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 causal 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, infant health, Malawi, Sub-Saharan Africa*
JEL: I10, J13, C21

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1 Introduction

There has been a strong and long-standing interest in economics in the production function of infant health, as it has been shown to be critical for the development of health and human capital more broadly throughout the entire life-cycle (Grossman, 1972; Heckman, 2006; Rosenzweig and Schultz, 1982). Infant health is a strong predictor of adult health and has long-lasting consequences on human capital and labor market outcomes (Aizer and Currie, 2014; Almond *et al.*, 2017; Barker, 1990; Case and Paxson, 2008a,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)[1]. As a result, understanding the determinants of infant health is crucial for any theories and policies addressing the development of human capital throughout the life course.

Specifically, researchers have been interested in infant health at the very beginning of life, arguing that health characteristics in the first two years of life are particularly crucial and sometimes difficult to compensate in the healthy development of a person, a concept that is often called critical-period programming (Maccini and Yang, 2009). A substantial body of research has therefore focused on the determinants of health early in life, with special interest in the effects of maternal inputs on infant health characteristics (Corman *et al.*, 2017; Frankenberg and Thomas, 2017).

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).

The key challenge in uncovering the causal effects of maternal inputs on infant health is to find exogenous variations in maternal inputs. To tackle this challenge, researchers have used exposures to stress linked to reforms in social welfare programs (Currie and Gruber, 1996a,b; Gray, 2001; Guldi *et al.*, 2018; Sonchak, 2015), plant lay off (Carlson, 2015), pollution (Agarwal *et al.*, 2010; Chay and Greenstone, 2003; Coneus and Spiess, 2012; Currie and Neidell, 2005; Currie and Walker, 2011; Currie *et al.*, 2009, 2011, 2013; Luechinger, 2014), famine (Painter *et al.*, 2008; Roseboom *et al.*, 2006; Wang *et al.*, 2017) and weather events (Currie and Rossin-Slater, 2013; Deschenes *et al.*, 2009; Maccini and Yang, 2009; Simeonova, 2011) as sources of exogenous variation in maternal inputs to determine their effects on infant health. The choice of these events is generally motivated by their

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

arguable exogeneity and policy-relevance.

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; 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 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

[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.

(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 causal 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 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. The country has experienced severe economic shocks over the past decades, most of them being climate-related external shocks due to drought and floods, leaving the country in a fragile and vulnerable state (Devereux, 1999; International Monetary Fund, 2017). 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).

[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.

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 proxies for informal safety net as measured by social participation and informal financial transfers. 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.

In addition to using economic shocks that are relatively common in Sub-Saharan Africa, the main contribution of this study is that it allows me to identify the kind of economic shocks that mothers experience during the year they have given birth. That is, the data allow me to identify the roots of these economic shocks, be they the death of a relative, poor crop yields or big changes in the price of grain, among others. Knowing the roots and possible consequences of these economic shocks is particularly important from a public policy perspective, as this information would allow policy makers to develop strategies that can help households better cope with the long-lasting effects that these specific economic shocks might have (Maccini and Yang, 2009).

My second contribution is that, unlike many previous studies that use national registries or medical records from hospitals (Bozzoli and Quintana-Domeque, 2014; Burlando, 2014; Carlson, 2015), I am using comparatively rich survey data to assess the effects that mothers' experience of negative economic shocks during pregnancy or shortly after childbirth has on child health. National registry data and medical records often lack detailed control variables that could confound the effects of economic shocks, biasing the causal estimates of the intended identification strategies. By using micro-level data, my study allows me to control for a wide range of factors that could confound my estimates. In addition, the rich survey data I use in this study allow me to contextualize my findings and explore the effects of coping strategies that mothers put in place to mitigate the deleterious impacts that negative economic shocks have on their children health.

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 nine 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"[6].

[6]There are several reasons that explain the difference in size between the "subjective health sample" and the "anthropometric sample". The main one is that anthropometric measurements of children could be collected only if children were physically present during the interview. In addition, the subjective health measures were collected during the main survey whereas weight and height were collected during a follow-up visit in which respondents were tested for HIV. That follow-up visit took place on average one week after the main survey. The difference in size can thus also be due to the fact that respondents who did not want to be tested for HIV avoided the follow-up visit. Respondents who were unavailable, tired of being asked questions or who moved during the period between the main survey and the follow-up visit did not participate in the follow-up survey either. To the extent that participation in the follow-up visit is independent to the occurrence of economic shocks and children health, the results below will not

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[7].

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. 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. 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

be biased. Note in addition that respondents who did not participate in the follow-up visit have an average number of reported shocks that is not statistically different from those who did participate in the follow-up visit.

[7] I decide to recode Likert-scale items into binary variables because my measures of subjective health are highly skewed, to the extent that there are very few children in poor subjective health (as reported by their mother). While such dichotomization involves some loss of information, the fact that I derive two different binary variables for each Likert-scale item allows me to explore, to some extent, the variation in subjective health among children not located at the extreme scale points.

Table 1: Descriptive statistics of the subjective health sample

	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)
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							

Note: Sample derived from the MLSFH data wave 5.

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.

Out of these six binary outcome variables, the ones derived from the question that asks mothers to rate the health of their child in general are perhaps the most unreliable as they are the most prone to reporting bias. On the contrary, the outcome variables derived from the questions about whether their child has been ill and about how the health of their child compares to other children are less prone to reporting errors because they are derived from scales that are relatively more objective.

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.

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)[8] in centimeters (cm).

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

[8] 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.

Table 2: Descriptive statistics of the anthropometric sample

	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)
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.

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 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.

Table 2 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 Table 2, 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 Tables 1 and 2) 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.

Moreover, assuming weight and height are normally distributed[9], I also estimate the effects of

[9]As robustness checks, I compute the z-scores of weight-for-age and height-for-age without assuming that the underlying distribution of weight and height is normal. To do so, I applied the so-called LMS formula (Cole and

negative economic shocks on weight-for-age and height-for-age z-scores, as defined as:

$$z_i = \frac{m_i - M_{s,a}}{sd_{s,a}} \tag{2}$$

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).

These z-scores also allow me to understand where the negative effects of economic shocks in the weight and height distributions take place. To do so, 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 (Black *et al.*, 2013; 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).

To illustrate how anthropometric measures of children in my Malawi sample compare to the WHO Child growth standards, I compute the weight-for-age and height-for-age z-scores of children in my sample using the WHO child growth table as reference groups (De Onis *et al.*, 1997). Figure 1 plots weight-for-age and height-for-age z-scores of children in my sample using sex- and age-specific WHO weight and height standards. The average z-score for weight in my sample is equal to -.65

Green, 1992) to my data:

$$z_i^n = \frac{\left(\frac{m_i}{M_{s,a}}\right)^{L_{s,a}} - 1}{S_{s,a} L_{s,a}} \tag{1}$$

Here, $L_{s,a}$ is the Box-Cox power parameter derived from i's reference group based on sex s and age a. $S_{s,a}$ is the coefficient of variation of the same reference group, defined as the ratio of the mean over the standard deviation and $M_{s,a}$ is the median.

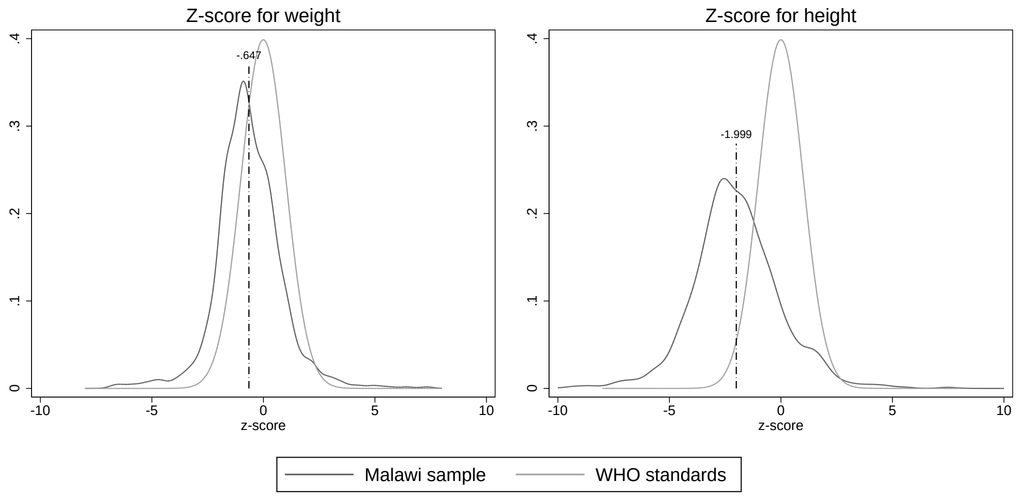

Figure 1: Weight-for-age (left) and height-for-age (right) z-scores of the children in my sample with respect to WHO anthropometric standards.

and the one for height is equal to -1.99 relative to WHO standards. The large shifts to the left of the weight-for-age and height-for-age z-scores displayed in Figure 1 suggest that all children in my sample, and not only those below a particular cutoff, are affected by poor child development.

Assuming the WHO standards to be an acceptable set of standard values[10], about 51% of the children in my sample suffer from stunting[11], indicating the severe conditions in which they live and demonstrating the seriousness of stunting as a major global health priority (De Onis and Branca, 2016).

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[12].

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

[10]Whether WHO Growth Standard Tables are to be used as reference has drawn a lot of attention and is subject to debate because of the possible growth potential differences of children of different ethnic background (De Onis and Branca, 2016). As robustness checks, I assess whether economic shocks have an effect on weight-for-age and height-for-age z-scores and the various cutoffs using the WHO standards as reference groups as well.

[11]This percentage is close to the one reported for Malawi in the WHO Global Database on Child Growth and Malnutrition, which indicate a percentage of 48.3 (De Onis *et al.*, 1997).

[12]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 3: Descriptive statistics of the 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
Average number of shock at birth	.36		.41	

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 shock in my Subjective health sample and the last two represent the same statistics for my Anthropometric sample.

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 any shock respondents could specify. Moreover, for the three most important shocks that they have experienced over the past five years, respondents are asked whether the shocks generated income and/or asset losses, when 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". It is worth noting that none of these shocks are associated with pregnancies or linked to any of the child that mothers were about to give birth to or just gave birth to when the shocks occurred.

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.

Table 3 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. Finally, the last row in Table 3 shows the average number of shocks experienced in utero or during the year of birth by children in my sample. The fact that these averages are higher than the percentages of children who experienced economic shocks indicates that some of the children have experienced more than one shock during their year of birth.

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 causal 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). 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)[13]. 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),

[13]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.

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. 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). 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.

The second half of Tables 1 and 2 shows that about half of the children in my subjective and objective samples are girls and the average age of the children at the interview is about 2.5 years old. As shown in these two tables, the average age of the mothers at child birth in the two samples is identical 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[14]. More specifically, I estimate the following simple model:

$$H_i = \alpha_0 + \alpha_1 S_i^* + X_i' \alpha_2 + \nu_i \tag{3}$$

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[15] . Set 3, in addition to the controls in set 2, includes variables

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

[15]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

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 1784 (789) children from 1153 (589) different mothers[16]. In all the results below, standard errors are clustered at the mother level.

4 Results

4.1 Subjective outcomes

4.1.1 Main results

Table 4 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

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.

[16] 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 4: Effects of negative economic shocks at birth on subjective health outcomes - linear probability model

Probability of being:	(1)	(2)	(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. Column 1 controls for age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Column 2 adds the marital status of the mother, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Column 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. Columns 4 and 5 include the same controls as Column 3. Estimates of the other coefficients are available upon request.

(Kraemer, 2000). Corresponding results using Logit and Probit models instead of linear probability models are presented in Tables 23 and 24 of Appendix A. The marginal effects presented in these two tables are very close to those obtained in Table 4.

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, which may not be completely accurate in reflecting the true health status of these children. Table 1 shows that 65% of the mothers in my sample consider their child to be in better health than children with the same characteristics in the same village. This percentage seems relatively high and may reflect some reporting bias. I now aim to assess the robustness of these findings by exploiting the fact that some mothers have several children in my sample. This allows me to run fixed-effect models to control for unobserved mother characteristics that are constant across births such as potential time-invariant reporting effects.

4.1.2 Fixed-effect analysis

Exploiting my relatively large subjective measure sample and the fact that I observe several children per mother in some cases, I can add mother fixed effect in the analysis to capture unobserved attributes of mothers and/or households that could be correlated with both child health and economic shocks. Analytically, the econometric specification takes the following form:

$$H_{im} = \alpha_1 S^*_{im} + X'_{im}\alpha_2 + g_m + \mu_{im} \tag{4}$$

where H_{im} is a subjective health measure of child i from mother m in 2008, S^*_{im} 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, X'_{im} is a set of control variables and g_m the unobserved mother or household effect that is constant over children within the same family.

This analysis restricts my sample to mothers who had at least two children during the period 2003-2008 and who reported different shock statuses for them[17]. Table 5 reports the estimates of my model adding mother fixed effects. As it was the case in Table 4, experience of negative economic shock at birth affects the subjective health of the children in my sample: those who experienced a shock during the year of their birth were about 10% points less likely to be in excellent health and 7% points less likely to be in much better health than children of the same sex and age in the same village. These results are very similar to those obtained in Table 4.

Appendix B presents the fixed-effect logit analog to the previous linear fixed-effect analysis. As explained in the appendix, the logit specification makes it possible to explicitly introduce reporting heterogeneity in the econometric model by allowing mothers to have different response behaviors when evaluating the health of their children. Results in Table 25 of Appendix B are very consistent to the ones obtained in the linear fixed-effect analysis.

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

[17]Note that this raises some issues of selection in case households with one child were different from those with several of them. That being said, the severity of this issue should be relatively low given the high fertility of the women in my sample, and in Malawi in general: the average number of children per mother in my sample was about 3.8 in 2008.

Table 5: Fixed-effect estimates on subjective health measures

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 effect. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively. Estimates of the other coefficients are available upon request.

4.2 Objective health measures

4.2.1 Main results

Table 6 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.*,

Table 6: Effects of negative economic shocks at birth on weight

Weight	(1)	(2)	(3)	Boy (4)	Girl (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)
Obs.	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. Column 1 controls for age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Column 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Column 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. Columns 4 and 5 include the same controls as Column 3. The reference category is a boy of age 0 from the central region of Malawi who did not experience any economic shock at birth.

1997).

Table 7 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. My measure of household wealth does not seem to have any effect on the weight and height of the children in my sample. Table 26 in Appendix C shows similar estimations, but using measures of household wealth defined differently. 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 26 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.

It is interesting to see that the level of education of the mother does not seem to have any statistically significant effect on the weight and height of the children. This could potentially mean that education in my setting is not a good proxy for parental characteristics or that the effect is captured by other variables included in the model. As another proxy for unobserved family background characteristics, Thomas and Strauss (1992) and Thomas *et al.* (1990) include mother's height in their estimations, arguing that, in addition to capturing genetic differences, it can be a proxy for human capital and investment in health and may also serve as a measure of family background. Including mother's height in my specification as control variable, although it positively predicts child anthropometric characteristics, does not affect the impact of my negative economic shock variable on weight and height[18].

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

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 estimate the effects of economic shocks on the probability of being d 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 ($d = 0$).

The first panel of Table 8 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

[18] Table 27 in Appendix D presents the results of these regressions.

Table 7: 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. Column 1 controls for age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Column 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Column 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. Columns 4 and 5 include the same controls as Column 3. The reference category is a boy of age 0 from the central region of Malawi who did not experience any economic shock at birth.

deviations lower than others and this effect is statistically significant at the 10%-level[19]. 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 than the reference group. These effect 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-

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

Table 8: 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. Set of controls 1 consists of age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Set of controls 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Set of controls 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household.

age z-score distribution. The effect appears to be quite homogeneous along the weight distribution, although the effects appear stronger at the median and are statistically significant only at both ends of the distribution and at the median.

The second panel of Table 8 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 however not precisely estimated. Table 28 in Appendix E presents the same sets of estimates but without assuming that weight and height are normally distributed. Using the LMS formula, I show that the results are rather similar and in the ballpark of those estimated assuming normal distributions.

So far, the results on z-scores were derived using my MLSFH sample as reference groups. One could also use the WHO standards as reference group instead and determine the effects of economic shocks on weight and height relative to median WHO standard values. Table 29 in Appendix F presents these results. As before, I observe a shift to the left in the weight-for-age and height-for-age z-scores of about .26 and .15 standard deviations, respectively. The effects appear to be stronger than the ones in which the MLSFH sample is used as reference group, but are in general similar.

Another anthropometric measure that is frequently used in the literature is weight-for-height (length) z-score (De Onis *et al.*, 1997; Onis, 2000; Thomas *et al.*, 1990; WHO, 1995). This measure has the advantage of being independent of age, which is in some contexts hard to assess with certainty, and is used as an indicator of wasting and thinness due to severe starvation and/or illness (WHO, 1995). Table 30 in Appendix G shows the results of the effects of economic shocks on weight-for-height z-score using WHO as standard values. I again observe a shift to the left due to economic shocks at birth and the effects seem to be particularly strong for z-scores below -1[20].

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. Children affected by economic shocks have weight-for-age and height-for-age z-scores that are shifted to the left by about 0.2 standard deviations. These effects appear to be quite homogenous along the weight and height distributions. I now further investigate the robustness of these findings and contextualize them.

4.2.3 Social participation and 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 and Nyshadham, 2016; Adhvaryu *et al.*, 2018; 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)[21]. Because 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)[22]. Social participation and social network of individuals could therefore be a coping mechanism for individuals who experience negative economic shocks. In my context, mothers who experience a negative economic shock during pregnancy or shortly after giving birth could seek out

[20] I unfortunately cannot assess whether these results hold using my MLSFH sample as standard values because of the size of my sample. Indeed, this would require many observations for every centimeter and for both boys and girls, which is the case only for few centimeters.

[21] 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).

[22] Such horizontal redistributive practices are widespread in rural Malawi. Devereux (1999) show that transfer can contribute to as much as 14 percentage of total income in household. He 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.

for help among persons in her village or community[23]. To investigate whether social participation and transfers can attenuate the effects of negative economic shocks on child health, I include in my estimation a set of control variables that proxy for informal safety nets. Specifically, in addition to the usual set of control variables, I add to my estimation a variable that represents the number of village committees the mother is a member of at the time of the interview as well as a variable that counts the number of social activities the mother has participated in during the month preceding the interview. In addition to these two variables that proxy for social participation and social network size, I include in my analysis a variable that represents the number of people the mother can seek out for help in case of crises and another that counts the number of people the mother has received financial and in-kind transfers from over the last two years. Under the assumption that negative economic shocks have increased the likelihood of mothers to seek out for help in their community, as Table 31 in Appendix H suggests, including such proxies for social participation and transfers in my econometric model would therefore attenuate the direct effects of economic shocks on child health.

Table 9 shows the results of these regressions. The effects of negative economic shocks on my two objective health measures –weight and height– are very similar to the ones I obtained in my benchmark results, which suggests that social participation and informal transfers do not attenuate the damaging effects of economic shocks on child health. My results also highlight that very few of my proxies for informal safety nets have an effect on weight and height.

The MLSFH also has a detailed module on public welfare programs that households in my sample have benefited from. Respondents are asked whether any members of their household has received help from various programs such as free food distribution, food- or cash-for work programs, or any financial or non-financial support from the governments, among others. I show in Table 32 of Appendix I that controlling for the number of programs the household has benefited from over the past three years, whether it has received agricultural input in the form of a coupon/voucher for seed or fertilizer and the total estimated value of the aid received by the household leaves the effects of economic shocks at birth on weight and height unchanged.

However, it is possible that social participation and transfers moderate the relationship between negative economic shocks and child health in the sense that they affect the direction and the strength of that relation. To investigate whether that is the case, I categorize mothers whose social partic-

[23]The experience of economic shocks at birth seems indeed to increase social participation and transfers in my sample. Table 31 in Appendix H shows that experiencing economic shocks at childbirth increases the number of village committees the mother is part of and the number of people the mother has received financial and in-kind transfers from over the two years preceding the interview.

Table 9: Effects of negative economic shocks on objective measures of health controlling for transfers and social participation

	(1)	(2)	(3)
1. Weight			
Shock at birth	-.309*	-.312*	-.337*
	(.165)	(.176)	(.177)
Number of village committees	-.092	-.136*	-.137*
	(.070)	(.080)	(.079)
Number of social activities	-.010	-.004	-.004
	(.008)	(.008)	(.008)
Potential help (number of person)	.025	-.014	-.004
	(.050)	(.057)	(.057)
Help received (number of person)	-.036	.016	.011
	(.046)	(.054)	(.053)
2. Height			
Shock at birth	-.671	-.308	-.301
	(.538)	(.589)	(.597)
Number of village committees	.398	.097	.089
	(.269)	(.245)	(.244)
Number of social activities	-.049**	-.033	-.033
	(.0234)	(.025)	(.025)
Potential help (number of person)	.046	-.017	-.018
	(.129)	(.142)	(.144)
Help received (number of person)	-.073	-.070	-.070
	(.115)	(.125)	(.125)
Obs.	788	633	633

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In addition to my three usual sets of control variables (Columns 1, 2 and 3), all regressions also include the number of village committees the respondent is a member of, the number of social activities the mother has been involved in over the last month, the number of people the respondent can seek help from in case of crises and the number of people the respondent has received financial and non-financial transfers over the last two years.

ipation and transfers 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. The first four Columns of Table 10 show that mothers' participation in village committees and social activities appears to exacerbate the negative effects of economic shocks on weight (panel 1) whereas it seems to have protective effects on height (panel 2). Similar results hold when looking at mothers who were categorized by whether the number of people they can seek out for help in case of crises was below (Column 5) or above (Column 6) the median value. The fact that social participation exacerbates the damaging effects of negative economic shocks on weight is surprising. One possible explanation could be that group membership in village committees and social activities incur short-term or periodic costs and mothers have to momentarily adjust their children's nutritional intake in order to be able to afford these activities.

Table 10: Effects of negative economic shocks on objective measures of health, splitting the sample by whether the mothers engage in social activities and transfers or not

	Number of village committees		Number of social activities		Potential help		Help received	
	Below	Above	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Weight								
Set of controls 1	-0.140	-0.539**	-0.061	-0.579**	-0.380	-0.307	-0.569**	-0.167
	(0.215)	(0.248)	(0.236)	(0.231)	(0.254)	(0.210)	(0.254)	(0.219)
Set of controls 2	-0.080	-0.512**	-0.261	-0.524**	-0.145	-0.458**	-0.585**	-0.197
	(0.238)	(0.260)	(0.262)	(0.242)	(0.280)	(0.223)	(0.286)	(0.221)
Set of controls 3	-0.083	-0.549**	-0.304	-0.542**	-0.111	-0.528**	-0.600**	-0.216
	(0.243)	(0.257)	(0.267)	(0.247)	(0.291)	(0.223)	(0.294)	(0.219)
2. Height								
Set of controls 1	-1.444**	0.117	-0.804	-0.616	-0.959	-0.638	-1.628**	-0.040
	(0.731)	(0.815)	(0.767)	(0.750)	(0.782)	(0.760)	(0.796)	(0.740)
Set of controls 2	-0.888	0.134	-0.974	-0.098	-0.467	-0.336	-1.851**	0.642
	(0.759)	(0.968)	(0.851)	(0.816)	(0.893)	(0.800)	(0.894)	(0.789)
Set of controls 3	-0.837	0.110	-0.933	-0.051	-0.400	-0.379	-1.978**	0.692
	(0.762)	(0.975)	(0.863)	(0.813)	(0.911)	(0.805)	(0.888)	(0.782)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The first and second panel show the effects of economic shocks at birth on weight and height, respectively. I split my sample by whether respondents are members of fewer (Column 1) or more (Column 2) village committees than the median value, by whether respondents engage in fewer (Column 3) or more (Column 4) social activities than the median value, by whether respondents can rely on fewer (Column 5) or more (Column 6) persons in case of crises than the median value and by whether respondents have received help from fewer (Column 7) or more (Column 8) persons than the median value in my sample.

The last two Columns of Table 10 show that mothers who received financial and in-kind transfers from their extensive 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[24].

4.2.4 Differences in negative asset and income shocks

The economic shock module in the MLSFH questionnaire allows me to categorize the negative economic shocks as asset shocks, income shocks or both. On the one hand, one can hypothesize that

[24] Using the same proxies for social participation and informal safety net but measured in 2006 instead of 2008 to partially deal with potential endogeneity issues shows similar patterns. More specifically, having an extensive network from which mothers have received help buffers the negative effects of economic shocks whereas social activities appears to exacerbate the effects of these shocks on both weight and height (see Table 33 in Appendix I). Note that the questions related to the number of village committees the respondents were members of at the time of the interview were not asked in 2006.

Table 11: Effects of negative income and asset shocks at birth on objective health measures

		(1)	(2)	(3)
1. Effects of income shocks	Weight	-.269*	-.224	-.245
		(.158)	(.167)	(.169)
	Height	-.666	-.256	-.252
		(.532)	(.578)	(.583)
2. Effects of asset shocks	Weight	-.145	-.030	-.051
		(.228)	(.232)	(.230)
	Height	-.668	-.800	-.817
		(.696)	(.747)	(.747)

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 effect of negative income shocks on weight and height while the second panel shows the effect of negative asset shocks on the same outcomes.

asset shocks experienced around birth might have more long-lasting damaging effects on health, and therefore on height, than income shocks. Income shocks, on the other hand, could be more transitory and could thus have more important effects on weight. The first panel of Table 11 shows the effects of experiencing a negative income shock around birth on weight and height whereas the second panel shows the results of the effects of negative asset shocks. While height seems to be more affected by asset than income shocks, and weight the other way around, as hypothesized, the coefficients are not precisely estimated and it is therefore difficult to draw any firm conclusions from these results.

4.2.5 Importance of economic shocks during the year of birth

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.

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

Table 12: 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. Note also that the number of observations is smaller because children born in 2003 and 2008 are not included in the analysis.

between 2004 and 2007, leading to a smaller sample[25].

Table 12 shows the results of my estimations. Results for weight 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.

Overall, the fact that the coefficients of interest remain largely unchanged after the inclusion of shocks occurring in years adjacent to the year of birth shows that it is indeed economic shocks at birth that impact child objective health measures.

[25] To facilitate comparisons, I provide the benchmark results using that restricted sample in Appendix J Table 34.

4.2.6 Effects of negative economic shocks on mortality

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 13 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)[26]. My results therefore seem not to be biased due to mortality selection[27,28].

The dataset, unfortunately, does not allow me to directly test whether economic shocks have any effect on miscarriage. Following Currie *et al.* (2018), I can however perform an indirect test and use the sex of the children at birth as a signal of changes to miscarriage rates, since male fetuses have a higher risk of miscarriage (Halla and Zweimüller, 2014; Sanders and Stoecker, 2015). As reported in Appendix L Table 37, I do not find any significant effect (even at 10%) of my negative economic shock dummy on the probability of being a female in both subjective health and anthropometric samples. This indicates that differential selection into birth because of miscarriages is also unlikely to bias my results.

The results above represent the causal 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.

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

[27]Corresponding results using Logit and Probit models instead of linear probability models are presented in Tables 35 and 36 of Appendix K, respectively. The marginal effects presented in these two tables are very close to those obtained in Table 13.

[28]The under-5 mortality rate (U5MR) prevailing in Malawi in 2008 is equal to 9.8% (The World Bank, 2008). 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 13 are biased towards 0.

Table 13: 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%). 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 3. Regressions are conducted separately for each type of shocks (rows).

5 The effects of community level shocks

5.1 Main results

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. One can for instance think of risky behaviors in terms of investments or consumption that are likely to have both an effect on child health and on the probability of experiencing some of the economic shocks listed in Table 3. 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"[29] and "big change in price of grain"[30] are plausibly more exogenous. These shocks, sometimes called

[29] 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.

[30] 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

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 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 14 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[31]. 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

important staple crop in Malawi, results from environmental factors, unpredictable domestic market interventions and export policies (Giertz *et al.*, 2015).

[31] Corresponding analyses using the two other sets of control variables are included in Appendix M Tables 38 and 39.

Table 14: 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 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.

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 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.

5.2 Objective measures of shocks

As detailed in Table 3, the two most prevalent shocks that mothers in my sample are confronted with are those associated with "poor crop yields or loss due to disease/pests" and "big changes in price of grain". The availability of external data on rainfall and grain market prices allows me to assess to what extent shocks derived from objective measures affect child health in my sample. I start my analysis with rainfall data.

5.2.1 Rainfall evidence

Individuals in my sample live in rural Malawi and are vulnerable to weather shocks because they largely rely on rain-fed agriculture with no extensive irrigation systems (Chin, 2010). Rainfall failure in the year of birth could therefore potentially have damaging effects on child health through its impact on foodcrop cultivation and alternative income generating activities such as cash cropping, livestock rearing and agricultural labor (Chin, 2010; Devereux, 1999).

The dataset I use in this section is derived from the Terrestrial Hydrology Research Group that makes meteorological datasets available online[32]: it is a hybrid of reanalysis, which relies on model-based predictions to estimate weather conditions, and real observations that span over the period 1948-2010 and cover the entire planet, with gridded daily data points for every 0.25 by 0.25 degrees (about 27 by 27km).

Figure 2 plots the cumulated precipitation in millimeter (mm)[33] from 1998 to 2008 for the three different regions of Malawi (Central, South and North) where the MLSFH data are collected. One can see the regular patterns of precipitation of the rainy season, extending from November to March, and the dry season, from April to October. Although some variations exist across the regions both in terms of the amount of precipitation and timing, I unfortunately cannot use this variation in rainfall due to insufficient variation in rainfall within the regions. Figure 3 plots the amount of precipitation in Malawi in January 2003 along with the GPS coordinates of the mothers in my sample (small black dots). Each side of the little squares on the map represents a distance of about 27km. As is clearly visible, most mothers in a given region are located in the same square and would thus have the same amount of precipitation for a given period of time. I therefore lack within-region variation in precipitation, which means that any effect of rainfall shocks based on my data would be

[32] The dataset can be downloaded from their webpage: http://hydrology.princeton.edu/home.php. See Sheffield *et al.* (2006) for more details on how the data is constructed, the bias correction and downscaling methodology. The data is considered as cutting-edge by experts in the field (Hsiang and Kopp, 2018).

[33] The unit of measurement in the original data is in $\frac{kg}{m^2 \times s}$ where kg stands for kilogram, m for meter and s for second. In order to translate that unit into mm/day, I multiply the value in the original dataset by 86400, given that there are 86400 seconds in a day and that $1kg \approx 1lt$ which, spread over $1m^2$, is 1mm deep.

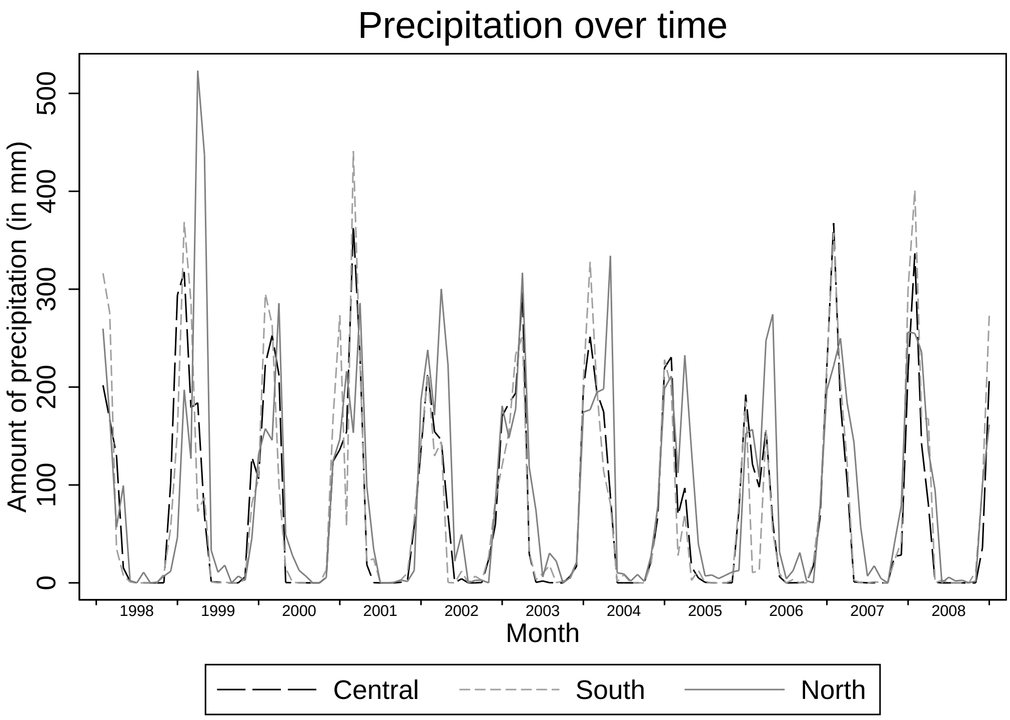

Figure 2: Amount of precipitation (in mm) in the three regions in Malawi where the MLSFH data were collected (Central= Mchinji, South= Balaka, North= Rumphi), between 1998 and 2008. The data comes from the Terrestrial Hydrology Research Group at Princeton University (Sheffield *et al.*, 2006).

confounded by region-specific events that occur in a given year. I would hence be unable to identify the direct effects of rain shocks on child health[34]. I therefore interact my measure of rain shocks, as defined below, with household characteristics, to derive rainfall shocks that vary within region.

Following Chin (2010), my rain shock variable is based on the variability in precipitation captured by the standard deviation of monthly rainfall in a specific year, where I define a year as a complete wet and dry season, going from June to May, rather than a calendar year[35]. More specifically, the shock variable is a dummy variable that takes the value 1 if σ_y, the standard deviation in the monthly precipitation in year y, is above the average annual standard deviation of monthly rainfall

[34]Note that even if mothers were located in different but adjacent squares, it is likely that I would still be lacking within-region variation as what I need is within-region variation in my rainfall shocks, as defined below, and not variation in precipitation per se.

[35]Adhvaryu *et al.* (2018) define rain shocks as level of annual precipitation that is one standard deviation below or above the annual mean in the same region computed over a period of ten years. Defining rain shocks that way does not lead to enough variation in the shock variable over the period 2003-2008. In fact, it would generate variation only in Mchinji and Rumphi for years 2005 and 2006.

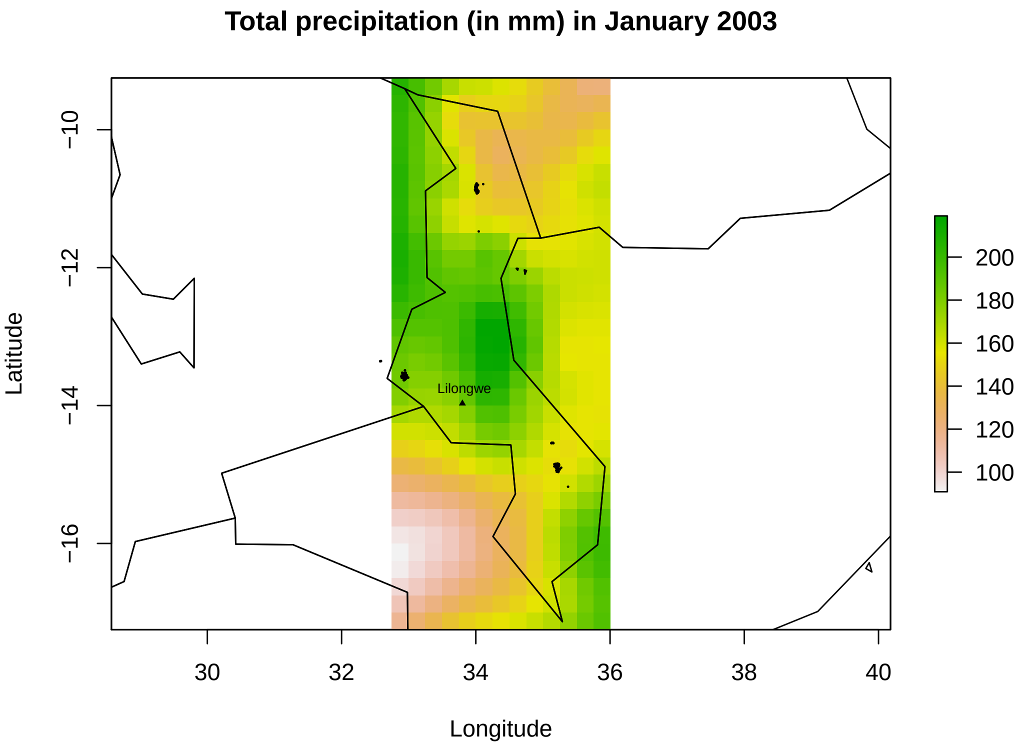

Figure 3: Amount of precipitation (in mm) in Malawi during January 2003. The data comes from the Terrestrial Hydrology Research Group at Princeton University (Sheffield *et al.*, 2006). The black dots represent the GPS coordinates of the MLSFH respondents in my sample. Lilongwe is the capital of Malawi

over the period 1998-2008, which I define as $\overline{\sigma}$:

$$Rain\ shock_y = \begin{cases} 1 & \text{if} \quad \sigma_y > \overline{\sigma} \\ 0 & \text{otherwise} \end{cases} \tag{5}$$

This measure accounts for within-year unpredictability in rainfall and can capture both droughts and floods[36].

As in Chin (2010), I interact this rain shock variable with land ownership (in tertiles) that the households possess in 2008. The interaction of these two variables is a plausible determinant of income and economic activity and its variability can proxy for the presence or absence of economic shock.

Table 15 presents the results of the regressions of weight (Columns 1-3) and height (Columns 4-6) on my usual sets of control variables, adding land, rain shock and their interaction into my econometric specification. While not precisely estimated, the direction of the effects of land owner-

[36]This is admittedly a mild definition of a rainfall shock but it allows to capture enough variation across regions.

Table 15: Effects of rain shocks on objective measures of health, interacted with land ownership

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Land = 1	0.424*	0.341	0.417	0.202	-0.265	-0.208
	(0.255)	(0.304)	(0.313)	(0.761)	(0.809)	(0.827)
Land = 2	0.081	0.239	0.270	1.600**	1.387*	1.415*
	(0.221)	(0.248)	(0.254)	(0.682)	(0.714)	(0.737)
Rain shock	-0.135	-0.066	-0.034	-0.167	0.061	0.065
	(0.268)	(0.315)	(0.314)	(0.808)	(0.938)	(0.944)
Land=1 ×Rain shock	-0.211	-0.093	-0.132	-1.135	0.089	0.087
	(0.384)	(0.444)	(0.436)	(1.074)	(1.218)	(1.224)
Land=2 ×Rain shock	-0.696**	-0.867**	-0.862**	-1.827*	-1.007	-0.972
	(0.336)	(0.373)	(0.374)	(1.017)	(1.101)	(1.110)
Obs.	788	639	639	788	639	639

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. The first 3 Columns are the results for weight and the last three are the results for height. Rain shock is a dummy variable that takes the value 1 if the variability in precipitation that respondents experienced in a given year is higher than the average annual variability in rainfall over the period 1998-2008. Land is measured in m^2 and is split in tertiles (0, 1 and 2). The reference category is a child who did not experience a rainfall shock at birth and has grown up in a household that owns little land (Land=0).

ship and rain shock is as one would expect: growing up in a household that owns more land leads to an increase in weight and height while experiencing a rain shock the year of birth results in a slight decrease in weight and height. The interaction terms however show that the more land a household owns, the more damaging the negative effects of rainfall shocks are. Children born in households in the top land tertile and who experienced a rain shock during the year of birth are about 800 grams lighter than children who experienced the same shock at birth but whose households were in the lowest land tertile. Although not statistically significant, my analysis suggests that the effects on height can also be important and could explain up to 1.8 centimeters in the height difference between children who did not experience rain shock at birth and whose households own little land, as compared to those who experienced such a shock and whose households own a large amount of land.

5.2.2 Change in prices of grain

In addition to the objective measure of precipitation, I also have access to monthly data on price of corn (maize) grain found in the main local market of each of the three regions over the period 1998-2008. Changes in the price of corn grain can affect child health in various ways. Perhaps the most obvious channel through which price variations affect child health is through the fact that consumption and income might be harder to smooth over time, resulting in possible periods of undernutrition for pregnant women and recently born children.

Price shocks cannot be determined based on averages or standard deviations over a given period due to trends and inflation. It is therefore important to separate trends from various shocks that can occur during the period under consideration. To do so, I apply region-specific Hodrick-Prescott (H-P) filter (Hodrick and Prescott, 1997) to my price data to separate the trends from the business cycle components of my times series (Arby, 2001; Chowdhury, 2004; Ehlers *et al.*, 2013; Elmi and Jahadi, 2011)[37,38]. Figures 4, 5 and 6 plot the prices of corn grain in the main local market in Balaka, Mchinji and Rumphi, respectively, over time (dark solid lines) along with their trends (dashed dark line) and their business cycle components (dashed light grey line). These figures show an important increase in the local price of corn grain in the three regions over time, along with significant deviations from the trends.

The price shock variable I define is based on the deviations from the trends. More specifically, I create an indicator variable that takes the value 1 if respondents have experienced a 50% deviation or more in the price of corn grain relative to the trend value in a given year. This price shock takes into account both large increases and drops in corn grain price, which is in line with the "big change in price of grain" shock that can be reported by the respondents, which can be either positive or negative.

Again, the information on price of corn grain is region-specific and I cannot use it to identify the effects of price shock on child health given that they could be confounded by other region-specific shocks that occur in a given year. I therefore proceed in the same way as I did before and interact price shock with land, again in tertiles.

Table 16 presents the results where I regress weight and height on my price shock and land variable along with their interactions. One can see that children who experienced a price shock at birth and who grew up in households that own little land don't have their weight and height that is statistically different from children who did not experience such shocks. However, the effects of price shocks on weight is statistically significant for children coming from households that own a large amount of land. The effect is large, about 700 grams, and is in the ballpark of the effects estimated in my rainfall shocks specification. The effects of price shocks on height are also large, about 1.2 centimeters, but are again not precisely estimated.

Table 40 in Appendix N presents corresponding results where I define a price shock based on the deviations of the price of corn grain relative to its trend value that lasted for at least three months

[37] As recommended by Ravn and Uhlig (2002), I use a smoothing parameter $\lambda = 129600$ given that I am using monthly data.

[38] Note that I linearly interpolated the price of corn grain in Rumphi for April, May and July 2000 as these values were missing.

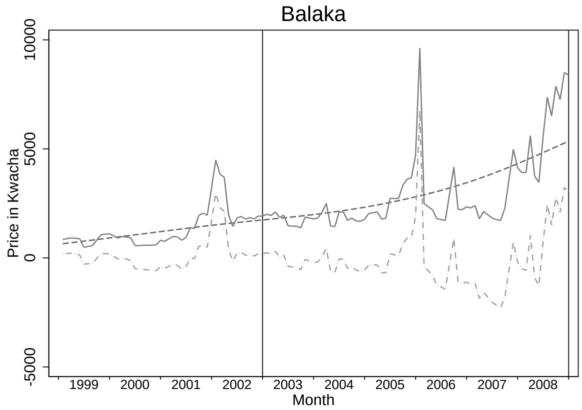

Figure 4: Time series of price of corn grain in the main market of the region of Balaka (South). The time series (solid line) is decomposed into two components, the trend (dark dashed line) and the business cycle component (light dashed line), using H-P filter with $\lambda = 129600$.

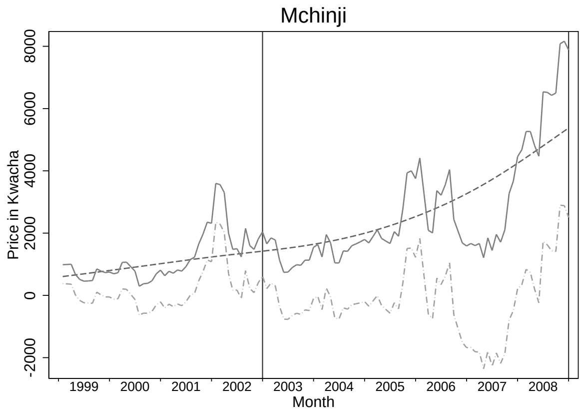

Figure 5: Time series of price of corn grain in the main market of the region of Mchinji (Central). The time series (solid line) is decomposed into two components, the trend (dark dashed line) and the business cycle component (light dashed line), using H-P filter with $\lambda = 129600$.

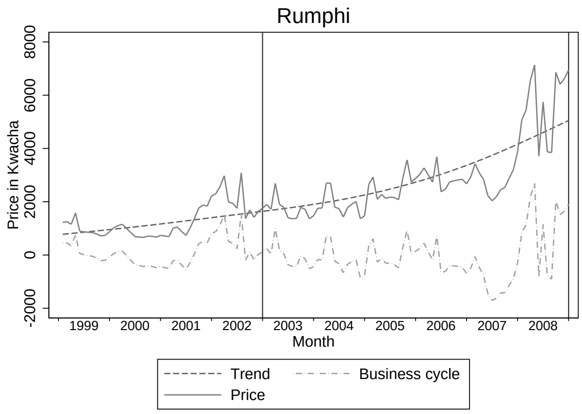

Figure 6: Time series of price of corn grain in the main market of the region of Rumphi (North). The time series (solid line) is decomposed into two components, the trend (dark dashed line) and the business cycle component (light dashed line), using H-P filter with $\lambda = 129600$.

Table 16: Effects of price shocks on objective measures of health, interacted with land ownership

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Land = 1	0.583**	0.384	0.490	-0.076	-0.418	-0.363
	(0.283)	(0.367)	(0.377)	(0.849)	(0.923)	(0.938)
Land = 2	0.295	0.321	0.372	1.596**	1.742**	1.776**
	(0.253)	(0.303)	(0.310)	(0.748)	(0.861)	(0.884)
Price shock	0.069	-0.005	-0.026	0.526	0.256	0.245
	(0.298)	(0.346)	(0.345)	(0.937)	(0.955)	(0.959)
Land = 1 × Price shock	-0.406	-0.139	-0.197	-0.290	0.270	0.255
	(0.372)	(0.445)	(0.444)	(1.103)	(1.175)	(1.173)
Land = 2 × Price shock	-0.777**	-0.645*	-0.661*	-1.177	-1.271	-1.270
	(0.340)	(0.383)	(0.385)	(1.000)	(1.080)	(1.085)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Price shock is a dummy variable that takes the value 1 if respondents have experienced a 50% deviation or more in the price of corn grain relative to the trend value in a given year. Land is measured in m^2 and is split in tertiles (0, 1 and 2). The reference category is a child who did not experience a price shock at birth and has grown up in a household that owns little land (Land=0).

in a given year. The magnitude of the effects is similar to the one found in Table 16[39].

It is possible that the direction of the changes in the price of corn grain has different effects on child health depending on whether the household of the child is a net buyer of corn grain (consumption higher than production) or a net seller (production higher than consumption). Indeed, net seller households could suffer more when grain prices are low whereas net buyer households may benefit from low prices. In Tables 43 and 44 of Appendix N, I investigate whether positive or negative price shocks, again defined as deviations equal or larger than 50% relative to the trend value, differently affect households, depending on the amount of land they own. The results suggest that the effects of positive price shocks on weight and height seem to be associated with land in an inverted-U shape pattern. However, as hypothesized, the effects of negative price shocks on weight and height appear to be large for children from households that own large amounts of land. This suggests that households that own little land, and are therefore more likely to be net buyers, seem to benefit from negative price shocks whereas households that own large amounts of land, and are therefore more likely to be net sellers, seem to suffer more from such price shocks.

From the economic module of the MLSFH, one can also determine the households' degree of diversification in crop production. Corn production is traditionally believed to represent the best protection against food insecurity in Malawi (Devereux, 1999; Mataya *et al.*, 1998) and one can conjecture that changes in price of corn grain, and therefore its unpredictability, may be particularly

[39] I also show in Appendix N that results are similar when I define price shocks as a dummy variable that takes the value 1 if the deviation from the trend is equal or larger than Malawian Kwacha (MKW) 10 in absolute value in a given year. Tables 41 and 42 show that defining price shocks as absolute deviation from the trend rather than relative deviation from it leads to similar results, both for price shocks that lasted (at least) 1 and 3 months in a given year, respectively.

damaging among households whose production focuses almost exclusively on corn. I therefore test this hypothesis and interact my price shock dummy with a dummy variable that takes the value 1 if the share of corn production in the household corresponds to at least 50% of the total value of the household crop production, conditioning on the household being in the two highest land ownership tertiles. Table 17 presents the results of this analysis. While the effect of corn specialization alone does seem to positively affect weight and height, one can see that children who experienced a price shock at birth and who grew up in households that did not specialize in corn production do not statistically differ in terms of weight and height from those who did not experience such price shocks at birth. However, children who were born when a price shock occurred and grew up in households that specialize in corn production were about 700 grams lighter and 1.9 centimeters shorter than other children. These results are robust to specifying price shocks as absolute deviations rather than relative deviations from the trend (Table 45 in Appendix N).

When breaking down my price shocks into positive or negative price shocks, the interaction between corn specialization and negative price shocks again appears to be particularly damaging for children's weight and height, suggesting that corn producers are more affected by negative corn price shocks than others (Tables 46 and 47 in Appendix N).

Ideally, one would want to use the objective measures of shocks derived from external rainfall and price data as instrumental variables (IV) for self-reported economic shocks to estimate the causal effects of these shocks on child health. These instruments could be relevant in my application because "Poor crop yields" and "big change in price of grain" shocks are the two most common shocks reported by the respondents. However, the shocks derived from these external data do not predict well enough the self-reported shocks (weak first-stage), which prevents me from deriving consistent causal estimates of these shocks using IV methods. One possible explanation is that, while there is a clear mapping between "big change in price of grain" and my price of corn grain data from local markets, "Poor crop yields, loss of crops due to disease or pests, or loss of livestock due to theft or disease, or loss of coupon", which is the most common shock reported, may be due to other reasons than rainfall shocks. Another explanation could be that my definitions of what characterizes a shock do not match with respondents' perceptions of what a shock is. I defined rainfall and price shocks in several different ways without being able to satisfactorily capture the shocks reported by the respondents. Finally, as shown in Table 3, an important fraction of the shocks reported by respondents are not related to changes in rainfall or price of grain, which can reduce the correlation between the IVs and self-reported economic shocks dummy variables.

Table 17: Effects of price shocks on objective measures of health, interacted with corn specialization

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Corn specialization	0.228	0.394	0.367	1.505**	1.621*	1.618*
	(0.274)	(0.334)	(0.334)	(0.761)	(0.896)	(0.894)
Price shock	-0.149	-0.098	-0.141	0.532	0.277	0.266
	(0.273)	(0.312)	(0.312)	(0.925)	(0.916)	(0.924)
Corn specialization × Price shock	-0.572	-0.722*	-0.668	-1.891**	-1.889*	-1.889*
	(0.368)	(0.426)	(0.428)	(0.956)	(1.047)	(1.061)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Price shock is a dummy variable that takes the value 1 if respondents have experienced a 50% deviation or more in the price of corn grain relative to the trend in a given year. Corn specialization is a dummy variable that takes the value 1 if the share of the production of corn corresponds to at least 50% of the total value of the household crop production, conditioning on the household being in the two highest land ownership tertiles. The reference category is a child who did not experience a price shock at birth and has grown up in a household that did not specialize in corn production and possess little land.

Overall, I show that shocks in rainfall and grain prices have negative effects on the health of children whose households own large amounts of land. My results also suggest that crop specialization exacerbates the detrimental effects of these shocks, particularly when the shocks in the price of grain are negative. Although not directly comparable to the effects of negative economic shocks derived from self-reports, this analysis provide further evidence on the importance of in utero, or shortly after birth, stress experienced by mothers in determining child health later in life.

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[40]. 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–

[40]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.

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:

$$H_i = \alpha_1 S_i^* + X_i' \alpha_2 + \nu_i \tag{6}$$

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 real vector of 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, $S^* \sim \text{Bern}(p)$.

$$S_i^* = \begin{cases} 1 & \text{if} \quad p \\ 0 & \text{if} \quad 1-p \end{cases} \tag{7}$$

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

$$d_{i,S^*} = \mathbb{1}(y_i^* \geqslant n \cap S_i^* = 0) - \mathbb{1}(y_i^* \leqslant m \cap S_i^* = 1) \tag{8}$$

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. The four possible scenarios that can occur can therefore be summarized as:

$$S_i = \begin{cases} 1 \begin{cases} S_i^* = 1 & \text{and} \quad y^* > m \quad \text{such that} \quad d_{i,S^*} = 0 & \Longrightarrow \quad \text{true reporting} \\ S_i^* = 0 & \text{and} \quad y^* \geqslant n \quad \text{such that} \quad d_{i,S^*} = 1 & \Longrightarrow \quad \text{false positive} \end{cases} \\ 0 \begin{cases} S_i^* = 1 & \text{and} \quad y^* \leqslant m \quad \text{such that} \quad d_{i,S^*} = -1 & \Longrightarrow \quad \text{false negative} \\ S_i^* = 0 & \text{and} \quad y^* < n \quad \text{such that} \quad d_{i,S^*} = 0 & \Longrightarrow \quad \text{true reporting} \end{cases} \end{cases}$$

I define y_i^* with a linear function as:

$$y_i^* = w_i'\gamma + u_i \tag{9}$$

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:

$$H_i = \alpha_1 S_i + X_i'\alpha_2 + \epsilon_i \tag{10}$$

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 but 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 my case.

Misreporting is, however, 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 6 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 Appendix

O that the asymptotic bias in case of endogenous misreporting can be written as follows:

$$plim(\hat{\alpha} - \alpha) = \frac{E[(1-p)\delta\sigma_\nu\phi(\frac{n-w_i'\gamma}{\sigma_u}) + p\delta\sigma_\nu\phi(\frac{m-w_i'\gamma}{\sigma_u})] - \alpha[E(S_i d_i) - E(S_i x_i')E(x_i x_i')^{-1}E(d_i x_i')]}{E(S_i^2) - E(S_i x_i')E(x_i x_i')^{-1}E(S_i x_i)} \tag{11}$$

where $\delta = corr(\nu_i, u_i)$ and σ_ν and σ_u the standard deviations of ν_i and u_i, respectively. 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 equation 11 as:

$$plim(\hat{\alpha} - \alpha) = \frac{\Gamma - \alpha\Lambda}{\Theta} \tag{12}$$

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 equation 6 that explain poor health can be positively correlated with factors that explain misreporting behaviors[41].

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

[41] 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 $\zeta = \frac{\Gamma}{\Lambda - \Theta}$.

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:

$$B_1 = \{i : \mathbb{1}[0 \leqslant |s_{m_i}| \leqslant 7] = 1\} \tag{13}$$

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[42]. I therefore define others B_i with $i = \{2, 3, 4, 5\}$ as

$$B_2 = \{i : \mathbb{1}[1 \leqslant |s_{m_i}| \leqslant 7] = 1\} \tag{14}$$

$$B_3 = \{i : \mathbb{1}[1 \leqslant |s_{m_i}| \leqslant 4] = 1\} \tag{15}$$

$$B_4 = \{i : \mathbb{1}[2 \leqslant |s_{m_i}| \leqslant 7] = 1\} \tag{16}$$

$$B_5 = \{i : \mathbb{1}[2 \leqslant |s_{m_i}| \leqslant 4] = 1\} \tag{17}$$

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. 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

[42] 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.

Table 18: Effects of negative economic shocks on objective measures of health using various control groups

	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.

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 18 reports the results of this analysis using set of controls 1[43]. Column 1 shows the results including all $i \in \{B_1\}$, which are the results in my benchmark specification in Columns 1 of Tables 6 and 7. 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 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 is to explicitly model misreporting by allowing observed and

[43] Results are similar when using set of controls 2 and 3. Table 48 in Appendix P presents these results.

unobserved characteristics of the mothers to explain true and false (positive and negative) reports[44]. The model below attempts to give some insights on the effect of endogenous misreporting and its consequences on the OLS estimator.

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 $w_i'\gamma + u_i \geqslant n$[45]. 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.

Again, the probability of reporting a shock ($P(S_i = 1)$) consists of either from the probability of truly experiencing a shock ($P(S_i^* = 1)$) and not falsely reporting it ($y_i^* > m$) or not experiencing a shock ($P(S_i^* = 0)$) and falsely reporting it ($y_i^* \geqslant n$, false positive). Similarly, the probability of not reporting a negative economic shock ($P(S_i = 0)$) is composed of truly not experiencing a shock ($P(S_i^* = 0)$) and having $y_i^* < n$ or experiencing a shock ($P(S_i^* = 1)$) but not reporting it ($y_i^* \leqslant m$).

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

$$P(S_i = 1) = P(S_i^* = 1 \cap w_i'\gamma + u_i > m) + P(S_i^* = 0 \cap w_i'\gamma + u_i \geqslant n) \tag{18}$$

$$P(S_i = 0) = P(S_i^* = 0 \cap w_i'\gamma + u_i < n) + P(S_i^* = 1 \cap w_i'\gamma + u_i \leqslant m) \tag{19}$$

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

$$P(S_i = 1) = p(1 - \Phi(m - w_i'\gamma)) + (1-p)(1 - \Phi(n - w_i'\gamma)) = P_i(p, \gamma : m, n) \tag{20}$$

$$P(S_i = 0) = (1-p)\Phi(n - w_i'\gamma) + p\Phi(m - w_i'\gamma) = 1 - P_i(p, \gamma : m, n) \tag{21}$$

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:

$$l_n(p, \gamma : m, n) = \prod_{i=1}^{N} P_i(p, \gamma : m, n)^{S_i} \,(1 - P_i(p, \gamma : m, n)^{1-S_i} \tag{22}$$

[44]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.

[45]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.

from which I can derive the log-likelihood function:

$$L_n(p,\gamma:m,n) = ln(l_n(p,\gamma:m,n)) = \sum_{i=1}^{N} S_i \, lnP_i(p,\gamma:m,n) + (1-S_i)\, ln(1-P_i(p,\gamma:m,n)) \quad (23)$$

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 is usually more likely than false positive report (Meyer *et al.*, 2018; Nguimkeu *et al.*, 2017)[46]. The variables in 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 a shock (false negative)[47].

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).

[46] 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.

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

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 $\mathrm{N}(0,1)$.

Finally the fourth variable is a dummy variable that exploits the available data on price of grain to identify respondents who "falsely" report a big change in the price of grain when such a change in fact did not occur[48].

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:

$$\hat{p} = \frac{N_{S'=1}}{N} = \frac{N_{S^*=1}}{N} \tag{24}$$

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:

$$R = |p - \hat{p}| \times N \tag{25}$$

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

[48]Ideally, one would want to use the rain data as well in order to identify individuals who falsely report "poor crop yields", the most prevalent shock reported by the respondents. That being said, this is not feasible as "poor crop yields" does not directly refer to rain shocks and I cannot be sure that rain shocks are the reason behind poor crop yields. In addition, the structure of the shock module in the MLSFH survey does not allow me to identify false negative report in reported "big change in price of grain" shocks in a given year. The reason is that respondents were asked details about the shocks they experienced only if they were the three most severe. In a given year, a shock that is not reported can either be a shock that did occur but was not reported (false negative) or a shock that was simply not considered by the respondents to be among the three most severe shocks. For this reason, I cannot be sure that a shock that is not reported by a respondent is due to misreporting. However, false positive reports can be defined, as I can compare the year of the "big change in the price of grain" shock reported by the respondents with the actual data on the price of grain in the local market and hence know whether such a change in the price of grain did occur or not.

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:

$$R_{i,j} = max(R_{i,0}, R_{0,j}) + R'_{i,j} \tag{26}$$

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[49]. 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 26, 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., $\hat{p} = \frac{N_{S'=1}}{N} = \frac{N_{S^*=1}}{N}$.

Table 19 reports the estimates of p that I derive from my maximum likelihood function using the four variables in w I described above[50]. 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).

Tables 20 and 21 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 cell in Table 20 is my benchmark estimate 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 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 Table 21: 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 0.5 cen-

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

[50] 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 19: Probabilites of experiencing 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*** (0.0163)	0.2610*** (0.0181)	0.2528*** (0.0184)	0.2321*** (0.0187)	0.1956*** (0.0189)
	2%	0.3301*** (0.0193)	0.2813*** (0.0185)	0.2689*** (0.0185)	0.2433*** (0.0188)	0.2035*** (0.0194)
	5%	0.3378*** (0.0198)	0.2983*** (0.0193)	0.2850*** (0.0193)	0.2567*** (0.0196)	0.2137*** (0.0202)
	10%	0.3497*** (0.0203)	0.3210*** (0.0204)	0.3076*** (0.0205)	0.2774*** (0.0208)	0.2305*** (0.0216)
	20%	0.3766*** (0.0213)	0.3637*** (0.0224)	0.3509*** (0.0227)	0.3193*** (0.0234)	0.2673*** (0.0248)

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.

timeters. 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 49 for weight and Table 50 for height in Appendix Q 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. I also investigate the robustness of my findings to the inclusion of the dummy variable for false positive report in w. As detailed above, this variable is only a proxy for false report and is likely to be poorly measured. I show in Tables 51, 52 and 53 of Appendix R that excluding this dummy variable in the vector w to estimate y^* does not affect the conclusion that I derive from my main results. Using region-specific differences 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 in w does not lead to important changes in my results either (Tables 54, 55 and 56 in the same appendix).

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 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 19), about 42%

Table 20: Effects of S_i' on weight

Effects of economic shocks on weight		Rate of false positive				
		0%	1%	2%	5%	10%
Rate of false negative	0%	-0.3280**	-0.2901*	-0.2959*	-0.1907	-0.2530
		(0.1669)	(0.1730)	(0.1760)	(0.1795)	(0.1900)
	2%	-0.4588***	-0.4554***	-0.4070**	-0.1371	0.0741
		(0.1602)	(0.1656)	(0.1690)	(0.1918)	(0.1979)
	5%	-0.4420***	-0.3698**	-0.3941**	-0.2549	0.0185
		(0.1576)	(0.1649)	(0.1673)	(0.1893)	(0.2010)
	10%	-0.3726**	-0.1522	-0.1824	-0.2295	0.0797
		(0.1591)	(0.1732)	(0.1768)	(0.1812)	(0.1990)
	20%	-0.2356	0.0008	-0.0454	-0.0646	0.0374
		(0.1574)	(0.1686)	(0.1695)	(0.1733)	(0.1829)

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 1. The sample is based on 775 observations.

Table 21: Effects of S_i' on height

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 using set of controls 1. The sample is based on 775 observations.

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 A word on rounding errors

The results above are derived using weight rounded at the nearest kilogram. One may wonder whether this could affect my estimates. Let's define m^* as the true, latent, anthropometric measurements of a child, and assume that weight follows a normal distribution $m^* \sim N(\mu_{m^*}, \sigma_{m^*})$. Instead of observing m^*, the econometrician observes m, with $m = m^* + \eta_m$ and where η_m is the rounding error. As in Schneeweiss *et al.* (2006), I can define a grid of equidistant points on the real line $\mathbb{R}$ with $\mathbb{R}^* := \mathbb{R}_h^* := \{ih, i \in \mathbb{Z}\}$ where h is the distance between two adjacent points of the grid. m will therefore be the point on $\mathbb{R}^*$ that is the closest to m^*, so that $m = ih$ if $m \in [ih \pm \frac{h}{2}]$ with $i \in \mathbb{Z}$. The classical assumption in measurement error theory is that η_m is independent of S^*, the real occurrence of negative economic shock, which means that measurement error in the dependent variable usually does not affect the consistency of α. However, the rounding error η_m is clearly not independent to the latent variable m^*, which is in turn affected by S^*, making the independence assumption between η_m and S^* invalid. In fact, η_m is deterministic and both η_m and m depend on m^*(Liu *et al.*, 2010)[51]. It is known that in cases where only the dependent variable is rounded and both the dependent and independent variables are continuous, then measurement errors will negligibly affect the OLS estimates and no correction is needed (Schneeweiß and Komlos, 2009). In my context however, the independent variable of interest is a dummy variable and I investigate below whether this has an effect on my OLS estimates. The OLS estimator of regressing m on S^* is given by:

$$plim\,\hat{\alpha} = \frac{cov(m, S^*)}{var(S^*)} = \frac{cov(m^* + \eta_m, S^*)}{var(S^*)} \tag{28}$$

$$= \frac{cov(m^*, S^*) + cov(\eta_m, S^*)}{var(S^*)} \tag{29}$$

$$= \alpha^* + \frac{cov(\eta_m, S^*)}{var(S^*)} \tag{30}$$

Clearly, in the classical measurement case, because η_m and S^* are assumed to be independent, α can be consistently estimated with $plim\,\hat{\alpha} = \alpha^*$. However, in the case where the dependent variable is rounded, η_m and S^* are dependent, leading to a bias of $\frac{cov(\eta_m, S^*)}{var(S^*)}$, which can be either positive or negative depending on the sign of $cov(\eta_m, S^*)$ (Schneeweiß and Komlos, 2009).

It is clear from the expression above that the bias decreases as h becomes smaller relative to the variance of m^*, since if the rounding error η_m is small, $cov(\eta_m, S^*)$ becomes negligible and the bias vanishes.

As before, I assume that S^* follows a Bernoulli distribution with parameter p, $S^* \sim \text{Bern}\,(p)$.

[51] As defined in Schneeweiss *et al.* (2006), the density of η_m, $f_{\eta_m}(.)$, conditional on m can be written as:

$$f_{\eta_m}(\eta_m|m) = \begin{cases} \frac{\phi(m-\eta_m)}{p(m)} & \text{for} \quad -\frac{h}{2} \leq \eta_m \leq \frac{h}{2} \\ 0 & \text{for} \quad \eta_m < -\frac{h}{2} \quad \text{or} \quad \eta_m > \frac{h}{2} \end{cases} \tag{27}$$

with $p(m^o) = \int_{m-\frac{h}{2}}^{m+\frac{h}{2}} \phi(m^*)dm^*$ for $m = m^o$ (m^o being an observed realization of the random variable m) and $\phi(.)$ the density function of the normal distribution.

This means that:

$$cov(\eta_m, S^*) = E(\eta_m S^*) - E(\eta_m)E(S^*) = E(\eta_m S^* | S^* = 1)Pr(S^* = 1) - pE(\eta_m) \tag{31}$$

$$= [E(\eta_m | S^* = 1) - E(\eta_m)]p \tag{32}$$

$$= [E(\eta_m | S^* = 1) - [E(\eta_m | S^* = 1)p + E(\eta_m | S^* = 0)(1-p)]]p \tag{33}$$

$$= [E(\eta_m | S^* = 1) - E(\eta_m | S^* = 0)]p(1-p) \tag{34}$$

$$= [E(\eta_m | S^* = 1) - E(\eta_m | S^* = 0)]var(S^*) \tag{35}$$

This implies that the bias above reduces to $E(\eta_m | S^* = 1) - E(\eta_m | S^* = 0)$, which can be estimated. To do so, I first need to estimate the first two moments of the latent variable m^*. First, the probability of observing $m = ih$ given μ_{m^*} and σ_{m^*} can be calculated as follows:

$$P(m = ih | \mu_{m^*}, \sigma_{m^*}) = \int_{ih-\frac{h}{2}}^{ih+\frac{h}{2}} \frac{1}{\sigma_{m^*}\sqrt{2\pi}} e^{-\frac{(m^*-\mu_{m^*})^2}{2\sigma^2_{m^*}}} dm^* \tag{36}$$

$$= \Phi_{\mu_{m^*},\sigma_{m^*}}(ih + \frac{h}{2}) - \Phi_{\mu_{m^*},\sigma_{m^*}}(ih - \frac{h}{2}) \tag{37}$$

over the range of i and where Φ is the cumulative distribution function of the normal distribution. With this set of probabilities for all m, one can then maximize the following likelihood function to uncover the estimates of μ_{m^*} and σ_{m^*} with:

$$L_n(m_1, m_2, ..., m_N | \mu_{m^*}, \sigma_{m^*}) = \prod_{j=1}^{N} \sum_{i=-\infty}^{\infty} I_i(m_j) \times P(m = ih | \mu_{m^*}, \sigma_{m^*}) \tag{38}$$

with $I_i(m_j)$ an indicator variable that takes the value 1 if $m_j = ih$ and 0 otherwise for individual $j = 1...N$. From this maximum likelihood function, one can estimate $\hat{\mu}_{m^*}$ and $\hat{\sigma}_{m^*}$. Once these two parameters are estimated, one can compute the conditional expectations above knowing that:

$$E(\eta_m) = \frac{1}{\hat{\sigma}_{m^*}\sqrt{2\pi}} \sum_{i=-\infty}^{\infty} \int_{ih-\frac{h}{2}}^{ih+\frac{h}{2}} (ih - m^*)\, e^{-\frac{(m^*-\hat{\mu}_{m^*})^2}{2\hat{\sigma}^2_{m^*}}} dm^* \tag{39}$$

In my analysis, $h = 1$ and $3 \leqslant i \leqslant 23$.

Results in Table 22 show that the means of m and m^*, conditional on S^*, are identical up to 5 digit decimal, which is not surprising given the assumed smooth and symmetric distribution of m^* and the small value of h relative to the variance of m^* (Schneeweiss *et al.*, 2006). The

Table 22: Estimates for weight

Estimates for weight		
	Mean	Standard deviation
m with $S^* = 1$	9.812766	3.016877
m^* with $S^* = 1$	9.812775	2.996572
m with $S^* = 0$	12.08664	3.411459
m^* with $S^* = 0$	12.08663	3.39613
η_m with $S^* = 1$	-0.0001751606	
η_m with $S^* = 0$	-0.0007743066	
$\Delta\eta_{m,\Delta S^*}$	0.0005991460	

Note: Results are derived using 554 observations in the case where $S^* = 0$ and 235 in the case where $S^* = 1$.

difference between the variance of m and m^* however is not negligible (Schneeweiß and Komlos, 2009; Schneeweiss *et al.*, 2006)[52]. The second panel of Table 22 shows the estimates of the rounding error η_m conditional on the occurrence of shocks. Given the very small values of these estimates and their difference, one can conclude under my assumptions that the bias of my estimate of the slope parameter of S^* due to rounding errors is negligible.

8 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

[52] The variance of m^* can be approximated using the Sheppard's correction $var(m^*) \approx var(m) - \frac{h^2}{12}$ (Liu *et al.*, 2010; Sheppard, 1897).

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.

There are several limitations in this study. First, it is possible that extreme economic shocks at birth induce families to migrate, which would mean that those who are the most affected by these shocks were potentially excluded from the analysis. This hypothesis is hard to verify in my sample. Second, the objective measures of shocks that I derive from external rainfall and price data could be used as instrumental variables for self-reported shocks. The correlation between the objective measures of shocks and the self-reported shocks is however weak and therefore the instruments would not be strong enough to derive consistent causal estimates using IV method. I do however believe that the various empirical strategies I put in place in this study provide me with an understanding of the causal effects of negative economic shocks experienced at birth on child health.

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. 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).

Appendix A

Table 23: 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	0.0110	0.0210	0.0190
	(0.0290)	(0.0330)	(0.0320)
ill for more than 1 month	0.0160	0.0160	0.0150
	(0.0130)	(0.0160)	(0.0150)
in very good health	-0.0160	-0.0370	-0.0360
	(0.0250)	(0.0290)	(0.0290)
in excellent health	-0.0330	-0.0700**	-0.0730**
	(0.0280)	(0.0310)	(0.0310)
in better health	-0.0290	-0.0580*	-0.0530*
	(0.0270)	(0.0310)	(0.0310)
in much better health	-0.0340	-0.0830***	-0.0820***
	(0.0270)	(0.0300)	(0.0300)
Obs.	1784	1382	1380

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Column 1 controls for age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Column 2 adds the marital statuts of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more detailts). Column 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. Columns 4 and 5 include the same controls as Column 3.

Table 24: 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	0.0110	0.0210	0.0190
	(0.0290)	(0.0320)	(0.0320)
ill for more than 1 month	0.0150	0.0150	0.0140
	(0.0130)	(0.0150)	(0.0150)
in very good health	-0.0160	-0.0360	-0.0350
	(0.0250)	(0.0290)	(0.0290)
in excellent health	-0.0330	-0.0700**	-0.0740**
	(0.0270)	(0.0310)	(0.0310)
in better health	-0.0280	-0.0570*	-0.0530*
	(0.0270)	(0.0310)	(0.0310)
in much better health	-0.0330	-0.0810***	-0.0810***
	(0.0270)	(0.0300)	(0.0300)
Obs.	1784	1382	1380

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Column 1 controls for age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Column 2 adds the marital statuts of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more detailts). Column 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. Columns 4 and 5 include the same controls as Column 3.

Appendix B

Controlling for reporting heterogeneity in self-reported health measures

One of the shortcomings of using subjective health measure is the potential lack of comparability across respondent's responses. Indeed, being ill, in a good/excellent health or in better/much better health can be interpreted very differently across mothers in my sample. One can exploit the fact that I have several observations per mother to control for reporting heterogeneity and assess the extent to which it affects my estimates.

I define y_{im} as the subjective binary health outcome reported by mother m for child i. I assume that there exists a continuous latent health variable y^*_{im} that represents the health status of child i and that y^*_{im} is explained linearly by my independent variables, that is $y^*_{im} = \beta_1 S^*_{im} + X'_{im}\beta_2 - \eta_{im}$, with η_{im} being an error term that follows a standard logistic distribution. I further assume that the relationship between the observed y_{im} and latent y^*_{im} takes the following form:

$$y_{im} = \begin{cases} 0 & \text{if} \quad y^*_{im} \leq c_m \\ 1 & \text{if} \quad y^*_{im} > c_m \end{cases} \tag{40}$$

Reporting heterogeneity arises when mothers have different thresholds c_m that link y^*_{im} to y_{im}. This means that children with different y^* may end up having the same y. Conversely, children with the same y^* may end up having different y. Because I assume that c_m is fixed at the mother level, one can use fixed-effect analysis to control for different reporting scales. The fixed-effect model is defined by the logistic probability of y_{im}:

$$f(y_{im}|S^*_{im}, X'_{im}, \beta_1, \beta_2, c_m) = P_{im}^{y_{im}}(1 - P_{im})^{(1-y_{im})} \tag{41}$$

with

$$\begin{aligned} P_{im} = P(y_{im} = 1|S^*_{im}, X'_{im}, \beta_1, \beta_2, c_m) &= P(-\eta_{im} > c_m - \beta_1 S^*_{im} - X'_{im}\beta_2) \\ &= P(\eta_{im} < \beta_1 S^*_{im} + X'_{im}\beta_2 - c_m) \\ &= \Lambda_\eta(\beta_1 S^*_{im} + X'_{im}\beta_2 - c_m) \\ &= \frac{1}{1 + e^{-\beta_1 S^*_{im} - X'_{im}\beta_2 + c_m}} \end{aligned} \tag{42}$$

where Λ_η is the cumulative distribution of the logistic distribution. One can then estimate this

Table 25: Effects of negative economic shocks on my subjective measures, controlling for reporting heterogeneity

	Set of control 1		Set of control 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's reporting heterogeneity using fixed-effect logit model 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 my "Being ill for more than 1 month" specification for it to be estimated when using set of controls 2.

model by means of conditional likelihood function to obtain the effects of economic shocks on my set of subjective measures and correct for reporting heterogeneity[53]. Table 25 shows the odd ratios resulting from this model, along with their 95% confidence intervals. After controlling for different thresholds in the relationship between y^*_{im} and y_{im}, one can see that the results are in line with the ones presented in the mother linear fixed-effect analysis. More specifically, the odds of being in excellent health and in being in much better health than children of the same sex and age in the village are 0.32 and 0.15 lower for those who experienced a shock at birth compared to those who did not experience such a shock.

[53] Note that fixed-effect model estimated with conditional likelihood function includes only observations of mother who report different values of y for their children. This leads to important reduction in my sample.

Appendix C

Table 26: Effects of negative economic shocks at birth on objective health outcomes - 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

Table 27: Effects of negative economic shocks at birth on weight, controlling for mother's height

	(1)	(2)	(3)	Male (4)	Female (5)
1. Weight					
Economic shock at birth	-0.320*	-0.305*	-0.325*	-0.610**	0.001
	(0.165)	(0.173)	(0.175)	(0.255)	(0.206)
Mother's height	0.030**	0.028*	0.028*	0.024	0.037***
	(0.013)	(0.015)	(0.015)	(0.021)	(0.014)
2. Height					
Economic shock at birth	-0.648	-0.439	-0.426	-1.501*	0.233
	(0.542)	(0.597)	(0.602)	(0.799)	(0.748)
Mother's height	0.122***	0.125***	0.128***	0.141**	0.113**
	(0.043)	(0.044)	(0.044)	(0.062)	(0.053)
Observations	784	636	636	373	411

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 weight and height. Column 1 controls for age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Column 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Column 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. Columns 4 and 5 include the same controls as Column 3. The reference category is a boy of age 0 from the central region of Malawi who did not experience any economic shock at birth. Mother's height is included in all regressions.

Appendix E

Table 28: Weight-for-age and height-for-age z-scores, assuming non-normal distributions

	Z-score	<-2	<-1	<0	<1	<2
1. Weight						
Set of controls 1	-.172*	.013	.014	.088**	.059*	.010
	(.097)	(.011)	(.027)	(.045)	(.030)	(.017)
Set of controls 2	-.133	.013	.020	.105**	.055*	.009
	(.096)	(.011)	(.030)	(.048)	(.033)	(.019)
Set of controls 3	-.145	.012	.020	.108**	.061*	.011
	(.097)	(.011)	(.030)	(.048)	(.033)	(.019)
2. Height						
Set of controls 1	-.123	.017	.054*	.066	.052	-.007
	(.091)	(.015)	(.032)	(.045)	(.032)	(.014)
Set of controls 2	-.056	.009	.031	.022	.039	-.015
	(.099)	(.015)	(.032)	(.048)	(.036)	(.017)
Set of controls 3	-.056	.008	.030	.018	.043	-.014
	(.099)	(.015)	(.032)	(.048)	(.037)	(.018)

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 z-score (Column 1) and on dummy variables that take value 1 if z-score is below d with $d = \{-2, -1, 0, 1, 2\}$ (Columns 2,3,4,5,6), assuming weight and height are not normally distributed. The first panel looks at the effect on weight and the second at height. Set of controls 1 consists of age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Set of controls 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Set of controls 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. All regressions also control for ethnicity and region.

Appendix F

Table 29: Weight-for-age z-score for weight, using WHO standards as reference group

	Z-score	<-2	<-1	<0	<1	<2
1. Weight						
Set of controls 1	-.250*	.032	.088**	.048	.042*	.020
	(.133)	(.028)	(.043)	(.04)	(.025)	(.019)
Set of controls 2	-.244*	.029	.096**	.043	.046	.019
	(.148)	(.029)	(.047)	(.045)	(.029)	(.022)
Set of controls 3	-.259*	.029	.101**	.048	.050*	.022
	(.149)	(.030)	(.047)	(.045)	(.029)	(.022)
2. Height						
Set of controls 1	-.261	-.001	.033	.037	.029	-.007
	(.194)	(.044)	(.039)	(.030)	(.026)	(.017)
Set of controls 2	-.146	-.023	.013	.020	.024	-.010
	(.219)	(.048)	(.044)	(.035)	(.030)	(.020)
Set of controls 3	-.138	-.021	.015	.020	.023	-.010
	(.221)	(.048)	(.044)	(.036)	(.030)	(.021)

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 z-score (Column 1) and on dummy variables that take value 1 if z-score is below d with $d = \{-2, -1, 0, 1, 2\}$ (Columns 2,3,4,5,6) using WHO standards as reference values. The first panel looks at the effect on weight and the second at height. Set of controls 1 consists of age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Set of controls 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Set of controls 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. All regressions also control for ethnicity and region.

Appendix G

Table 30: Weight-for-height z-score, using WHO standards as reference group

	Z-score	<-2	<-1	<0	<1	<2
Weight-for-height						
Set of controls 1	-.157	.000	.050*	.036	.013	.008
	(.22)	(.022)	(.030)	(.042)	(.043)	(.036)
Set of controls 2	-.245	.004	.077**	.069	.031	.035
	(.246)	(.024)	(.034)	(.046)	(.048)	(.039)
Set of controls 3	-.265	.007	.078**	.074	.037	.038
	(.247)	(.025)	(.035)	(.047)	(.047)	(.039)

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 weight-for-length z-score (Column 1) and on dummy variables that take value 1 if z-score is below d with $d = \{-2, -1, 0, 1, 2\}$ (Columns 2,3,4,5,6) using WHO standards as reference values. Set of controls 1 consists of age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Set of controls 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Set of controls 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household. All regressions also control for ethnicity and region.

Appendix H

Table 31: Effects of negative economic shocks on social participation and transfers

Effects of economic shocks at birth on	(1)	(2)	(3)
Number of ville committees	0.141*	0.159*	0.169*
	(0.082)	(0.091)	(0.090)
Number of social activities	0.460	0.811	0.839
	(0.671)	(0.771)	(0.766)
Potential help (number of person)	0.108	0.309	0.339
	(0.207)	(0.218)	(0.218)
Help received (number of person)	0.215	0.479**	0.482**
	(0.220)	(0.240)	(0.243)
Observations	789	633	633

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 measures of social participation and informal transfers. Column 1 controls for age (dummy for each age category), sex of the child, age of the mother at birth, region (dummy for each region) and ethnicity (dummy for each ethnicity). Column 2 adds the marital status of the mother at birth, the birth order of the child, the level of education of the mother and a continuous measure of wealth score (see text for more details). Column 3 adds 10 quantile measures of household crop production, total household expenditure and total children expenditure in the household.

Appendix I

Table 32: Effects of negative economic shocks on objective measures of health, controlling for public welfare program participation

	(1)	(2)	(3)
1. Weight			
Economic shock at birth	-0.328**	-0.319*	-0.348**
	(0.165)	(0.174)	(0.175)
Nb of programs the HH has benefited from	-0.038	-0.053	-0.059
	(0.084)	(0.093)	(0.093)
Agricultural Input Supply Program	0.134	0.065	0.065
	(0.279)	(0.311)	(0.314)
Total amount received in 1000 MKW	-0.006	-0.010	-0.011
	(0.015)	(0.018)	(0.017)
2. Height			
Economic shock at birth	-0.691	-0.361	-0.360
	(0.536)	(0.580)	(0.584)
Nb of programs the HH has benefited from	-0.168	-0.039	-0.040
	(0.221)	(0.229)	(0.231)
Agricultural Input Supply Program	1.225	1.400	1.409
	(0.871)	(1.021)	(1.030)
Total amount received in 1000 MKW	0.100**	0.064	0.064
	(0.048)	(0.048)	(0.048)
Obs.	788	639	639

Note: *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 weight (first panel) and height (second panel). In addition to including my usual set of regressors in Column 1, 2 and 3, my specifications control for the number of programs the respondent's household has benefited from over the past three years, whether the household has received agricultural input in the form of coupon/voucher for seed or fertilizer and the total estimated value the household has received in thousands MKW.

Table 33: Effects of negative economic shocks on objective measures of health, splitting the sample by whether the mothers engage in social activities and transfers or not measured in 2006.

	Number of social activities		Potential help		Help received	
	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)
1. Weight						
Set of controls 1	-0.254	-0.262	-0.301	-0.271	-0.405	-0.215
	(0.258)	(0.265)	(0.284)	(0.238)	(0.283)	(0.238)
Set of controls 2	-0.121	-0.309	-0.320	-0.122	-0.440	-0.062
	(0.235)	(0.278)	(0.295)	(0.234)	(0.321)	(0.226)
Set of controls 3	-0.150	-0.387	-0.350	-0.166	-0.448	-0.089
	(0.235)	(0.273)	(0.295)	(0.234)	(0.324)	(0.224)
1. Height						
Set of controls 1	-0.309	-1.391	-0.271	-0.605	-0.891	-0.040
	(0.772)	(0.921)	(0.924)	(0.727)	(1.021)	(0.670)
Set of controls 2	-0.152	-0.980	-0.323	0.117	-0.557	0.228
	(0.807)	(0.940)	(0.978)	(0.743)	(1.092)	(0.677)
Set of controls 3	-0.286	-0.949	-0.439	0.118	-0.595	0.206
	(0.823)	(0.933)	(0.987)	(0.730)	(1.097)	(0.672)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The first and second panel show the effects of economic shocks at birth on weight and height, respectively. I split my sample by whether respondents engage in fewer (Column 1) or more (Column 2) social activities than the median value, by whether respondents can rely on fewer (Column 3) or more (Column 4) persons in case of crises than the median value and by whether respondents have received help from fewer (Column 5) or more (Column 6) persons than the median value in my sample. These variables are measured in 2006. The questions related to the number of village committees the mothers participate in is not available in 2006.

Appendix J

Table 34: Effects of economic shocks at birth using the same sample as the one in Table 12

	(1)	(2)	(3)
1. Weight			
Economic shock at birth	-0.359**	-0.373*	-0.403**
	(0.182)	(0.194)	(0.195)
2. Height			
Economic shock at birt	-0.805	-0.466	-0.477
	(0.569)	(0.628)	(0.634)
Observations	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 and second panel show the effects of negative economic shocks on weight and height respectively, using the same sample as the one used in Table 12, that is, the observations of children born in 2003 and 2008 have been discarded.

Appendix K

Table 35: Marginal effects of economic shocks on mortality, Logit regressions

Effects on Mortality			
	(1)	(2)	(3)
Shock at birth	0.009	-0.002	-0.001
	(0.012)	(0.015)	(0.014)
Idiosyncratic shock at birth	-0.004	0.001	0.001
	(0.019)	(0.021)	(0.021)
Common shock at birth	0.018	0.004	0.005
	(0.013)	(0.016)	(0.016)
Obs.	1939	1508	1506

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Logit regressions. I am not controlling for the age of the children in the regressions. Sample consists of 1939 children, 1808 are alive and 131 are dead (6.76%). Idiosyncratic shocks are shocks affecting the household of the respondents only. Common shocks are shocks that affect other households as well.

Table 36: Marginal effects of economic shocks on mortality, Probit regressions

Effects on Mortality			
	(1)	(2)	(3)
Shock at birth	0.009	-0.001	0.001
	(0.012)	(0.014)	(0.014)
Idiosyncratic shock at birth	-0.005	0.001	0.000
	(0.019)	(0.021)	(0.020)
Common shock at birth	0.019	0.005	0.007
	(0.013)	(0.016)	(0.015)
Obs.	1939	1508	1506

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Probit regressions. I am not controlling for the age of the children in the regressions. Sample consists of 1939 children, 1808 are alive and 131 are dead (6.76%). Idiosyncratic shocks are shocks affecting the household of the respondents only. Common shocks are shocks that affect other households as well.

Appendix L

Table 37: Effects of economic shocks on the probability that the child is a girl

Effects on the probability that the child is a girl	(1)	(2)	(3)
Subjective health sample			
Shock at birth	.017	.040	.039
	(.029)	(.034)	(.034)
Obs.	1784	1384	1382
Anthropometric sample			
Shock at birth	.006	-.022	-.023
	(.046)	(.051)	(.051)
Obs.	789	639	639

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 the probability that the child is a girl. Columns 1, 2 and 3 include set of controls 1, 2 and 3, respectively.

Appendix M

Table 38: 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 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 39: 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 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 N

Table 40: Effects of price shocks, for at least 3 months in a given year, on objective measures of health, interacted with land ownership

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Land = 1	0.420*	0.393	0.492*	-0.226	-0.199	-0.129
	(0.223)	(0.268)	(0.285)	(0.663)	(0.681)	(0.716)
Land = 2	-0.034	0.070	0.119	1.147*	1.157*	1.202*
	(0.196)	(0.224)	(0.232)	(0.590)	(0.629)	(0.666)
Price shock	0.127	0.152	0.171	0.061	-0.168	-0.176
	(0.321)	(0.349)	(0.351)	(1.033)	(1.072)	(1.076)
Land = 1 × Price shock	-0.351	-0.351	-0.441	0.002	-0.063	-0.109
	(0.428)	(0.453)	(0.451)	(1.327)	(1.391)	(1.392)
Land = 2 × Price shock	-0.661	-0.709	-0.706	-1.410	-0.922	-0.917
	(0.453)	(0.485)	(0.486)	(1.450)	(1.540)	(1.551)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Price shock is a dummy variable that takes the value 1 if respondents have experiences a 50% deviation or more in the price of corn grain relative to the trend for at least 3 months in a given year. Land is measured in m^2 and is split in tertiles (0, 1 and 2). The reference category is a child who did not experience a price shock at birth and has grown up in a household that owns little land (Land=0).

Table 41: Effects of price shocks (absolute), for at least 1 month in a given year, on objective measures of health, interacted with land ownership

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Land = 1	0.538*	0.288	0.361	-0.457	-0.962	-0.916
	(0.291)	(0.396)	(0.410)	(0.893)	(0.979)	(1.011)
Land = 2	0.248	0.201	0.263	1.610*	1.173	1.203
	(0.270)	(0.326)	(0.332)	(0.820)	(0.855)	(0.884)
Price shock (absolute)	0.138	-0.124	-0.163	0.283	-0.484	-0.497
	(0.362)	(0.413)	(0.419)	(1.110)	(1.124)	(1.139)
Land = 1 × Price shock (absolute)	-0.312	0.024	0.021	0.395	1.133	1.133
	(0.350)	(0.431)	(0.434)	(1.122)	(1.189)	(1.192)
Land = 2 × Price shock (absolute)	-0.682**	-0.427	-0.461	-1.217	-0.305	-0.297
	(0.334)	(0.377)	(0.381)	(1.059)	(1.097)	(1.103)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Price shock is a dummy variable that takes the value 1 if respondents have experienced a deviation from the trend in price of grain of more than MKW 10 in a given year. Land is measured in m^2 and is split in tertiles (0, 1 and 2). The reference category is a child who did not experience a price shock at birth and has grown up in a household that owns little land (Land=0).

Table 42: Effects of price shocks (absolute), for at least 3 months in a given year, on objective measures of health, interacted with land ownership

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Land = 1	0.511*	0.384	0.496	-0.766	-0.898	-0.848
	(0.263)	(0.355)	(0.373)	(0.845)	(0.956)	(0.994)
Land = 2	0.245	0.318	0.400	1.004	0.710	0.738
	(0.264)	(0.315)	(0.321)	(0.769)	(0.819)	(0.847)
Price shock (absolute)	0.231	0.153	0.145	-0.654	-1.219	-1.209
	(0.319)	(0.347)	(0.352)	(1.024)	(1.062)	(1.072)
Land = 1 × Price shock (absolute)	-0.313	-0.144	-0.213	1.017	1.150	1.134
	(0.331)	(0.394)	(0.400)	(1.041)	(1.131)	(1.139)
Land = 2 × Price shock (absolute)	-0.740**	-0.666*	-0.733**	-0.094	0.568	0.574
	(0.322)	(0.357)	(0.362)	(1.023)	(1.068)	(1.074)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Price shock is a dummy variable that takes the value 1 if respondents have experienced a deviation from the trend in price of grain of more than MKW 10 for at least 3 months in a given year. Land is measured in m^2 and is split in tertiles (0, 1 and 2). The reference category is a child who did not experience a price shock at birth and has grown up in a household that owns little land (Land=0).

Table 43: Effects of positive price shocks on objective measures of health, interacted with land ownership

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Land = 1	0.270	0.153	0.211	-0.729	-0.693	-0.640
	(0.260)	(0.318)	(0.320)	(0.749)	(0.808)	(0.833)
Land = 2	-0.003	-0.030	0.017	1.112*	1.325*	1.364*
	(0.227)	(0.264)	(0.265)	(0.660)	(0.755)	(0.780)
Positive price shock	-0.009	-0.213	-0.246	0.306	-0.401	-0.413
	(0.290)	(0.338)	(0.336)	(0.938)	(0.979)	(0.984)
Land = 1 × Positive price shock	0.165	0.344	0.368	1.167	1.120	1.112
	(0.373)	(0.432)	(0.428)	(1.103)	(1.155)	(1.156)
Land = 2 × Positive price shock	-0.353	-0.071	-0.087	-0.468	-0.784	-0.788
	(0.331)	(0.354)	(0.355)	(1.033)	(1.042)	(1.045)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Positive price shock is a dummy variable that takes the value 1 if respondents have experience a 50% increase or more in the price of corn grain relative to the trend in a given year. Land is measured in m^2 and is split in tertiles (0, 1 and 2). The reference category is a child who did not experience a price shock at birth and has grown up in a household that owns little land (Land=0).

Table 44: Effects of negative price shocks on objective measures of health, interacted with land ownership

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Land = 1	0.475**	0.388	0.478	0.283	0.156	0.217
	(0.230)	(0.276)	(0.290)	(0.699)	(0.709)	(0.744)
Land = 2	0.008	0.144	0.185	1.208*	1.221*	1.267*
	(0.200)	(0.230)	(0.239)	(0.625)	(0.662)	(0.695)
Negative price shock	0.231	0.293	0.312	0.255	0.876	0.870
	(0.300)	(0.337)	(0.336)	(0.939)	(1.041)	(1.049)
Land = 1 × Negative price shock	-0.515	-0.331	-0.393	-1.943	-1.223	-1.227
	(0.460)	(0.512)	(0.505)	(1.207)	(1.311)	(1.324)
Land = 2 × Negative price shock	-0.656	-0.834*	-0.827*	-1.178	-0.806	-0.810
	(0.416)	(0.464)	(0.462)	(1.210)	(1.349)	(1.355)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Negative price shock is a dummy variable that takes the value 1 if respondents have experience a 50% drop or more in the price of corn grain relative to the trend in a given year. Land is measured in m^2 and is split in tertiles (0, 1 and 2). The reference category is a child who did not experience a price shock at birth and has grown up in a household that owns little land (Land=0).

Table 45: Effects of price shocks (absolute) on objective measures of health, interacted with corn specialization

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Corn specialization	0.348	0.463	0.440	0.880	0.913	0.897
	(0.286)	(0.354)	(0.345)	(0.720)	(0.797)	(0.796)
Price shock (absolute)	0.008	-0.083	-0.145	0.176	-0.252	-0.269
	(0.339)	(0.384)	(0.390)	(0.968)	(1.041)	(1.055)
Corn specialization × Price shock (absolute)	-0.814**	-0.841**	-0.792**	-0.947	-0.784	-0.766
	(0.344)	(0.385)	(0.381)	(1.002)	(1.062)	(1.080)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Price shock (absolute) is a dummy variable that takes the value 1 if respondents have experienced a 50% deviation or more in the price of corn grain relative to the trend in a given year. Corn specialization is a dummy variable that takes the value 1 if the share of the production of corn corresponds to at least 50% of the total value of the household crop production, conditioning on the household being in the two highest land ownership tertiles. The reference category is a child who did not experience a price shock at birth and has grown up in a household that did not specialize in corn production and possess little land.

Table 46: Effects of positive price shocks on objective measures of health, interacted with corn specialization

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Corn specialization	0.029	0.170	0.152	0.580	0.804	0.791
	(0.265)	(0.315)	(0.307)	(0.683)	(0.803)	(0.798)
Positive price shock	-0.022	-0.039	-0.085	0.569	-0.262	-0.279
	(0.275)	(0.325)	(0.327)	(0.945)	(0.877)	(0.886)
Corn specialization × Positive price shock	-0.363	-0.553	-0.499	-0.570	-0.908	-0.892
	(0.386)	(0.423)	(0.419)	(0.974)	(1.055)	(1.060)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Positive price shock is a dummy variable that takes the value 1 if respondents have experienced a 50% increase or more in the price of corn grain relative to the trend in a given year. Corn specialization is a dummy variable that takes the value 1 if the share of the production of corn corresponds to at least 50% of the total value of the household crop production, conditioning on the household being in the two highest land ownership tertiles. The reference category is a child who did not experience a price shock at birth and has grown up in a household that did not specialize in corn production and possess little land.

Table 47: Effects of negative price shocks on objective measures of health, interacted with corn specialization

	Weight			Height		
	(1)	(2)	(3)	(4)	(5)	(6)
Corn specialization	-0.034	0.042	0.039	0.717	0.810	0.800
	(0.219)	(0.247)	(0.249)	(0.628)	(0.727)	(0.726)
Negative price shock	0.024	0.109	0.109	-0.184	0.697	0.684
	(0.290)	(0.329)	(0.329)	(0.844)	(0.922)	(0.925)
Corn specialization × Negative price shock	-0.329	-0.367	-0.338	-1.490	-1.355	-1.342
	(0.468)	(0.522)	(0.512)	(1.139)	(1.292)	(1.296)
Obs.	786	637	637	786	637	637

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 4, 2 and 5, and 3 and 6 include set of controls 1, 2 and 3, respectively. Negative price shock is a dummy variable that takes the value 1 if respondents have experienced a 50% drop or more in the price of corn grain relative to the trend in a given year. Corn specialization is a dummy variable that takes the value 1 if the share of the production of corn corresponds to at least 50% of the total value of the household crop production, conditioning on the household being in the two highest land ownership tertiles. The reference category is a child who did not experience a price shock at birth and has grown up in a household that did not specialize in corn production and possess little land.

Appendix O

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 10 and the Frisch-Waugh theorem that

$$MH = \alpha_1 MS + M\epsilon \tag{43}$$

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:

$$\hat{\alpha}_1 = (S'MS)^{-1}S'MH \tag{44}$$

where I use the idempotence of the projection matrix M and the fact that $M = M'$. Plugging in the expression for MH in 44 and rearranging yields:

$$\hat{\alpha}_1 - \alpha_1 = (S'MS)^{-1}S'M\epsilon \tag{45}$$

Taking the expectation, I then get:

$$E(\hat{\alpha}_1 - \alpha_1|X,S) = (S'MS)^{-1}S'ME(\epsilon_i|X,S) \neq 0 \tag{46}$$

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:

$$\hat{\alpha}_1 - \alpha_1 = (S'MS)^{-1}S'M\epsilon \tag{47}$$

$$\hat{\alpha}_1 - \alpha_1 = \left(\frac{S'MS}{N}\right)^{-1}\frac{S'M\epsilon}{N} \tag{48}$$

$$\hat{\alpha}_1 - \alpha_1 = \Bigg(\underbrace{\frac{S'MS}{N}}_{①}\Bigg)^{-1}\Bigg(\underbrace{\frac{S'M\nu}{N}}_{②} - \underbrace{\frac{S'M\alpha d}{N}}_{③}\Bigg) \tag{49}$$

I derive now each of the three terms on the right hand side of 49, starting with ①.

$$① = \frac{S'MS}{N} = \frac{S'[I - X(X'X)^{-1}X']S}{N} = \frac{S'S}{N} - \frac{S'X(X'X)^{-1}X'S}{N} \tag{50}$$

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

$$① \xrightarrow{p} E(S_i^2) - E(S_i x_i')E(x_i x_i')^{-1}E(S_i x_i) \tag{51}$$

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

$$①^{-1} = \left(\frac{S'MS}{N}\right)^{-1} \xrightarrow{p} [E(S_i^2) - E(S_i x_i')E(x_i x_i')^{-1}E(S_i x_i)]^{-1} \tag{52}$$

Similarly, ② can be written as follows:

$$② = \frac{S'M\nu}{N} \xrightarrow{p} E(S_i\nu_i) - E(S_i x_i')E(x_i x_i')^{-1}E(x_i\nu_i) = E(S_i\nu_i) \tag{53}$$

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:

$$E(S_i\nu_i) = E(\nu_i \mathbb{1}(w_i'\gamma + u_i \geqslant n \cap S_i^* = 0) - \nu_i \mathbb{1}(w_i'\gamma + u_i \leqslant m \cap S_i^* = 1) + \nu_i S_i^*) \tag{54}$$

$$= E((1-p)Pr(u_i \geqslant n - w_i'\gamma)E(\nu_i|u_i \geqslant n - w_i'\gamma) - pPr(u_i \leqslant m - w_i'\gamma)E(\nu_i|u_i \leqslant m - w_i'\gamma)) \tag{55}$$

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, 55 simplifies to:

$$E(S_i\nu_i) = E[(1-p)\delta\sigma_\nu\phi(\frac{n - w_i'\gamma}{\sigma_u}) + p\delta\sigma_\nu\phi(\frac{m - w_i'\gamma}{\sigma_u})] \tag{56}$$

such that

$$② = \frac{S'M\nu_i}{N} \xrightarrow{p} E(S_i\nu_i) = E[(1-p)\delta\sigma_\nu\phi(\frac{n - w_i'\gamma}{\sigma_u}) + p\delta\sigma_\nu\phi(\frac{m - w_i'\gamma}{\sigma_u})] \tag{57}$$

Turning now to ③, I have

$$③ = \frac{\alpha S'Md}{N} = \frac{\alpha S'[I - X(X'X)^{-1}X']d}{N} \tag{58}$$

$$= \frac{\alpha S'd}{N} - \frac{\alpha S'X(X'X)^{-1}X'd}{N} \tag{59}$$

$$\xrightarrow{p} \alpha E(S_i d_i) - \alpha E(S_i x_i')E(x_i x_i')^{-1}E(d_i x_i') \tag{60}$$

This leads to:

$$plim(\hat{\alpha} - \alpha) = \frac{E[(1-p)\delta\sigma_\nu\phi(\frac{n - w_i'\gamma}{\sigma_u}) + p\delta\sigma_\nu\phi(\frac{m - w_i'\gamma}{\sigma_u})] - \alpha[E(S_i d_i) - E(S_i x_i')E(x_i x_i')^{-1}E(d_i x_i')]}{E(S_i^2) - E(S_i x_i')E(x_i x_i')^{-1}E(S_i x_i)} \tag{61}$$

Appendix P

Table 48: Effects of negative economic shocks on objective measures of health using various control groups and sets of controls

	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 Q

Table 49: 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 50: Effects of S_i' on weight, 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 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.

Appendix R

Table 51: Probabilities of experiencing negative economic shocks, allowing for misreporting and excluding the false positive report dummy in w

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. I exclude the false positive report dummy in the estimation of $\hat{p}$.

Table 52: Effects of S_i' on weight, excluding the false positive report dummy in w

Effects of economic shocks on weight		Rate of false positive 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)

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, 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. I exclude the false positive report dummy in the estimation of $\hat{p}$.

Table 53: Effects of S_i' on height, excluding the false positive report dummy in w

Effects of economic shocsk on height		Rate of false positive				
		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, 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. I exclude the false positive report dummy in the estimation of $\hat{p}$.

Table 54: Probabilities of experiencing negative economic shocks, allowing for misreporting and using region-specific interviewer shock rate in w

		Rate of false positive				
Estimates of the parameter p, $\hat{p}$		0%	1%	2%	5%	10%
Rate of false negative	0%	0.2929***	0.2712***	0.2676***	0.2457***	0.2167***
		(0.0163)	(0.0183)	(0.0180)	(0.0183)	(0.0180)
	2%	0.3238***	0.2907***	0.2806***	0.2542***	0.2130***
		(0.0194)	(0.0181)	(0.0186)	(0.0187)	(0.0193)
	5%	0.3325***	0.3043***	0.2931***	0.2656***	0.2220***
		(0.0201)	(0.0187)	(0.0190)	(0.0194)	(0.0201)
	10%	0.3464***	0.3241***	0.3127***	0.2842***	0.2378***
		(0.0208)	(0.0198)	(0.0200)	(0.0205)	(0.0214)
	20%	0.3796***	0.3643***	0.3536***	0.3244***	0.2738***
		(0.0221)	(0.0220)	(0.0222)	(0.0230)	(0.0245)

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. I use region-specific interviewer shock rate in w in the estimation of $\hat{p}$.

Table 55: Effects of S_i' on weight, using region-specific interviewer shock rate in w

Effects of economic shocks on weight		Rate of false positive				
		0%	1%	2%	5%	10%
Rate of false negative	0%	-0.3280**	-0.3384**	-0.3132*	-0.2875	-0.2471
		(0.1669)	(0.1700)	(0.1701)	(0.1793)	(0.1754)
	2%	-0.2970*	-0.2860*	-0.1634	-0.0984	0.1494
		(0.1601)	(0.1592)	(0.1742)	(0.1788)	(0.1875)
	5%	-0.2300	-0.2444	-0.1048	-0.1593	0.0984
		(0.1655)	(0.1661)	(0.1705)	(0.1767)	(0.1847)
	10%	-0.1419	-0.2346	-0.2078	-0.1326	0.0242
		(0.1650)	(0.1665)	(0.1651)	(0.1727)	(0.1821)
	20%	-0.1237	0.0288	-0.0132	-0.0083	-0.0882
		(0.1562)	(0.1522)	(0.1531)	(0.1580)	(0.1714)

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, 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. I use region-specific interviewer shock rate in w in the estimation of $\hat{p}$.

Table 56: Effects of S_i' on height, using region-specific interviewer shock rate in w

Effects of economic shocks on height		Rate of false positive				
		0%	1%	2%	5%	10%
Rate of false negative	0%	-0.5222	-0.4218	-0.3090	-0.3379	-0.9318*
		(0.5404)	(0.5408)	(0.5386)	(0.5482)	(0.5390)
	2%	-0.5556	-0.4129	-0.3917	-0.6787	0.6005
		(0.5160)	(0.5073)	(0.5135)	(0.5098)	(0.6037)
	5%	-0.4709	-0.3942	-0.0149	-0.6717	0.4303
		(0.5078)	(0.4954)	(0.5066)	(0.5127)	(0.6011)
	10%	-0.4517	-0.2846	-0.3440	-0.3990	0.3109
		(0.4984)	(0.5487)	(0.4828)	(0.4981)	(0.5894)
	20%	-0.1050	-0.3998	-0.4011	-0.2146	-0.0104
		(0.4777)	(0.5039)	(0.5064)	(0.5218)	(0.5554)

Note: Clustered standard errors at the mother level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Estimated effects of S' on height, 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. I use region-specific interviewer shock rate in w in the estimation of $\hat{p}$.

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