cancel each other out in S', so that $R'_{i,j}$ does not reflect the real number of misreports[28]. By taking $max(R_{i,0}, R_{0,j})$, I follow a more conservative approach and make sure that the number of false reports is at least as big as $R_{i,0}$ and $R_{0,j}$ for each corresponding i and j. I therefore change the shock status of at least $max(R_{i,0}, R_{0,j})$ individuals on both ends of the y^* distribution. By adding $R'_{i,j}$ to the $max(.)$ function in Eq. 21, I make sure that after changing the shock status of these $R_{i,j}$ individuals, the newly created shock vector S' equals S^*, i.e., $\hat{p} = \frac{N_{S'=1}}{N} = \frac{N_{S^*=1}}{N}$.

[28] If the number of false positive and false negative reports is identical, then $\hat{p} = \frac{N_{S'=1}}{N} = \frac{N_{S=1}}{N} = p$.

Table K.1 in the Online Appendix K reports the estimates of p that I derive from my maximum likelihood function using the three variables in w I described above[29]. Not surprisingly, as the rate of false positive reporting increases (Columns), the probability of "truly" experiencing an economic shocks decreases, whereas that same probability increases when the rate of false negative report goes up (Rows). When both false positive and false negative reports occur, the effects cancel each other out so that the $\hat{p}$ estimated in the diagonal elements of the p-matrix are close to the case where there is no misreporting (first cell in the p-matrix).

Panels A and B of Table 9 report OLS estimates of the effects of economic shocks on weight and height, respectively, after changing the shock status of $R_{i,j}$ respondents at the tails of $\hat{y}^*$ to take endogenous misreporting into account. The first cells of Panels A and B in Table 9 are my benchmark estimates and corresponds to the case where there is no false positive and false negative reports. When allowing for false positive reports (Columns) to be at maximum 5% and false negative reports (Rows) to be at maximum 10%, one can see in Panel A that the effects of S' on weight are robust and fairly precisely estimated, with effects of economic shocks on weight ranging from about 200 to 450 grams. However, when allowing for higher rates of false positive and false positive reports, the negative effect of shocks disappears.

Similarly for my estimates of S' on height in the second panel of Table 9: the effects of negative economic shocks seem to be quite robust for low rates of false reports, with an effect ranging from 0.2 to 0.7 centimeters. However, the negative effect disappears when allowing higher rates of misreporting. Note that, as was the case in my benchmark specification, these effects are not precisely estimated and fail to be statistically significant.

The estimates above use my first set of control variables as regressors in the OLS estimations. Table K.2 for weight and Table K.3 for height in the Online Appendix K show that the results above are robust to the inclusion of additional control variables in my econometric specification (set of control variables 3), which reduces my sample of observations from 775 to 629.

Overall, my results on the negative effects of economic shocks at birth on weight and height appear to be robust to misreporting as long as the rates of false positive and false negative reports remain relatively low. It is worth noting that, unsurprisingly, the negative effects disappear when these rates increase given the conservative approach that I put in place. Indeed, in the case where I allow 10% of false positive and 20% of false negative reports, this amounts to changing the

[29] Note that the results below are derived using my first set of control variables and are based on a sample of 775 observations. The difference in the number of observations is due to missing information in the variables in w.

Table 9: Effects of S_i' on weight and height

		Rate of false positive				
A. Effects of economic shocks on weight						
		0%	1%	2%	5%	10%
Rate of false negative	0%	-0.3280**	-0.3134*	-0.3755**	-0.2678	-0.2746
		(0.1669)	(0.1714)	(0.1743)	(0.1794)	(0.1921)
	2%	-0.4188***	-0.4535***	-0.4850***	-0.2555	0.0393
		(0.1553)	(0.1640)	(0.1686)	(0.1726)	(0.1948)
	5%	-0.4427***	-0.3960**	-0.4341***	-0.4177**	-0.0500
		(0.1552)	(0.1632)	(0.1672)	(0.1738)	(0.1987)
	10%	-0.3780**	-0.1936	-0.2924*	-0.3351**	0.0160
		(0.1567)	(0.1742)	(0.1758)	(0.1807)	(0.1977)
	20%	-0.2333	-0.0014	0.0063	-0.0857	0.0236
		(0.1548)	(0.1713)	(0.1737)	(0.1770)	(0.1807)
B. Effects of economic shocks on height						
		0%	1%	2%	5%	10%
Rate of false negative	0%	-0.5222	-0.4377	-0.6545	-0.2969	-0.2469
		(0.5404)	(0.5360)	(0.5439)	(0.5570)	(0.5760)
	2%	-0.7134	-0.3160	-0.4812	-0.1362	-0.0712
		(0.4941)	(0.5802)	(0.5860)	(0.5848)	(0.6416)
	5%	-0.4828	-0.3478	-0.3894	-0.2513	0.0114
		(0.5617)	(0.5550)	(0.5715)	(0.5777)	(0.6228)
	10%	-0.4258	-0.3168	-0.3389	-0.4354	-0.0112
		(0.5503)	(0.5356)	(0.5422)	(0.5610)	(0.6013)
	20%	-0.2484	-0.1316	-0.0139	-0.0526	0.1001
		(0.5258)	(0.5161)	(0.5155)	(0.5382)	(0.5644)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Estimated effects of S' on weight (Panel A) and height (Panel B), allowing for different rates of false positive reports (Columns) and false negative reports (Rows), using set of controls 1. The sample is based on 775 observations.

shock status of 75 children on the left tail of the y^* distribution (from 0 to 1, that is false negative reports) and 95 children on the right tail of the y^* distribution (from 1 to 0, that is false positive reports). Out of the 29.29% of children who experienced a shock at birth (first cell in Table K.1), about 42% were therefore considered as not having experienced a shock while about 14% of those who did not experience a shock were assumed to have experienced one. These changes represent a significant amount of misreporting in the analysis, which explains the differences in the results between the cases where I allow for low versus high rates of false positive and false negative reports.

7 Discussion and conclusion

In this study, I estimate the effects of negative economic shocks during pregnancy or the year of childbirth on child health in Malawi, a Sub-Saharan African country where poverty is deep and wide. I show that negative economic shocks have effects on both subjective and objective measures of child health. More specifically, I find that children who experience a negative economic shock at birth are about 7 percentage points less likely to be reported to be in excellent health and 8 percentage points less likely to be reported to be in much better health than children of the same sex and age in the same village by their mothers. I show that these effects are robust to reporting heterogeneity and unobserved mother and household effects that are constant over children from the same family. I also show that children who experience a shock at birth were about 300 grams lighter and 0.4 centimeters shorter, although the latter effect fails to be precisely estimated in some of my specifications. All these effects are particularly strong for boys.

I also explore the plausibility of the exogeneity of the economic shocks used in my analysis and issues around the self-reporting of shocks. With regard to the exogeneity assumption, my dataset allows me to identify shocks that were triggered exogenously –independently of mothers' characteristics– and that affected the community as a whole. I show that taking into account these covariate shocks leads to similar results.

With regards to the fact that shocks are self-reported, I propose a simple model that allows to control for endogenous misreporting by identifying respondents who are likely to misreport. I show that changing the shock status of those who are likely to misreport generate similar results, as long as the rates of false positive and false negative reports are not too high.

My study sheds light on the consequences of negative economic shocks that mothers experience while pregnant or the year they give birth on child health. This constitutes further evidence of the intergenerational transmission of poverty and inequality in developing countries (Bhalotra and Rawlings 2013). These results also highlight the indirect consequences of economic instability on child health and malnutrition and draw further attention to the particular economic vulnerability of families living in Malawi, and perhaps more broadly in Sub-Saharan Africa. Indeed, I believe my findings speak not only to the Malawian context but also to Sub-Saharan African countries in general. Malawi shares many socio-economic and socio-demographic characteristics with its neighbouring countries (Chin 2010) and it is likely that negative economic shocks have identical effects in settings that share similar fragility and vulnerability.

From a policy perspective, my results imply that economic shocks at a specific time in life can have long-lasting effects and that families cannot rely on social network and informal safety nets to protect themselves against shocks that affect the community as a whole. Policies aiming to protect families with young children and particularly pregnant women against negative economic shocks can help mitigate the deleterious consequences of these shocks, especially in terms of food security and health care use. Given the substantial economic costs of undernutrition and the now demonstrated dramatic benefits of investing in nutrition, where the return for every dollar invested can be up to 35 dollars (Shekar *et al.* 2017), guaranteeing food security to vulnerable individuals to ensure their healthy development is not only the right thing to do, but it is also a smart investment. This could improve the well-being of not only the mothers who are subject to economic shocks but also their children who start their life with lower initial health capital. This resonates well with the new direction that international organizations such as the World Bank and the United Nations are taking, when placing human capital development, especially early in life, at the center of their agendas (The World Bank Group 2018).

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ONLINE APPENDIX

Appendix A

Table A.1: Marginal effects of negative economic shocks at birth on subjective health outcomes - Logit regressions

Effects of economic shocks at birth on: Probability of being	(1)	(2)	(3)
ill in the last 12 months	0.011	0.021	0.019
	(0.029)	(0.033)	(0.032)
ill for more than 1 month	0.016	0.016	0.015
	(0.013)	(0.016)	(0.015)
in very good health	-0.016	-0.037	-0.036
	(0.025)	(0.029)	(0.029)
in excellent health	-0.033	-0.070**	-0.073**
	(0.028)	(0.031)	(0.031)
in better health	-0.029	-0.058*	-0.053*
	(0.027)	(0.031)	(0.031)
in much better health	-0.034	-0.083***	-0.082***
	(0.027)	(0.030)	(0.030)
Obs.	1784	1382	1380

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively.

Table A.2: Marginal effects of negative economic shocks at birth on subjective health outcomes - Probit regressions

Effects of economic shocks at birth on:			
Probability of being	(1)	(2)	(3)
ill in the last 12 months	0.011	0.021	0.019
	(0.029)	(0.032)	(0.032)
ill for more than 1 month	0.015	0.015	0.014
	(0.013)	(0.015)	(0.015)
in very good health	-0.016	-0.036	-0.035
	(0.025)	(0.029)	(0.029)
in excellent health	-0.033	-0.070**	-0.074**
	(0.027)	(0.031)	(0.031)
in better health	-0.028	-0.057*	-0.053*
	(0.027)	(0.031)	(0.031)
in much better health	-0.033	-0.081***	-0.081***
	(0.027)	(0.030)	(0.030)
Obs.	1784	1382	1380

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively.

Appendix B

Table B.1: Estimates of the effects of negative economic shocks on subjective health measures controlling for mother fixed effects (linear)

Probability of being:	(1)	(2)	(3)
ill in the last 12 months	.003	.010	.009
	(.039)	(.050)	(.050)
ill for more than 1 month	.004	-.003	-.003
	(.018)	(.024)	(.024)
in very good health	-.042	-.028	-.028
	(.031)	(.045)	(.045)
in excellent health	-.042	-.099**	-.103**
	(.029)	(.040)	(.040)
in better health	-.009	-.012	-.012
	(.034)	(.049)	(.049)
in much better health	-.042	-.073**	-.073**
	(.029)	(.035)	(.035)
Obs.	1203	769	767

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Effect of negative economic shock during the year of birth on subjective health outcomes controlling for mother fixed effects. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively. Estimates of the other coefficients are available upon request.

Table B.2: Estimates of the effects (odd-ratios) of negative economic shocks on subjective health measures controlling for mother fixed effects (logit)

	Set of controls 1		Set of controls 2	
	Odd ratios	Conf. Int. (95%)	Odd ratios	Conf. Int. (95%)
Probability of being:	(1)	(2)	(3)	(4)
ill in the last 12 months	1.003	[1.676-0.601]	0.821	[1.695-0.398]
Obs.	406		256	
ill for more than 1 month	1.791	[7.288-0.440]	n.a.	n.a.
Obs.	107	.		
in very good health	0.694	[1.290-0.373]	0.856	[1.821-0.403]
Obs.	277		190	
in excellent health	0.613	[1.281-0.293]	0.323**	[0.956-0.109]
Obs.	243		154	
in better health	0.803	[1.339-0.481]	0.781	[1.507-0.405]
Obs.	318		212	
in much better health	0.592	[1.217-0.405]	0.145**	[0.668-0.032]
Obs.	226		137	

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Effects of negative economic shocks during the year of birth on subjective health outcomes controlling for mother fixed-effect with a logit specification by means of conditional likelihood function. Columns 1 and 3 include set of controls 1 and 2, respectively. The results using set of controls 3 are similar to the ones derived using set of control 2 as there is no within-mother variation in the variables that are added to the model. Estimates of the other coefficients are available upon request. There was not enough variation in the "Being ill for more than 1 month" specification for it to be estimated when using set of controls 2. Estimates presented are odd-ratios.

Appendix C

Table C.1: Effects of negative economic shocks at birth on objective health outcomes using different wealth scores

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
1. Wealth measure 1						
Economic shock at birth	-0.336**	-0.284*	-0.316*	-0.705	-0.587	-0.550
	(0.164)	(0.164)	(0.166)	(0.539)	(0.548)	(0.549)
Wealth score		-0.130***	-0.138***		0.208	0.156
		(0.047)	(0.049)		(0.168)	(0.176)
Obs.	789	769	768	789	769	768
2. Wealth measure 2						
Economic shock at birth	-0.336**	-0.419*	-0.473**	-0.705	-1.278*	-1.288*
	(0.164)	(0.228)	(0.231)	(0.539)	(0.700)	(0.695)
Wealth score		-0.136**	-0.132**		-0.019	-0.026
		(0.064)	(0.064)		(0.154)	(0.168)
Obs.	789	487	486	789	487	486

Note: *Note*: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1-3 represent the results for weight and Columns 4-6 for height. Wealth measure 1 is the wealth of the household in 2004, subsequently changing missing values with the wealth level of the household in 2006 and then in 2008. Wealth measure 2 only takes into account the household wealth level in 2004, not exploiting the information from the 2006 and 2008 waves.

Appendix D

Effects on weight-for-age and height-for-age z-scores

One can assess where in the weight and height distributions the effects of negative economic shocks are taking place using z-scores. Assuming weight and height are normally distributed in the population, I can estimate the effects of negative economic shocks on weight-for-age and height-for-age z-scores, as defined as:

$$z_i = \frac{m_i - M_{s,a}}{sd_{s,a}} \tag{D.1}$$

where m_i is my objective anthropometric measure (weight or height) of child i, $M_{s,a}$ is the median and $sd_{s,a}$ the standard deviation of m from i's reference group based on sex s and age a in my sample. Weight-for-age and height-for-age z-scores are widely used anthropometric measures and deficits in these measures are often seen as evidence of malnutrition (Fishman *et al.* 2004, WHO 1995). As an indicator of thinness and wasting, low weight-for-age z-score implies recent or continuing current severe weight loss and is the strongest anthropometric predictor of child malnutrition and long-term mortality in developing countries used in the literature (Fishman *et al.* 2004, WHO 1995). Low height-for-age reflects shortness and stunted growth, which is a failure to reach optimal health potential. This is often characterized by early and long-term exposure to adverse conditions due, for instance, to illness and malnutrition (De Onis *et al.* 1997, WHO 1995).

I derive binary variables that take the value 1 if the z-score in consideration is less than d and 0 otherwise, with $d = \{-2, -1, 0, 1, 2\}$. Children with z-scores of weight-for-age and height-for-age lower than -2 are categorized as suffering from moderate to severe undernutrition (De Onis *et al.* 1997). More specifically, childhood stunting, a good indicator of children well-being and malnutrition (De Onis and Branca 2016), corresponds to a height-for-age z-score below -2 (WHO 1995) and moderate to severe underweight corresponds to a weight-for-age z-score lower than -2. A weight-for-age z-score between -2 and -1 represents the case of children who are mildly underweight (Fishman *et al.* 2004).

The first panel of Table D.1 reports the results of the effects of economic shocks experienced by children on their weight-for-age z-score. Column 1 shows the effects of economic shock on the z-score when considering the z-score as a continuous measure. The table shows that the weight of the children who experienced a shock at birth is on average about 0.16 standard deviation lower than the median weight.

When discretizing my continuous measures into categories, Column 2 shows that children who experienced a shock at birth were 3 percentage points more likely to have a weight that is 2 standard deviations lower than others and this effect is statistically significant at the 10%-level[30]. The same children were also about 10 percentage points more likely to be lower than median weight and 3 percentage points more likely to have a weight that is less than +2 standard deviations above the median. These effects are statistically significant at the 1%-level. This shows that experiencing a negative economic shock at birth results in a shift towards the left of the weight-for-age z-score distribution. The effect appears to be quite homogeneous along the weight distribution, although the effects are stronger at the median and are statistically significant only at both ends of the distribution and at the median.

The second panel of Table D.1 shows the results of the same regressions, but looking at height instead of weight. Negative economic shocks in utero or during the year of birth appear to have a negative effect on the height-for-age z-score (Column 1) and a positive effect on the probability of having a z-score that is below the various z-score thresholds. These effects are

[30] As mentioned above, a weight-for-age z-score of -2 and below characterizes children as being underweight (De Onis *et al.* 1997).

Table D.1: Weight-for-age and height-for-age z-scores, assuming normal distributions

	Z-score	<-2	<-1	<0	<1	<2
1. Weight						
Set of controls 1	-.168**	.028*	.027	.088**	.037	.025***
	(.084)	(.016)	(.030)	(.045)	(.030)	(.010)
Set of controls 2	-.157*	.031*	.020	.105**	.030	.026**
	(.090)	(.017)	(.032)	(.048)	(.032)	(.012)
Set of controls 3	-.169*	.031*	.020	.108**	.035	.027**
	(.091)	(.017)	(.032)	(.048)	(.032)	(.012)
2. Height						
Set of controls 1	-.124	.011	.049	.066	.046	-.007
	(.091)	(.014)	(.032)	(.045)	(.033)	(.015)
Set of controls 2	-.062	.010	.027	.022	.031	-.013
	(.098)	(.016)	(.032)	(.048)	(.037)	(.018)
Set of controls 3	-.061	.008	.027	.018	.033	-.012
	(.099)	(.016)	(.032)	(.048)	(.037)	(.018)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Effects of negative economic shocks during the year of birth on z-score (Column 1) and on dummy variables that take value 1 if z-score is below d with $d = \{-2, -1, 0, 1, 2\}$. The first panel looks at the effects on weight and the second at the effects on height.

however not precisely estimated.

Appendix E

Table E.1: Effects of negative economic shocks on objective measures of health, splitting the sample by whether the mothers have received more or less transfers than the median value in 2006

Financial and in-kind transfers received (in 2006)				
	Weight		Height	
	Below	Above	Below	Above
	(1)	(2)	(3)	(4)
Set of controls 1	-0.405	-0.215	-0.891	-0.040
	(0.283)	(0.238)	(1.021)	(0.670)
Set of controls 2	-0.440	-0.062	-0.557	0.228
	(0.321)	(0.226)	(1.092)	(0.677)
Set of controls 3	-0.448	-0.089	-0.595	0.206
	(0.324)	(0.224)	(1.097)	(0.672)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. I split my sample by whether respondents have received informal transfers, be they financial or in-kind, from fewer (Columns 1 and 3) or more (Columns 2 and 4) persons than the median value in my sample (in 2006).

Appendix F

Table F.1: Effects of economic shocks one year before, one year after and during the year of birth on objective health measures

	(1)	(2)	(3)
1. Weight			
Economic shock a year before birth	.171	-.026	-.031
	(.213)	(.222)	(.221)
Economic shock at birth	-.348*	-.380*	-.411**
	(.182)	(.195)	(.196)
Economic shock a year after birth	-.115	-.139	-.154
	(.168)	(.185)	(.188)
2. Height			
Economic shock a year before birth	.649	.685	.662
	(.657)	(.748)	(.754)
Economic shock at birth	-.792	-.424	-.438
	(.569)	(.634)	(.639)
Economic shock a year after birth	.331	.087	.066
	(.520)	(.581)	(.580)
Obs.	645	524	524

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively. The first panel shows the effects of negative economic shocks on weight for shocks that occur one year prior to birth (row 1), during the year of birth (second row) and one year after the year of birth (row 3). The second panel shows the results for height instead of weight. The sample underlying this specification is smaller than in my previous analyses because shocks reported in wave 5 of MLSFH cover only the period from 2003 to 2008. I therefore don't know whether those born in 2003 experienced a shock in 2002 and those born in 2008 experienced a shock in 2009. I thus discard these observations and keep for this analysis only children who are born between 2004 and 2007, leading to a reduced sample size.

Appendix G

Table G.1: Effects of economic shocks on mortality

Effects on mortality	(1)	(2)	(3)
Shock at birth	.009	-.002	-.001
	(.013)	(.015)	(.014)
Idiosyncratic shock at birth	-.004	.001	.001
	(.018)	(.021)	(.021)
Common shock at birth	.020	.004	.005
	(.016)	(.017)	(.016)
Obs.	1939	1510	1508

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Age of the children is not controlled for in these regressions. Sample consists of 1939 children, 1808 are alive and 131 are dead (6.76%) in 2008. Idiosyncratic shocks are shocks affecting the household of the respondents only. Common shocks are shocks that affect other households as well, as defined in Table 2. Regressions are conducted separately for each type of shocks (rows).

Appendix H

Table H.1: Effects of covariate shocks on objective health outcomes for various levels of negative economic shocks, including set of controls 2

	(1)	(2)	(3)	(4)	(5)	(6)
1. Weight						
Covariate shocks	-.106		-.021		-.074	
	(.196)		(.223)		(.246)	
Poor crop yields,loss of crops due to disease or pests		.117		.198		-.179
		(.238)		(.280)		(.351)
Big change in price of grain		-.200		-.206		.070
		(.239)		(.266)		(.265)
2. Height						
Covariate shocks	-.437		-.995		-1.073	
	(.639)		(.714)		(.812)	
Poor crop yields, loss of crops due to disease or pests		-.183		-.673		-1.159
		(.702)		(.813)		(1.128)
Big change in price of grain		-.453		-.566		-.463
		(.852)		(.944)		(.978)
Including shocks affecting only HH	y	y				
Excluding shocks affecting only HH			y	y		
Including only shocks affecting most or all HH in community only					y	y
Obs.	639	639	639	639	6339	639

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. These regressions include set of controls 2. Covariate shock is a dummy variable that combines shocks due to poor crop yields/disease/pest and those due to big changes in price of grain. Columns 1 and 2 include shocks affecting all households, including those that have affected only the household of the respondents. Columns 3 and 4 exclude shocks that have affected only the household of the respondents. Columns 5 and 6 take into account only shocks that have affected most or all households in the community.

Table H.2: Effects of covariate shocks on objective health outcomes for various levels of negative economic shocks, including set of controls 3

	(1)	(2)	(3)	(4)	(5)	(6)
1. Weight						
Covariate shocks	-.132		-.061		-.124	
	(.197)		(.225)		(.244)	
Poor crop yields,loss of crops due to disease or pests		.101		.155		-.259
		(.237)		(.279)		(.345)
Big change in price of grain		-.221		-.217		.067
		(.238)		(.268)		(.261)
2. Height						
Covariate shocks	-.433		-1.009		-1.093	
	(.642)		(.716)		(.815)	
Poor crop yields, loss of crops due to disease or pests		-.184		-.700		-1.232
		(.706)		(.815)		(1.139)
Big change in price of grain		-.444		-.546		-.430
		(.859)		(.959)		(.992)
Including shocks affecting only HH	y	y				
Excluding shocks affecting only HH			y	y		
Including only shocks affecting most or all HH in community only					y	y
Obs.	639	639	639	639	639	639

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. These regressions include set of controls 3. Covariate shock is a dummy variable that combines shocks due to poor crop yields/disease/pest and those due to big changes in price of grain. Columns 1 and 2 include shocks affecting all households, including those that have affected only the household of the respondents. Columns 3 and 4 exclude shocks that have affected only the household of the respondents. Columns 5 and 6 take into account only shocks that have affected most or all households in the community.

Appendix I

The derivation of the asymptotic bias in case of endogenous misreporting follows closely Nguimkeu *et al.* (2017) and the results they derive in their one-sided model. I know from Eq. 6 and the Frisch-Waugh theorem that

$$MH = \alpha_1 MS + M\epsilon \tag{I.1}$$

where I omit the subscript X on the projection matrix M for ease of notation (M_X). It follows that the OLS estimator of α_1 is:

$$\hat{\alpha}_1 = (S'MS)^{-1}S'MH \tag{I.2}$$

where I use the idempotence of the projection matrix M and the fact that $M = M'$. Plugging in the expression for MH in Eq. I.2 and rearranging yields:

$$\hat{\alpha}_1 - \alpha_1 = (S'MS)^{-1}S'M\epsilon \tag{I.3}$$

Taking the expectation, I then get:

$$E(\hat{\alpha}_1 - \alpha_1|X,S) = (S'MS)^{-1}S'ME(\epsilon_i|X,S) \neq 0 \tag{I.4}$$

as explained above due to both $E(\nu_i|X,S) \neq 0$ and $-\alpha_1 E(d_{i,S^*}|X,S)) \neq 0$.

To determine the inconsistency of the OLS estimator, I can express the above expression as:

$$\hat{\alpha}_1 - \alpha_1 = (S'MS)^{-1}S'M\epsilon \tag{I.5}$$

$$\hat{\alpha}_1 - \alpha_1 = \left(\frac{S'MS}{N}\right)^{-1}\frac{S'M\epsilon}{N} \tag{I.6}$$

$$\hat{\alpha}_1 - \alpha_1 = \left(\underbrace{\frac{S'MS}{N}}_{①}\right)^{-1}\left(\underbrace{\frac{S'M\nu}{N}}_{②} - \underbrace{\frac{S'M\alpha d}{N}}_{③}\right) \tag{I.7}$$

I derive now each of the three terms on the right hand side of Eq. I.7, starting with ①.

$$① = \frac{S'MS}{N} = \frac{S'[I - X(X'X)^{-1}X']S}{N} = \frac{S'S}{N} - \frac{S'X(X'X)^{-1}X'S}{N} \tag{I.8}$$

which, following the Weak Law of Large Numbers and the Slutsky theorem, leads to

$$① \xrightarrow{p} E(S_i^2) - E(S_i x_i')E(x_i x_i')^{-1}E(S_i x_i) \tag{I.9}$$

and then, using the Continuous Mapping theorem, I know that:

$$①^{-1} = \left(\frac{S'MS}{N}\right)^{-1} \xrightarrow{p} [E(S_i^2) - E(S_i x_i')E(x_i x_i')^{-1}E(S_i x_i)]^{-1} \tag{I.10}$$

Similarly, ② can be written as follows:

$$② = \frac{S'M\nu}{N} \xrightarrow{p} E(S_i\nu_i) - E(S_i x_i')E(x_i x_i')^{-1}E(x_i\nu_i) = E(S_i\nu_i) \tag{I.11}$$

where I use the fact that $E(x_i\nu_i) = E(x_i)E(\nu_i) = 0$. To define $E(S_i\nu_i)$, I remember that

$S_i = \mathbb{1}(w_i'\gamma + u_i \geqslant n \cap S_i^* = 0) - \mathbb{1}(w_i'\gamma + u_i \leqslant m \cap S_i^* = 1) + S_i^*$ so that:

$$E(S_i\nu_i) = E(\nu_i\mathbb{1}(w_i'\gamma + u_i \geqslant n \cap S_i^* = 0) - \nu_i\mathbb{1}(w_i'\gamma + u_i \leqslant m \cap S_i^* = 1) + \nu_i S_i^*) \tag{I.12}$$
$$= E((1-p)Pr(u_i \geqslant n - w_i'\gamma)E(\nu_i|u_i \geqslant n - w_i'\gamma) - pPr(u_i \leqslant m - w_i'\gamma)E(\nu_i|u_i \leqslant m - w_i'\gamma)) \tag{I.13}$$

where I use the exogeneity of S_i^*, the law of iterated expectations and the fact that $E(\nu_i) = 0$.

I assume $\binom{\nu_i}{u_i} \sim \mathrm{N}(0, \Sigma)$ with $\Sigma = \begin{pmatrix} \sigma_\nu^2 & \delta\sigma_\nu\sigma_u \\ \delta\sigma_\nu\sigma_u & \sigma_u^2 \end{pmatrix}$ and $corr(\nu_i, u_i) = \delta$. After some arrangements, Eq. I.13 simplifies to:

$$E(S_i\nu_i) = E[(1-p)\delta\sigma_\nu\phi(\frac{n - w_i'\gamma}{\sigma_u}) + p\delta\sigma_\nu\phi(\frac{m - w_i'\gamma}{\sigma_u})] \tag{I.14}$$

such that

$$② = \frac{S'M\nu_i}{N} \xrightarrow{p} E(S_i\nu_i) = E[(1-p)\delta\sigma_\nu\phi(\frac{n - w_i'\gamma}{\sigma_u}) + p\delta\sigma_\nu\phi(\frac{m - w_i'\gamma}{\sigma_u})] \tag{I.15}$$

Turning now to ③, I have

$$③ = \frac{\alpha S'Md}{N} = \frac{\alpha S'[I - X(X'X)^{-1}X']d}{N} \tag{I.16}$$
$$= \frac{\alpha S'd}{N} - \frac{\alpha S'X(X'X)^{-1}X'd}{N} \tag{I.17}$$
$$\xrightarrow{p} \alpha E(S_i d_i) - \alpha E(S_i x_i')E(x_i x_i')^{-1}E(d_i x_i') \tag{I.18}$$

This leads to:

$$plim(\hat{\alpha} - \alpha) = \frac{E[(1-p)\delta\sigma_\nu\phi(\frac{n - w_i'\gamma}{\sigma_u}) + p\delta\sigma_\nu\phi(\frac{m - w_i'\gamma}{\sigma_u})] - \alpha[E(S_i d_i) - E(S_i x_i')E(x_i x_i')^{-1}E(d_i x_i')]}{E(S_i^2) - E(S_i x_i')E(x_i x_i')^{-1}E(S_i x_i)} \tag{I.19}$$

In case of endogenous misreporting, both attenuation bias and expansion bias can occur. As detailed below and discussed in Nguimkeu *et al.* (2017), there can even be cases where the OLS estimates can have the wrong sign.

Because by assumption α is negative, attenuation bias exists when $plim(\hat{\alpha} - \alpha) > 0$ and expansion bias exists when $plim(\hat{\alpha} - \alpha) < 0$. There are also cases where $\hat{\alpha} < 0 < \alpha$ or $\hat{\alpha} > 0 > \alpha$, that is, OLS estimates can have the wrong sign. To see this, rewrite Eq. I.19 as:

$$plim(\hat{\alpha} - \alpha) = \frac{\Gamma - \alpha\Lambda}{\Theta} \tag{I.20}$$

Because the denominator Θ is positive (by the Cauchy-Schwartz inequality), the direction of the bias is determined by the sign of the numerator.

First, one can show that expansion (attenuation) bias occurs if $\frac{\Gamma}{\Lambda} < (>)\alpha$ and $\Lambda > (<)0$ or when $\Lambda < (>)0$ and $\frac{\Gamma}{\Lambda} > (<)\alpha$. One can also show that $\hat{\alpha}$ and α can have opposite signs. Again, assuming that $\alpha < 0$, then $\hat{\alpha} > 0$ if $\Gamma > 0$ and $0 > \alpha > \frac{\Gamma}{\Lambda - \Theta}$ with $\Theta - \Lambda > 0$. In case $\Theta - \Lambda < 0$, then α will have to be smaller than $\frac{\Gamma}{\Lambda - \Theta}$ for $\hat{\alpha}$ to have the opposite sign of α. It is worth noting that these last conditions can be met in my setting. Recall that $\Gamma = E(S_i\nu_i)$, such that $\Gamma > 0$ holds if $\delta = corr(\nu_i, u_i) > 0$. This can potentially be the case as unobserved factors in Eq. 1 that explain poor health can be positively correlated with factors that explain misreporting behaviors[31].

[31] It is interesting to note that the sign switching region basically depends on the size of $\frac{\Gamma}{\Lambda - \Theta}$. One can show

that $\frac{\partial \zeta}{\partial \delta} > 0$, $\frac{\partial \zeta}{\partial \sigma_\epsilon^2} > 0$, $\frac{\partial \zeta}{\partial m} > 0$ and $\frac{\partial \zeta}{\partial n} < 0$ with $\zeta = \frac{\Gamma}{\Lambda - \Theta}$.

Appendix J

Table J.1: Effects of negative economic shocks on objective measures of health using various sets of mothers and sets of control variables

	B_1 (1)	B_2 (2)	B_3 (3)	B_4 (4)	B_5 (5)
1. Set of controls 2					
Weight	-.325*	-.301*	-.342*	-.264	-.320*
	(.173)	(.173)	(.176)	(.190)	(.193)
Height	-.355	-.389	-.530	-.420	-.594
	(.583)	(.581)	(.582)	(.644)	(.646)
Obs.	639	613	588	502	477
2. Set of controls 3					
Weight	-.353**	-.324*	-.370**	-.295	-.358*
	(.175)	(.174)	(.176)	(.190)	(.192)
Height	-.356	-.379	-.522	-.428	-.614
	(.588)	(.585)	(.587)	(.650)	(.653)
Obs.	639	613	588	502	477

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The results are derived using the set of controls 2 (first panel) and 3 (second panel). B_1 corresponds to my benchmark sample. B_2 restricts my sample to mothers who experienced at least one shock between 2003 and 2008. B_3 includes mothers who have experienced at least one shock but less than 5 and B_4 restricts my analysis to mothers who experienced at least 2 shocks. B_5 includes only mothers who reported between 2 and 4 negative shocks between 2003 and 2008.

Appendix K

Table K.1: Probabilities of experiencing negative economic shocks, allowing for misreporting

Estimates of the parameter p, $\hat{p}$		Rate of false positive 0%	1%	2%	5%	10%
Rate of false negative	0%	0.2929***	0.2629***	0.2545***	0.2333***	0.1964***
		(0.0163)	(0.0181)	(0.0183)	(0.0185)	(0.0186)
	2%	0.3339***	0.2840***	0.2710***	0.2445***	0.2043***
		(0.0196)	(0.0181)	(0.0182)	(0.0185)	(0.0190)
	5%	0.3430***	0.3013***	0.2873***	0.2580***	0.2145***
		(0.0202)	(0.0188)	(0.0188)	(0.0191)	(0.0196)
	10%	0.3569***	0.3237***	0.3097***	0.2787***	0.2314***
		(0.0209)	(0.0197)	(0.0198)	(0.0201)	(0.0209)
	20%	0.3879***	0.3653***	0.3524***	0.3204***	0.2683***
		(0.0221)	(0.0216)	(0.0219)	(0.0226)	(0.0239)

Note: Estimated probabilities of experiencing a negative economic shock, allowing for different rates of false positive reports (Columns) and false negative reports (Rows). These probabilities are estimated with maximum likelihood using set of controls 1. The sample is based on 775 observations.

Table K.2: Effects of S_i' on weight, including set of controls 3

Effects of economic shocks on weight		Rate of false positive 0%	1%	2%	5%	10%
Rate of false negative	0%	-0.3703**	-0.4788***	-0.4350**	-0.4278**	-0.4435**
		(0.1771)	(0.1790)	(0.1817)	(0.1877)	(0.1904)
	2%	-0.4111**	-0.4255**	-0.4374**	-0.4920**	-0.1200
		(0.1665)	(0.1768)	(0.1789)	(0.1812)	(0.2228)
	5%	-0.3879**	-0.2557	-0.4266**	-0.5297***	-0.0848
		(0.1676)	(0.1932)	(0.1755)	(0.1794)	(0.2137)
	10%	-0.2871*	-0.2138	-0.2175	-0.3104	-0.0471
		(0.1725)	(0.1874)	(0.1886)	(0.1905)	(0.2081)
	20%	-0.1467	0.0965	-0.0112	-0.0779	-0.2032
		(0.1766)	(0.1854)	(0.1876)	(0.1899)	(0.1994)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Estimated effect of S' on weight, allowing for different rates of false positive reports (Columns) and false negative reports (Rows), using set of controls 3. The sample is based on 629 observations and the estimations include set of controls 3.

Table K.3: Effects of S_i' on height, including set of controls 3

Effects of economic shocks on height		Rate of false positive				
		0%	1%	2%	5%	10%
Rate of false negative	0%	-0.5222	-0.3135	-0.3625	-0.1762	-0.4306
		(0.5404)	(0.5458)	(0.5469)	(0.5590)	(0.5769)
	2%	-0.4683	-0.2479	-0.3302	0.0995	0.1228
		(0.5809)	(0.5868)	(0.5916)	(0.5906)	(0.6461)
	5%	-0.5093	-0.1431	-0.3413	-0.2778	0.3027
		(0.5675)	(0.5628)	(0.5745)	(0.5777)	(0.6264)
	10%	-0.3980	-0.1056	0.0220	-0.3276	0.2446
		(0.5566)	(0.5458)	(0.5510)	(0.5614)	(0.6028)
	20%	-0.3061	-0.2847	-0.4790	-0.1342	0.2437
		(0.5362)	(0.5058)	(0.5132)	(0.5366)	(0.5732)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Estimated effect of S' on height, allowing for different rates of false positive reports (Columns) and false negative reports (Rows), using set of controls 3. The sample is based on 629 observations and the estimations include set of controls 3.