ESSAYS ON EARLY LIFE SHOCKS AND HUMAN CAPITAL PRODUCTION

A thesis

by

HABTAMU ALI BESHIR

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In writing this thesis, I compiled data from different sources. The second chapter uses data which come from Young Lives, a 15-year study of the changing nature of childhood poverty in Ethiopia, India, Peru and Vietnam. The third chapter employed data that is obtained from Ghana Statistical Service. The fourth chapter conducted using data from General Statistics Office of Vietnam. I owe thanks to all for making these datasets available.

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Declaration

I declare that this thesis, or any part of it, has not been submitted or will not be submitted to any other institution to get an award of any other degree. Nonetheless, a version of chapter two is already published as a working paper at the department of economics, Lancaster University, under the title: In utero seasonal food insecurity and cognitive development: Evidence from Ethiopia. Another version of it is also submitted to a journal for publication.

Dedication

To Seada and Hakeem

Chapter 1

Introduction

In the last two decades Economists have become increasingly interested in connecting adulthood (as well as later childhood) conditions to in utero and early life experiences. This is following the fetal origins hypothesis (FOH), a theory that relates in utero nutritional deprivation to chronic health conditions in adulthood (Barker, 1990). More importantly, Economists extend the fetal origins hypothesis (FOH) literature to explore the effect of early life (not just prenatal) exposure to any shock (not just nutritional) on different outcomes (not just health) observed during the life cycle (not just adulthood). The FOH literature now exploits the effects of early life exposure to famine (Almond et al., 2010; Dercon and Porter, 2014; Chen and Zhou, 2007), natural disasters (Fuller, 2014), conflicts and wars (Akresh and De Walque, 2008; Akresh et al., 2017; Singhal, 2018), and disease epidemics (Almond, 2006; Lin and Liu, 2014) on health, cognitive, education and labour market outcomes. The literature used famine, natural disasters, conflicts and wars, and disease epidemics as a natural experiment to identify the causal effect of the early life circumstances on different childhood or adulthood outcomes. These shocks are dramatic and disastrous, yet, they are rare events.

Recently, the related literature considers the impacts more moderate fetal shock. A typical example is the study of Ramadan exposure (Almond and Mazumder, 2011; Van Ewijk, 2011; Almond et al., 2015; Majid, 2015). This motivates the study of the second chapter of this thesis. In the this chapter, we study the impact of a regular and more moderate fetal shock: in utero exposure to seasonal food insecurity. In Ethiopia during the rainy/planting season households experience severe food shortages. In this study, we explore the impacts of in utero exposure to this seasonal food insecurity on maths and grade-for-age outcomes. Exploiting a unique dataset from the Young Lives Ethiopia study and applying a novel identification strategy, we estimate the effect of variation in the number of days of exposure to prenatal food insecurity on these cognitive development outcomes. We find that in utero exposure to food insecurity reduces maths and the odds of being on the correct grade. In addition, we are more interested to investigate if these effects are significantly different by gender. There are both biological (mortality selection and scarring effect that varies by gender) and behavioural (parental compensatory or reinforcing investment which may differ for sons and daughters) reasons to expect that these effects might be different by gender. We find that the effects of the exposure are significantly different for boys and girls. We argue (with supportive evidence) that boys are strongly affected by the shock due to the scarring effects that accumulate overtime. So, by studying the effects of exposure to a shock that many Ethiopians often experience, we contribute to an emerging literature seeking to identify the consequences of relatively mild, though frequent, shocks.

Does human capital production respond positively (negatively) to economic booms (busts)? Theoretically, this is ambiguous due to opposing income and substitution effects. The empirical literature also documents heterogeneous effects of economic fluctuations on human capital investments in developing countries. For instance, Jensen (2000) and Beegle et al. (2006) find income effect dominates and, thus, human capital investment improves (declines) during booms (busts). However, Duryea et al. (2007), Kruger (2007) and Shah and Steinberg (2017) document the opposite, where schooling declines (increases) and child labour increases (decreases) during economic booms (busts). Age of children and the context of countries may explain part of these heterogeneous results. Exploiting rainfall variation overtime and across provinces in India, Shah and Steinberg (2017) document that income effect dominates for for young children while substitution effect is important for school age (older) children. In the third chapter of this thesis, I also consider the differential effect of real cocoa price fluctuations on human capital production of young and old children in Ghana. Ghana is one of the major exporters of cocoa to the world market. Cocoa is a key source of income and livelihood in cocoa producing regions of Ghana. In Ghana, primary school (also junior high school) is compulsory and free. However, Senior high school is expensive. Exploiting these facts and using Ghana Living Standard Surveys (GLSS1; GLSS2; GLSS3; GLSS4; GLSS5; and GLSS6), I test the effect of exposure to contemporaneous (school age) price shock on schooling outcomes. I find that children surveyed during cocoa price boom in cocoa producing regions are significantly less likely to attend school and more likely to engage in work.

The effects are driven by impacts on primary and junior high school age children. With free education for primary and junior high children, only the substitution effect would be the driving force. Hence, cocoa price booms lead to less schooling. No effect is found for senior high school age children. With expensive cost of education for senior high children, not only substitution effect, but also income effect is at play here. The null effect found for this group may be as a result of the net effect of the two opposing forces being zero. So, substitution effect is dominant for old children as long as education cost is free.

Moreover, Barker (1990) hypothesize that access to nutrition in utero important for fetal development. The empirical literature also documents that both short-term and long-term health and socio-economic outcomes are significantly affected by access to nutrition in utero (Hoynes et al., 2011, 2016; Black et al., 2007). Inspired by the fetal origins hypothesis (FOH), in the second part of the chapter, I tried to show the effects cocoa price fluctuations on young (in utero) children. In particular, I show how income effect (more investment in nutritious consumptions due to more income during economic booms, for instance) is dominant for young (in utero) children. Using Ghana Living Standards Survey round 2 (GLSS2, 1988/89) and Ghana Education Impact Evaluation Survey (GEIES, 2003), I test the effect of in utero real cocoa price fluctuation exposure on Raven/IQ test of children 9 to 17 years old. I find that in utero cocoa price boom significantly increases Raven/IQ score. In addition, exploiting the Ghana Living Standard Surveys (GLSS1; GLSS2; GLSS3; GLSS4; GLSS5; and GLSS6), I also estimate the impact of in utero price boom on grade attainment for children of ages 6 to 17. Higher in utero real producers price of cocoa increases grade attainment.

Currently, not only the FOH literature is looking at the effect of shocks on outcomes of individuals exposed to the shock either in utero or early life, but also expanding to examining if the effects are transmitted through generations (Lee, 2014; Akresh et al., 2017). In chapter four, I study both first and second generation effects of early life exposure to the most intense aerial bombing episode in history: bombing Vietnam. I investigate the effects of the shock on the education and labour market outcomes of the first generation, and education outcomes of the second generation. I exploit 15% sample of the Vietnam Population and Housing Census and data on bombing intensity at province (also district) level. I find that exposure to bombing significantly reduces the education and labour market outcomes of the first generation. This could be due to school closure (or destruction) as a result of bombing, lack of nutrition due to food shortage or maternal stress due to the conflict. No effect is found on the second generation, though. This may be as a result of government's effort in distributing more state investments to more bombed provinces that reached the second generation on time.

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Chapter 2

In utero seasonal food insecurity and cognitive development: Evidence on gender imbalances from Ethiopia

Habtamu Ali Beshir Jean-François Maystadt

Department of Economics, Lancaster University

Abstract

Food insecurity is pervasive and highly seasonal in Ethiopia. In this study, we investigate the effect of seasonal food insecurity on child development. Exploiting the Young Lives Ethiopia dataset, we study the gender-specific impact of in utero exposure to seasonal food insecurity on cognitive development and the probability of being on the expected grade for children of age 8 up to 12. We find that at age 8 in utero exposure to food insecurity negatively affects cognitive development, only for boys. At age 12, such exposure significantly reduces cognitive development for all children, but with a significantly higher magnitude for boys. The impact is almost three times bigger compared to the one estimated for girls. Corroborated with other outcomes, we explain such gender imbalances by the accumulative nature of the scarring effect rather than the culling effect or gender differences in parental investment.

Keywords: Ethiopia; Food Insecurity; Shocks In Utero; Gender Imbalances; Cognitive Development; Human Capital.

JEL Classification: I15; O13; O15

2.1 Introduction

Early cognitive abilities play an important role in determining long-term schooling and wages (Currie and Thomas, 2001). The development of these skills begins in utero and continues to evolve over the life-cycle through a dynamic process of skill formation (Heckman, 2007). Large-scale shocks such as famine, natural disasters, and civil wars experienced during prenatal and early life environment have been found to be strong predictor of future outcomes (Almond and Currie, 2011; Currie and Vogl, 2013). Nonetheless, food shortages are much more frequent and potentially more detrimental on most children's life cycle. Each year, more people die from hunger than AIDS, malaria and tuberculosis combined (WFP, 2013).

Ethiopia is a case in point. According to FAO (2009), about 44 percent of the total population in Ethiopia were undernourished between 2004 and 2006. This could be attributed to chronic food insecurity, a pervasive phenomenon in the country. A substantial number of people in Ethiopia are facing difficulties in feeding themselves on a regular basis around the rainy and planting seasons. According to the International Food Policy Research Institute and the Ethiopian Development Research Institute, more than 25 percent of households in Tigray region, close to 30 percent of households in Oromia (the most populous region) and 25 percent of households in Southern Nations, Nationalities, and Peoples(SNNP) region reported food gaps during the rainy season in 2006. For Amhara (the second most populous) region the food gap stands at less than 20 percent (Hoddinott et al., 2011).[1] In the same year, close to 20 percent and 15 percent of households reported food gaps for 3 months and 4 months, respectively. Such chronic under-nutrition, in particular at early age, is likely to have long-term consequences in terms of health, schooling and socio-economic outcomes (Alderman et al., 2006; Miller, 2017). The positive impact of early childhood nutrition on education has also been established (Glewwe et al., 2001; Maluccio et al., 2009). The impact of prenatal exposure to seasonal food insecurity is largely unknown.

[1]The data may not be representative of the country since the information is obtained from chronically food-insecure woredas (districts).

In this study, we examine the impact of in utero exposure to seasonal food insecurity on cognitive development and grade-for-age. We exploit a unique dataset from the Young Lives Ethiopia study and apply a novel identification strategy. We estimate the effect of variation in the number of days of exposure to prenatal food insecurity on cognitive development outcomes, controlling for community and birth month fixed effects together with child and household characteristics. We find that a standard deviation increase in food insecurity exposure in utero results in lower maths achievements score at age 12 by about 0.175 standard deviations. Exposure also decreases the odds of being on the correct educational track. More importantly, we shed light on the gender-specific impact of seasonal food insecurity in utero. We find that there are significant gender imbalances. Both at ages 8 and 12, in utero shock decreases boys' maths score more severely than girls'. At age 12, we find that boys are significantly less likely to be on the right grade for their age.

Our paper directly relates to the emerging literature exploring the effect of prenatal shock on human capital development of children (Neelsen and Stratmann, 2011; Almond et al., 2015). The so called 'fetal origins' hypothesis advocated by Barker describes that conditions in utero (for instance, nutritional deficiencies) have long lasting health effects (Barker, 1990; Almond and Currie, 2011). Prenatal nutrition shocks should also have significant detrimental effects on brain development (Almond and Mazumder, 2011; Almond et al., 2015; Umana-Aponte, 2011). To establish causal effects, studies exploit famines and other shocks like natural disasters, wars, and disease epidemics as exogenous natural experiments. Almond and Currie (2011) and Currie and Vogl (2013) provide extensive review of the literature.[2] More directly related to the context of our study, there is a large number of studies investigating the impact of seasonality, price shocks and weather shocks on households vulnerability and child development in Ethiopia (Dercon, 2004; Dercon and Krishnan, 2000; Alem and Söderbom, 2012; Porter, 2012; Dercon and Porter, 2014; Hill and

[2]The literature on the long-term effect of in utero shocks has relied on rare and extreme events such as famine, war, terrorist attacks. In addition to the likely fiercer selection in utero, it has been difficult to distinguish the nutritional impact of shocks from the psychological stress associated with the shock (Currie and Vogl, 2013). We are not able to distinguish between these insults but in our case, similar to Miller (2017) and Nilsson (2017), we are more likely to directly capture the nutritional impact of shocks.

Porter, 2017; Abay and Hirvonen, 2017; Miller, 2017). However, this literature has not considered the individuals exposure to shock in utero, except for Dercon and Porter (2014) and Miller (2017). Dercon and Porter (2014) find detrimental impact of the 1984/85 Ethiopian famine on height of young adults. However, no effect is found from exposure in utero. On the contrary, Miller (2017) finds significant effects of seasonal food scarcity in utero on height at ages 8 and 12. Our paper extends Miller (2017)'s work by exploring the impact of seasonal food insecurity on cognitive development and by investigating possible gender imbalances in such an impact.

Boys have been found to be more vulnerable to shocks in utero such as famine (Almond et al., 2010; Roseboom et al., 2011; Hernández-Julián et al., 2014), conflict (Valente, 2015; Dagnelie et al., 2018), alcohol consumption (Nilsson, 2017) or a parental grief (Black et al., 2016).[3] However, the nature of gender imbalances in the effect of in utero and early life shocks on different health and socio-economic outcomes differs across existing studies. While the Great Chinese Famine has been found to be more detrimental for girls in terms of health and education (Luo et al., 2006; Mu and Zhang, 2011), stronger effects on boys have been found from famines during World War II in Greece, Germany and the Netherlands (Berg et al., 2016) and during the Dutch Potato Famine in the mid-nineteenth century (Lindeboom et al., 2010). Nilsson (2017) also finds stronger effect for boys of in utero exposure to increased alcohol availability on long-term labour market and educational outcomes and cognitive and non-cognitive ability. The differences in the results are puzzling. The use of different outcome variables and contextual differences may be behind the mixed nature of the evidence but the impact of in utero shocks on outcomes later in life may result from different mechanisms (Valente, 2015; Nilsson, 2017; Dagnelie et al., 2018). The scarring effects result from a downward shift of the entire foetal health distribution. Since male foetuses disproportionally stand at the low end of that distribution, deficiencies due to the scarring effects may accumulate overtime and explain more detrimental effects for boys later in life. On the contrary , the culling effect directly relates to selective mortality in utero. If selection in utero is

[3]The vulnerability of boys in utero is consistent with the medical literature (Shettles, 1961; Mizuno, 2000; Kraemer, 2000; Eriksson et al., 2010; Catalano et al., 2006).

significant, surviving male children would be stronger since in utero shocks have more detrimental effects on boys than girls. As a result, we may find small, or no, effects on boys. Selection effects are likely to be particularly severe for large-scale shocks such as famines and civil wars (Neelsen and Stratmann, 2011; Gørgens et al., 2012). In our case of relatively mild shocks in food insecurity, we expect the culling effect (selection in utero) to be less of a concern. Results presented in Section 2.4.1 confirm that prior. Finally, interpreting the impact of shocks in utero on later outcomes requires to consider possible compensating (or exacerbating) investments made by parents in children in response to health endowments after birth (Almond and Mazumder, 2013; Adhvaryu and Nyshadham, 2016). For instance, Ayalew (2005) finds evidence of compensating health investment in Ethiopia. However, the same author shows evidence of reinforcing investment in terms of education. In our study, we confirm Miller (2017) in finding little evidence of subsequent investment responses by parents. Therefore, our results tend to support the existence of scarring effects that accumulate overtime and dominate possible selection effects or compensating mechanisms.

The remainder of the paper is organized as follows. Section 2.2 presents data and identification strategy. In Section 2.3 and Section 2.4, we discuss the main results and discussion, respectively. Section 2.5 deals with some additional analyses that include notes on sensitivity, heterogeneity and identification threats. Finally, Section 2.6 concludes.

2.2 Data and Identification Strategy

We exploit data from the Young Lives Ethiopia (YLE) surveys. YLE is part of the Young Lives Project, an international study of childhood poverty tracking 12,000 children in four countries (Ethiopia, Peru, Vietnam, and India) over a 15-year period. The Ethiopian data originate from 20 sites located in four regions of the country and Addis Ababa, in which more than 96 percent of the Ethiopian children live. These regions include: Amhara, Oromia, Tigray, and the Southern Nations, Nationalities, and Peoples Region (SNNPR) (Figure A.1 in Appendix A.1.1). To choose the 20 sites of the study in each country, a sentinel site sampling approach was applied (Barnett et al., 2013). In Ethiopia the purposive sampling process follows the following three principles: (1) oversampling of food deficit districts (2) the profile of the selected districts/sites should reflect the diversity of the country (3) the possibility of tracking children in the future at reasonable cost. The sites in Ethiopia are selected in such a way that: first, four regional states (Amhara, Oromia, SNNPR, Tigray), and one city administration (Addis Ababa) were chosen; second, up to five woredas (districts) were selected from each region (this accounts for 20 districts in total); third, from each woreda at least one kebele (local administrative area) was selected. The selected community may be a sentinel site itself or could be combined with neighbouring communities to create a site. Finally, 100 households with a child born in 2001-2002 that constitute the younger cohort and 50 households with a child born in 1994-1995 that make up the older cohort were randomly chosen from each site.[4] The YLE survey contains information on children's health, education, schooling, time-use, feelings and attitudes, and cognitive tests. Household information includes: family background, education, consumption, social networks, livelihoods and wealth indicators. In this study, we exploit information about the so-called young cohort. The young cohort for Ethiopia comprises 1,999 children born between 2001 and 2002 in the 20 sites across the country. In the baseline survey of 2002, these children were aged between 6 and 18 months old.[5] These children were then surveyed again in 2006, 2009 and 2013 (Figure A.2 in Appendix A.1.1). We focus on 24 out of 26 communities, since two communities lack the food security information needed for our analysis.

We seek to identify the causal impact of in utero exposure to food insecurity on cognitive development and educational progression using the following ordinary least-square specification.[6] To shed light on the gender imbalances in the effect of the shock, we estimate equation (2.1) separately for boys and girls.

[4]See http://www.younglives.org.uk/content/sampling-and-attrition for details.

[5]The survey also collects similar information for the older cohort, born around 1994-1995.These children were 7-8 years old during the first round survey in 2002. We do not have birth information such as prematurity for this cohort that are essential for computing our exposure variables. Thus, this cohort cannot be exploited for our main analysis. We will nonetheless use the information about this cohort to assess the relationship between cognitive development and long-term education outcomes to shed light on the long-run significance of our results.

[6]For binary outcomes, logistic regressions are used instead.

$$Y_{idc} = \alpha_c + \theta_m + \beta \text{Exposure}_{dc} + X_{idc} + \varepsilon_{idc}, \tag{2.1}$$

where Y_{idc} is the outcome variable designated by various cognitive development measures for individual i, born on date d, in community c. Exposure$_{dc}$ is the number of days of exposure to seasonal food insecurity in utero, based on each child's date of birth.[7] In the analysis, we use the treatment variable -Exposure-Std$_{dc}$- that is obtained after Exposure$_{dc}$ is standardized to have a mean of zero and a standard deviation of one within each community to reduce the influence of communities with more severe periods of food insecurity.[8] X_{idc} denote the household and child characteristics.[9] We also introduce community and month of birth fixed effects, α_c and θ_m. Our coefficient of interest, β captures the average effect of a standard deviation change (within a community) in exposure to seasonal food insecurity on maths score and grade-for-age. Standard errors are clustered at the community level to deal with correlation within location of residence. Given the low number of communities which might underestimate intra-group correlation, we also show the robustness of our results to the use of wild bootstrapping method (Cameron et al., 2008; Cameron and Miller, 2015). We report both the robust standard errors clustered at the community level and the wild bootstrap p-values for our main results.

Our specification deals with several identification concerns. Community fixed effects deal with the threat of systematic differences across communities. For instance, food security is known to vary significantly across communities, mainly due to diverse agro-ecological zones and differences in terms of access to infrastructure. Stifel and Minten (2017) indeed find that households in Ethiopia living in remote areas are systematically more likely to be food insecure. Cognitive developments are also likely to differ across communities. We therefore not only control for household and child characteristics, X_{idc}, but also for community fixed effects, α_c. Another issue relates

[7]Similar to Miller (2017), date of birth for each child is calculated using age of child in days and the date of interview from the first survey round.

[8]In Section 2.5.2, we discuss the importance of the standardization, together with other functional assumptions (e.g. linearity). Moreover, we show the robustness of our results to using non-standardized treatment variable.

[9]To assess the risk of bad controls (Angrist and Pischke, 2009), we present our results without and with the household controls.

to the confounding role of seasonality. The season of birth has indeed been found to be a strong predictor of health during childhood and later life outcomes (McEniry and Palloni, 2010; Lokshin and Radyakin, 2012; Buckles and Hungerman, 2013). Similarly, experiencing Ramadan fasting during pregnancy has been found to impact short-term and long-term health (Almond and Mazumder, 2011; Van Ewijk, 2011) and education outcomes (Almond et al., 2015; Majid, 2015). To deal with national seasonality effects that are unrelated to food insecurity (e.g. Ramadan, national policies), we introduce month of birth fixed effects, denoted θ_m. In Section 2.4 and Section 2.5.4, we discuss further threats to identification.

We now discuss the variables in turn. The dependent variables, designated by Y_{idc}, are maths achievement scores used to measure children's quantitative skills, and a measure of grade-for-age.[10] We define grade-for-age as a binary variable that takes 1 if a child is in the correct grade for his or her age. The YLE survey only contains completed grade. We need current grade to indicate whether the child is on one's educational expected track. We calculate the current grade level using the information on whether the child is currently enrolled and data on completed grade. Specifically, current grade is equal to completed grade plus 1 if the child is enrolled.

Panel A in Table 2.1 shows the descriptive statistics of our outcome variables, maths score and grade-for-age. As indicated in column (10) the mean values for boys and girls are not statistically different from each other. These descriptive statistics only reveal general patterns in our outcomes and nothing about the role of food insecurity exposure in utero. In our statistical analysis, we use the maths scores standardized within a sample to have a mean of 0 and a standard deviation of 1. Maths achievement tests and grade-for-age have been widely used to measure

[10] We also report results from other cognitive development measures collected by the YLE study: Early Grade Reading Assessment (EGRA) and Peabody Picture Vocabulary Test (PPVT). The Early Grade Reading Assessment (EGRA) is orally assessed only at age 8. It is implemented to measure the most basic skills for literacy acquisition in the early grades. It involves recognising letters of the alphabet, reading simple words, understanding sentences and paragraphs, and listening with comprehension. The Peabody Picture Vocabulary Test is a widely used test of receptive vocabulary. These tests are adapted to different languages spoken in the country. Difficulty levels may have changed during translation, and as a result, it is recommended that comparison must be within languages (Cueto and Leon, 2012; Singh, 2015). We cannot limit our data to a certain language in the country since the geographical concentration of languages in Ethiopia would cancel out the variation in the exposure variable. As a result, we are cautious about interpreting results from these two tests.

cognitive development and educational progression (Almond et al., 2015; Shah and Steinberg, 2017).

Table 2.1: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full sample			Boys			Girls			Mean diff(Boys-Girls)
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	P-values
Panel A: Outcome variables										
Maths score, restricted sample										
Maths Age 8	7.153	5.421	1461	7.253	5.493	768	7.043	5.342	693	0.461
Maths Age 12	10.615	6.032	1461	10.551	6.002	768	10.685	6.069	693	0.670
Maths score, unrestricted sample										
Maths Age 8	6.525	5.368	1695	6.670	5.448	878	6.370	5.280	817	0.250
Maths Age 12	10.503	6.053	1508	10.428	6.030	796	10.587	6.080	712	0.611
Grade-for-age										
Grade-for-age Age 8	0.606	0.489	1768	0.601	0.490	920	0.612	0.488	848	0.638
Grade-for-age Age 12	0.410	0.492	1757	0.393	0.489	916	0.428	0.495	841	0.135
Panel B: Exposure variable										
Exposure, 9 months	111.050	49.696	1875	111.385	48.895	970	110.691	50.564	905	0.762

Source: Young Lives Study (Survey), Ethiopia

To understand the response of parents towards children and whether it is related with exposure to the shock, we employ several parental investment outcomes. These include: an indicator to school enrolment; the number of study hours at home (including extra tuition); and an indicator to whether a child is enrolled into a private or a public school; an indicator if parents paid for school fees or tuition (last 12 months); an indicator if parents paid any medical expenditure (last 12 months); the number of meals a child had in the last 24 hours; and the total number of food variety a child experienced in the last 24 hours.[11] Panel D in Table A.1 in Appendix A.1.4 reports the descriptive statistics of these variables.

Our main variable of interest, Exposure$_{dc}$ seeks to capture seasonal food insecurity in utero, by exploiting both food security information at the community level and variations at the individual level based on the date of birth. At the community level, food insecure months are identified in the YLE community surveys, where the community leaders are asked in which months of the year food becomes harder or more expensive to obtain. The alternative is to use weather shocks as a proxy for food insecurity. However, it has been established that early life weather shocks

[11] Parents/children were asked 7 yes/no questions related to meal frequency for a child. Specifically, they were asked if the child ate any food before breakfast, breakfast, food between breakfast and midday meal, midday meal, food between midday meal and evening meal, evening meal, and food after the evening meal. We computed meal frequency for each round (age) as the sum of these frequencies. We top coded at six meals. Moreover, parents/children were asked whether the child ate different types of foods in the last 24 hours. They were asked 17 (at age 8) and 15 (at age 12) yes/no questions. We computed food variety variables for each round (age) as the sum of these frequencies. We top coded at 10 food types.

affect long-term outcomes through many channels such as maternal stress, nutritional changes, and infectious diseases (Aguilar and Vicarelli, 2011; Thai and Falaris, 2014; Rosales-Rueda, 2018; Shah and Steinberg, 2017; Rocha and Soares, 2015). Using community level reported seasonal food insecurity data instead of rainfall variability, for instance, has nonetheless an advantage of estimating the direct effect of hunger on cognitive development. One disadvantage of reported food insecurity data may be the risk of systematic reporting bias. However, the fact that we are using data collected at community level from community representatives (not at household or individual level) makes the reporting bias minimal. The food insecurity information requested from community leaders was not a measure of food insecurity experienced at personal level that can be subject to erroneous and biased reporting. In addition, community leaders were asked about months that food becomes expensive and scarce in their respective community. Since this is a recurrent occurrence, we believe community representatives would be accurate in their reporting. We use information on community-level food insecurity from the community survey that was conducted in the second round (2006). The same information was also collected in the first round (2002). However, the pattern does not correspond to the conventionally observed seasonality in Ethiopia.[12] In particular, the 2002 survey on food insecurity reports higher average relative food insecurity from October to January. But, this period coincides with post harvest in Ethiopia, and is thus characterised by relatively higher availability of food and lower prices. Thus, the information must have been reported and documented with errors. On the contrary, the food insecurity information reported in 2006 corresponds with the reality in Ethiopia. This is further corroborated by monthly food price data. Figure 2.1 depicts that relative food insecurity is reported from May to September. Figures A.7 and A.8 in Appendix A.1.3 also show that food prices both in rural and urban parts of the country are higher from May to September. This is also further confirmed by specific grain prices during 2001-2002 (see, Figures A.9; A.10; A.11, A.12 in Appendix A.1.3)

As indicated in Figure 2.1, food insecurity is more likely to be reported during the

[12] Note that using food insecurity information from 2002 community survey confirms our results but with much lower magnitude. Results are discussed in Section 2.5.2.

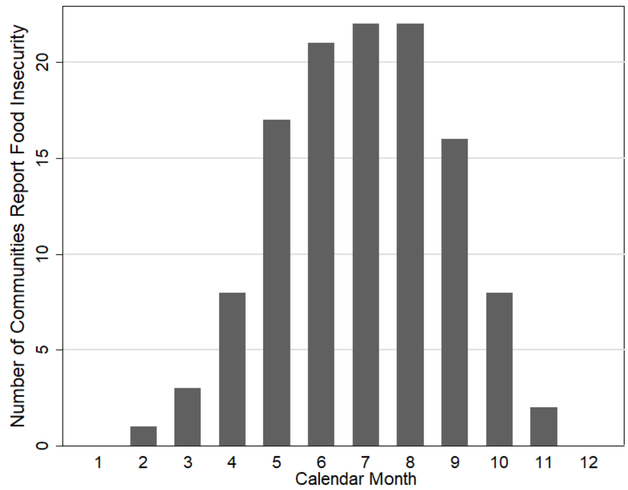

FIGURE 2.1. Reported Seasonal Food Insecurity by Calendar Month
Source: Authors' calculations using data from Young Lives Study, Ethiopia

rainy and planting periods of the main harvesting season. Such harvesting seasons vary across agro-ecological zones but the main harvesting season would usually fall from October to December. In each month from June to August, more than 20 of the surveyed communities report relative food insecurity. More than 15 of them also report relative food insecurity in May or September. The rest of the year is largely food secure. The seasonal pattern of food insecurity should not come as a surprise. In rural Ethiopia where subsistence agriculture is the prominent form of livelihood, households experience severe food shortages during the rainy/ planting season. Post harvest, farmers have usually enough food with a high level of supply associated with relatively low prices (Figures A.7 and A.8 in Appendix A.1.3). That is why we observe less food insecurity following harvests (from November to April). But when the rainy and planting seasons come, food availability decreases and pushes market prices upward, threatening food security. More than 60 percent communities report food insecurity for 4 to 5 months in a similar range to Hoddinott et al. (2011) (Figure A.3 in Appendix A.1.2).

The community-level measurement of food insecurity is then used to determine how much a child is exposed to food insecurity in utero.[13] Similar to Miller (2017), we compute the number of days a child has faced a food insecure environment while

[13]It is clear that the community level food insecurity data represent the seasonality in Ethiopia. However, do the data reflect the food insecurity situation when the children were in utero? We describe the reliability of the community-level food insecurity information in the construction of our in utero exposure in Appendix A.1.3.

he/she was in utero. One lives in utero for approximately 38 weeks or 266 days starting from conception. Premature births may be an issue here. 8.7 percent of the children in our sample are indeed born before the end of the term. We have data on the number of weeks of prematurity for only 73 percent of pre-term babies. For the remaining 27 percent, we substitute the missing observations by the median weeks of prematurity, 2 weeks. Thus, for premature babies, the number of days of exposure are calculated after adjustment is made for the reported number of weeks of prematurity. Miller (2017) adopts the same correction. As a result, our measure of food insecurity exposure in full 9 months is calculated as the number of days a child is facing food insecurity in utero from conception to birth in those 266 days of prenatal experience. The calculation of our prenatal food insecurity exposure is described in Table 2.2. Assume for example, a child is conceived in a particular community on 26 May 2001. In theory the child will be born on 16 February 2002. In this community, food is relatively unavailable in May, June, July, August and September. The child born in that community will be exposed to prenatal food insecurity for 4 months (June, July, August and September) and 6 days (from May), resulting in 126 days of prenatal food insecurity exposure. Panel B shows a child born in another community on 11 January 2002. This child will be exposed to 3 months (June, July, August) of prenatal food insecurity, resulting in 91 days of exposure.

Table 2.2: Calculating the number of days a child exposed to prenatal seasonal food shortage

Panel A, Community X									
Date	Conceived on 26 May 2001								Born on 16 Feb 2002
Month	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb
Food insecurity	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Panel B, Community Y										
Date	Conceived on 10 Apr 2001									Born on 11 Jan 2002
Month	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan
Food insecurity	No	No	Yes	Yes	Yes	No	No	No	No	No

Panel B in Table 2.1 reports the means and standard deviations of exposure in full 9 months. On average, a child has experienced 111 days (3.70 months) of food insecurity out of 266 days.[14] Panel B of Table 2.1 also show that both boys and girls

[14]In Appendix A.1.2, Figure A.4 (for all children); Figure A.5 (for boys); and Figure A.6 (for girls) also show that the histograms of number of days of in utero food insecurity exposure.

are equally affected by food insecurity in utero.

2.3 Results

Table 2.3 presents the estimated effects of in utero exposure to food insecurity on maths score and the probability of being on the correct educational track at ages 8 and 12.[15] For each outcome, the first panel presents the results without household controls, while the second panel introduces such control variables. Columns (1) and (2) provide estimates from regressions pooling boys and girls together, while the following columns contrast the results between boys (columns 3-4) and girls (columns 5-6). Column (1) in Panels A and B indicates a non-significant effect of exposure on maths score at age 8. However, columns (3) and (5) show there is a significant difference between boys and girls. While the coefficient remains non-significant for girls, maths scores for boys are between 0.09 and 0.12 standard deviation lower as a result of one standard deviation change in the exposure to food insecurity (column 3). The detrimental effects of in utero exposure seem to accumulate with age to the point where in utero exposure to food insecurity has a significant and detrimental impact on cognitive development at age 12 for both sexes. This is consistent with the idea highlighted by Heckman and Masterov (2007): disadvantages just like advantages accumulate overtime. Gender imbalances are further confirmed. At age 12, the decrease in maths score for boys by almost one third of a standard deviation (0.27-0.29, in column (4)) is significantly different from the decrease in girl's score (about 0.1 standard deviation, in column (6)). Gender imbalances are also apparent with the other outcome. At age 12, a standardised deviation increase in food insecurity exposure in utero decreases the odds of being on the correct grade for one's age, but only for boys.[16] The gender imbalances in in utero exposure echo recent findings by Nilsson (2017) of higher vulnerability of male foetuses to alcohol

[15] Detailed results of Table 2.3 including control variables are provided in Tables A.2 and A.3 of Appendix A.1.4.

[16] Gender imbalances in the effect of exposure on other tests is also apparent in Table A.4 in Appendix A.1.4. Exposure decreases reading at age 8, more significantly so for boys. Though largely we find no significant effect of the exposure on PPVT, exposure has unexpected and positive effect on girl's PPVT score at age 12. We do not, however, interpret further results from these two outcomes given the lack of accuracy of the cognitive tests in Ethiopia (see footnote 12).

consumption in utero.

Table 2.3: Estimated effect of in utero food insecurity exposure, (full pregnancy)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
Panel A: Maths without HH controls						
Exposure-Std	-0.016	-0.169***	-0.120**	-0.290***	0.071	-0.090*
	(0.027)	(0.035)	(0.056)	(0.067)	(0.047)	(0.052)
	[0.606]	[0.002]	[0.082]	[0.002]	[0.100]	[0.082]
P-value Boys=Girls (Age 8)					0.034	
P-value Boys=Girls (Age 12)						0.038
Observations	1,461	1,461	768	768	693	693
Panel B: Maths with HH controls						
Exposure-Std	-0.017	-0.175***	-0.093*	-0.268***	0.055	-0.111*
	(0.023)	(0.039)	(0.053)	(0.066)	(0.045)	(0.059)
	[0.504]	[0.002]	[0.092]	[0.004]	[0.18]	[0.05]
P-value Boys=Girls (Age 8)					0.086	
P-value Boys=Girls (Age 12)						0.089
Observations	1,441	1,441	755	755	686	686
Panel C: Grade-for-age(odds ratio) without HH controls						
Exposure-Std	0.977	0.804**	0.934	0.713**	1.029	0.860
	(0.113)	(0.086)	(0.103)	(0.119)	(0.152)	(0.166)
Observations	1,768	1,757	909	916	844	841
Panel D: Grade-for-age(odds ratio) with HH controls						
Exposure-Std	0.945	0.781**	0.920	0.701*	0.982	0.817
	(0.102)	(0.086)	(0.100)	(0.128)	(0.147)	(0.170)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. Wild bootstrap p-values in brackets. The asterisks next to the coefficients are for p-values associated with our main (non-wild bootstrap) regressions. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are standardized maths score and grade-for-age at age 8 and 12. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure) standardized to have mean 0 and standard deviation 1 with in each community. Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages.

2.4 Discussion

Three broad classes of factors may drive our results on the gender imbalances in seasonal food insecurity in utero. First, gender imbalances may be explained by the fact male foetuses are more vulnerable than girls in utero. Deficiencies in human capital development, the so-called scarring effects, may accumulate overtime. Second, the higher vulnerability of boys in utero may result in higher selective mortality in utero, the so-called culling effect and the survival of the stronger boys at and after births. Such alternative explanation would bias the coefficient downward. Third, gender discrimination is usually expected against girls in such a context. Compensating mechanisms would therefore have mitigated the gender imbalances found in the previous section.[17]

2.4.1 Mortality selection

Our sample only includes surviving children. Although our prenatal shock is of relatively mild (and frequent) nature, we cannot exclude that mortality in utero would drive our estimates towards zero. Surviving children may appear to be the strongest, the healthiest, and those with better genes. Similarly, the gender-based analysis could be biased due to differentiated mortality risk for boys and girls. The medical research indeed documents that male fetuses are more vulnerable to shocks and at greater mortality risk than female fetuses (Shettles, 1961; Mizuno, 2000; Kraemer, 2000; Catalano et al., 2006; Eriksson et al., 2010). Empirical studies also document how negative prenatal exposure could alter sex composition at birth (Almond et al., 2010; Van Ewijk, 2011; Almond and Mazumder, 2011; Valente, 2015; Nilsson, 2017; Dagnelie et al., 2018).

We cannot directly test the effect of the exposure on prenatal death differential between boys and girls. We do not have information about miscarriages and prenatal deaths. However, following Van Ewijk (2011), we test the role of selection by estimating the exposure effect on the probability of being a male at ages 1, 5, 8, and 12. We do not find strong evidence for mortality selection. Food insecurity shocks in

[17]Those mechanisms can equally be seen as threats to the general identification but help us to understand the gender imbalances. Other identification threats, with no obvious gender bias, may affect the magnitude of our coefficients. In Section 2.5.4, we therefore also examine how our results may be threatened by (1) fertility selection; (2) reporting errors; (3) the existence of other mechanisms; (4) attrition and missing data; and (5) after birth exposure. Some of these identification threats are also tested on gender-stratified samples to assess their possible consequences on the consistency of our results on the gender imbalances of seasonal food security in utero.

utero do not seem to translate into changes in the sex ratio (Table 2.4). Only at age 5, we find a positive coefficient significantly different from zero at 90 percent level of confidence. Such coefficient cannot explain the stronger detrimental impact for boys compared to girls at ages 8 and 12. So, the causal interpretation of our main results is not threatened by mortality selection in utero or after birth. Gender imbalances in cognitive development cannot be explained by selective mortality.

Table 2.4: Effect of exposure on the probability of the child surveyed is male

	Logit odds ratio			
	(1) Age 1	(2) Age 5	(3) Age 8	(4) Age 12
	Child surveyed is male, without controls			
Exposure-Std	1.108	1.112	1.108	1.105
	(0.074)	(0.076)	(0.079)	(0.078)
Observations	1,875	1,793	1,768	1,754
	Child surveyed is male, with controls			
Exposure-Std	1.094	1.109*	1.106	1.098
	(0.065)	(0.070)	(0.071)	(0.071)
Observations	1,846	1,770	1,745	1,731
Community FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is dummy indicating child born is boy. The main independent variable is standardized prenatal exposure to seasonal food insecurity (exposure in whole nine months).

2.4.2 Parental Responses vs Biological Effects

Parents may respond to in utero shocks by adapting their investment towards children either to compensate or reinforce children's endowments. If investment responses are compensatory, the effect of prenatal food insecurity shock will tend to understate biological effects. However, parents may also decide to reinforce children's endowment. In that case our baseline results may overestimate the true biological effect. Recent empirical studies reviewed in Almond and Mazumder (2013) indeed find that parental investments reinforce initial endowment differences. In our case, that would mean that parents discriminate against boys more vulnerable in utero.

That would be quite surprising given the abundant report on gender discrimination against girls in Ethiopia (Ayalew, 2005). On the contrary, compensatory investments would attenuate the established gender imbalances in the previous section.

Following Adhvaryu and Nyshadham (2016) and using the YLE survey, we assess whether the behavioural response from parents is driven by food insecurity shock in utero. Specifically, we test the effect of the shock on parental investments at ages 8 and 12 to investigate parental response once the cognitive endowment is realized.[18] In Table 2.5, we explore the role of parental investments which are directly related to education that happened at age 8 and 12.[19] Overall, we confirm the conclusions by Miller (2017) that there is limited role for parental investment. One exception is the fact the shock decreases the odds of being enrolled in school for girls at age 12. Under-investment in girls' education at age 12 would tend to attenuate the gender imbalances against boys found earlier. Such under-investment is not confirmed using the time available for study at home or the probability to be sent to a private school or expenditures on school fees or tuition (educational expenditures).

Table 2.6 reports results from parental health and nutritional investments such as: medical expenditures; meal frequency or the food variety. In this case too, we find little evidence that parents respond to the shock through health and nutritional investments. At age 12, in utero exposure to food insecurity decreases the number of meal frequency, but with no apparent significant difference between boys and girls.[20]

[18]We focus on investment carried out at ages 8 and 12. On the one hand, parents at this stage can observe the realized cognition of their children to decide to reinforce or compensate it. On the other hand, it helps us understand whether differential investment at ages 8 and 12 could explain the difference in the observed effect of the shock on cognition between ages 8 and 12.

[19]Nonetheless, in Table A.5, we also report results on investment on preschool, an educational investment that happened on or before age 5. We find no significant effect of exposure on preschool investment.

[20]Gender-specific pre-natal investment is not expected since sex detection before birth is very uncommon in Ethiopia. We nonetheless test the impact of in utero exposure on pre-natal and neo-natal (BCG) investments. We do not find any significant impact of in utero exposure to food insecurity (Table A.6). Furthermore, other sources of heterogeneity may explain why the effects accumulate overtime, indirectly shedding light on differentiated ability of households to deal with food insecurity in utero. Further heterogeneities are commented and discussed in Section 2.5.3.

Table 2.5: Childhood parental educational investments

	Full sample		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Enrolled in to school					
Exposure-Std	0.892*	0.652	0.836	0.726	1.015	0.359**
	(0.061)	(0.188)	(0.150)	(0.364)	(0.093)	(0.184)
Observations	1,629	1,398	749	666	742	437
	Panel B: Study hour at home(including extra tuition)					
Exposure-Std	-0.005	0.043	-0.003	0.034	-0.005	0.052
	(0.025)	(0.031)	(0.043)	(0.043)	(0.036)	(0.052)
Observations	1,744	1,732	904	900	840	832
	Panel C: In private school					
Exposure-Std	1.192	0.940	1.144	0.996	1.277	0.945
	(0.278)	(0.166)	(0.356)	(0.264)	(0.653)	(0.260)
Observations	757	851	269	353	323	308
	Panel D: Education expenditures (school fees or tuition)					
Exposure-Std	0.868	1.035	1.072	0.836	0.787	1.247
	(0.101)	(0.126)	(0.222)	(0.117)	(0.129)	(0.225)
Observations	1,555	1,631	782	825	724	723
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

For binary outcomes (indicators of school enrolment and type of school enrolled in to), Logit odds ratio are reported. Robust standard errors (clustered at the community level) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable are indicator to school enrolment (panel A), study hours at home (including extra tuition) (panel B), and indicator to whether a child is enrolled in to private or public school (panel C), indicator if parents paid for school fees or tuition for the child (panel D). Controls include (X): age of child in months, household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity. In the school enrolment regressions many observations are dropped because in several communities all children reported being in school.

Table 2.6: Childhood parental health and nutritional investments

	Full sample		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
Panel A: Medical expenditures						
Exposure-Std	1.145	1.140	1.097	1.214	1.203	1.094
	(0.133)	(0.163)	(0.192)	(0.328)	(0.235)	(0.283)
Observations	1,746	1,734	905	901	841	829
Panel B: Meal frequency in the last 24 hours						
Exposure-Std	0.010	-0.052*	-0.004	-0.055	0.019	-0.064
	(0.021)	(0.027)	(0.040)	(0.048)	(0.035)	(0.041)
Observations	1,746	1,728	905	897	841	831
Panel C: Food variety in the last 24 hours						
Exposure-Std	-0.054	-0.030	-0.092	0.041	-0.040	-0.077
	(0.054)	(0.073)	(0.137)	(0.124)	(0.144)	(0.078)
Observations	1,745	1,727	905	897	840	830
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

For binary outcomes (indicators of school enrolment and type of school enrolled in to), Logit odds ratio are reported. Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable are indicator if parents paid any medical expenditure to the child (panel A), the number of meals a child ate in the last 24 hours (panel B); and total number of food variety a child ate in the last 24 hours (panel C). Controls include (X): age of child in months, household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity.

2.5 Additional Analysis

In this section, we present discussions from additional results. These include: analysis of the shock by trimester; sensitivity analysis; heterogeneity analysis based on socio-economic status of households and community level access to infrastructures; and threats of identification. We present these results in Appendix A.1.4.

2.5.1 Timing of Shocks

We investigate the effect of food insecurity exposure on cognitive development by pregnancy trimester. The evidence so far is quite mixed. While the first and second trimester seem to be crucial for academic outcomes, the third trimester is especially important for short term health outcomes like birth weight. Almond et al. (2015) establish that the early stage of prenatal Ramadan experience (first and to some extent second trimester) is very important for child academic development. Schwandt (2017) finds evidence of a labour market effect of influenza exposure in the second trimester. On the contrary, Painter et al. (2005) and Schwandt (2017) identify stronger impacts resulting from shocks occurring at later stage of pregnancy (third trimester) on birth weight.

To stratify the exposure to food insecurity by trimester, we compute the number of days the child has been exposed to food insecurity in each trimester of gestation. In our study, the first and second trimesters (after conception) are 90 days each and the third trimester accounts for 86 days. All exposure variables are then standardized to have mean zero and standard deviation one within community. Table A.7 presents the estimated effects of food insecurity exposure in each trimester on maths achievements score and grade-for-age. For the pooled sample, we find significant and negative effects of exposures from all trimesters for maths score at age 12 (column (2), panel A) and only from second trimester for grade-for-age at age 12 (column (2), panel B). Specifically, a standard deviation increase in exposure to in utero food insecurity during the first or second trimester decreases the maths score at age 12 by about 0.16 standard deviations. Even though, the effect from exposures in the first and second trimester seems to be larger, a closer look in to the point estimates suggest that the effect from third trimester is not statistically different from either the first or second trimester (see p-values in column (2) panel A).

Moreover, gender imbalances are observed across all stages of gestation for both outcomes. The effects are stronger for boys similar to the baseline results. For boys' maths score at age 12, exposures from all stages of trimester are significant and the effect from the third trimester is not statistically different from the first or the second trimester. For girls' maths score, however, only the first and second trimesters are significant and the effect from the third trimester is indeed statistically different from the first or the second trimester (see p-values in column (6) panel A). For the grade-for-age outcome at age 12 the second trimester is the most important one, even though we find significant negative effect from exposure in the third trimester at age 8. By and large, food insecurity shocks from all trimesters are detrimental for later childhood cognitive development (except for girls' maths outcome).

Furthermore, it should be noted that we find significant effects from the third trimester that are comparable to first and second trimester effects (especially for pooled maths sample and boys maths sample). This suggest that, for cognitive development, not only nutritional shocks in the early stages of gestation that directly affect brain development, but also shocks from later stage of gestation (probably through affecting health outcomes such as birth weight) are important.

2.5.2 Sensitivity Analysis

We explore the robustness of our results to not standardizing the measure of exposure to food insecurity in utero (more subject to high-leverage communities); to using round 1 food insecurity information (similar to Miller (2017)) to capture seasonal food insecurity at the community level; to non-linear effects in exposure; to using maths score standardized within community similar to the exposure and also using both maths and exposure variables that are standardized within the sample as opposed to within community; and to relaxing the restriction of using only children whom we observe the maths score at all ages (rounds) (in the regressions using the maths score as a dependent variable).

Table A.8 shows the results from not standardizing the measure of exposure to food insecurity in utero. In this case, exposure is converted into monthly units by dividing the number of exposure days by 30 so that our results can be interpreted on a monthly basis. Our results are robust to not standardizing exposure to food insecurity in utero within community. However, the point estimates are larger in the case of not standardizing the measure of exposure to food insecurity. For instance, in panel B column 2, an extra month exposure to food insecurity in utero decreases maths score at age 12 by 0.13 standard deviation. This implies that a standard deviation (50 days, see Table 2.1) increase in exposure to food insecurity in utero would decrease maths score at age 12 by 0.22 standard deviation. Miller (2017) also find similar results.

Given the fact a child in our analytical sample faces on average 111 days of in utero food insecurity, a child loses on average approximately 0.49 standard deviations of maths scores at age 12 as a result of in utero shock. For boys it is equivalent to a loss of 0.75 standard deviations. These are large effects compared to other existing studies. Berhane et al. (2016), for instance, document the effect of childhood positive shock (exposure to productive safety net) and negative shock (drought) in Ethiopia on Peabody Picture Vocabulary Test (PPVT). They find that exposure to drought reduces child cognitive skills by 0.18 standard deviations, while access to safety net increases cognition by 0.18 standard deviations. We provide evidence that exposure to food shortages at the prenatal stage has a greater impact to that of childhood exposure to drought and safety net. Such detrimental impact is likely to have long-term consequences on socio-economic outcomes. Using data from the older cohort at ages 18 or 19 in round 4 (2013) and conditional on the same individual control variables, we estimate correlations between cognitive development (maths scores) at age 12 and graduating from high school or joining college at ages 18 or 19. The maths score of the older cohort was collected in round 2 (2006).[21] We

[21] The maths questionnaire used in round 2 (when older cohort were tested at age 12) has fewer questions compared to the maths questionnaire used in round 4 (when the younger cohort were tested at age 12). For the sake of comparison, we therefore convert maths scores to percentages of correct answers. One standard deviation of the maths score is equal to 24.6% for older cohort and that is equivalent to 21.5% for the younger cohort in the restricted sample (analytical sample).

find correlations of about 0.17 and 0.09 between maths score and the probability of graduating from high school or joining college, respectively. This analysis is presented in Table A.9. For boys, the correlations increase to 0.2 and 0.13, respectively. In other words, one standard deviation (24.6% among the old cohort) increase in maths score, for instance, is associated with a 17 percentage points and 9 percentage points increase in graduating from high school or joining college, respectively. Given a standard deviation in maths score is equal to 21.5 percent in our analytical sample (young cohort), the correlations are equivalent to 15 percentage points and 7.9 percentage points increases in graduating from high school or joining college, respectively. Given that a child on average has lost 0.49 (0.75 for boys) standard deviations in maths score as a result of the in utero food insecurity exposure, we can conjecture that exposed children would have a 7.4 percentage points and 3.8 percentage points lower probability of graduating from high school or joining college, respectively. For boys, this would be 11.3 percentage points and 6 percentage points lower probability of graduating from high school or joining college, respectively. We have to be cautious in interpreting these results as they are predicted from correlations rather than causal relationships between maths and long-term schooling outcomes. The predicted effects of the exposure on graduating from high school or joining college are likely to be upper bound estimates.

Table A.10 presents results from exploiting round 1 (2002) food insecurity information instead of round 2. Exposure to food insecurity has significant effect on cognitive development. The effect is more pronounced for maths test than the other outcome. However, the estimated effects in this case are much smaller (about a quarter) than the baseline.

In Table A.11, we categorize children into four groups based on the number of days they are exposed to food insecurity in utero:<60, 60-120, 120-180, >180 days. Then, defining the group with the least number of days of exposure as a reference group, we run regressions where the interest variables are now indicators of whether a child is exposed within a certain range of days (60-120 days, 120-180, or >180). The results show that the effect of the shock may be driven by children who are exposed to more than 120 days.

Our analysis is based on exposure standardized within community. This is done to reduce the influence of communities with more severe (longer) periods of food insecurity in our estimations. However, the outcome variables such as maths score are standardized within the sample. In Table A.12, we check if our results are robust in using maths score standardized within community similar to the exposure variable (in Panel A and B); and in using both maths and exposure variables standardized within the sample rather than within community (in panel C and D). In both cases, the results are similar with the baseline.

Finally, in the baseline analysis where maths score is the outcome, we restrict the sample to include only children whom we observe the outcomes of interest at all ages (rounds). In Table A.13, we relax this restriction. Even though the coefficients seem to be a little smaller compared to the baseline, the table shows that all the estimates are similar to our baseline results.

2.5.3 Further Heterogeneity

In this section, we investigate whether the impact of food insecurity in utero and in particular the gender imbalances found in this paper are conditional on socio-economic status or access to markets and roads at the community level.

According to Tables A.14 for maths score there is no heterogeneity based on household wealth. For grade-for-age outcome, however, there seems to be some evidence that children from wealthier families are less affected by the shock. In Table A.15, we find that even though the effects are not significant, closer access to market diminishes the effect of the shock. For age 12 maths and grade-for-age outcomes, albeit insignificantly, having access to market within 10 kilometre distance diminishes the negative effect of the shock. Table A.16 presents heterogeneous effect based on access to different types of road. Access to cement road slows down the negative effect of in utero shock.

2.5.4 Threats to Identification

In this section, we examine how our results may be threatened by (1) fertility selection; (2) reporting errors; (3) the existence of other mechanisms; (4) attrition and missing data and (5) after birth exposure.

A. Fertility Selection

If parents plan the timing of having children and if this is correlated with seasonal food insecurity patterns, our results may be biased. For instance, Do and Phung (2010) find that parents may give birth during good years and these planned children tend to have more years of schooling. In our case, parents may end up investing more in children whose birth was planned during less food insecure periods. Therefore, our results might not be due to exposure to food insecurity but due to unplanned pregnancies in bad times. Given that about 37 percent of pregnancies in our sample were unplanned, this may be a non-trivial issue.

Moreover, inclusion of birth month fixed effects only controls for seasonality effects that happen at the country level. However, even within community, fertility patterns may vary based on socio-economic characteristics of women. If poorer, unmarried and less educated women conceived during the period of food insecure seasons, the effect of food insecurity exposure on our outcome variables might be a result of the attributes of women rather than exposure. Indeed, studies like Buckles and Hungerman (2013) document that women that give birth in different seasons have different attributes.

To address these issues, first, we check whether the raw birth data show seasonal fertility pattern. We graph the timing of all births by calender date. Figure A.13 report the percentage of all children born in a given time. The figure shows that fertility declines in August and September of 2001 followed by a hike in the next period. Births also decrease in January-March of 2002, followed by a spike in the following period. Similar pattern is also observed in Figure A.14 and Figure A.15, which depict boys' and girls' birth date, respectively. More importantly, in all cases, there seems to be no correlation between the fertility patterns and the seasonal food insecurity data presented in Figure 2.1.

Second, similar to Lokshin and Radyakin (2012) and Miller (2017), we investigate if the unplanned pregnancies in our sample coincide with food insecure seasons. If this is true, we should find correlation between unplanned pregnancies and our measure of exposure to food insecurity. We estimate the effect of being unplanned (an indicator that takes the value of one if the pregnancy was reportedly unwanted) on the number of food insecure days in utero. A positive and significant coefficient of unplanned with large magnitude would imply unplanned pregnancies experiencing more exposure days in utero. Columns (1) and (2) of Table A.17 indicate that there is a negative weakly significant relationship between the two. Contrary to our expectations, unwanted pregnancies faced 3.4 days less exposure. So, fertility selection problem due to planning of pregnancies does not threaten the causal interpretation of our results.

Third, we explore if family characteristics influence fertility patterns and thereby food insecurity exposure within communities. Specifically, we test the correlation between household and mother characteristics and our exposure variable. We regress the days of exposure against our control variables including community fixed effects.[22] Columns (3) of Table A.17 reports that except for an indicator for mothers having attended between 5 and 8 years of education, other characteristics are insignificant. The correlation with an intermediate level of education is even of small magnitude. As a result, we may not expect substantial fertility selection. Nonetheless, the result strengthens the case for controlling for such characteristic in our main analysis.

B. Reporting Errors

Our estimates assume that within a month the timing of birth can be considered as random. One concern may be that dates of birth are reported with errors and such reporting errors would be correlated with household socio-economic characteristics. We do not have any prior on the direction of the resulting bias. To explore the importance of the issue, we estimate the probability of being born in a particular week of a month as a function of mother education and household wealth. Our dependent variable birth week has unordered structure of four responses. The appropriate model

[22]Regressing the household and mother background characteristic against the exposure to see if there is difference between women in these characteristics based on exposure also gives same result.

to estimate the relationship between birth week and household characteristics is a multinomial logit model. We have also done the same exercise for month of birth. In Table A.18, we present results from multinomial logit regression by defining four possible outcomes depending on the week a child is born within a month. In Table A.19, we report the analysis on birth of month. In both cases, we do not find any systematic evidence that being born at the beginning or at the end of a month and also at any given month is correlated with socio-economic characteristics.

C. Other Mechanisms

Our results may be driven by omitted factors that vary by month and community. We see two possibilities, either exposure to more and harder work during pregnancy, or the occurrence of Ramadan. The first concern is that mothers may engage in more physically demanding work during their pregnancy period. This may impact on more calories burned, which could in turn affect child development in utero (Strand et al., 2011; Miller, 2017). The concern is that pregnancies may coincide with seasonal variation in labour demand/supply. Labour demand/supply is seasonal in Ethiopia due to the nature of seasonality in agricultural production. The causal impact on cognitive development might be due to an increase in work requirements and the resulting stress that coincides with food insecure times rather than the direct effect of food insecurity exposure. To assess the importance of this alternative mechanism, we estimate the main specification, augmented with a proxy for exposure to work during pregnancy.[23] Panels A and B of Table A.20 show that work exposure does not have a significant impact on cognitive development. The inclusion of this auxiliary variable does not alter the main coefficients of interest that capture the impact of seasonal food insecurity.

Second, the literature tends to show that Ramadan has a detrimental effect on academic test scores (Almond et al., 2015; Majid, 2015). If the observance of Ramadan coincides with food insecure months, our results may be explained by a

[23]We use the following question to construct exposure to work in utero: "In which months of the year is there relatively more work to do?" In utero work exposure is constructed in a similar way to that of exposure to food insecurity.

higher proportion of Ramadan-exposed Muslim pregnancies during times of relative food scarcity as opposed to the exposure to seasonal food insecurity. Although the introduction of month of birth fixed effects deals with seasonality effect induced by the adoption of Ramadan at the national level, we cannot exclude the possibility that even children born the same month, may end up with different days of exposure to Ramadan.[24] Given the fact about 17 percent of children in our analytical sample are originating from Muslim households, the issue cannot be overlooked. We assess the importance of that channel by augmenting the model with a Ramadan effect. Panels C and D of Table A.20 report the results that include effects of Ramadan on the test scores. The main coefficient of interest remains virtually unchanged even after controlling for Ramadan effects.

D. Attrition and Missing Data

Attrition appears to be small in our sample. The attrition on the younger cohort between round 1 and round 4 is 2.2 percent.[25] Missing data with respect to our measures of cognitive developments is a larger concern, especially in round 4 (at age 12). In round 4, 13 percent of children have missing information on maths outcome. If the probability of having missing information is correlated with our exposure measure, our results might be biased. Moreover, the significant result that we found at age 12 might be driven by missing information on the outcome variable. In particular, if strongest children are missing (have missing outcome) by age 12 and that is systematically correlated with our exposure measure, the estimated coefficients would be biased upwards. We therefore assess if the probability to have missing data on maths score is related to exposure; and an interaction term between exposure and children's height (to measure children's strength, height at the first round of the survey is used as a proxy). Table A.21 reveals no significant correlations. Moreover, our results of the effect of exposure on maths outcome are based on a longitudinal sample where the same children are considered in all rounds. Nonetheless, not

[24]Ramadan in this paper is defined as an indicator variable equal to 1 if a child is exposed to the Ramadan fasting even for few days in utero.

[25]See http://www.younglives.org.uk/content/sampling-and-attrition and also Barnett et al. (2013).

imposing such a sample restriction does not alter our main results (Tables A.13).

E. After Birth Exposure

Our analysis only considers the effect of exposure during the 9 months of gestation. However, the seasonal nature of food insecurity in Ethiopia means that we may capture the cumulative effect of food insecurity during childhood. Children affected by the shock in utero may also be affected after birth in childhood. To assess the importance of that issue, we follow Hoynes et al. (2016) who investigate childhood exposure to participation into a safety net program. Hoynes et al. (2016) study the effect of early life exposure to safety net on long-term health and economic outcomes in the US. Individuals exposed to the introduction of safety net early in life also have been exposed to it later in childhood. So, their comparison is based on the additional number of months of safety net exposure in early life, conditional on exposure during later childhood. Similarly, given the level of exposure to food insecurity after birth, our specification identifies the effect of additional days of food insecurity exposure in utero. However, given the variation in age in months during the time of interview, we cannot be certain the coefficient of interest will only capture the effect of exposure in utero. We therefore show the robustness of our result in controlling for exposure to food insecurity from birth to interview date. Specifically, we calculate the number of days of exposure between birth and the interview date at round 3 (age 8) as well as the number of days of exposure between birth and the interview date at round 4 (age 12). As indicated in Table A.22, the effects of in utero food insecurity exposure are robust to controlling for after birth shocks (especially for Maths outcome).

2.6 Conclusions

We examine the effect of in utero seasonal food insecurity on childhood cognitive development and grade-for-age. We exploit a unique dataset from the Young Lives Ethiopia. We estimate the effect of variation in the number of days of exposure to prenatal food insecurity on these outcomes, controlling for community and birth month fixed effects together with child and household characteristics. The inclusion of community and month of birth fixed effects means our estimations are unlikely to be affected by seasonality effects, or unobserved heterogeneity at the community level (for example, climatic conditions). We find that at age 8, maths are adversely affected by in utero exposure to seasonal food insecurity, but only for boys. At age 12, gender imbalances exacerbate. At age 12, a standard deviation increase in food insecurity in utero decreases maths scores by about one third of a standard deviation for boys, almost three times the decrease observed for girls. Moreover, at age 12, we find that food insecurity in utero decreases the odds of being on the correct grade, but only for boys. Based on the lack of selective mortality in utero and weak evidence for differentiated parental investment, we conjecture that scarring effects, particularly fierce for male foetuses, accumulate overtime.

Such detrimental impacts are likely to have long-term consequences on socio-economic outcomes. Policy interventions that address seasonal food insecurity and programs that target pregnant women to enhance their resilience to seasonal food shortages could protect the development of children and minimize the long-term economic cost. Social safety net or cash transfer programs together with nutrition and micro-nutrient supplementation programs are the obvious policy options. In Ethiopia, starting from 2005, the Productive Safety Net Programme (PSNP) aims at addressing seasonal food insecurity. Unfortunately, our data do not allow us to investigate the mitigating effect of the PSNP since the sampled children were in utero between 2000 and 2002, before the implementation of the PSNP. Understanding how specific programs build resilience to seasonal food insecurity is a path for future research.

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Appendix A.1 Chapter 2 Appendix

A.1.1 Young Lives Study Area and Cohorts

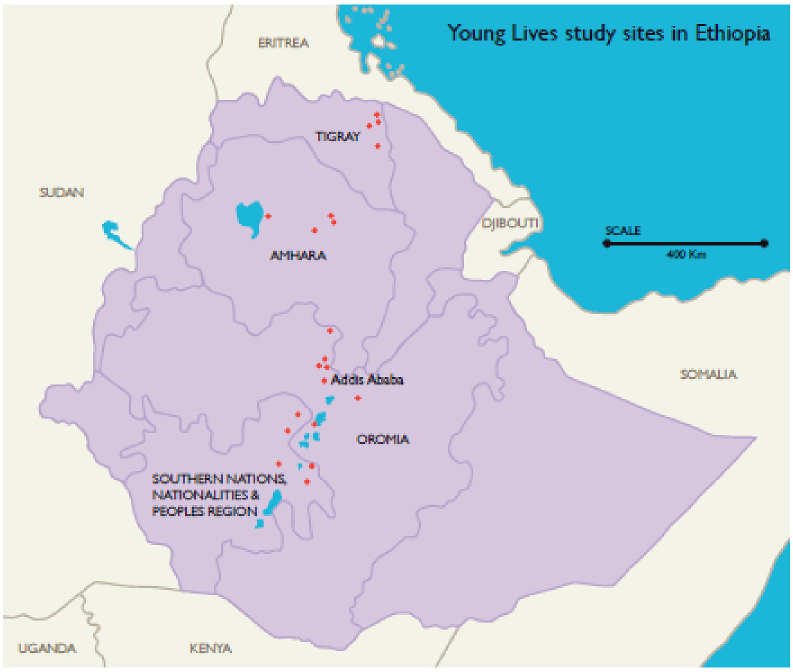

FIGURE A.1. Young Lives study sites in Ethiopia

Source: (http://www.younglives.org.uk/content/sampling-and-attrition)

FIGURE A.2. Young Lives longitudinal cohort

Source:(http://www.younglives.org.uk/content/sampling-and-attrition)

A.1.2 Intensity of Food Insecurity and Exposure

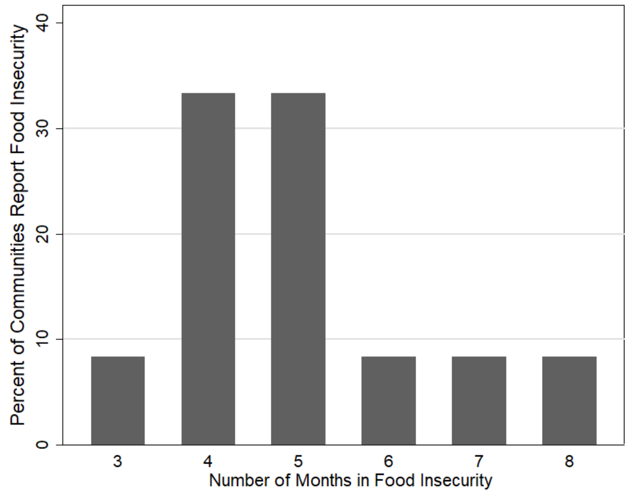

FIGURE A.3. Number of Reported Months of Seasonal Food Insecurity

Source: Authors' calculations using data from Young Lives Study, Ethiopia

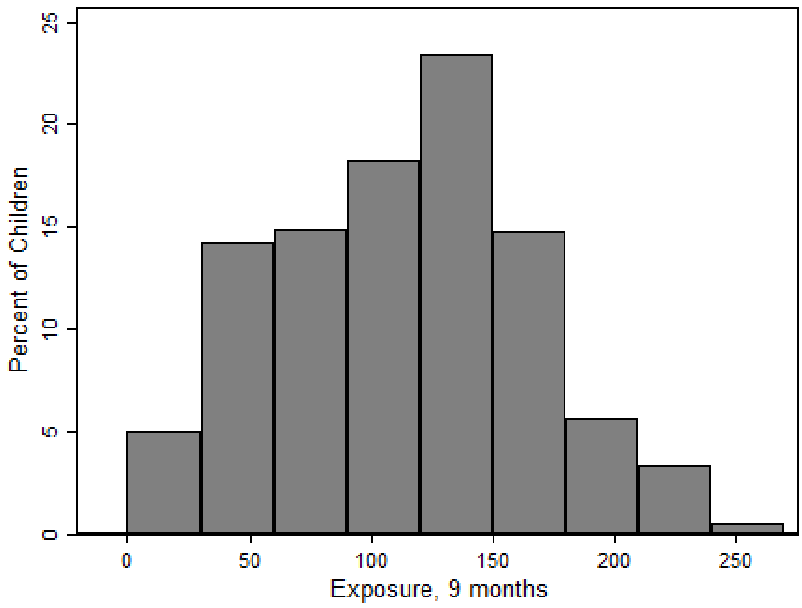

FIGURE A.4. Prenatal days exposure to reported seasonal food insecurity, all children

Source: Authors' calculations using data from Young Lives Study, Ethiopia. Histogram is calculated with 30 days window for each bin.

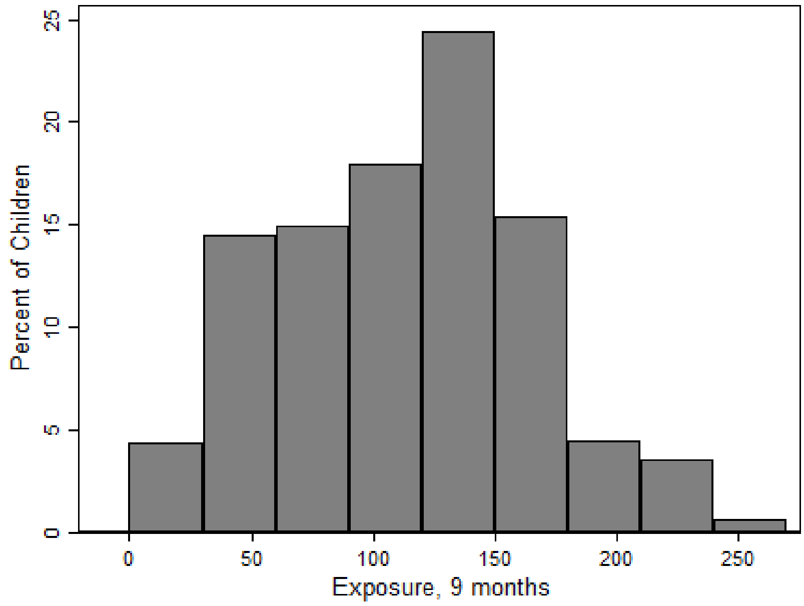

FIGURE A.5. Prenatal days exposure to reported seasonal food insecurity, boys
Source: Authors' calculations using data from Young Lives Study, Ethiopia. Histogram is calculated with 30 days window for each bin.

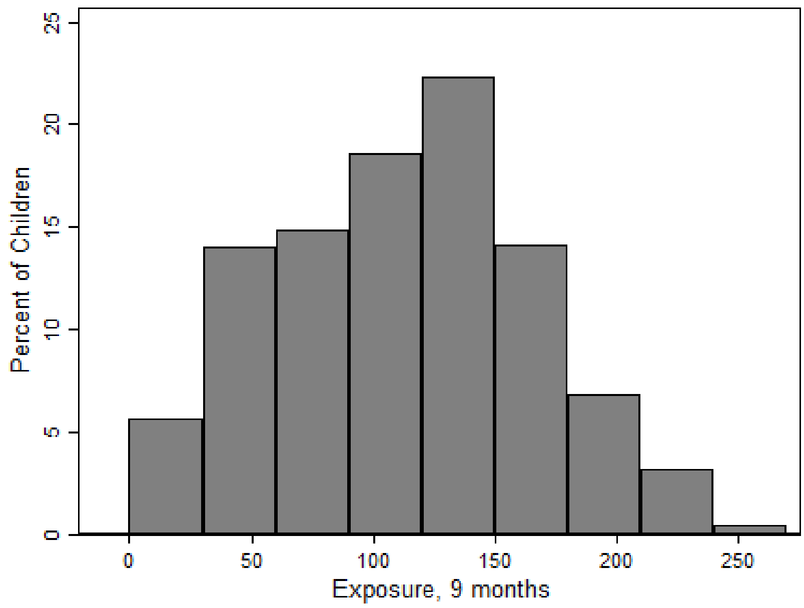

FIGURE A.6. Prenatal days exposure to reported seasonal food insecurity, girls
Source: Authors' calculations using data from Young Lives Study, Ethiopia. Histogram is calculated with 30 days window for each bin.

A.1.3 Food Insecurity Data vs Time the Children were In Utero

Do the food insecurity data reflect the time the children were in utero? Children in our sample were in utero between July, 2000 and June, 2002. The seasonal variation in food insecurity is defined from 2006 data. This gap may be a concern. However price data (Figures A.7 and A.8) confirm the repeated nature of the seasonal pattern in the country. To be more precise, we provide information on the seasonality of prices in major grains harvested in Ethiopia. We use data from Central statistical Authority of Ethiopia monthly price data.[26] We use the price data on Teff, Wheat, Barley, and Sorghum. Figures A.9; A.10; A.11, A.12 show that nationally averaged monthly prices from July, 2001 to June, 2002. The graphs show higher prices from May to October and lower from November to April. Moreover, Figure 2.1 shows many communities report food insecurity from May to September and few report from November to March. By comparing and contrasting the price information with food insecurity data, one can conclude that the variations in the prices during the period children were in utero show similar seasonality to the food insecurity data we used in this study.

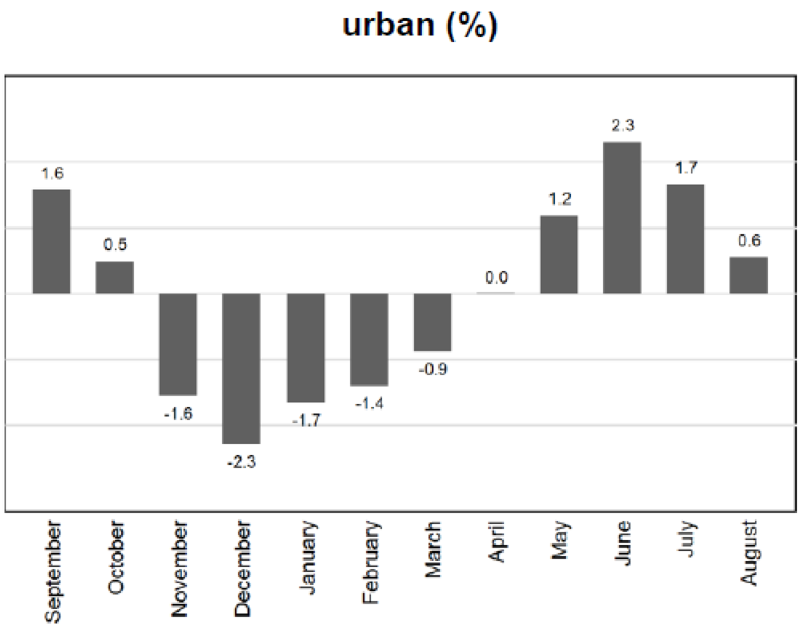

FIGURE A.7. Monthly food price deviation from annual average in urban Ethiopia
Source:Hirvonen et al. (2016). Notes: It is calculated from Central statistical Authority of Ethiopia price data spanning 2002-2011. Price deviations reflect the average monthly departures from the annual mean of the seasonal food price index.

[26]The children were in utero between July, 2000 and June, 2002. Unfortunately the price data only covers dates after July, 2001.

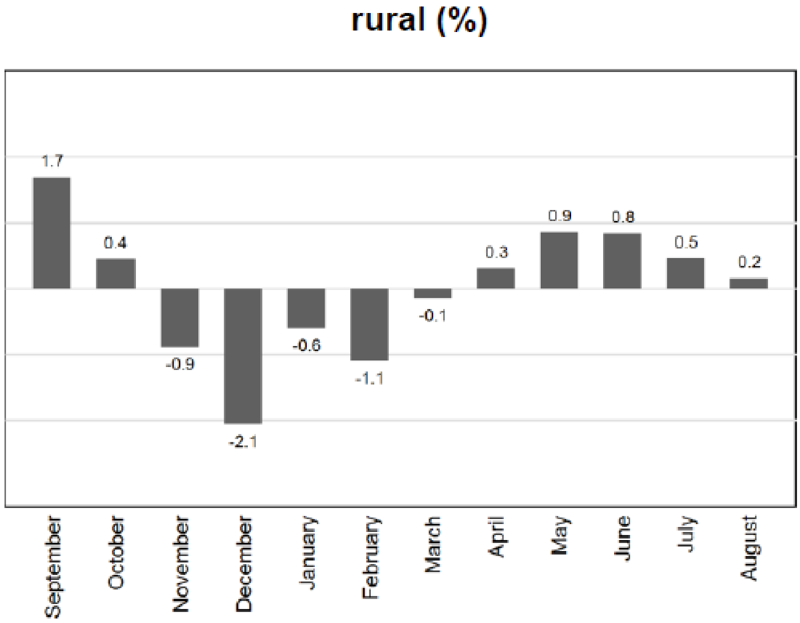

FIGURE A.8. Monthly food price deviation from annual average in rural Ethiopia
Source:Hirvonen et al. (2016). Notes: It is calculated from Central statistical Authority of Ethiopia price data spanning 2002-2011. Price deviations reflect the average monthly departures from the annual mean of the seasonal food price index.

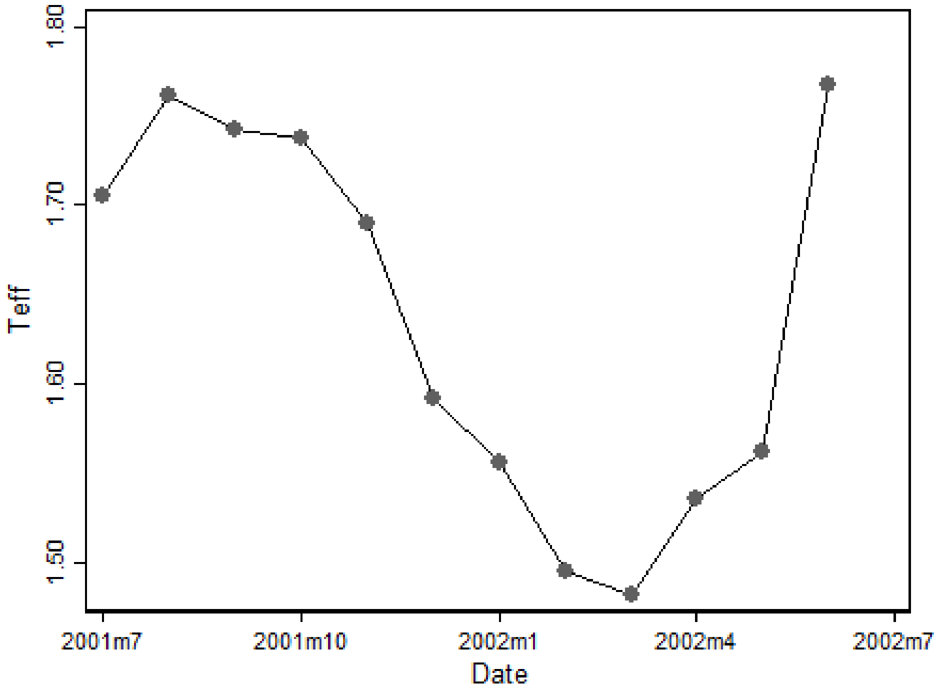

FIGURE A.9. Seasonality in the price of Teff
Source: Authors' calculation using Central statistical Authority of Ethiopia price data in 2001 and 2002.

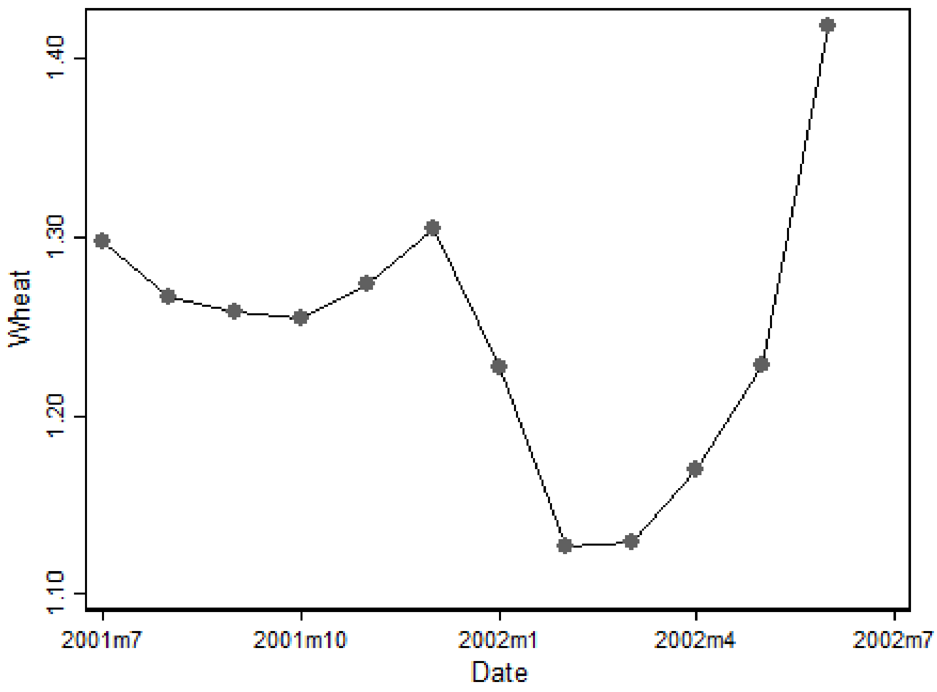

FIGURE A.10. Seasonality in the price of Wheat

Source: Authors' calculation using Central statistical Authority of Ethiopia price data in 2001 and 2002.

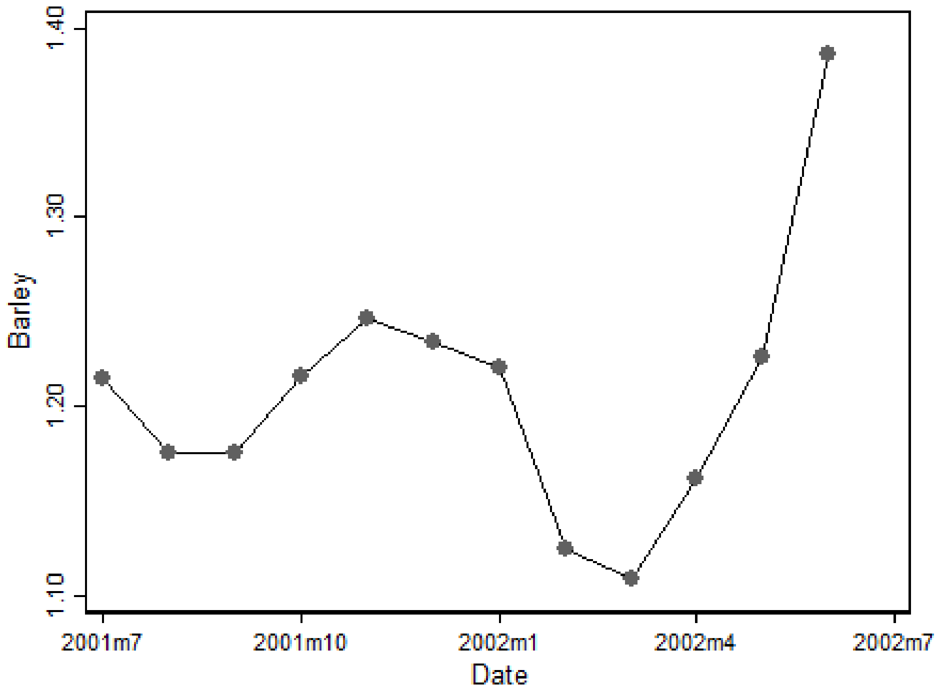

FIGURE A.11. Seasonality in the price of Barley

Source: Authors' calculation using Central statistical Authority of Ethiopia price data in 2001 and 2002.

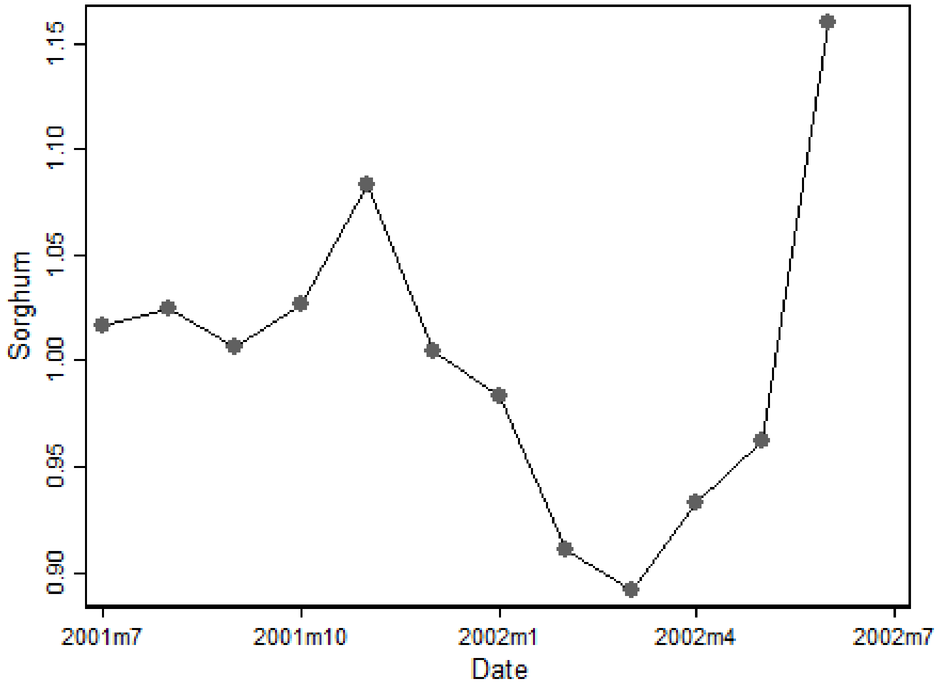

FIGURE A.12. Seasonality in the price of Sorghum

Source: Authors' calculation using Central statistical Authority of Ethiopia price data in 2001 and 2002.

A.1.4 Tables and Figures for Additional Analysis

Table A.1: Additional descriptive statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full sample			Boys			Girls		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Panel A: Other Cognitive outcomes									
EGRA Age 8	5.148	3.102	1762	5.151	3.080	915	5.145	3.127	847
PPVT Age 8	79.052	44.5	1741	79.532	44.582	903	78.535	44.431	838
PPVT Age12	38.322	8.878	1523	38.474	8.849	795	38.157	8.912	728
Panel B: Other Exposure variables									
Exposure in 1st Trimester	37.945	35.261	1875	37.951	35.520	970	37.938	35.000	905
Exposure in 2nd Trimester	37.513	36.824	1875	37.671	36.540	970	37.343	37.145	905
Exposure in 3rd Trimester	35.599	34.015	1875	35.763	34.039	970	35.424	34.008	905
Work Exposure	100.570	53.076	1875	98.186	53.343	970	103.125	52.698	905
Panel C: Household variables									
Female headed	0.141	0.348	1874	0.135	0.342	969	0.147	0.354	905
Wealth round 1	0.212	0.174	1857	0.215	0.178	958	0.209	0.170	899
Number of older siblings	2.132	1.981	1875	2.101	1.913	970	2.166	2.052	905
Mom educ. (none)	0.609	0.488	1865	0.605	0.489	966	0.614	0.487	899
Mom educ. (1 to 4 years)	0.144	0.351	1865	0.136	0.343	966	0.154	0.361	899
Mom educ. (5 to 8 years)	0.157	0.364	1865	0.164	0.370	966	0.150	0.357	899
Mom educ.(>8 years)	0.090	0.286	1865	0.096	0.295	966	0.082	0.275	899
Panel D: Individual variables									
Age round 3	97.476	3.767	1875	97.560	3.770	970	97.385	3.765	905
Age round 4	145.655	3.976	1875	145.700	3.947	970	145.608	4.008	905
Child Boy	0.517	0.500	1875	1.000	0.000	970	0.000	0.000	905
Premature	0.087	0.282	1875	0.097	0.296	970	0.076	0.266	905
Amhara	0.297	0.457	1875	0.313	0.464	970	0.278	0.448	905
Tigrian	0.234	0.424	1875	0.244	0.430	970	0.223	0.417	905
Oromo	0.180	0.385	1875	0.173	0.379	970	0.188	0.391	905
Wolayta	0.056	0.230	1875	0.049	0.217	970	0.063	0.243	905
Gurage	0.082	0.274	1875	0.073	0.261	970	0.091	0.287	905
Other	0.151	0.359	1875	0.146	0.354	970	0.157	0.364	905
Panel E: Additional variables									
Muslim	0.171	0.376	1875	0.166	0.372	970	0.176	0.381	905
Ramadan X Muslim	0.127	0.334	1875	0.121	0.326	970	0.135	0.342	905
Ramadan	0.808	0.394	1875	0.793	0.406	970	0.824	0.381	905
Unplanned	0.374	0.484	1779	0.372	0.484	932	0.377	0.485	847
Enrolled in school									
Age 8	0.764	0.425	1765	0.752	0.432	919	0.777	0.417	846
Age 12	0.943	0.232	1756	0.925	0.264	915	0.963	0.189	841
Study hour at home(including extra tuition)									
Age 8	0.977	0.861	1767	0.985	0.867	919	0.968	0.856	848
Age 12	1.404	0.874	1755	1.354	0.890	915	1.458	0.854	840
In private school									
Age 8	0.114	0.318	1166	0.121	0.326	596	0.107	0.309	570
Age 12	0.060	0.238	1654	0.065	0.247	847	0.056	0.230	807
Meal frequency (in the last 24 hours)									
Age 8	3.925	0.698	1769	3.934	0.682	920	3.916	0.715	849
Age 12	3.918	0.766	1751	3.921	0.762	912	3.914	0.771	839
Food variety (in the last 24 hours)									
Age 8	5.027	1.779	1768	5.032	1.803	920	5.021	1.755	848
Age 12	5.371	1.584	1750	5.264	1.546	912	5.487	1.617	838
Access to market									
Market with in <1 km	0.5	0.5	1600	0.49	0.5	820	0.51	0.5	780
Market with in 2 to 10 km	0.25	0.433	1600	0.246	0.431	820	0.254	0.435	780
Market with in >10 km	0.25	0.433	1600	0.263	0.441	820	0.236	0.425	780
Access to road									
Cement/tar	0.276	0.447	1809	0.289	0.454	934	0.263	0.44	875
Gravel/dirt	0.502	0.5	1809	0.482	0.5	934	0.525	0.5	875
None	0.221	0.415	1809	0.229	0.42	934	0.213	0.409	875

Source: Young Lives Study (Survey), Ethiopia

Table A.2: Estimated effect of in utero food insecurity exposure on maths outcome, with controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
Exposure-Std	-0.017	-0.175***	-0.093*	-0.268***	0.055	-0.111*
	(0.023)	(0.039)	(0.053)	(0.066)	(0.045)	(0.059)
Age round 3	0.039***		0.042***		0.039***	
	(0.010)		(0.014)		(0.014)	
Age round 4		0.030***		0.034***		0.032**
		(0.008)		(0.012)		(0.016)
Child is boy	0.003	-0.026				
	(0.045)	(0.051)				
Female headed	-0.187***	-0.240***	-0.047	-0.207***	-0.347***	-0.315***
	(0.062)	(0.048)	(0.091)	(0.068)	(0.071)	(0.078)
Wealth round 1	1.352***	1.225***	1.217***	1.408***	1.469***	0.939*
	(0.225)	(0.245)	(0.391)	(0.401)	(0.371)	(0.498)
Number of older siblings	0.004	0.002	0.006	-0.000	0.006	0.007
	(0.011)	(0.010)	(0.018)	(0.019)	(0.012)	(0.014)
Mom educ. (1 to 4 years)	0.092	0.151	0.128	0.058	0.057	0.246
	(0.083)	(0.113)	(0.084)	(0.106)	(0.104)	(0.155)
Mom educ. (5 to 8 years)	0.229***	0.219***	0.288**	0.205*	0.129	0.205*
	(0.086)	(0.070)	(0.115)	(0.115)	(0.101)	(0.110)
Mom educ.(>8 years)	0.366***	0.459***	0.376***	0.421***	0.359**	0.471***
	(0.097)	(0.084)	(0.107)	(0.122)	(0.156)	(0.156)
Premature	-0.038	-0.096*	-0.122	-0.142**	0.086	-0.006
	(0.058)	(0.057)	(0.088)	(0.068)	(0.111)	(0.107)
Amhara	0.094	-0.043	0.015	0.134	0.031	-0.240
	(0.166)	(0.104)	(0.123)	(0.145)	(0.259)	(0.165)
Tigrian	0.070	0.099	0.147	0.077	-0.010	0.057
	(0.110)	(0.109)	(0.195)	(0.149)	(0.145)	(0.143)
Oromo	0.002	-0.077	-0.063	0.168	-0.027	-0.372***
	(0.129)	(0.127)	(0.089)	(0.135)	(0.190)	(0.137)
Wolayta	-0.121	-0.362***	-0.313	-0.241	-0.045	-0.563**
	(0.176)	(0.130)	(0.335)	(0.162)	(0.230)	(0.239)
Gurage	0.242*	0.206*	0.363**	0.444***	-0.084	-0.085
	(0.128)	(0.108)	(0.176)	(0.134)	(0.170)	(0.152)
Observations	1,441	1,441	755	755	686	686
R-squared	0.082	0.082	0.085	0.102	0.111	0.090
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Estimated effect of in utero food insecurity exposure on grade-for-age (odds ratio), with controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
Exposure-Std	0.945	0.781**	0.920	0.701*	0.982	0.817
	(0.102)	(0.086)	(0.100)	(0.128)	(0.147)	(0.170)
Age round 3	0.894***		0.880**		0.896*	
	(0.032)		(0.049)		(0.054)	
Age round 4		0.927**		0.944		0.923
		(0.036)		(0.053)		(0.052)
Child is boy	0.830	0.696**				
	(0.133)	(0.120)				
Female headed	0.739	0.596**	0.842	0.665	0.692	0.487*
	(0.147)	(0.134)	(0.275)	(0.193)	(0.184)	(0.206)
Wealth round 1	12.406***	6.384**	4.375	4.995	34.086***	12.081*
	(8.433)	(5.631)	(4.281)	(6.435)	(36.821)	(17.203)
Number of older siblings	0.930*	0.970	0.945	0.971	0.908*	0.952
	(0.036)	(0.039)	(0.046)	(0.059)	(0.049)	(0.055)
Mom educ. (1 to 4 years)	1.789***	1.682**	1.631*	1.467	1.612**	1.684
	(0.288)	(0.352)	(0.458)	(0.386)	(0.319)	(0.588)
Mom educ. (5 to 8 years)	1.292	1.416	1.478	1.509	1.130	1.187
	(0.310)	(0.336)	(0.443)	(0.489)	(0.459)	(0.481)
Mom educ.(>8 years)	1.412	1.641*	1.587	2.374***	1.744	1.265
	(0.419)	(0.492)	(0.477)	(0.761)	(0.972)	(0.642)
Premature	1.035	0.892	0.761	0.653	1.591	1.436
	(0.267)	(0.229)	(0.280)	(0.198)	(0.479)	(0.600)
Amhara	0.822	0.589	1.194	0.539	0.366	0.484
	(0.313)	(0.262)	(0.357)	(0.334)	(0.295)	(0.333)
Tigrian	0.670	0.611	1.540	0.600	0.179**	0.566
	(0.387)	(0.462)	(1.345)	(0.780)	(0.138)	(0.447)
Oromo	1.195	0.906	0.744	0.677	2.128	1.165
	(0.364)	(0.342)	(0.239)	(0.447)	(1.176)	(0.616)
Wolayta	0.943	0.452**	1.088	0.151**	0.610	0.590
	(0.398)	(0.174)	(1.228)	(0.142)	(0.531)	(0.461)
Gurage	2.440	0.955	5.520*	0.663	0.791	1.026
	(1.382)	(0.448)	(4.956)	(0.535)	(0.619)	(1.062)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Estimated effect of in utero food insecurity exposure on EGRA and PPVT

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Early Grade Reading Assessment (EGRA)					
Exposure-Std	-0.055**		-0.114**		-0.006	
	(0.023)		(0.049)		(0.040)	
	[0.004]		[0.008]		[0.832]	
Observations	1,739		900		839	
	Panel B: PPVT					
Exposure-Std	0.017	0.020	-0.015	-0.047	0.056	0.084**
	(0.027)	(0.030)	(0.035)	(0.042)	(0.049)	(0.034)
Observations	1,718	1,504	888	783	830	721
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are standardized PPVT at age 8 and 12 and reading score (EGRA) at age 8. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure) standardized to have mean 0 and standard deviation 1 with in each community. Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education.

Table A.5: Preschool investments before age 5 (odds ratio)

	(1)	(2)	(3)
	Full sample	Boys	Girls
Exposure-Std	1.000	1.259	0.834
	(0.174)	(0.434)	(0.244)
Observations	1,386	687	508
Community FE	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes s
Controls	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is whether or not the child attended preschool since age 3. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure) standardized to have mean 0 and standard deviation 1 with in each community. Controls include (X): age of child in months, household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity.

Table A.6: Prenatal and neonatal investments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Prenatal		Delivery		Vaccination		
	Antenatal visit	Birth size	Formal Delivery	Assisted Delivery	BCG		
	Full sample	Full sample	Full sample	Full sample	Full sample	Boys	Girls
Exposure-Std	0.998	0.984	1.129	0.834	1.048	1.097	1.025
	(0.117)	(0.085)	(0.201)	(0.105)	(0.129)	(0.244)	(0.098)
Observations	1,790	1,781	1,639	1,680	1,691	874	795
Community FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

For binary outcomes (indicators of school enrolment and type of school enrolled in to), logit odds ratio are reported. Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable across columns are indicators to antenatal visit, birth size above average, formal delivery, assisted delivery, BCG vaccination. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure) standardized to have mean 0 and standard deviation 1 with in each community. Controls include (X): household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity.

Table A.7: Estimated effect of in utero food insecurity exposure, by trimester

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths					
First Trimester	-0.008	-0.157***	-0.071	-0.210***	0.044	-0.112**
	(0.028)	(0.038)	(0.053)	(0.062)	(0.051)	(0.056)
Second Trimester	-0.024	-0.160***	-0.080	-0.212***	0.029	-0.151*
	(0.030)	(0.047)	(0.061)	(0.064)	(0.058)	(0.080)
Third Trimester	0.011	-0.122**	-0.068	-0.243***	0.101*	0.013
	(0.033)	(0.050)	(0.051)	(0.077)	(0.054)	(0.078)
p-value First Trimester=Third Trimester	0.551	0.266	0.950	0.517	0.284	0.092
p-value Second Trimester=Third Trimester	0.389	0.552	0.814	0.605	0.247	0.056
Observations	1,441	1,441	755	755	686	686
	Panel B: Grade-for-age(odds ratio)					
First Trimester	0.917	0.864	1.035	0.832	0.852	0.935
	(0.122)	(0.130)	(0.166)	(0.168)	(0.151)	(0.203)
Second Trimester	0.989	0.690**	0.851	0.626*	1.177	0.705
	(0.104)	(0.115)	(0.095)	(0.158)	(0.179)	(0.225)
Third Trimester	0.865	0.879	0.943	0.764	0.763*	0.940
	(0.104)	(0.106)	(0.145)	(0.135)	(0.107)	(0.166)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are standardized maths score and grade-for-age at age 8 and 12. The variables of interest are standardized prenatal exposure to seasonal food insecurity (exposure at trimester level). Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages.

Table A.8: Estimated effect of in utero food insecurity exposure , round 2 exposure (non-standardized)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
Panel A: Maths without HH controls						
Exposure	-0.015	-0.129***	-0.092**	-0.221***	0.046	-0.069*
	(0.020)	(0.029)	(0.043)	(0.055)	(0.036)	(0.040)
Observations	1,461	1,461	768	768	693	693
Panel B: Maths with HH controls						
Exposure	-0.014	-0.133***	-0.070*	-0.204***	0.036	-0.084*
	(0.018)	(0.032)	(0.041)	(0.055)	(0.035)	(0.046)
Observations	1,441	1,441	755	755	686	686
Panel B: Grade-for-age (odds ratio) without HH controls						
Exposure	0.962	0.827**	0.926	0.740**	0.998	0.882
	(0.091)	(0.067)	(0.080)	(0.089)	(0.123)	(0.136)
Observations	1,768	1,757	909	916	844	841
Panel D: Grade-for-age (odds ratio) with HH controls						
Exposure	0.939	0.806***	0.914	0.725**	0.968	0.854
	(0.082)	(0.067)	(0.078)	(0.098)	(0.120)	(0.140)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are standardized maths score and grade-for-age at age 8 and 12. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure). Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages.

Table A.9: Correlation between cognition and long-term academic achievements

	(1)	(2)	(3)	(4)
	Marginal Effects			
	Graduated from high school		Went to college	
	Panel A: Full sample			
1 standard deviation=24.6 percent				
Maths age 12 (% correct)	0.162***	0.166***	0.095***	0.090***
	(0.012)	(0.013)	(0.013)	(0.014)
Observations	797	785	750	738
Outcome mean(obs, 908)	0.222		0.12	
	Panel B: Boys			
Maths age 12 (% correct)	0.198***	0.204***	0.135***	0.131***
	(0.023)	(0.025)	(0.023)	(0.023)
Observations	375	371	377	373
Outcome mean(obs, 488)	0.198		0.12	
	Panel C: Girls			
Maths age 12 (% correct)	0.175***	0.172***	0.096***	0.085***
	(0.021)	(0.023)	(0.023)	(0.027)
Observations	377	369	318	310
Outcome mean(obs, 419)	0.251		0.131	
Community FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Robust standard errors (clustered at the community level) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variables across columns are indicators that show whether a child graduates from high school (column 1 and 2) and go to college (column 3 and 4) at age 18 or 19. The independent variable is percentage correct in maths score at age 12 (standardized to have mean 0 and standard deviation 1). Controls include (X): age of child in months, household wealth index, and dummies for gender, mothers education, and child ethnicity. Some observations are dropped because in some communities there are no variations in the outcome variables (i.e. all observations have either 1 or 0 values with in those communities).

Table A.10: Estimated effect of in utero food insecurity exposure , exposure using round 1

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths without HH controls					
Exposure-miller-Std	0.001	-0.032*	-0.009	-0.066*	0.000	-0.003
	(0.021)	(0.017)	(0.033)	(0.036)	(0.033)	(0.034)
Observations	1,481	1,481	782	782	699	699
	Panel B: Maths with HH controls					
Exposure-miller-Std	-0.013	-0.044***	-0.021	-0.076**	-0.022	-0.024
	(0.023)	(0.017)	(0.032)	(0.034)	(0.033)	(0.030)
Observations	1,459	1,459	768	768	691	691
	Panel C: Grade-for-age (odds ratio) without HH controls					
Exposure-miller-Std	1.009	0.982	0.932	0.930	1.099	1.033
	(0.069)	(0.065)	(0.055)	(0.088)	(0.109)	(0.123)
Observations	1,791	1,780	938	934	849	846
	Panel D: Grade-for-age (odds ratio) with HH controls					
Exposure-miller-Std	0.997	0.947	0.939	0.894	1.052	0.993
	(0.073)	(0.063)	(0.059)	(0.090)	(0.101)	(0.128)
Observations	1,766	1,755	922	918	840	837
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are standardized maths score and grade-for-age at age 8 and 12. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure) standardized to have mean 0 and standard deviation 1 with in each community. Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages.

Table A.11: Non-linear effect of in utero food insecurity exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths					
Exposure days (60 to 120)	0.029	-0.124	-0.061	-0.168	0.108	-0.095
	(0.065)	(0.105)	(0.114)	(0.149)	(0.115)	(0.130)
Exposure days (120 to 180)	-0.041	-0.248**	-0.028	-0.242	-0.081	-0.333**
	(0.102)	(0.126)	(0.170)	(0.208)	(0.141)	(0.143)
Exposure days (>180)	-0.085	-0.426**	-0.131	-0.565*	-0.048	-0.376**
	(0.130)	(0.170)	(0.243)	(0.309)	(0.184)	(0.186)
Observations	1,441	1,441	755	755	686	686
	Panel B: Grade-for-age (odds ratio)					
Exposure days (60 to 120)	0.958	0.989	1.060	1.034	0.812	0.931
	(0.213)	(0.218)	(0.323)	(0.312)	(0.198)	(0.267)
Exposure days (120 to 180)	1.006	0.549**	0.999	0.590	1.115	0.496*
	(0.222)	(0.134)	(0.313)	(0.277)	(0.431)	(0.200)
Exposure days (>180)	1.045	0.500*	0.969	0.521	1.144	0.421
	(0.409)	(0.182)	(0.467)	(0.294)	(0.816)	(0.286)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are standardized maths score and grade-for-age at age 8 and 12. The variable of interest are dummies of prenatal exposure to seasonal food insecurity (full 9 months exposure). Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages.

Table A.12: Both Maths and exposure standardized within community (panel A and B) and Both Maths and exposure standardized within the sample (panel C and D)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths without HH controls					
Exposure-Std	-0.019	-0.190***	-0.150**	-0.318***	0.085	-0.105*
	(0.037)	(0.039)	(0.075)	(0.066)	(0.061)	(0.063)
Observations	1,461	1,461	768	768	693	693
	Panel B: Maths with HH controls					
Exposure-Std	-0.019	-0.197***	-0.114	-0.295***	0.064	-0.128*
	(0.032)	(0.044)	(0.073)	(0.068)	(0.057)	(0.073)
Observations	1,441	1,441	755	755	686	686
	Panel C: Maths without HH controls					
Exposure-Std-Sample	-0.025	-0.214***	-0.153**	-0.368***	0.077	-0.115*
	(0.034)	(0.048)	(0.071)	(0.091)	(0.060)	(0.067)
Observations	1,461	1,461	768	768	693	693
	Panel D: Maths with HH controls					
Exposure-Std-Sample	-0.024	-0.222***	-0.116*	-0.340***	0.059	-0.140*
	(0.030)	(0.053)	(0.069)	(0.092)	(0.059)	(0.077)
Observations	1,441	1,441	755	755	686	686
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in panel A is maths score at age 8 and 12 standardized with in community. The dependent variable in panel B is maths score at age 8 and 12 standardized with in the sample. The variable of interest captures prenatal exposure to seasonal food insecurity (standardized within community in panel A and standardized within the sample in panel B). Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education.

Table A.13: Estimated effect of in utero food insecurity exposure on maths outcome, not restricting the sample to those followed overtime

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
Panel A: Maths without controls						
Exposure-Std	-0.013	-0.123***	-0.074**	-0.214***	0.031	-0.076
	(0.023)	(0.032)	(0.037)	(0.051)	(0.035)	(0.048)
Observations	1,695	1,508	878	796	817	712
Panel B: Maths with HH controls						
Exposure-Std	-0.017	-0.136***	-0.071*	-0.213***	0.024	-0.101*
	(0.021)	(0.036)	(0.041)	(0.056)	(0.032)	(0.057)
Observations	1,674	1,486	865	781	809	705
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is standardized maths score at age 8 and 12. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure) standardized to have mean 0 and standard deviation 1 with in each community. Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education.

Table A.14: Estimated effect of in utero food insecurity exposure, heterogeneity by wealth

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
			Panel A: Maths			
Exposure-Std	0.009	-0.156***	-0.054	-0.248***	0.042	-0.104
	(0.031)	(0.040)	(0.056)	(0.064)	(0.060)	(0.074)
Exposure-Std X Wealth	-0.100	-0.072	-0.156	-0.083	0.048	-0.025
	(0.096)	(0.098)	(0.114)	(0.168)	(0.154)	(0.188)
Observations	1,441	1,441	755	755	686	686
			Panel B: Grade-for-age (odds ratio)			
Exposure-Std	0.958	0.838	0.940	0.646**	0.993	1.048
	(0.122)	(0.123)	(0.120)	(0.112)	(0.152)	(0.257)
Exposure-Std X Wealth	0.937	0.762	0.900	1.361	0.954	0.391*
	(0.315)	(0.246)	(0.370)	(0.659)	(0.483)	(0.213)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variables are standardized maths score and grade-for-age at age 8 and 12. The variables of interest capture standardized prenatal exposure to seasonal food insecurity measures (full 9 months exposure) and its interaction with household wealth. Controls include (X): age of child in months, household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages.

Table A.15: Estimated effect of in utero food insecurity exposure, heterogeneity by access to market

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths					
Exposure-Std	0.047	-0.200***	-0.114*	-0.314***	0.191*	-0.100
	(0.055)	(0.042)	(0.063)	(0.075)	(0.113)	(0.100)
Exposure-Std X Market<1km	-0.085	0.010	-0.038	0.036	-0.122	-0.021
	(0.056)	(0.041)	(0.059)	(0.060)	(0.102)	(0.080)
Exposure-Std X Market2-10km	-0.051	0.031	-0.006	0.036	-0.082	0.031
	(0.051)	(0.027)	(0.034)	(0.053)	(0.102)	(0.068)
Observations	1,236	1,236	649	649	587	587
	Panel B: Grade-for-age (odds ratio)					
Exposure-Std	1.100	0.765**	0.941	0.531***	1.230	0.937
	(0.162)	(0.088)	(0.170)	(0.099)	(0.242)	(0.191)
Exposure-Std X Market<1km	0.910	1.118	0.885	1.375	0.896	0.998
	(0.108)	(0.144)	(0.132)	(0.271)	(0.165)	(0.154)
Exposure-Std X Market2-10km	0.975	1.129	1.062	1.169	0.914	1.342*
	(0.103)	(0.137)	(0.147)	(0.149)	(0.113)	(0.211)
Observations	1,489	1,479	765	762	724	717
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are standardized maths score and grade-for-age at age 8 and 12. The variables of interest capture standardized prenatal exposure to seasonal food insecurity measures (full 9 months exposure) and its interaction with the types of access to market to the community of birth. Controls include (X): age of child in months, household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages. We lost many observations due to several of the communities do not have available information with regard to access to market. The comparison group is Market with in >10 km

Table A.16: Estimated effect of in utero food insecurity exposure, heterogeneity by access to road

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths					
Exposure-Std	0.006	-0.151***	-0.058	-0.248***	0.047	-0.102
	(0.035)	(0.051)	(0.068)	(0.095)	(0.042)	(0.067)
Exposure-Std X Cement	0.029	-0.001	-0.010	0.028	0.091*	0.006
	(0.052)	(0.052)	(0.080)	(0.068)	(0.053)	(0.102)
Exposure-Std X Dirt/Gravel	-0.033	-0.031	-0.069	-0.034	0.025	0.009
	(0.040)	(0.039)	(0.054)	(0.059)	(0.058)	(0.078)
Observations	1,385	1,385	726	726	659	659
	Panel B: Grade-for-age (odds ratio)					
Exposure-Std	0.996	0.772*	0.894	0.540**	1.071	0.944
	(0.091)	(0.112)	(0.097)	(0.133)	(0.184)	(0.192)
Exposure-Std X Cement	0.953	1.189	0.860	1.667*	0.928	0.839
	(0.095)	(0.218)	(0.154)	(0.478)	(0.162)	(0.174)
Exposure-Std X Dirt/Gravel	1.002	1.038	1.110	1.180	0.948	1.038
	(0.099)	(0.169)	(0.143)	(0.239)	(0.164)	(0.199)
Observations	1,686	1,675	874	870	808	805
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variables across columns are standardized maths score and grade-for-age at age 8 and 12. The variables of interest capture standardized prenatal exposure to seasonal food insecurity measures (full 9 months exposure) and its interaction with indicator of access to the type of road in the community. We categorized responses given by the community in to no road, access to gravel/dirt road, and access to cement/tar road. Controls include (X): age of child in months, household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages. The comparison group is None (no access to road)

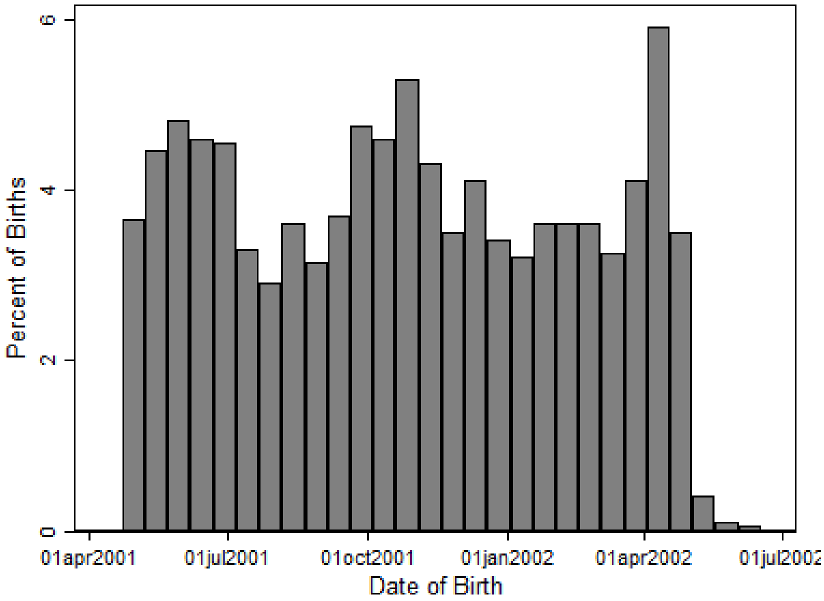

Figure A.13. Date of birth, all children
Source: Authors' calculations using data from Young Lives Study, Ethiopia. Histogram is calculated with 15 days window for each bin.

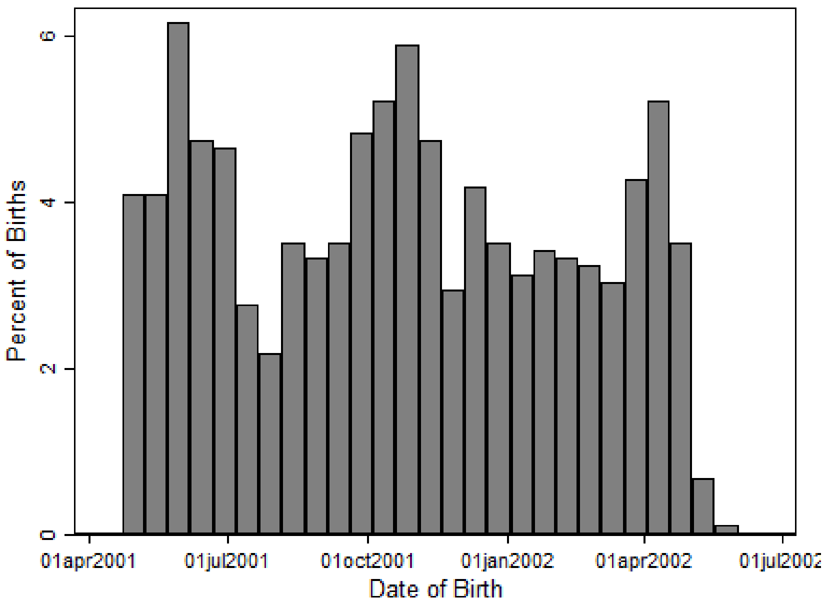

Figure A.14. Date of birth, boys
Source: Authors' calculations using data from Young Lives Study, Ethiopia. Histogram is calculated with 15 days window for each bin.

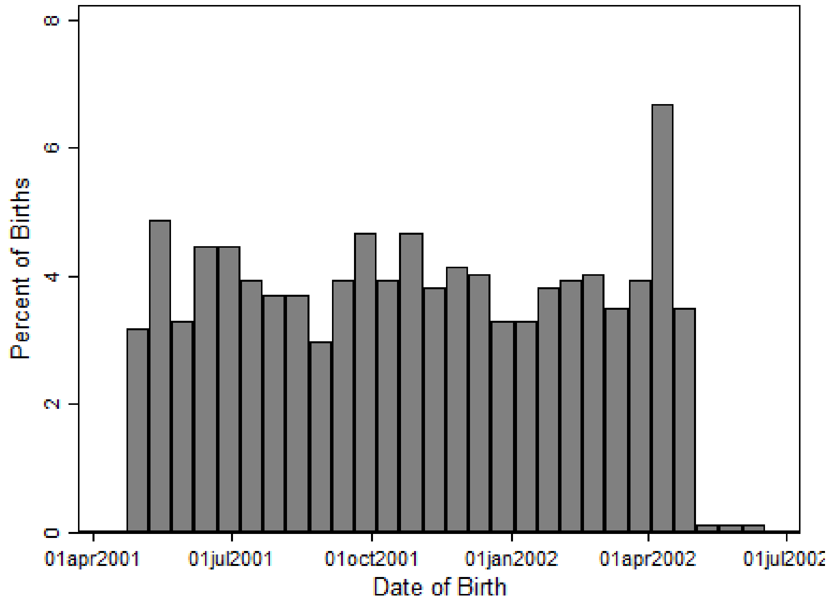

FIGURE A.15. Date of birth, girls
Source: Authors' calculations using data from Young Lives Study, Ethiopia. Histogram is calculated with 15 days window for each bin.

Table A.17: Relationship between prenatal days of exposure and individual and household characteristics

	(1)	(2)	(3)
Unplanned	-3.595*	-3.391*	
	(1.895)	(1.797)	
Wealth round 1			-6.491
			(11.495)
Number of older siblings			-0.357
			(0.575)
Mom educ. (1 to 4 years)			1.689
			(3.096)
Mom educ. (5 to 8 years)			7.673***
			(2.938)
Mom educ.(>8 years)			3.103
			(3.299)
Observations	1,779	1,761	1,846
Controls	No	Yes	Yes

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is days of exposure to seasonal food insecurity during full gestation. The main independent variables are an indicator whether the baby is desired in the first and second columns and household characteristics in the third column. Other unreported controls include: number of older siblings, and dummies for gender, gender of household head, child ethnicities, prematurity, and community .

Table A.18: Relationship between household characteristics and probability of being born in a certain week

	(1)	(2)	(3)
	Multinomial Logit		
	Born in 1st week	Born in 3rd week	Born in 4th week
	Full sample		
Wealth round 1	0.694	0.102	0.451
	(0.556)	(0.511)	(0.478)
Mom educ. (1 to 4 years)	-0.197	0.024	0.203
	(0.227)	(0.201)	(0.191)
Mom educ. (5 to 8 years)	-0.139	0.016	0.019
	(0.237)	(0.217)	(0.206)
Mom educ. (>8 years)	-0.112	-0.098	0.216
	(0.320)	(0.310)	(0.279)
Observations		1,846	
Log-likelihood value		-2509.53	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable across columns is the probability of being born in the first, second, third or fourth week of a certain month. The second week is left as a base/reference. The variables of interest are household and mother socio-economic characteristics: education of the mother and household wealth. We also controlled for number of older siblings, and set of dummies for gender, child ethnicity, and prematurity.

Table A.19: Relationship between household characteristics and probability of being born in a certain month

	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Multinomial Logit										
	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
	Full sample										
Wealth round 1	0.352	1.162	0.086	0.724	0.481	1.368	0.906	0.626	-0.386	0.564	0.535
	(0.922)	(0.947)	(0.834)	(0.860)	(0.834)	(0.902)	(0.955)	(0.885)	(0.845)	(0.886)	(0.956)
Mom educ. (1 to 4 years)	-0.023	-0.378	-0.184	-0.395	-0.366	-0.091	-0.498	-0.485	-0.135	-0.479	-0.189
	(0.371)	(0.368)	(0.324)	(0.351)	(0.345)	(0.356)	(0.384)	(0.367)	(0.351)	(0.369)	(0.370)
Mom educ. (5 to 8 years)	0.479	-0.422	-0.316	-0.356	0.433	-0.102	-0.164	0.202	0.696*	0.309	0.320
	(0.424)	(0.449)	(0.412)	(0.408)	(0.392)	(0.433)	(0.449)	(0.400)	(0.384)	(0.408)	(0.410)
Mom educ. (>8 years)	0.000	-0.461	0.129	-0.357	-0.500	-0.449	-0.314	-0.637	-0.167	-0.039	-0.030
	(0.551)	(0.565)	(0.468)	(0.488)	(0.512)	(0.533)	(0.568)	(0.543)	(0.525)	(0.499)	(0.543)
Observations						1,846					
Log-likelihood value						-4474.485					

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable across columns are the probability of being born at a certain month of the year. The first month is left as a base/reference. The variables of interest are household and mother socio-economic characteristics: education of the mother and household wealth. We also controlled for number of older siblings, and set of dummies for gender, child ethnicity, and prematurity.

Table A.20: Controlling for Work and Ramadan exposures

	(1)	(2)	(3)	(4)	(5)	(6)
	Add work exposure					
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths					
Exposure-Std	-0.020	-0.179***	-0.091*	-0.284***	0.047	-0.111*
	(0.025)	(0.037)	(0.049)	(0.063)	(0.046)	(0.057)
Work Exposure	-0.010	-0.015	0.004	-0.042	-0.037	-0.001
	(0.019)	(0.016)	(0.029)	(0.027)	(0.028)	(0.039)
Observations	1,441	1,441	755	755	686	686
	Panel B: Grade-for-age (odds ratio)					
Exposure-Std	0.942	0.761**	0.914	0.691**	0.978	0.786
	(0.101)	(0.089)	(0.099)	(0.123)	(0.149)	(0.171)
Work Exposure	0.980	0.922	0.952	0.962	0.969	0.867
	(0.059)	(0.057)	(0.066)	(0.103)	(0.097)	(0.088)
Observations	1,745	1,734	895	901	836	833
	Add Ramadan exposure					
	Panel C: Maths					
Exposure-Std	-0.018	-0.172***	-0.092*	-0.262***	0.053	-0.108*
	(0.024)	(0.039)	(0.053)	(0.066)	(0.045)	(0.059)
Ramadan X Muslim	-0.078	-0.072	-0.165	-0.109	-0.084	-0.179
	(0.106)	(0.105)	(0.183)	(0.179)	(0.159)	(0.114)
Observations	1,441	1,441	755	755	686	686
	Panel D: Grade-for-age (odds ratio)					
Exposure-Std	0.949	0.784**	0.916	0.703**	0.987	0.814
	(0.103)	(0.087)	(0.098)	(0.124)	(0.147)	(0.167)
Ramadan X Muslim	0.892	0.557	1.164	0.439	0.643	0.631
	(0.265)	(0.206)	(0.937)	(0.289)	(0.397)	(0.246)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Control	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable are standardized maths scores and grade-for-age at age 8 and 12. The variables of interest are prenatal exposure to seasonal food insecurity and work (in Panel A, B, and C) exposure to food insecurity and Ramadan (in Panel D, E, and F). In panel D, E, and F we also controlled for Muslim and Ramadan exposure dummies. Controls include (X): age of child in months, household wealth index, number of older siblings, and dummies for gender, gender of household head, mothers education, child ethnicity, prematurity. For maths outcome, we restrict the sample to children for which we observe the outcomes of interest at all age (round) stages.

Table A.21: Correlation between exposure and probability of missing

	(1) Maths missing Age 8	(2) Maths missing Age 12	(3) Maths missing Age 8	(4) Maths missing Age 12
	Panel A: Full sample			
Exposure-Std	-0.004	-0.003	0.025	-0.023
	(0.015)	(0.014)	(0.079)	(0.103)
Height Round 1 X Exposure			-0.000	0.000
			(0.001)	(0.001)
Observations	1,875	1,875	1,825	1,825
	Panel B: Boys			
Exposure-Std	0.000	-0.022	-0.147	-0.022
	(0.020)	(0.023)	(0.111)	(0.141)
Height Round 1 X Exposure			0.002	-0.000
			(0.001)	(0.002)
Observations	970	970	945	945
	Panel C: Girls			
Exposure-Std	-0.007	0.017	0.128	-0.102
	(0.014)	(0.028)	(0.135)	(0.149)
Height Round 1 X Exposure			-0.002	0.002
			(0.002)	(0.002)
Observations	905	905	880	880
Community FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable across columns is a dummy variable that shows whether the particular outcome is missing at that specific age (round). The independent variables are prenatal exposure to seasonal food insecurity and an interaction of round 1 height and the exposure measure.

Table A.22: Estimated effect of in utero food insecurity exposure, including childhood food exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Boys		Girls	
	Age 8	Age 12	Age 8	Age 12	Age 8	Age 12
	Panel A: Maths					
Exposure-Std	-0.057	-0.173***	-0.149**	-0.241***	0.024	-0.134*
	(0.036)	(0.044)	(0.071)	(0.060)	(0.064)	(0.079)
Birth to Age 8 Interview Date Exposure	-0.002*		-0.003		-0.002	
	(0.001)		(0.002)		(0.002)	
Birth to Age 12 Interview Date Exposure		0.000		0.001		-0.001
		(0.001)		(0.001)		(0.002)
Observations	1,441	1,441	755	755	686	686
	Panel B: Grade-for-age (odds ratio)					
Exposure-Std	1.020	0.867	1.042	0.797	1.081	0.893
	(0.138)	(0.121)	(0.198)	(0.178)	(0.214)	(0.197)
Birth to Age 8 Interview Date Exposure	1.003		1.005		1.004	
	(0.003)		(0.006)		(0.004)	
Birth to Age 12 Interview Date Exposure		1.006		1.007		1.005
		(0.005)		(0.007)		(0.005)
Observations	1,745	1,734	895	901	836	833
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the community level) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is standardized maths score at age 8 and 12. The variable of interest captures prenatal exposure to seasonal food insecurity (full 9 months exposure) standardized to have mean 0 and standard deviation 1 with in each community. Birth to Age 8 Interview date Exposure is seasonal food insecurity exposure between birth to age at round 3 (age 8) interview date. Birth to Age 12 Interview date Exposure is seasonal food insecurity exposure between birth to age at round 4 (age 12) interview date. Ind. controls include : age of child in months, number of older siblings, and dummies for gender, child ethnicity, prematurity. HH Controls include household wealth index, and dummies for gender of household head, and mothers education.

Chapter 3

Impacts of contemporaneous and early life price shocks on human capital production

Habtamu Ali Beshir

Department of Economics, Lancaster University

Abstract

The effect of economic shocks on human capital is theoretically ambiguous due to opposing income and substitution effects. Using child level information on schooling, child labour, and test scores and time series data on real producer price of cocoa from Ghana, in this study, I investigate the effect of cocoa price fluctuations on human capital production. In doing so, I link the fetal origins hypothesis literature with the old income vs substitution effects debate. For old (school age) children the substitution effect dominates. Contemporaneous (school age) price boom decreases schooling and increases child labour. Specifically, I find that a standard deviation increase in the current year real producer price of cocoa significantly decreases current school attendance by 8 percentage points. In addition, a standard deviation increase in the previous year real producer price of cocoa significantly decreases the likelihood of being on the correct grade in the following year by 6.7 percentage points. For young children, however, the income effect dominates. In utero real producer price of cocoa boom significantly increases Raven/IQ score and grade attainment. Specifically, a standard deviation increase in in utero real producer price of cocoa increases the correct Raven/IQ items answered by a child by 4.2 percentage points and increases completed grade by 0.38 year.

Keywords: Ghana; Cocoa Price Shocks, Child Labour; Schooling; Cognitive Development; Human Capital.

JEL Classification: I25; J1; O12

3.1 Introduction

An active literature investigates the effect of economic booms and recessions on human capital investment in developing countries. The effect of economic fluctuations on human capital investment is theoretically ambiguous due to opposing income and substitution effects. Income effect results from the fact that economic fluctuations leads to variation in available resources for households to invest on their children. Households send their children to school and invest in them more during economic booms due to budget constraint slackness. Substitution effect arises from the case that economic changes affect wages that in turn alter the opportunity cost of staying in school (for children) and caring for children (for parents). Children may leave school to join the labour market during economic booms as wage/income from the outside option improves. The effect of economic shocks on human capital investments can be pro-cyclical or countercyclical depending whether the income effect is dominant over the substitution effect. If the substitution effect dominates the income effect, there is countercyclical investment in human capital and vice versa.

Empirically, there are two broad sets of evidences that deal with the effect of economic shocks on human capita production. The first strand of the literature focuses on the effect of variation in permanent income or wealth on human capital investment (Binder, 1999; McKenzie, 2003; Schady, 2004; Cogneau and Jedwab, 2012). The second instead documents the effects of short-run or temporary economic fluctuations (Jensen, 2000; Beegle et al., 2006; Duryea et al., 2007; Kruger, 2007; Shah and Steinberg, 2017). Some of these studies find that income effect dominates: schooling increases while child labour declines (Jensen, 2000; Beegle et al., 2006). Others document that the substitution effect is particularly important: human capital investment in schooling drops as child labour increases (Duryea et al., 2007; Kruger, 2007; Shah and Steinberg, 2017). It is interesting to investigate why there is such mixed results in the literature. Could it be that the kind of shock considered matters: whether it is temporary economic fluctuations vs permanent (longer) crises? Could it be that the setting of the studies matter: whether they are based in developing vs developed countries? It may also be the case that absence/presence of credit and insurance markets to smooth out the income shocks is important (Ferreira and Schady, 2009). The specific country context (such as availability of free education) may be a factor too.

In this study, following Shah and Steinberg (2017), I argue that age of children plays an important role. For old (school age) children, both income and substitution effects are important, yet in the context of free education the substitution effect may dominate the income effect. Thus, during temporary economic booms, parents may decide not to send their children to school to put them into work and send them back to school after the economic boom is over (Kruger, 2007). For young (in utero) children, income effect may be particularly relevant. Substitution effect may also arise as a result of mothers substituting work for time intensive child care (prenatal care). However, in utero, compared to later childhood, time intensive investments (such as providing good hygiene) are not much required. Instead, in utero, income intensive investment in nutritious consumptions and prenatal medical care could be much needed and important. So, in utero economic booms translate in to higher human capital outcomes later in childhood.

In this study, I investigate the effect of exposure to contemporaneous (school age) and in utero (i.e., a year lag to year of birth) cocoa price fluctuations on children human capital development in Ghana. I focus on Ghana as it is one of the largest cocoa exporters and cocoa is a key source of household incomes in its cocoa producing regions.

In Ghana where primary and junior high school education are free (or at least cheap in comparison to senior high school education), school age price boom would drive children out of school to child labour. Exploiting Ghana Living Standard Surveys (GLSS1; GLSS2; GLSS3; GLSS4; GLSS5; and GLSS6), I test the effect of exposure to contemporaneous price shock on school attendance and grade-for-age. I estimate a difference-in-difference model where I exploit variation in the real cocoa price over time and compare those who are from cocoa producing regions with those from non-cocoa producing regions. Controlling for interview year and region fixed effects, I compare children from the same region but interviewed in different years. Children surveyed during cocoa price boom in cocoa producing regions are significantly less likely to attend school. Specifically, one standard deviation increase in the current year real producer price of cocoa significantly decreases current attendance by 8 percentage points. This is equivalent to 10% of the average attendance rate. In addition, a cocoa price boom in the previous year significantly decreases the likelihood of a child to be in the right grade for her age in the following year. A standard deviation increase in the previous year real producer price of cocoa significantly decreases the likelihood of being on the correct grade in the following year by 6.7 percentage points. This is equivalent to 30% of the average grade-for-age rate. Moreover, I show that these effects are driven by impacts on primary and junior high school age children with stronger effect on primary school age children. However, no effect is found for senior high school age children. For this group of children, education cost is very expensive. In this case, both income and substitution effect would be relevant. Cocoa price boom increases household income that can be directed towards the expensive schooling expenditures. It also increases the opportunity cost of staying in school for these children and they may leave school and join the labour force as a result. The net effect would be zero due to these two opposing effects at play.

Next, I estimate the effect of in utero cocoa price shock exposure on cognitive development outcomes: Raven/IQ test score and grade attainment. In this analysis, I follow a difference-in-difference identification strategy. Exploiting variation in real cocoa price over years, I compare children born in cocoa producing with non-cocoa producing regions. The rationality behind the identification is children of households born in cocoa producing regions during cocoa price boom experience more resources as opposed to children who are born in households from non-cocoa producing regions. Children that experience this positive income shock early in their life (in utero) will have better nutrition and investments and grow up to be healthy and acquire higher cognitive skills compared to their peers that are born in to households from non-cocoa producing regions. Controlling for region of birth and year of birth fixed effects, the specification allows me to compare children born the same region but in different years. Using Ghana Living Standards Survey round 2 (GLSS2, 1988/89) and Ghana Education Impact Evaluation Survey (GEIES, 2003), I test the effect of in utero real cocoa price fluctuation exposure on Raven/IQ test of children 9 to 17 years old. In utero cocoa price boom significantly increases Raven/IQ score. A standard deviation increase in utero real producer price of cocoa increases the correct Raven/IQ items answered by a child by 4.2 percentage points, which is equivalent to 11% of the average score. Exploiting the Ghana Living Standard Surveys (GLSS1; GLSS2; GLSS3; GLSS4; GLSS5; and GLSS6), I also estimate the impact of in utero price boom on grade attainment for children of ages 6 to 17. A standard deviation increase in real producer price of cocoa increases grade attained by 0.38 year, which is 13% of the average grade attained by the children in the sample.

The early life shock analysis of this study is related with evidences on fatal origins hypothesis (Almond and Currie, 2011; Barker, 1992). Most of these evidences focus on the effect of early life (in utero) exposures on adult health or human capital outcomes skipping the middle ages (years). This means there is lack of knowledge regarding the developmental trajectories over the life cycle (Almond et al., 2017). Recently, an emerging body of evidences is testing the effects of early life shocks on outcomes measured in school ages (Almond et al., 2015; Shah and Steinberg, 2017; Figlio et al., 2014). This study contributes to that literature by exploiting the effect of exposure to in utero cocoa price shock on childhood cognitive outcomes in Ghana.

The remainder of the paper is organized as follows. The next section presents the relevant background information on Ghana. Section 3.3 and Section 3.4 deal with data and identification strategy, respectively. In Section 3.5 and Section 3.6, I discuss the main results and some heterogeneity analyses, respectively. Section 3.7 addresses some identification threats and finally, Section 3.8 concludes.

3.2 Background

The Cocoa Coast. Over the years, after its independence, Ghana, formerly known as the Gold Coast, remains to be one of the major exporters of cocoa. Ghana with Brazil and Cote dIvoire is one of the largest producers of cocoa. Though it varies over time, the cocoa sector is an important force in the economy of the country. For instance its share of the GDP of the country in the 1970s was more than 5 percent. Between 2001-2005, the sector contributed only 2 percent to the GDP, but constituted about 10 percent of agricultural GDP (Kolavalli and Vigneri, 2018). Moreover, it is one of the major sources of export revenue. For example, in 2009/10 Ghana exported more than half a million (566,700) MT of cocoa beans, which accounts about 21% of total exports (The World Bank , 2013). Furthermore, according to The World Bank (2013), at the micro level, the cocoa sector is a major source of livelihood for about 800,000 farmers and substantial number of other people that are involved in trade, transportation, and processing of cocoa.

Ghana Education Structure. After the reform in 1987, education in Ghana is structured as 6 years of Primary School (age 6 to 11); 3 years of Junior High School (age 12 to 14) and 3 years of Senior High School (age 15 to 17) (see B.1 in Appendix B.1). Senior High School education was extended to 4 years during the 2007 reform, only to be reversed back to 3 years in 2009. In terms of cost of education, even though, primary education is always free in principle in Ghana, localities in different schools charge fees. Since 1996, the country has implemented free compulsory universal basic education (FCUBE) in which education is free of cost for primary and junior secondary school children (White, 2004; Akyeampong, 2009). Despite FCUBE, education is not completely free. For example in 2005-2006, Table B.1, Table B.2 and Table B.3 in Appendix B.1 show that families in Ghana still pay educational expenses. Expenditures to primary school age students, however, are the smallest followed by expenditures for junior high school students. Parents who have a child in senior high school endure considerably higher cost of education.[1]

Schooling. School enrolment has shown a remarkable progress in Ghana in the last three decades. In 1987/88 about 61% of children aged between 6 to 17 were in school, while that number rose to 87% in 2012/13. The variation in attendance by age shows that fewer students attend school as age increases. In 1987/88, 63% of children aged

[1]A free Senior high school (SHS) policy has been adopted since 2017 in the country.

between 6 to 11 attended school; 64% of children aged between 12 to 14 attended school; whereas only 50% of children aged between 14 to 17 were in school. However, in 2012/13, 90% of children aged between 6 to 11 were in school; 90% of children aged between 12 to 14 attended school; yet only 76% of children aged between 14 to 17 went to school. In 1987/88, there was gender disparities in school attendance, where 67% of boys and only 55% girls of school age went to school. Nonetheless, in 2012/13 there was a convergence in boys and girls attendance rate: the attendance rate for both school age boys and girls was similar at 87%.

Child Labour. Child labour is one of the main reasons why children are dropping out school in many developing countries. In Ghana, about 29% of school age children are involved in child work activities. A slightly higher percentage of boys (30%) and around 27% girls participate in any work activity mostly for their families. However, more girls are involved in household chores (83% girls to 75% boys).

3.3 Data

3.3.1 Main Variables

The study has two interrelated parts. The first part investigates the contemporaneous effects of price shock on child schooling and labour supply. The second part deals with the effect of early life price fluctuation on cognitive development and school attainment outcomes. To examine these short-term and early life effects, I exploit various datasets.

For contemporaneous analysis, I use the Ghana Living Standard Surveys (GLSS1; GLSS2; GLSS3; GLSS4; GLSS5; and GLSS6). The Living standard surveys are carried out in different countries with the aim of collecting information on individuals, households, communities and prices, to gauge the living standards of a given population and inform evidence based policy making (The World Bank , 1999). The GLSS surveys are implemented by the Ghana Statistical Service. These surveys are nationally representative samples. GLSS1 conducted in 1987/88 is the first living standard survey of Ghana. The second survey, GLSS2, was conducted in 1988/89 followed by the third Ghana living standard survey, which was conducted in 1991/1992. The fourth, fifth and sixth living standard surveys were conducted in 1998/99, 2005/06 and 2012/13, respectively. The GLSS1 consists of 3,136 households and 15,492 individuals; the GLSS2 collects information from 3,192 households and 14,924 individuals; the GLSS3 comprises of 4552 household and over 20,000 individuals; the GLSS4 covers a sample of 5,998 households containing 25,855 household members; the GLSS5 covers a sample of 8,687 households containing 37,128 household members and the GLSS6 surveys close to 17,000 households consisting of more than 72,000 individuals. They contain individual, household, community and price information. In this study, I focus on the household module. I pool these six waves to create the working sample. Because the analysis is conducted at a child level, I restrict the sample to individuals aged 6 to 17 years.

The main outcome variables in the contemporaneous analysis are attendance and grade-for-age. In GLSS, the surveys ask whether the household members are currently attending school. Attendance takes 1 if the member of the household is currently attending school and 0 otherwise. Another schooling variable is grade-for-age. I define grade-for-age as a binary variable that takes 1 if a child is in the correct grade for his or her age. Panel A in Table 3.1 reports summary statistics of these outcome variables and controls used in this analysis. The table reports that 79% of children in the pooled sample (all children) are currently attending school. Moreover, on average children in the sample attained 3 years of schooling and only 22% of children are in the right grade for their age.

I also use these surveys to show the effect of the current year price fluctuation on child labour supply. I employ the following variables: whether a child is engaged in any work; engaged in agricultural self employment including contributing to family work; participate in non-agricultural self employment including contributing family business and working in household chores. Panel A in Table B.4 in Appendix B.1 presents the summary statistics of these outcomes.

To gauge the effect of price shock on young children, I analyse the effect of in utero price shock on cognitive development. Following Glewwe et al. (2001), Field et al. (2009), and Ampaabeng and Tan (2013), I focus on Raven/IQ test and grade attainment as measures of cognition. To understand the impact of early life shock on direct effect of intelligence or IQ, I focus on Raven test score (Raven's Progressive Matrices).[2] This test comes from two data sources: The Ghana Living Standards Survey Round 2 (GLSS2) and the Ghana Education Impact Evaluation Survey (GEIES), conducted in 2003.[3] The GEIES is a nationally representative sample survey. The GLSS2 is considered to be the precursor of the GEIES. The GLSS2 has an education module, which tested cognitive development and achievement skills of household members, and teachers in 85 sampling clusters randomly selected from the entire GLSS2 sample of 170 clusters. In 2003, the GEIES is conducted in 84 of the 85 clusters where, in GLSS2(1988/89), educational achievements scores were collected from. The 2003 survey collected data from 1,740 households and 8,000 individuals. In both of the surveys, household members aged 9 to 55 years took the tests. In this study, I restrict the sample only to include children aged from 9 to 17, as the aim of paper focuses on child cognitive development. So, the data I use for investigating the effect of the price shock on Raven/IQ outcome is obtained by pooling these two surveys.[4] The summary statistics of Raven/IQ outcome is presented in Panel B of Table 3.1. It shows that on average children answer 49% of Raven/IQ tests correctly. For grade attainment outcome, I exploit Ghana Living Standard Surveys (GLSS1; GLSS2; GLSS3; GLSS4; GLSS5; and GLSS6). The summary statistics of grade outcome is presented in Panel B of Table 3.1. As mentioned above, Table 3.1 reports that on average children in the sample attained 3 years of schooling.

The other data I use is the series of real producer price of cocoa in Ghana. The source is Teal (2002).The data contains a time series of real producer price of cocoa

[2]Figure B.2 in the Appendix B.1 shows a sample of Raven/IQ test.

[3]One of the clusters surveyed in GLSS2, 1988/89 was no longer inhabited in 2003.

[4]The datasets have also collected information on other achievement scores, namely: simple English, and maths tests. However, only subset of household members who have three and more years of schooling were given the easy maths and easy reading tests. Moreover, those who scored 50 percent or more on these tests were asked to take the advanced tests in English and maths. Panel B in Table B.4 in Appendix B.1 reports the summary statistics of these variables. These tests suffer from sample selection problem. I do not focus on these scores in the main analysis. Nonetheless, I have also reported the effect of the in utero shock on these scores by including grade as one control in the regressions for the easy tests and the inverse mills ratio (IMR) in the advanced tests regressions (Ampaabeng and Tan, 2013; Heckman, 1979). Furthermore, the effect of the shock on these scores may be mediated by the impact of the shock on Raven/IQ and grade attainment.

for Ghana. The data is published by Oxford University, Centre for the Study of African Economies. The advantage of using this data is the possibility to measures the true and farm-gate (producer) price faced by cocoa-growing households. Teal (2002) computes the real producer price for cocoa exports as follows:

$$\frac{P_X^P}{P^C} = \frac{P_X}{P_M}\frac{P_M ER}{P^C}(1 - tax) \tag{3.1}$$

where, P_X^P is the cedi (Ghanaian Money) price received by cocoa producers. This price is then deflated by P^C the price of domestic goods to get real producer price in cedi. Thus, real producer price in cedi is a function of $\frac{P_X}{P_M}$, the export price in foreign currency divided by the price of imports in foreign currency, the official exchange rate, ER, and the tax rate tax, which encompasses both export duties and the difference between world cocoa prices and the lower prices often set by the monopolistic cocoa board. Table 3.1 (panel A contemporaneous prices and panel B early life prices) shows the descriptive statistics of the logarithm of real producer price of cocoa and the variable derived from it by dividing the logarithm of real producer price of cocoa by its standard deviation over the sample period. The price data is expressed this way so that reported coefficients can be interpreted as the effect of a standard deviation price change. The table reports that there is a considerable variation in the price in each sample. For instance, over the period of 1987 to 2013, which the contemporaneous analysis sample covers, the standard deviation of real producer price of cocoa is close to 0.36. In addition, I graph B.3 and B.4 to depict the fluctuation in real producer price of cocoa in Ghana over time. Figure B.3 illustrates that real producer price of cocoa data used in the contemporaneous analysis, where the vertical lines represent the interview years. Real producer prices were relatively lower for the year before 1987, 1991,and 1998. Interview years such as 1989, 2005, 2006, 2012 and 2013, are characterised by higher price events preceding them and lower price events following them. However, cocoa prices are relatively lower before the interview years 1988, 1992 and 1999, and relatively lower following these years. Moreover, Figure B.4 depicts price fluctuation employed in the early life analysis. The graph shows troughs in late 1970s, early 1980s, and mid 1990s; and peaks in late 1980s, and mid 2000s.

Finally, I exploit the EGC-ISSER Ghana Panel Survey conducted in 2009-2010 to construct an indicator for whether a region is cocoa producer. In the survey, individuals were asked to list all plots of land, the size of land in hectares, and the type of crops grown on these plots. Using this information, I calculate the total area of farm land in hectares by region. I also compute the total area of land in hectares occupied by cocoa. Then, I compute the percentage of farm land occupied by cocoa by region as a fraction of land devoted to cocoa divided by total farm land area. A region is treated as cocoa producing, if the fraction is greater than 0%. Later in the robustness analysis, I dropped regions with less than 20% of their farm land occupied by cocoa (Greater Accra and Volta). Panel C in Table 3.1 shows the summary statistics. Furthermore, Figure B.5 in Appendix B.1 reports a map of regions in Ghana that have suitable soil for growing cocoa. This map resembles the categorization of regions as cocoa producers using EGC-ISSER Ghana Panel Survey.

3.3.2 Additional Variables

Apart from the main analysis, I also investigated several mediation and robustness analyses. For example, as a mediation analysis, I explore the effect of contemporaneous price shock on mothers' health and early life shocks on prenatal, at-birth and childhood investments. To that end, I use the Ghana Demographic and Health Survey (GDHS) collected in 1988, 1993, 1998, 2003, 2008. Specifically, for contemporaneous mothers' health analysis, I exploit the Individual's recode. Individual's Recode contains information on nationally representative sample of women aged 15-49 at the time of the survey. The data have information on womans year of birth, region of residence, years of education, rural residence, age, occupation, religion, ethnicity, height and weight. To investigate early life shocks on prenatal, at-birth and childhood investments, I exploit the Children's Recode. This data contains information on every child born to women interviewed and recorded in the individual recode in the last five years preceding the interview. The data contain information on children predetermined characteristics (such as year of birth, birth order, gender, the child's current age in months); children vaccination histories and how long they are breastfed; and pregnancy and postnatal care and immunization carried out by the mother (such as prenatal visits to doctors, vaccines at-birth, and method of delivery of the child). Moreover, as a robustness check, I carried out mortality and fertility selection checks. To do so, I exploit the GDHS Birth's Recode. The data record information on every child ever born to women interviewed and recorded in the individual recode. I use these information to investigate the selection effects. Specifically, I use the data in order to test whether in utero price shocks have impacts on in utero mortality and women fertility behaviour. Panel C in Table B.4 in Appendix B.1 reports the summary statistics of these variables.

As further robustness check, I control for annual average rainfall both in the contemporaneous and early life analysis to minimize bias from potential omitted variables. The source for the data is the University of East Anglia Climatic Research Unit (UEA-CRU). Panel D in Table B.4 in Appendix B.1 reports the summary statistics.

Table 3.1: Descriptive statistics of main variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Panel A: Contemporaneous											
	(1) Total sample			(2) Primary			(3) Junior High			(4) Senior High		
	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD	Obs.
Individual and HH variables												
Attendance	0.788	0.409	59101	0.815	0.388	31827	0.828	0.378	14957	0.669	0.471	12317
Grade-for-age	0.217	0.413	50069	0.315	0.465	26690	0.130	0.336	12737	0.077	0.266	10642
Child age	11.100	3.397	60223	8.366	1.687	32354	12.918	0.817	15222	15.906	0.812	12647
Child is male	0.513	0.500	60223	0.511	0.500	32354	0.516	0.500	15222	0.515	0.500	12647
HH head is male	0.732	0.443	60125	0.746	0.435	32299	0.718	0.450	15201	0.714	0.452	12625
Year	2002.675	9.588	60223	2002.448	9.620	32354	2002.861	9.559	15222	2003.035	9.526	12647
Current year shock	29.043	17.035	60223	28.721	17.186	32354	29.411	16.846	15222	29.422	16.855	12647
Previous year shock	23.491	13.786	60223	23.227	13.906	32354	23.790	13.634	15222	23.806	13.645	12647
Real Cocoa Producers Price												
Log(current real cocoa producer price)	13.83	0.357	11									
Log(current real cocoa producer price)/SD	38.696	1	11									
	Panel B: Early Life											
	Mean	SD	Obs.									
	Raven/IQ Outcome											
Individual and HH variables												
Raven/IQ	48.968	18.201	2826									
Child is male	0.515	0.5	2826									
HH head is male	0.680	0.466	2826									
Year of Birth	1982.891	7.582	2826									
In utero Shock	32.929	14.084	2826									
Real Cocoa Producers Price												
Log(in utero cocoa producer price)	13.384	0.346	24									
Log(in utero cocoa producer price)/SD	38.653	1	24									
	Grade outcome											
Individual and HH variables												
Grade	2.94	2.941	49737									
Child is male	0.515	0.5	49737									
HH head is male	0.742	0.438	49640									
Year of Birth	1992.454	10.653	49737									
In utero Shock	26.129	16.339	49737									
Real Cocoa Producers Price												
Log(in utero cocoa producer price)	13.515	0.372	38									
Log(in utero cocoa producer price)/SD	36.285	1	38									
	Panel C: Fraction of farm area under Cocoa, by Region											
Western	53.95%											
Central	34.51%											
Greater Accra	0.09%											
Volta	4.38%											
Eastern	26.20%											
Ashanti	44.36%											
Brong Ahafo	31.80%											
Northern	0.00%											
Upper West	0.00%											
Upper East	0.00%											

Source: GLLS1; and GLLS2; GLLS3; GLLS4; GLLS5 and GLLS6(for Contemporaneous analysis and for Early Life analysis in the case of Grade outcome);GLLS 2, 1989; and GEIES,2003 (for Early Life analysis in the case of Raven/IQ outcome); Teal(2002) and Ghana Cocoa Board (sources of real Cocoa producer price); EGC-ISSER Socio-economic Panel Survey (for computing Fraction of farm area under Cocoa, by Region)

3.4 Empirical Strategy

The main objective of the study is to explore the differential effect of cocoa price fluctuation on young and older children. To that end, the ideal strategy is to investigate the effect of school age cocoa price on school age outcomes such us attendance and child labour; and the impact of early life cocoa price on outcomes of children before age 5. However, it is impossible to conduct the later one due to lack of human capital outcomes before age 5 especially in developing country context. To get evidence on the potential effect on young (in utero) children, I follow the 'fetal origin hypothesis' literature- which shows the persistence effect of early life (in utero) shocks- and investigate the effect of in utero cocoa price fluctuation on cognition and grade attainment.

To understand the impacts of cocoa price fluctuation on older children, I investigate the contemporaneous (school age) price shocks on schooling and child labour outcomes. Specifically, I follow the following difference-in-difference specification that is estimated using linear probability model (LPM) method.[5] In the regressions, I use survey weights.[6]

$$S_{irt} = \alpha_r + \theta_t + \delta_r t + \beta \text{CocoaPrice}_t \times \text{CocoaProducer}_r + X'_{irt}\Upsilon + \varepsilon_{irt}, \tag{3.2}$$

where S_{irt} is schooling or child labour outcome of child i, from region r, surveyed at year t. CocoaPrice_t is the normalized real cocoa price. It is constructed by dividing $\ln(\text{RealCocoaProducerPrice})_t$ by its standard deviation over the sample period so that reported coefficients can be interpreted as the effect of a standard deviation price change. CocoaProducer_r indicates whether cocoa is produced in region r. The interaction of CocoaPrice_t and CocoaProducer_r gives us the current year price shock. X_{irt} is the household and child characteristics (such as: age of the child, gender of the child, and gender of the household head). I also control for α_r, the region fixed effects and γ_t, the interview year fixed effects, and $\delta_r t$, the region specific time trends.[7] The parameter of interest β estimates the differential effect of current year price on schooling and child labour in regions that produce cocoa. Standard errors are clustered at the region level to deal with correlation within location of

[5]As a robustness check, I also report results estimated using logistic regressions.

[6]In the Living Standard Surveys that are not self-weighting disproportionately larger samples were selected from smaller regions. As a result, for consistent estimation, I use survey weights in the regressions. As a robustness check, however, I also report results from the regressions that don't use survey weights in the appendix.

[7]Outcomes may change due to other economic reasons at region level during the sample years. To control for potential omitted variables, I include these region specific trends (Dube and Vargas, 2013).

residence. Given the low number of clusters (10 regions) used to cluster the standard errors, the precision of the estimates may be affected. I show the robustness of the results to the use of wild bootstrapping method (Cameron et al., 2008; Cameron and Miller, 2015). Identification of the causal effect of price shocks on schooling outcomes depends on the assumption that, conditional on survey year and region fixed effects, and region specific time trends, contemporaneous price shocks are not related with omitted factors that affect the schooling outcomes. In Section 3.7, I discuss some of the threats of this identifying assumption.

To estimate the impact of price fluctuation on young children, I estimate the effect of in utero cocoa price shock exposure on cognitive development outcomes. Specifically, I follow the following difference-in-difference specification that is estimated using ordinary least square (OLS) method. For the reason explained above, for estimating effects on grade outcome (the source of data for Raven/IQ outcome is self-weighting samples), I use survey weights.[8]

$$Y_{irb} = \alpha_r + \theta_b + \delta_r b + \gamma \text{CocoaPrice}_{b-1} \times \text{CocoaProducer}_r + X'_{irb}\Upsilon + \varepsilon_{irb}, \qquad (3.3)$$

where Y_{irb} is an outcome variable of a child, such as Raven/IQ, and grade attainment for individual i, born in region r, and year b.

CocoaPrice_{b-1} is the normalized real cocoa price. It is constructed by dividing $\ln(\text{RealCocoaProducerPrice})_{b-1}$ by its standard deviation over the sample period. Due to lack of precision in the date of birth information, in utero is defined as the year before the year of birth. This strategy is widely used in the literature (Adhvaryu et al., 2015; Shah and Steinberg, 2017). CocoaProducer_r is an indicator variable that shows whether cocoa is produced in the region of birth r.[9] The interaction of

[8]As a robustness check, however, I also report the estimation that doesn't use survey weights in the appendix.

[9]Note that I avoid the concern related to migration selection, usually encountered in the literature (Akresh et al., 2012; Shah and Steinberg, 2017). Unobserved migration by households (children) has been found to potentially bias results on early-life shocks since in utero exposure to shocks could be incorrectly assigned based on the child's current region of residence. In my case, the data used in this paper reports the region of birth for children. Migration selection is therefore not a major issue.

CocoaPrice_{b-1} and CocoaProducer_r captures shocks occurring in utero. X_{irb} is the household and child characteristics. In addition, I introduce α_r, region fixed effects; θ_b, year of birth fixed effects; as well as $\delta_r b$, region specific time trends. The parameter of interest β captures the effect of exposure to in utero (income) shock. Specifically, it measures the differential effect of in utero cocoa price on cognitive development outcomes in regions that produce cocoa. Standard errors are clustered at the region level to deal with correlation within location of residence. Given the low number of clusters (10 regions) used to cluster the standard errors, the precision of the estimates may be affected. I show the robustness of the results to the use of wild bootstrapping method (Cameron et al., 2008; Cameron and Miller, 2015). Identification of the causal effect of in utero price shock on cognitive outcomes rests on the assumption that, conditional on birth year and region of birth fixed effects, and region specific time trends, in utero price shocks were not correlated with omitted factors that also impact the cognitive development outcomes. In Section 3.5.3 (subsection B.) and Section 3.7, I discuss some of the threats of this identifying assumption.

3.5 Results

3.5.1 Contemporaneous Effects

Table 3.2 reports the main results on the contemporaneous effects of price shock estimated from equation (3.2). It presents the estimated effects of contemporaneous price shock on child current attendance and educational progression. Panel A reports the effect of current year price shock on the probability of current attendance, while panel B shows the effect of previous year price shock on the probability of a child being on the right educational track.[10] Column (1) provides estimates on pooled sample (children aged 6 to 17); column (2) presents estimates on sample of primary school aged children (aged 6 to 11); column (3) presents estimates on sample of junior high school aged children (aged 12 to 14); and column (4) presents estimates on sample of

[10]If children experience price boom last year and decided to drop out of school, they fall behind in the current year grade attainment. Thus, it is intuitive to use previous year price shock instead of current year price shock in the case of the grade-for-age analysis.

senior high school aged children (aged 15 to 17). Column (1) indicates that current and previous year price shocks significantly decrease current school attendance and grade-for-age. For children surveyed in regions that produce cocoa, when real cocoa price increases school attendance and grade-for-age decrease. In panel A column (1), a standard deviation increase in the current year real producer price of cocoa significantly decreases current attendance by 8 percentage points. This is equivalent to 10% of the average attendance rate. In panel B column (1), a standard deviation increase in the previous year real producer price of cocoa significantly decreases the likelihood of being on the correct grade by 6.7 percentage points. This is equivalent to 30% of the average grade-for-age rate. In columns (2) and (3), I show that these effects are driven by impacts on primary and junior high school aged children with stronger effect on primary school age children. However, no effect is found for older children (senior high school age children).

To provide corroborating evidence to the impacts on schooling above, I conducted the effect of current year cocoa price on child labour supply outcomes. Table 3.3 presents these results. The table presents the effect of the shock on whether a child is engaged in any work (panel A), on agricultural self employment including contributing to family work (panel B), on non-agricultural self employment including contributing family business (panel C), and on household chores (panel D). The results show that during cocoa price boom children engage significantly more into non-agricultural business activities and household chores. In regions that produce cocoa, an increase in current year real cocoa price increases employment in non-agricultural activities for all groups of children (column 1 to 4 of panel C) and participation in household chores for children aged 12 to 14 (column 3 of panel D). In panel C column (1) depicts that, for the whole sample, a standard deviation increase in the current year real cocoa price significantly increases the probability of participating in non-agricultural work by 2.2 percentage points (51% of the average). Column (2) shows that, for the primary school age children, a standard deviation increase in price increases the probability of participating in non-agricultural work by 1.3 percentage points (56% of the average). In column (3), a standard deviation increase in cocoa price significantly raises

Table 3.2: Estimated effect of current and previous year cocoa price shock on schooling

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Current Attendance			
Current year shock	-0.080***	-0.106***	-0.057**	-0.030
	(0.024)	(0.019)	(0.023)	(0.042)
	[0.088]	[0.002]	[0.134]	[0.498]
Observations	59,004	31,773	14,936	12,295
R-squared	0.140	0.172	0.159	0.107
	Panel B: Grade-for-age			
Previous year shock	-0.067***	-0.093***	-0.023**	-0.029
	(0.010)	(0.013)	(0.011)	(0.019)
	[0.002]	[0.002]	[0.002]	[0.048]
Observations	49,972	26,636	12,716	10,620
R-squared	0.143	0.162	0.047	0.041
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. Wild-bootstrap p-values in brackets. The asterisks next to the coefficients are for p-values associated with the main (non-wild bootstrap) regressions. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child, child age, and gender of household head. Panel A reports results on current attendance, and panel B is shows the effect of price shock on grade-for-age. Survey weights are used in the regression estimations.

the probability of participating in non-agricultural business activities for children aged between 12 to 14 by 3.5 percentage points (67% of the average). For older children, a standard deviation increase in price is significantly associated with 2.6 percentage points (33% of the average) more participation in to non-agricultural business activities (column 4).[11] The results on the effect of price shock on labour supply outcomes only capture the extensive margin of labour supply. As a result, they do not tell the full story and may be underestimation of the full effect of price boom. Nonetheless, the results are suggestive evidence that during cocoa price boom

[11] I also estimate the effects of current year cocoa price fluctuation on adult labour supply outcomes. The results are presented in Table B.5 in Appendix B.1. A price boom is significantly related with higher labour supply to non-agricultural business activities by adults. This is another suggestive evidence that household income improves during cocoa price boom due to higher employment.

Table 3.3: Estimated effect of current year cocoa price shock on child labour

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Any Work			
Current year shock	0.021	0.005	0.030	0.035
	(0.026)	(0.028)	(0.028)	(0.025)
Observations	48,331	24,230	13,036	11,065
	Panel B: Agri. self emp't or contributing to family work			
Current year shock	-0.004	-0.012	-0.009	0.005
	(0.022)	(0.024)	(0.022)	(0.026)
Observations	48,309	24,222	13,028	11,059
	Panel C: Non-Agri. self emp't or contributing to family work			
Current year shock	0.022***	0.013***	0.035***	0.026*
	(0.008)	(0.005)	(0.013)	(0.014)
Observations	48,309	24,222	13,028	11,059
	Panel D: Household chores			
Current year shock	-0.005	-0.023	0.026**	-0.011
	(0.015)	(0.020)	(0.013)	(0.020)
Observations	54,921	27,803	14,876	12,242
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child, child age, and gender of household head. Panel A reports results on any work, and panel B shows the effect of price shock on agricultural employment and panel C shows the effect of price shock on non-agricultural employment. Survey weights are used in the regression estimations.

economic activities (even those that are not in agricultural sector) flourish in cocoa producing regions. It may be a little surprising, however, that the results in panel B of Table 3.3 show no significant effect of price shock on agricultural employment. Large share of children in the sample (see Table B.4 in Appendix B.1) already engage in agricultural work. The dependent variable (employment in agriculture) measures only the extensive margin of labour supply. There may no be room for further change in the extensive margin. Hence, during cocoa price boom, it may be the case that only change in the intensive margin (the number of hours worked in agricultural activities)- not change in the extensive margin- happened. The results in Table 3.2 and Table 3.3 imply that parents may temporarily pull their children out of school or delay their enrollment and put them to work to reap the benefits of an economic boom. Shah and Steinberg (2017) from India and Kruger (2007) from Brazil also find similar effects that during economic booms children drop out of school to engage in child labour activities.

3.5.2 Effects of Early Life Price Shocks

A. Effects of In Utero Shock

Table 3.4 presents the effects of in utero price shock on Raven/IQ test an grade attainment estimated from equation (3.3). Column (1) provides estimates of in utero price shock on Raven/IQ test score for sample of children aged 9 to 17. Column (2) shows the effect of the in utero cocoa price shock on grade attainment for sample of children of aged 6 to 17. The in utero price shock has significant positive effect on both outcomes. Children conceived during higher cocoa prices and born in cocoa producing regions have grown to have higher Raven/IQ score and higher grade attainment. A standard deviation increase in real producer price of cocoa increases the correct Raven/IQ items answered by a child by 4.2 percentage points, which is equivalent to 11% of the average score. It also increases grade attained by 0.38 year, which is 13% of the average grade attained by the children in the sample. Similarly, Shah and Steinberg (2017) also find in utero and early life economic booms strongly and positively affect human capital development in India.

In Table 3.4, a standard deviation increase in prenatal year cocoa price is associated with 4.2% percentage points more in Raven/IQ test score. To gauge the economic impact of the shock, I estimate the impact of Raven/IQ test on different cognitive achievement test scores. Table B.6 in Appendix B.1 reports these results. Thus, the effect of the gain in Raven/IQ due to in utero price boom translates in to a gain of 2.73 percentage points in simple reading test score, 2 percentage points in simple maths test score, 1.12 percentage points in advanced reading and 0.63

Table 3.4: Estimated effect of in utero cocoa price shock on cognition

	Age 9 to 17	Age 6 to 17
	(1)	(2)
	Raven/IQ	Grade
Shock in utero	4.214***	0.379***
	(1.357)	(0.047)
	[0.048]	[0.000]
Observations	2,826	49,621
R-squared	0.152	0.361
Region of birth FE	Yes	Yes
Year of birth FE	Yes	Yes
Region of birth trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. Wild-bootstrap p-values in brackets. The asterisks next to the coefficients are for p-values associated with the main (non-wild bootstrap) regressions. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child and gender of household head. The Raven/IQ includes children of ages 9 to 17, while grade is reported for children of age 6 to 17. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

percentage points in advanced maths test score.

Furthermore, in Table B.7 in Appendix B.1 I report the impacts of in utero price shock on other different cognitive achievement scores. Except for simple reading score, in utero price shock does not significantly affect achievement scores.

B. Early Life Shock Timing

The 'fetal origins' hypothesis predicts that the period in utero is the most critical in a terms of health and human capital development. Nutritional shocks during that period have persistent effects (Barker, 1990, 1992; Dobbing, 1972; Almond and Currie, 2011). However, the literature has questioned that assertion by extending the critical period to early life after birth. Studies like Maccini and Yang (2009) concludes that the [rain fall] shock in the year of birth that results in nutritional shock in the following year is crucial for long-term health and socio economic development. Glewwe and King (2001) suggest that nutritional shocks at the second year of life have greater effect than shocks in the first year of life. Another set of studies find that both the period in utero and the first year of life are equally important (Dobbing, 1972; Akresh et al., 2012), while some others find evidence that all early years are important and there is no special unique period among the early life years (Adhvaryu et al., 2014; Shah and Steinberg, 2017). These evidences show that not just price shock in utero but also shocks in other early life years matter for cognitive development. In addition, real cocoa price can be serially correlated over time. As a result, the results in the baseline (Table 3.4) may be attributed not just to price shock in utero but also shocks in other early life years. In other words there might be omitted variable bias in the estimated effects in the baseline as a result of not controlling for price shocks in other years than in utero period. In this section, to control for possible omitted variables and to test if the early life shocks that happen from in utero to 3 years after that have variant effect on cognition, I re-estimate equation (3.3).[12] I include not only in utero price shock but also shocks that occur just before and after in utero.[13] The results are depicted in Table 3.5. Column (1) shows the effect of price shocks in the year before in utero, in utero, year of birth, year of birth plus 1 year, and year of birth plus 2 years for Raven/IQ test. Similarly, column (2) shows the same analysis for grade attainment outcome. For Raven/IQ test, though the coefficients on in utero and year of birth price shocks are positive they are insignificant.[14] Column (2) shows that in utero price shock is significantly related with grade attainment and the coefficient is comparable with the baseline effect in Table 3.4. This indicates that the result in the baseline is indeed due to the effect of shock in utero. There is also a marginally significant effect on grade attainment from price shock at year of birth plus 1 year.

[12]This discussion is in line with Maccini and Yang (2009).

[13]Note that the literature on early life shock considers all years before the 5^{th} year birth day as critical period of development. Therefore, to incorporate all these early life years and as an alternative to the specification thus far, I conducted another exercise as robustness. In this case, shock is constructed as interaction between the average prices in early life (in utero, year of birth, year of birth plus 1 year, year of birth plus 2 years, year of birth plus 3 years and year of birth 4 years) with the indicator if the region of birth is cocoa producer. Table B.8 in Appendix B.1 presents these results. It shows that average early life price boom is positively related with later childhood cognitive outcomes.

[14]The data for Raven test score consists of children born between 1971 to 1994. The short period of time considered in the analysis may affect the precision of estimates when serially correlated variables are included in the same regression.

Table 3.5: Cocoa price shock timing and cognition

	Age 9 to 17	Age 6 to 17
	(1)	(2)
	Raven/IQ	Grade
Shock year 0 - 2	-3.449	0.075
	(2.458)	(0.104)
Shock year 0 - 1 (in utero)	2.757	0.277***
	(4.437)	(0.044)
Shock year 0 (year of birth)	6.471	-0.043
	(5.532)	(0.040)
Shock year 0 + 1	-3.267	0.141*
	(2.325)	(0.078)
Shock year 0 + 2	0.815	0.038
	(1.394)	(0.039)
Observations	2,826	49,621
Region of birth FE	Yes	Yes
Year of birth FE	Yes	Yes
Region of birth trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child and gender of household head. The Raven/IQ includes children of ages 9 to 17, while grade is reported for children of age 6 to 17. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

3.5.3 Mechanisms of Effects of Early Life Shocks on Cognitive Outcomes

A. Nutrition

Access to nutrition in utero is critical for health and human capital development (Barker, 1990). Empirical evidences such as Hoynes et al. (2011) and Hoynes et al. (2016) from USA and Black et al. (2007) from Norway show that both short-term and long-term health and socio-economic outcomes are significantly impacted by access to nutrition in utero. As a result, it is plausible to think that the positive effects of prenatal price shock on cognition and grade found above may be due to improvement in nutritional intake in utero through improved household income. To test this, similar to Adhvaryu et al. (2014), using GDHS (1993, 1998, 2003, and 2008), I estimate whether mother's health responds to contemporaneous price shocks. An increase in contemporaneous price can improve mother's weight and BMI through increasing consumption. Table 3.6 presents these results. Column (1) shows the effect of contemporaneous cocoa price on mother weight and Column (2) shows the effect of contemporaneous cocoa price on mother BMI. The table shows that increase in current real cocoa price improves mother's weight and BMI. This can be taken as a suggestive evidence that children conceived during cocoa price boom might have received better nutrition in utero.

Table 3.6: Estimated health effect of contemporaneous cocoa price shock on mothers' health

	(1) Mother weight	(2) Mother BMI
Current year shock	1.299***	0.286***
	(0.299)	(0.107)
Observations	14,472	14,045
Region FE	Yes	Yes
Survey Year FE	Yes	Yes
Region time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Mother education, rural, age of mother, dummies for Ethnicity and religion. Survey weights are used in the regression estimations.

B. Investments

Increase in real cocoa producer price raises households' income and relaxes their budget constraint. This improvement in the available resources in the household may lead to investment on better prenatal care, delivery and other at-birth investments like vaccinations. To investigate if this is the case in Ghana, using GDHS (1988, 1993, 1998, 2003, and 2008), I estimate the effects of price shock before year of birth on prenatal and at-birth investments. The results are presented in Table 3.7 panel A. Column (1) presents estimates on whether the mother had doctor assist prenatal care, column (2) presents estimates on whether the mother had doctor assist delivery, column (3) presents estimates on whether the child received BCG vaccination, column (4) presents estimates on whether the child received polio 0 dose vaccination. There is some evidence that increase in real cocoa price increases prenatal and at-birth investments: higher in utero price leads to mothers to have doctor assist prenatal care and children to receive BCG vaccination at the time of birth.[15]

Furthermore, parents may respond to children's endowments by investing either in compensatory or reinforcing manner (Adhvaryu and Nyshadham, 2016). Parents may respond to improved children endowments through reinforcing investment behavior. To test for this parents investment behavior, I estimate the effect of in utero price shock on childhood investments. Table 3.7 panel B reports these results. Column (1) presents estimates on No. polio doses, column (2) presents estimates on No. DPT doses, column (3) presents estimates on Measles vaccination, column (4) presents estimates on No. of total vaccination and column (5) presents estimates on number of months of breast feeding. In the childhood investment analysis, price shock has significant positive effect on most of these investments: increase in in utero price leads to higher childhood investments. Similar to Adhvaryu and Nyshadham (2016) and Adhvaryu et al. (2014), I find evidence that parents in Ghana reinforce children's endowment through further childhood investment later in infancy. I also find that prenatal and at-birth investments are positively related to in utero cocoa price as a result of more income and relaxed budget constraint. Either way, the positive and significant effect on cognition and grade attainment found in the baseline could stem from investment in addition to improved nutrition.

[15]In 2004 the Ghanaian government implemented National Health Insurance Scheme (NHIS) (Bonfrer et al., 2016; Mensah et al., 2010). The insurance program was supposed to solve the very high medical treatment cost Ghanaians face. Pregnant mothers that need ante-natal, delivery and post-natal health care services are among the beneficiaries of the NHIS. The results presented here using GDHS (1988, 1993, 1998, 2003, and 2008) may not tell us the pure effect that stem from relaxed budget constraint, because the data also contain information after the NHIS. Table B.9 in Appendix B.1 reports results after the 2008 GDHS (data collected after the implementation of NHIS) is excluded from the dataset. The results are not altered.

Table 3.7: Estimated health effect of in utero cocoa price shock on investments

	(1)	(2)	(3)	(4)	(5)
	Panel A: Prenatal/at-birth investments				
	Doctor assist prenatal care	Doctor assist delivery	Received BCG	Received Polio 0 dose	
Shock in utero	0.046***	0.009	0.025**	0.023	
	(0.017)	(0.007)	(0.012)	(0.063)	
Observations	11,798	13,322	11,905	9,101	
	Panel B: Childhood investments				
	No. of polio doses	No. of DPT doses	Measles	No. of total vaccinations	Months of breastfeeding
Shock in utero	0.129***	0.153***	0.008	0.266***	0.715
	(0.031)	(0.042)	(0.011)	(0.102)	(0.464)
Observations	11,962	11,853	11,825	11,980	13,152
Region of birth FE	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes
Region of birth trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Household size, birth order, mother education, rural, gender of child, and dummies for ethnicity and religion. Panel A presents results on childhood investments and panel B focuses results on birth investments. Survey weights are used in the regression estimations.

3.6 Heterogeneity Analysis

In this section, I discuss the heterogeneous effects of the price shock by gender. Conceptually, when there is cocoa price boom boys in cocoa producing regions may work in the cocoa production farms and in the sectors that are connected to cocoa production. Girls may also engage in additional household chores (to substitute adults who have gone to the fields). It is possible to expect both boys and girls to work more and to attend school less. However, the empirical evidence in gender imbalance in the effect of income shock on schooling/child labour is mixed. For instance, Kruger (2007) finds that in Brazil both boys and girls are less likely to leave school and more likely to work during improvement in economic activities. Similar result is found in Shah and Steinberg (2017) from India. However, Edmonds (2006) documents that boy's schooling and labour supply are more impacted by income shocks than girl's in South Africa. In this section, I estimate the effect of contemporaneous shocks separately for boys and girls. Table 3.8 reports the results on contemporaneous effects estimated for boys and girls. Column (1) to (4) report results on boy's outcomes and column

(5) to (8) present results on girl's outcomes. In line with Kruger (2007) and Shah and Steinberg (2017), the estimates show that current year cocoa price boom has no differential effect for boy's and girl's attendance.

Table 3.8: Heterogeneous effect of contemporaneous price shocks by gender

	Boys				Girls			
	All	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)	All	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Current Attendance							
Current year shock	-0.073**	-0.098***	-0.053*	-0.025	-0.086***	-0.115***	-0.054**	-0.040
	(0.030)	(0.027)	(0.030)	(0.044)	(0.019)	(0.014)	(0.022)	(0.045)
Observations	30,364	16,261	7,718	6,385	28,640	15,512	7,218	5,910
Previous year shock	-0.070***	-0.099***	-0.022	-0.032	-0.062***	-0.084***	-0.021	-0.026
	(0.013)	(0.020)	(0.020)	(0.020)	(0.011)	(0.015)	(0.014)	(0.027)
Observations	25,754	13,635	6,633	5,486	24,218	13,001	6,083	5,134
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Child age, and gender of household head. Panel A reports results on current attendance, and panel B is shows the effect of price shock on grade-for-age. Columns 1 to 4 reports on effects on boys, while columns 5 to 8 reports on results on girls. Survey weights are used in the regression estimations.

Biologically boys are more vulnerable in utero than girls (Shettles, 1961; Mizuno, 2000; Kraemer, 2000; Catalano et al., 2006; Eriksson et al., 2010). As a result, larger effect of in utero price boom on boys outcomes is expected. To test if this is the case, I estimate the effect of in utero price shock on cognition and grade attainment for boys and girls separately. Table 3.9 shows these results. Columns (1) and (2) show effects on boy's outcomes and columns (3) and (4) results on girl's outcomes. The results indicate that the effects seem to be larger for boys, albeit insignificantly.

Table 3.9: Heterogeneous effect of in utero shock on cognition by gender

	Boys		Girls	
	(1)	(2)	(3)	(4)
	Raven/IQ	Grade	Raven/IQ	Grade
Shock in utero	5.271***	0.448***	2.798**	0.303***
	(1.905)	(0.031)	(1.343)	(0.080)
Observations	1,454	25,575	1,372	24,046
Region of birth FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
Region of birth trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of household head. Columns 1 to 2 reports on effects on boys, while columns 3 to 5 reports on results on girls. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

3.7 Robustness

3.7.1 Confounding Weather Shocks

Contemporaneous rainfall shocks are associated with schooling outcomes (Jensen, 2000; Shah and Steinberg, 2017). Studies like Shah and Steinberg (2017), Maccini and Yang (2009), and Thai and Falaris (2014) also provide evidence that in utero and early childhood rainfall shocks determine later childhood and long-term adulthood human capital outcomes. Since cocoa is an agricultural crop, rainfall variability impacts its yield and hence, its price. As a result, the baseline results might suffer from omitted variable bias. In that case the results might as well be due to fluctuations in rainfall and the established effect might be due to the fact that rainfall was not included in the regressions. I re-estimate equation (3.2) and equation (3.3) by including contemporaneous annual average rainfall at region of survey level (for contemporaneous analysis) and in utero annual average rainfall at region of birth level (for early life analysis). The results are reported in Table B.10 and Table B.11 in Appendix B.1. Controlling for rainfall does not alter the baseline results.

3.7.2 Mortality and Fertility Selection

The sample exploited in the effect of early life shock analysis is comprised of surviving children. Economic slump in utero may result in higher probability of in utero death. Surviving children included in the sample may, as a result, be the strongest, and the healthiest. This mortality selection would drive the baseline estimates towards zero and, as a result, may not be a concern. Generally, one may expect that exposure to positive economic shocks (booms) at prenatal stage do not reduce the number of live birth (Hoynes et al., 2016). But, one can also argue that price boom may increase in utero (infant) mortality if pregnant women (mothers) work more in the cocoa sector, to get advantage of the boom, instead of devoting time to prenatal (child) care (Miller and Urdinola, 2010). In fact, I find that during cocoa price boom women (mothers) work more in non-agricultural employment activities (see Table B.12 in Appendix B.1). However, this may not be at the expense of prenatal care, since in utero, there may not be requirement for that demanding and time intensive child care. Nonetheless, if cocoa price boom results in prenatal death, those who escaped death and reached birth may be the strongest and healthiest. The results in the baseline would be biased to the upward direction. This selection is a concern.

Boys are more fragile in utero than girls (Kraemer, 2000; Eriksson et al., 2010). In other words, in the fetal health distribution, the lower tail is occupied by male foetuses (Dagnelie et al., 2018). In theory, in utero shocks may affect sex ratio in favor of girls (Trivers and Willard, 1973). Many empirical studies also document that in utero shocks reduce male birth (Almond and Mazumder, 2011; Valente, 2015; Dagnelie et al., 2018). In the absence of data on in utero mortality or miscarriages, I use this established stylized fact in order to test for in utero mortality selection. Specifically, to test if in utero price shock leads to in utero mortality that disproportionately affects boys, I estimate equation (3.3) where the outcome variables is indicator of whether the child born is a boy. For this purpose, I use the birth recode of GDHS. A negative and statistically significant effect would mean in utero price boom leads to sex ratio in favor of female, which is an indication of selection effect. The result is presented in column (1) of Table B.13 in Appendix B.1. A cocoa price boom doesn't significantly affect sex ratio. Mortality selection in utero doesn't seem to be an issue for the baseline results.

The other selection issue regarding the early life analysis is related to fertility. Women may prefer to conceive and give birth during boom years and these planned children may grow up to achieve better cognition and more years of schooling as a result of more investment (Do and Phung, 2010). Moreover, if the characteristics of women who get pregnant and give birth during boom versus slump time are different, the baseline results would be biased. If younger and less educated women, for instance, give birth during slump period, the baseline effects might also attribute the effects of these characteristics. In fact, studies like Buckles and Hungerman (2013) document that women that give birth in different seasons have different attributes. To investigate if women who plan pregnancy during boom versus slump are different, following Akresh et al. (2012) and Dagnelie et al. (2018), I regress mother and household characteristics (education, age, height, number of children and husband's occupation) against the price shock during the year prior to the year of birth. Columns (2) to (6) of Table B.13 in Appendix B.1 reports these results. No attributes of women are related to in utero price shock: fertility selection is not a major issue.

Nonetheless, to correct for any potential mortality and fertility selection, I follow Shah and Steinberg (2017) and Dercon and Porter (2014) and re-run the baseline results above by introducing household fixed effects to compare outcomes of siblings. Table B.14 in Appendix B.1 and reports the results. The baseline results are robust to sibling comparison.

3.7.3 Other Robustness Checks

Using Cocoa Production Intensity. The baseline results use an indicator whether or not a region is producing cocoa. A region is treated as cocoa producing, if the fraction of land devoted to cocoa is greater than 0%. As a robustness analysis, I use the cocoa production intensity (panel C, Table 3.1). The interest variable is constructed now as a multiplication of the intensity variable by contemporaneous price (in the case of contemporaneous analysis) and by in utero price (in the case of early life analysis). I re-estimate the both the contemporaneous and early life regressions using this measure of shock. Table B.15 and Table B.16 in Appendix B.1 provide the results. While the contemporaneous effects are consistently robust to this specification, the early life effects are not.[16]

Dropping Regions Low Production of Cocoa. I also test for the robustness of the result by dropping regions with very low production of cocoa (Greater Accra and Volta). Tables B.17 and B.18 report the results. The estimates are largely similar with the baseline effects.

High Prices vs Low Prices. In Table B.19 and Table B.20, I report results from regressions that split the real cocoa price in to high price events and low price events. Specifically, using decile ranking, I split the cocoa price series in to three groups: high price, low price; and a reference category. High price category is an indicator that takes 1 if price is above and equal to Price Xtile 8 (top three deciles) and 0 otherwise. Low price category is an indicator that takes 1 if price is below and equal to Price Xtile 3 (bottom three deciles) and 0 otherwise. The reference group is an indicator that takes 1 if price is in between Price Xtile 3 and Price Xtile 8; and 0 otherwise. Table B.19 shows the results of contemporaneous analysis. The table shows that when prices are high, schooling decreases and when price are low schooling increases. For instance, in panel A, high prices lead to reduction in current attendance and low prices drive increase in attendance. I also provide p-values that show if the effects of high prices are significantly different from effects of low prices in absolute value. The magnitudes of effects of high prices on attendance are not significantly different from the effects of low prices. However, especially for primary school aged children, grade-for-age seems to be significantly more sensitive to low prices. Table B.20 reports similar analysis for the early life effects of cocoa price shocks. For both outcomes, the effects of low prices are significantly larger than the

[16]This may be due to the fact that the intensity measure is constructed from dataset surveyed as recently as 2012 and may not capture the intensity of cocoa production when children were in utero.

effects of higher prices. For raven/IQ outcome, high prices result in positive and significant effects, while low prices lead to significant negative effects. However, high prices yield significant negative effects in the case of grade outcome. To understand the reason for this negative effect, in columns (3), I report a regression of grade on the interaction of cocoa producer region and dummies that indicate if prices belong to a specific decile leaving the first decile (Price Xtile 1) as a reference group. The result show that highest positive effects are obtained from deciles around and just above the median not at the highest extreme of the price distribution. Adhvaryu et al. (2014) also found similar results.

Fixed Effect Logit Specification. Table B.21 report the analysis using fixed effect logistic specification. The results show that the point estimates are similar with the baseline. However, the magnitude of the effects are almost half of the benchmark for attendance outcome and a little bigger than the benchmark results for the grade-for-age outcome.

Results from Avoiding Survey Weights. Thus far the results are based on applying survey weights in the regression estimations. In Table B.22 and Table B.23, I report results from regressions that don't use survey weights. The tables show that the results from regressions that don't use survey weights are very similar to the baseline results.

3.8 Conclusions

In this study, I explore if there is differential effect of cocoa price fluctuation on young and older children. I find that cocoa price boom positively affects human capital production of young children, while it is negatively related with human capital investments on older children. In other words, substitution effect is dominant for old (school age) children, while income effect is dominant for young (in utero) children.

Exploiting Ghana Living Standard Surveys (GLSS1; GLSS2; GLSS3; GLSS4; GLSS5; and GLSS6) and difference-in-difference specification, I estimate the impacts of real cocoa price fluctuations on schooling, child labour, and grade-for-age outcomes. Controlling for region and survey year fixed effects, and region specific time trends, the coefficients estimate the differential effect of current year price on attendance and child labour and previous year price on grade-for-age in regions that produce cocoa. I find that cocoa price boom negatively affects attendance and grade-for-age, but is positively related with child labour. A standard deviation increase in the current year real producer price of cocoa significantly decreases current attendance by 8%. This is equivalent to 10% of the average attendance rate. A standard deviation increase in the previous year real producer price of cocoa significantly decreases the likelihood of being on the correct grade in the following year by 6.7%. This is equivalent to 30% of the average grade-for-age rate. These effects are driven by impacts on primary school aged children. For school age children substitution effect is important.

Exploiting the Ghana Living Standards Surveys and the Ghana Education Impact Evaluation Survey (GEIES) and difference-in-difference specification, I also explore the effect of the price fluctuations on cognitive development and grade attainment. In utero cocoa price booms increase Raven/IQ and grade attainment. A standard deviation increase in utero real producer price of cocoa increases the Raven/IQ score by 4.2 percentage points and increases grade attained by 0.38 year. I find evidence that these effects could result from both improved prenatal nutrition, and prenatal and childhood investments. For young (in utero) children income effect is dominant.

In many developing countries, designing social safety net policies that integrate public work programs is increasingly becoming popular. The National Rural Employment Guarantee Act that started in 2005 in India, the Productive Safety Net Program that is implemented since 2005 in Ethiopia, the Productive Safety Net Program that came in place since 2012 in Tanzania and the Productive Safety Net Program that started recently in Ghana are some examples. In these programs, there is a public work program in which beneficiaries engage in public work activities for relatively good wages. The results from this study suggest that, even though access to such kind of resources early in life increases cognitive development outcomes later in childhood, for old children it might have detrimental schooling effect. When outside options improve (wages in the localities increase), children may substitute work for school and as a result human capital production decreases. Policy makers should take in to account such negative consequence of social safety net programs. In that regard, lump sum grants might minimize this unintended consequence (Shah and Steinberg, 2017).

The study argues that when cocoa price increases, children are less likely to attend school because they are working in non-agricultural activities. However, it should be noted that the magnitude in school attendance reduction during good cocoa periods doesn't match the increase in child labour. Specifically, the magnitude of the percentage decrease in school attendance is larger than the percentage increase in the probability of engaging in non-agricultural activities. This can be regarded as one of the limitations of the study.

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Appendix B.1 Chapter 3 Appendix

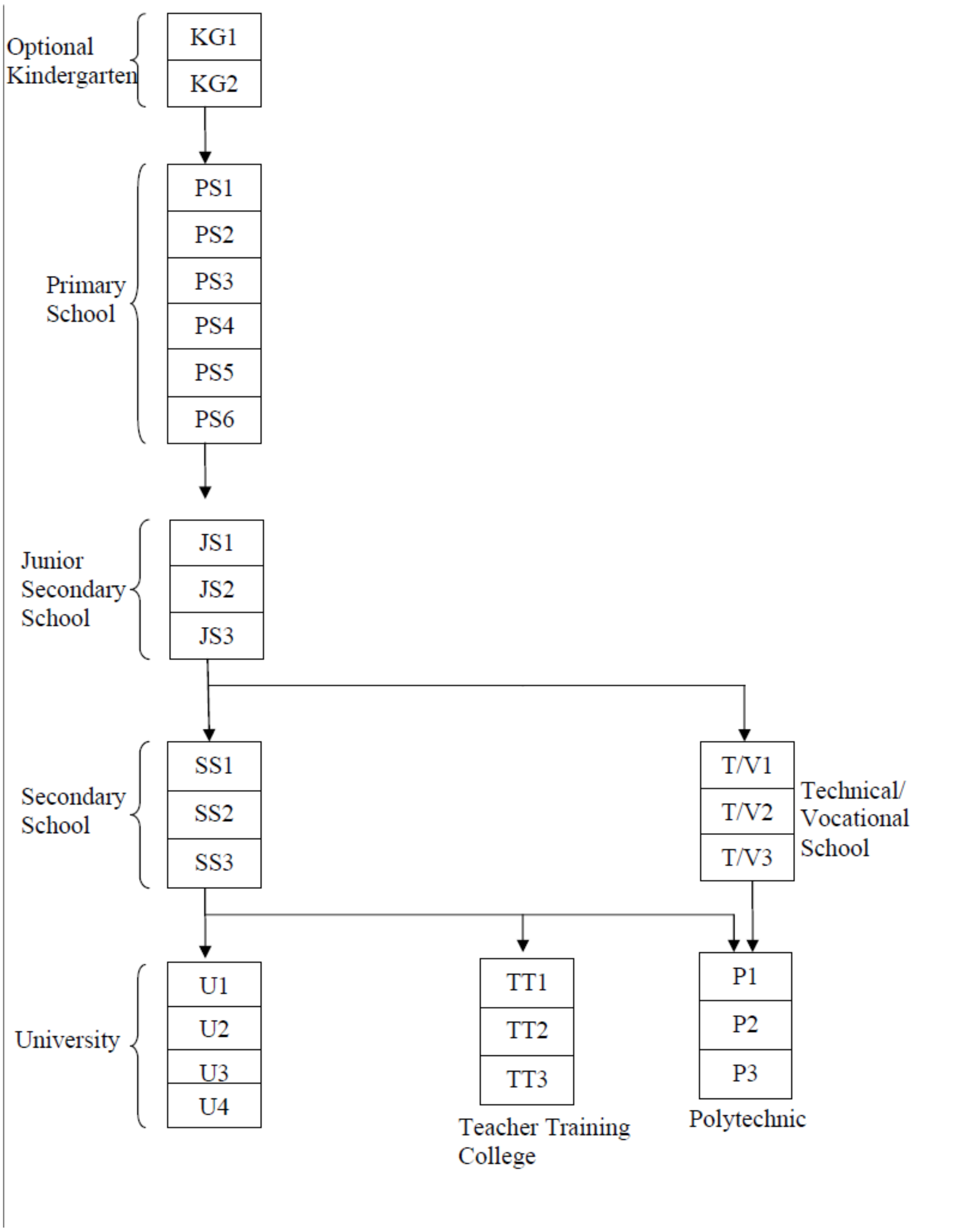

Figure B.1. Ghanas Structure of Education (After 1987)

Source: Akyeampong et al. (2007)

Table B.1: Average amount paid per person attending primary school in the last 12 months, in 2005/06

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Locality						
	Accra	Other Urban	Rural Coastal	Rural Forest	Rural Savannah	Ghana	Percent
School and registration fee	72.45	16.05	4.96	4.19	1.25	10.92	20.7
Contribution to PTA	2.42	1.37	0.65	0.58	0.41	0.83	1.6
Uniforms and sports cloths	8.22	5.76	4.37	4.00	2.85	4.43	8.4
Books and school supplies	20.41	6.56	4.00	2.77	1.28	4.53	8.6
Transportation to and from school	24.16	3.62	2.6	1.61	0.14	3.14	5.9
Food, board and lodging	70.21	33.42	26.37	25.13	7.25	24.96	47.3
Expenses on extra classes	19.87	5.82	1.98	2.02	0.26	3.63	6.9
In-kind expenses	1.24	0.37	0.58	0.4	0.13	0.39	0.7
Total	218.98	72.96	45.5	40.7	13.55	52.81	100

Source: Ghana Statistical Service (2008)

Table B.2: Average amount paid per person attending JSS in the last 12 months, in 2005/06

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Locality						
	Accra	Other Urban	Rural Coastal	Rural Forest	Rural Savannah	Ghana	Percent
School and registration fee	73.88	22.89	4.93	11.2	2.92	19.62	22.1
Contribution to PTA	2.56	1.99	0.96	0.88	0.6	1.33	1.5
Uniforms and sports cloths	9.24	6.42	5.0.7	5.28	4.39	5.88	6.6
Books and school supplies	24.14	10.24	6.32	7.13	3.83	9.18	10.3
Transportation to and from school	17.33	6.45	5.2	4.09	0.38	5.65	6.4
Food, board and lodging	84.58	42.43	31.92	34.34	10.76	37.5	42.2
Expenses on extra classes	25.92	12.3	4.91	6.15	0.87	9.12	10.3
In-kind expenses	1.93	0.26	0.49	0.55	0.23	0.55	0.6
Total	239.59	102.97	59.8	69.61	23.98	88.83	100

Source: Ghana Statistical Service (2008)

Table B.3: Average amount paid per person attending SSS in the last 12 months, in 2005/06

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Locality						
	Accra	Other Urban	Rural Coastal	Rural Forest	Rural Savannah	Ghana	Percent
School and registration fee	192.7	113.54	51.86	91.71	31.86	104.98	42.9
Contribution to PTA	7.99	6.21	3.65	6.41	3.86	6.01	2.5
Uniforms and sports cloths	11.68	7.26	9.76	9.51	6.83	8.44	3.5
Books and school supplies	41.18	24	17.56	21.72	11.16	23.77	9.7
Transportation to and from school	36.18	14.5	3.34	15.15	3.25	16.65	6.8
Food, board and lodging	125.74	64.18	47.5	72.24	21.23	67.56	27.6
Expenses on extra classes	25.93	17.11	22.8	3.55	16.21	6.6	
In-kind expenses	1.57	0.96	0.38	1.34	0.35	1.03	0.4
Total	442.96	247.77	156.86	223.09	82.08	244.65	100

Source: Ghana Statistical Service (2008)

Table B.4: Descriptive statistics of additional variables

Panel A: Child and adult labour outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	(1) Total sample			(2) Primary			(3) Junior High			(4) Senior High			(5) Adult		
	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N
Any work	0.291	0.454	48433	0.202	0.402	24283	0.349	0.477	13059	0.417	0.493	11091	0.766	0.424	73604
Agri. self employed or family work	0.262	0.440	48410	0.191	0.393	24275	0.318	0.466	13051	0.354	0.478	11084	0.469	0.499	73559
Non-Agri. self employed or family work	0.043	0.204	48410	0.023	0.149	24275	0.052	0.221	13051	0.079	0.270	11084	0.259	0.438	73559
Household chores	0.792	0.406	55015	0.710	0.454	27851	0.877	0.328	14898	0.873	0.333	12266	0.821	0.383	73484

Panel B: Other test scores

	Mean	SD	Obs.
Simple reading	60.636	35.385	1375
Simple maths	60.686	25.187	1971
Advanced reading	48.432	19.041	596
Advanced maths	24.557	14.774	759

Panel C: GDHS, Individual's Recode, Children's Recode, Birth Recode

	Mean	SD	Obs.
Mother's health analysis (Individual's Recode)			
Mother Weight	57.677	12.205	14502
BMI	22.467	3.602	14075
Investment analysis (Children's Recode)			
No. of polio doses (max=3)	2.25	1.102	12743
No. of DPT doses (max=3)	2.235	1.146	12624
Measles	0.646	0.478	12598
No. total vaccination (max=7)	5.095	2.529	12762
No. months breastfeeding	14.923	8.675	16150
Mortality and fertility selection analysis (Birth Recode)			
Child is boy	0.511	0.5	67676
Mother year of education	3.908	5.019	67656
Age of mother	36.128	7.803	67676
Height of mother	173.544	110.202	42383
No births	5.38	2.606	67676
Husband in self employed agriculture	0.584	0.493	66463

Panel D: Rainfall

	Mean	SD	Obs.
For Contemporaneous analysis			
Mean annual rainfall	103.495	17.685	110
Year	1998.182	9.195	110
For Grade Outcome			
Mean annual rainfall (in utero)	103.172	19.578	380
YOB	1988.5	10.98	380
For Raven/IQ Outcome			
Mean annual rainfall (in utero)	104.068	20.832	240
YOB	1982.5	6.937	240

Source: GLLS1; and GLLS2; GLLS3; GLLS4; GLLS5 and GLLS6(for Contemporaneous analysis in the case of child labour outcomes); GLLS 2, 1989; and GEIES,2003 (for the analysis on other test scores); Rainfall data from University of East Anglia Climatic Research Unit (UEA-CRU); GDHS 1988 1993, 1998, 2003, and 2008(Mother's health, investment, and mortality and fertility analyses)

Table B.5: Estimated effect of current year cocoa price shock on adult labour supply

	(1)	(2)	(3)	(4)
	Any work	Agri. self emp. or family	Non-agri. self or family	HH chores
Current year shock	0.000	-0.026	0.030**	-0.009
	(0.019)	(0.029)	(0.015)	(0.024)
Observations	73,448	73,403	73,403	73,331
R-squared	0.063	0.041	0.055	0.132
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): gender, age, and gender of household head. Column (1) reports results on any work, and column (2) shows the effect of price shock on agricultural employment, column (3) shows the effect of price shock on non-agricultural employment and Column (4) shows the effect of price shock on household chores. Survey weights are used in the regression estimations.

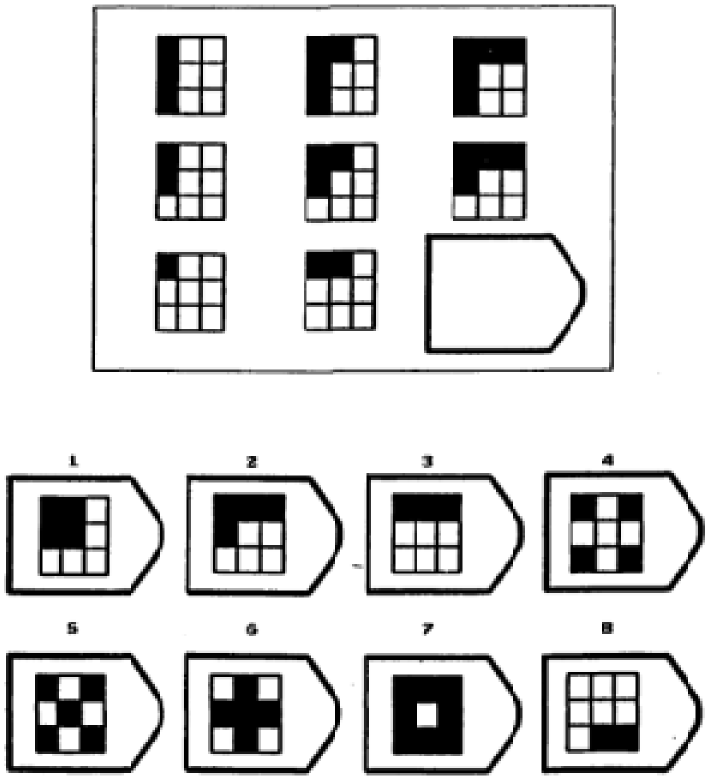

Figure 4a. A problem to illustrate the quantitative pairwise progression rule. The number of black squares in the top of each row increases by one from the first to the second column and from the second to the third column. The number of black squares along the left remains constant within a row, but changes between rows from three to two to one (The correct answer is #3)

FIGURE B.2. Sample of Raven test

Table B.6: the effect of Raven test on other tests

	(1) Simple reading	(2) Simple math	(3) Advanced reading	(4) Advanced math
Raven	0.605***	0.476***	0.267***	0.150***
	(0.043)	(0.028)	(0.040)	(0.033)
Observations	1,359	1,953	566	625
Region of birth FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
Region of birth trends	Yes	Yes	Yes	Yes
Child Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Controls include (X): Gender of the child and gender for household head. The source of data is self weighted.

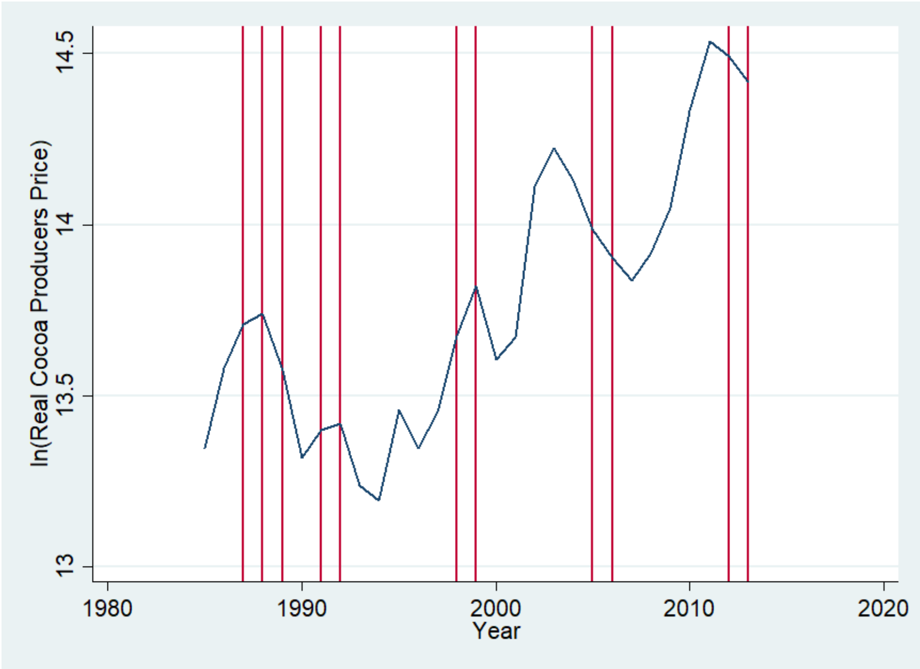

FIGURE B.3. Real Producer Price of Cocoa (Time Series) Used in the Contemporaneous Analysis. The vertical lines represent the interview years.

Source: Author's computation using data from Teal(2002) and Ghana Cocoa Board

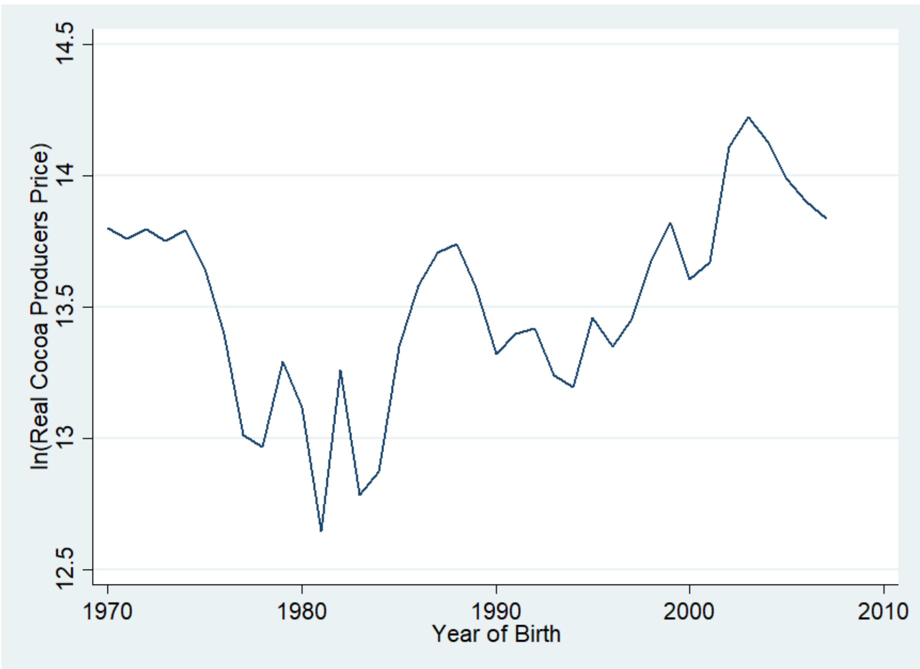

FIGURE B.4. Real Producer Price of Cocoa (Time Series) Used in the Early Life Analysis

Source: Author's computation using data from Teal(2002) and Ghana Cocoa Board

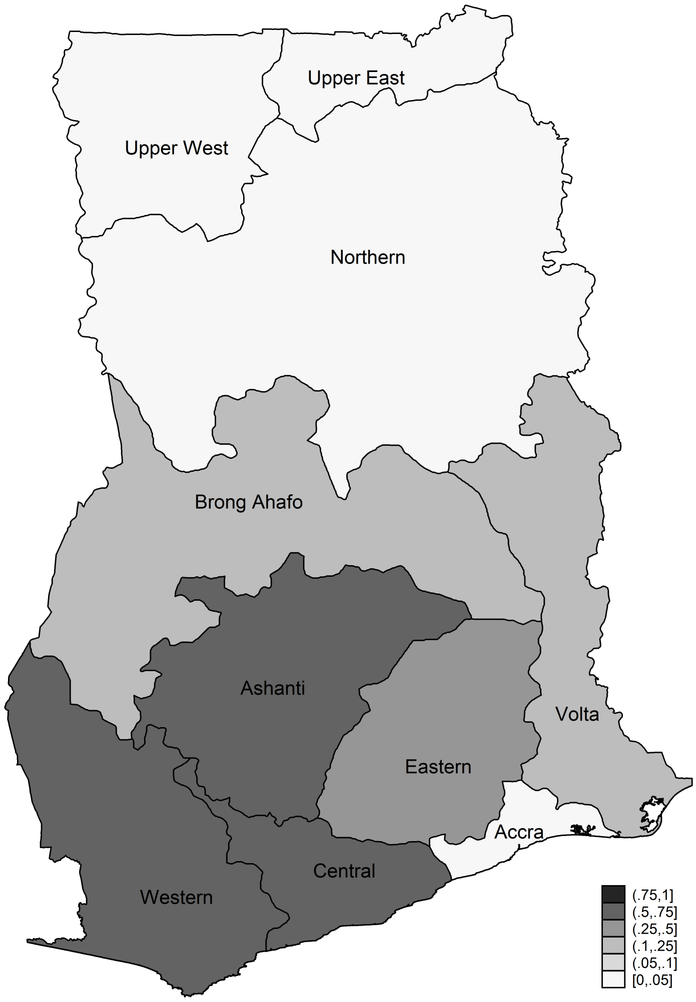

FIGURE B.5. Regions suitable for cocoa production

Source: Adhvaryu et al. (2014)

Table B.7: The effect of in utero price shock on other tests

	(1) Simple reading	(2) Simple math	(3) Advanced reading	(4) Advanced math
Shock in utero	12.881***	2.977	4.821	3.865
	(3.990)	(3.394)	(13.026)	(4.275)
Observations	1,373	1,969	571	627
Region of birth FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
Region of birth trends	Yes	Yes	Yes	Yes
Child Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child and gender of household head. Only subset of household members who have three and more years of schooling were given the easy maths and easy reading tests. So I include grade attainment for the regression of these outcomes. In addition, those who scored 50 percent or more on these tests were asked to take the advanced tests in English and maths. This implies that those who sat for the advanced tests are selected from the sample of individuals based on their score in the easy tests, which creates a concern of sample selection problem. To address, this sample selection problem, I include the inverse mills ratio (IMR) in the advanced tests regressions. The inverse mills ratio (IMR) is computed as follows:

$$IMR(\lambda) = \frac{\phi(0.5 - X'_{it}\beta)}{1 - \Phi(0.5 - X'_{it}\beta)}, \tag{3.4}$$

where $\phi(.)$ and $\Phi(.)$ are the Gaussian pdf and CDF, 0.5 is the selection criteria (in which students are required to score 50% in the simple tests to sit for the advanced tests) and $X'_{it}\beta$ is the fitted value from the probit regression of sitting for advanced tests on simple test scores). Furthermore, the raw scores of all tests are converted in to percentages of correct answers. The source of data is self-weighting samples.

Table B.8: Estimated effect of average early life cocoa price shock on cognition

	Age 9 to 17	Age 6 to 17
	(1) Raven/IQ	(2) Grade
Average early life shock	5.033***	0.467***
	(1.201)	(0.093)
Observations	2,826	49,621
R-squared	0.152	0.361
Region of birth FE	Yes	Yes
Year of birth FE	Yes	Yes
Region of birth trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child and gender of household head. The Raven/IQ includes children of ages 9 to 17, while grade is reported for children of age 6 to 17. Shock in this case is constructed as interaction between the average prices in early life (in utero, year of birth, year of birth plus 1 year, year of birth plus 2 years, year of birth plus 3 years and year of birth 4 years) with the indicator if the region of birth is cocoa producer. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

Table B.9: Estimated health effect of in utero cocoa price shock on investments, excluding 2008 GDHS

	(1)	(2)	(3)	(4)	(5)
	Panel A: Prenatal/at-birth investments				
	Doctor assist prenatal care	Doctor assist delivery	Received BCG	Received Polio 0 dose	
Shock in utero	0.056**	0.001	0.065***	-0.026	
	(0.024)	(0.009)	(0.020)	(0.057)	
Observations	9,798	10,642	9,236	6,421	
	Panel B: Childhood investments				
	No. of polio doses	No. of DPT doses	Measles	No. of total vaccinations	Months of breastfeeding
Shock in utero	0.246***	0.240***	-0.023	0.395**	1.768***
	(0.037)	(0.068)	(0.030)	(0.170)	(0.290)
Observations	9,287	9,188	9,155	9,302	10,530
Region of birth FE	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes
Region of birth trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Household size, birth order, mother education, rural, gender of child, and dummies for ethnicity and religion. Panel A presents results on childhood investments and panel B focuses results on birth investments. Survey weights are used in the regression estimations.

Table B.10: Controlling for rainfall, contemporaneous

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Current Attendance			
Current year shock	-0.080***	-0.101***	-0.058***	-0.044
	(0.021)	(0.016)	(0.021)	(0.043)
Mean annual rainfall	0.000	-0.001	0.000	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	59,004	31,773	14,936	12,295
	Panel B: Grade-for-age			
Previous year shock	-0.064***	-0.088***	-0.022*	-0.032*
	(0.009)	(0.013)	(0.011)	(0.018)
Mean annual rainfall	-0.000	-0.001	-0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.000)
Observations	49,972	26,636	12,716	10,620
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Mean annual rainfall at region of survey level, gender of the child, child age, and gender of household head. Panel A reports results on current attendance, and panel B shows the effect of price shock on grade-for-age. Survey weights are used in the regression estimations.

Table B.11: Controlling for rainfall, in utero

	Age 9 to 17	Age 6 to 17
	(1)	(2)
	Raven/IQ	Grade
Shock in utero	3.879***	0.379***
	(1.310)	(0.047)
Mean annual rainfall	0.055*	-0.002
	(0.033)	(0.003)
Observations	1,820	49,621
Region of birth FE	Yes	Yes
Year of birth FE	Yes	Yes
Region of birth trends	No	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Mean annual rainfall at region of birth level, gender of the child and gender of household head. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

Table B.12: Estimated effect of current year cocoa price shock on labour supply of mothers

	(1)	(2)	(3)	(4)
	Any work	Agri. self employed or contributing to family	Non-agri. self employed or contributing to family	HH chores
Current year shock	-0.006	-0.045	0.045***	-0.004
	(0.026)	(0.034)	(0.015)	(0.008)
Observations	30,261	30,244	30,244	30,212
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): gender, age, and gender of household head. Column (1) reports results on any work, and column (2) shows the effect of price shock on agricultural employment, column (3) shows the effect of price shock on non-agricultural employment and Column (4) shows the effect of price shock on household chores. Survey weights are used in the regression estimations.

Table B.13: Mortality and Fertility selection checks

	Mortality selection	Fertility selection				
	(1)	(2)	(3)	(4)	(5)	(6)
	Boy birth	Mother years of education	Mother age	Mother height	Number of births	Husband in agriculture
Shock in utero	-0.003					
	(0.005)					
Shock in utero		0.039	-0.072	1.407	0.037	0.001
		(0.064)	(0.061)	(1.750)	(0.040)	(0.005)
Observations	67,676	67,656	67,676	42,383	67,676	66,463
Birth region FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Survey weights are used in the regression estimations.

Table B.14: Correction for selection, cognitive outcomes

	(1)	(2)
	Raven/IQ	Grade
Shock in utero	3.185*	0.384***
	(1.736)	(0.037)
Observations	2,228	42,711
Household FE	Yes	Yes
Region of birth FE	Yes	Yes
Year of birth FE	Yes	Yes
Region of birth trends	Yes	Yes
Child Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child and gender of household head. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

Table B.15: Robustness check using intensity of cocoa production, contemporaneous

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Current attendance			
Current year shock(intensity)	-0.094	-0.136*	-0.100	0.037
	(0.072)	(0.080)	(0.070)	(0.074)
Observations	59,004	31,773	14,936	12,295
	Panel A: Grade-for-age			
Previous year shock(intensity)	-0.098***	-0.119**	-0.082***	-0.060
	(0.032)	(0.049)	(0.029)	(0.041)
Observations	49,972	26,636	12,716	10,620
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child, child age, and gender of household head. Panel A reports results on current attendance, and panel B shows the effect of price shock on grade-for-age. Survey weights are used in the regression estimations.

Table B.16: Robustness check using intensity of cocoa production, in utero

	(1) Raven/IQ	(2) Grade
Shock in utero (intensity)	0.257	0.266
	(3.522)	(0.236)
Observations	2,285	49,621
Region of birth FE	Yes	Yes
Year of birth FE	Yes	Yes
Region of birth trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of child and gender of household head. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

Table B.17: Robustness check after drooping Accra and Volta from the sample production, contemporaneous

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Current attendance			
Current year shock	-0.076***	-0.103***	-0.056**	-0.018
	(0.024)	(0.021)	(0.025)	(0.042)
Observations	47,551	25,773	11,977	9,801
	Panel B: Grade-for-age			
Previous year shock	-0.071***	-0.095***	-0.029***	-0.042*
	(0.010)	(0.013)	(0.010)	(0.022)
Observations	40,643	21,763	10,314	8,566
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child, child age, and gender of household head. Panel A reports results on current attendance, and panel B shows the effect of price shock on grade-for-age. Survey weights are used in the regression estimations.

Table B.18: Robustness check after dropping Accra and Volta from the sample, in utero

	(1) Raven/IQ	(2) Grade
Shock in utero	3.420***	0.361***
	(1.305)	(0.053)
Observations	2,210	40,803
Region of birth FE	Yes	Yes
Year of birth FE	Yes	Yes
Region of birth trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of child and gender of household head. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

Table B.19: High price vs low price, contemporaneous

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Current attendance			
High Price X Cocoa_producer	-0.123***	-0.148***	-0.106***	-0.084*
	(0.030)	(0.027)	(0.031)	(0.043)
Low Price X Cocoa_producer	0.112***	0.147***	0.083***	0.029
	(0.027)	(0.022)	(0.020)	(0.090)
p-value (High Price X Cocoa Producer)= (Low Price X Cocoa Producer)	0.605	0.962	0.545	0.328
Observations	59,004	31,773	14,936	12,295
	Panel B: Grade-for-age			
High Price X Cocoa_producer	0.023	0.026	0.035	0.028
	(0.024)	(0.026)	(0.037)	(0.018)
Low Price X Cocoa_producer	0.103***	0.135***	0.057**	0.066*
	(0.023)	(0.031)	(0.023)	(0.034)
p-value (High Price X Cocoa Produce)r= (Low Price X Cocoa Producer)	0.000	0.000	0.546	0.241
Observations	49,972	26,636	12,716	10,620
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child, child age, and gender of household head. Panel A reports results on current attendance, and panel B shows the effect of price shock on grade-for-age. High price category is an indicator that takes 1 if price is above and equal to Price Xtile 8 and 0 otherwise. Low price category is an indicator that takes 1 if price is below and equal to Price Xtile 3 and 0 otherwise. Survey weights are used in the regression estimations.

Table B.20: High price vs low price, in utero

	(1)	(2)	(3)
	Raven/IQ	Grade	Grade
High Price X CocoaProducer	3.903***	-0.393***	
	(1.381)	(0.132)	
Low Price X CocoaProducer	-7.726***	-1.554***	
	(2.352)	(0.144)	
Price Xtile 2 X CocoaProducer			0.895***
			(0.099)
Price Xtile 3 X CocoaProducer			0.839***
			(0.099)
Price Xtile 4 X CocoaProducer			1.995***
			(0.176)
Price Xtile 5 X CocoaProducer			2.243***
			(0.188)
Price Xtile 6 X CocoaProducer			1.674***
			(0.130)
Price Xtile 7 X CocoaProducer			2.068***
			(0.170)
Price Xtile 8 X CocoaProducer			2.601***
			(0.390)
Price Xtile 9 X CocoaProducer			1.473***
			(0.160)
Price Xtile 10 X CocoaProducer			1.269***
			(0.121)
p-value (High Price X Cocoa Producer)= (Low Price X Cocoa Producer)	0.030	0.000	
Observations	2,826	49,621	49,621
ROB FE	Yes	Yes	Yes
YOB FE	Yes	Yes	Yes
ROB Trends	Yes	Yes	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of child and gender of household head. High Price is a dummy equal to 1 if Price Xtile is above or equal to 8 and 0 otherwise; Low Price is a dummy equal to 1 if Price Xtile below or equal to Price Xtile 3 and 0 otherwise. In column (3), Price Xtile 1 X CocoaProducer is the omitted (and thus the reference) group. The source of data for Raven/IQ outcome is self-weighting samples. However, survey weights are used in the regression estimation of grade outcome.

Table B.21: Fixed effect logit (Marginal effects)

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Current Attendance			
Current year shock	-0.040**	-0.039**	-0.019	-0.017
	(0.020)	(0.015)	(0.016)	(0.041)
Observations	59,004	31,773	14,936	12,295
	Panel B: Grade-for-age			
Previous year shock	-0.087***	-0.116***	-0.052	-0.056***
	(0.009)	(0.016)	(0.035)	(0.020)
Observations	49,972	26,636	12,716	10,620
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X):Gender of the child, child age, and gender of household head. Panel A reports results on current attendance, and panel B shows the effect of price shock on grade-for-age. Survey weights are used in the regression estimations.

Table B.22: Regressions that don't use survey weights, contemporaneous

	All (age 6-17)	Primary (age 6-11)	Junior High (age 12-14)	Senior High (age 15-17)
	(1)	(2)	(3)	(4)
	Panel A: Current attendance			
Current Shock	-0.107***	-0.127***	-0.090***	-0.072
	(0.027)	(0.023)	(0.023)	(0.051)
Observations	59,004	31,773	14,936	12,295
	Panel B: Grade-for-age			
Previous Shock	-0.061***	-0.084***	-0.021*	-0.030
	(0.009)	(0.011)	(0.011)	(0.019)
Observations	49,972	26,636	12,716	10,620
Region FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Region time trends	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the region of interview level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): Gender of the child, child age, and gender of household head. Panel A reports results on current attendance, and panel B shows the effect of price shock on grade-for-age. In this case, regressions don't use survey weights.

Table B.23: Regressions that don't use survey weights, in utero

	(1)
	Grade
Shock in utero	0.366***
	(0.051)
Observations	49,640
Region of birth FE	Yes
Year of birth FE	Yes
Region of birth trends	Yess
Controls	Yes

Robust standard errors (clustered at the region of birth level) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Controls include (X): Gender of child and gender of household head. In this case, the regression doesn't use survey weights.

Chapter 4

The first and second generation effects of bombing Vietnam

Habtamu Ali Beshir

Department of Economics, Lancaster University

Abstract

The most intense aerial bombing episode in history happened during the Vietnam War. In this study, I investigate the long-term impacts of early life exposure to bombing Vietnam. Exploiting a unique U.S. military dataset on bombing intensity at the province level and using difference-in-difference and instrumental variable methods, I estimate exposure to the war at early life on education and labour market outcomes of adults. I also explore if the impacts of the exposure to the bombing transmitted to the second generation. I find that, for the first generation sample, exposure to bombing decreases the odds of earning upper secondary education and being a wage worker. Exposed individuals are more likely to engage in self employment. Specifically, a standard deviation increase in bombing intensity decreases the likelihood of having upper secondary education and engaging in wage work by 11% and 9%, respectively. A standard deviation increase in bombing intensity increases the likelihood of engaging in self employment by 5%. For the second generation sample, however, no significant effect is found on the education attainment of children whose parents were exposed to bombing at early life.

Keywords: Vietnam; Bombing, Labour Market; Education; Human Capital.

JEL Classification: I12; I25; J13 O12

4.1 Introduction

The first half of the 20^{th} century experienced two major catastrophic events: WWW I and WWW II.[1] Figure C.1 in Appendix C.1 shows that, even though there were certain peaks, after the end of the World War II the number of war related deaths has been declining. One of these peaks was due to the Vietnam war that happened between 1965-1975. Bombing Vietnam resulted in a loss of millions of lives. Since 2015, a Saudi Arabia-led coalition supported by USA has been bombing Yemen. This has created a humanitarian crisis (Nicholas Kristof, 2018). In addition to the toll of human life as a result of a war, those who experienced the conflict at early life and survived would have worse educational and economic advantage following them to adult life. The long-term effects of exposure to war at a young age on socio-economic outcomes need to be explored. More importantly so, in order to design appropriate policies to curve the potential transmission of the adverse effects to the second generation.

In this study, I investigate the long-term effects of early life exposure to bombing Vietnam on the education and labour market outcomes of individuals who were directly exposed to the war (first generation). The study also explores if the effects of the bombing transmitted to the second generation. I exploit two unique datasets: 15% sample of the Vietnam Population and Housing Census conducted in 2009 and province level military data on bombing intensity. I estimate a difference-in-difference model as a baseline. In order to account for omitted variable bias, I estimate a combination of instrumental variable and difference-in-difference methods where I instrument the bombing intensity (and its interaction with indicator if cohort was exposed to the war) with distance from the 17^th parallel (and its interaction with indicator if cohort was exposed to the war). I find significant and negative effect of bombing on the education and labour market outcomes of those who were directly exposed to the bombing (first generation). However, no effect is found on the second generation. For the first generation sample, exposure to bombing decreases the odds of earning upper secondary education and being a wage worker. Exposed individuals

[1] In WWW II alone about 39 million people died in Europe (Kesternich et al., 2014).

are more likely to engage in self employment. Specifically, a standard deviation increase in bombing intensity decreases the likelihood of having upper secondary education and engaging in wage work by 11% and 9%, respectively. A standard deviation increase in bombing intensity increases the likelihood of engaging in self employment by 5%.

Several mechanisms could play a role in explaining the results on the first generation outcomes. Bombing may have destructed school infrastructures. In addition, schools in heavily bombed areas may have been closed due to security concerns. For instance, Dell and Querubin (2017) find that the bombing decreases access to primary and secondary schools. As a result, school aged children were not able to attend schools (which adversely determines adulthood schooling and labour market outcomes). War may obstruct production and trade Gráda (2007), which results in food shortage in conflict affected areas. Malnutrition at early life as a result of the exposure to food shortage may be a significant factor for adverse adulthood effects documented here. For example, Singhal (2018) finds that exposure to bombing Vietnam decreases the long-term height acquired by adults, which is a manifestation of childhood deprivation of nutrition. Moreover, as documented in (Camacho, 2008; Mansour and Rees, 2012; Lee, 2014) maternal stress due to the conflict may be another determining factor for the results found. In this study, however, I do not intend to (nor I am able to) distinguish among these mechanisms. One or all of these factors could be the potential mechanisms of the adverse results of the war. With regard to the estimated effect on the second generation outcomes, I argue that compensatory state investment that came in time to the second generation might be the reason for no transmission of adverse bombing effects to the next generation.

The study is related to a growing number of evidences that are investigating the causal impacts of childhood exposure to conflicts and wars on adulthood outcomes in developing countries. For example, Alderman et al. (2006) study the effect of exposure to civil war [and also drought] in Zimbabwe. They find that war exposure led to significant reduction in child height that in turn reduced adolescent height and grade attainment. Akresh and De Walque (2008) investigate the impact of exposure to

Rwandan genocide. They find that exposed children acquired less years of schooling. Leon (2012) document the effect of civil conflict on human capital accumulation in Peru. The study finds that individuals exposed to the conflict accumulated fewer years of schooling on average. Akresh et al. (2012) report that growing up during the Nigerian civil war has a significantly adverse effect on adult height. More importantly, Akresh et al. (2017) study both the first and second generation effects of Biafran war. For women, exposure to the war reduces height, increases the probability of being overweight. Exposed women also have children earlier, and attain lower level of education. Furthermore, these adverse effects of the war on mothers were transmitted to the their children.

More directly related to this study, a few studies explore the long-term effects of the Vietnam War. Miguel and Roland (2011) focuses on the long-term aggregate effects of the war on local poverty rates, consumption levels, infrastructure, literacy or population density. At local aggregate level, Miguel and Roland (2011) find no effect on poverty and other economic outcomes. Palmer et al. (2016) also document the effect of the war on district level health outcomes (disability rate). Unlike, Miguel and Roland (2011), Palmer et al. (2016) show bombing intensity at district level increases the prevalence of severe disability. These studies, however, do not consider the effect of the war based on individuals' early life war exposure. Singhal (2018) investigates the long-term effects of the Vietnam war exposure on different health outcomes using individual level early life war exposure. He finds that individual level early life exposure decreases mental health at adulthood. In this study, I add to this literature by exploring the effect of early life individual level exposure to the bombing not just on the socio-economic outcomes of people directly exposed to the war (first generation) but also their offspring (second generation).

The remainder of the paper is organized as follows. The next section presents the relevant background information on the Vietnam War. Section 4.3 and Section 4.4 deal with data and identification strategy, respectively. In Section 4.5 and Section 4.6, I discuss the main results and discussion, respectively. In Section 4.7, I present some heterogeneity analysis. Section 4.8 addresses some identification threats and finally, Section 4.9 concludes.

4.2 The Vietnam War

Following the conclusion of the first Indochina war (1946-1954) and the end of French colonization of the region, Vietnam was dividend in to a communist North Vietnam and a pro-West South Vietnam (Do, 2009). The two independent countries were created North and South of the 17^{th} parallel following the Geneva conference on July 21^{st}, 1954. In 1960 the National Liberation Front (NFL) or the Viet Cong (VC) was established. The same year, led by the VC and supported by the North Vietnam army (NVA), a communist insurgency started in the South Vietnam.

After US ships were attacked at the Gulf of Tonkin in August 1964, the US started military deployment in South Vietnam. President Lyndon Johnson deployed 200,000 American troops to South Vietnam in 1965 and by 1969 the U.S. troops in Vietnam reached at over half a million (Dell and Querubin, 2017).

In terms of weight (tonnage), the bombing of Vietnam is estimated to be at least three times more than the bombing during the World War II, and about fifteen times the bombing in the Korean War (Miguel and Roland, 2011). It is considered to be one of the most intense in history. Although, bombing also happened in the South Vietnam in order to to disrupt the NFL movements and assist the operation of the US troops on the ground, heavy bombing occurred in the border between the North and South Vietnam around the 17^{th} parallel. The war ended on April 30, 1975 after communist forces entered Saigon, capital of South Vietnam. The war resulted in a staggering amount of Vietnamese deaths. It is estimated that from 1 million (Hirschman et al., 1995) to 3.8 million (Obermeyer et al., 2008) Vietnamese died as a result of the war.

4.3 Data

The study exploits two datasets from different sources. For individual characteristic (both outcomes and controls), I use Vietnam 2009 census data. I use 15% sample of the Vietnam Population and Housing Census. The census was conducted by General Statistics Office of Vietnam (GSO) in April 2009. It comprises of 3,692,042 households and 14,177,590 individuals. The data is obtained from IPUMS-international.[2]

The dataset contains information both at household and individual level. In this study, I exploit education and labour market outcomes as outcome variables and some predetermined household and child characteristics as controls. I also employ birth year and month information to locate the cohorts that were exposed to the Vietnam war. I restrict the sample only to include individuals born between 1960 to 1986. Panel A in Table 4.1 shows the summary statistics of these variables. Columns (1) to (3) report the summary statistics of variables employed in the first generation analysis. Columns (4) to (6) presents descriptive statistics of the characteristics of individuals whose mothers were exposed to the war. Finally, columns (7) to (9) depict descriptive statistics of variables for individuals whose fathers were exposed to the war. In column (1), on average, 28% individuals in the first generation sample have upper secondary education (education more or 10 years of schooling). They have close to 8 years of schooling; 28% of them are in wage employment; and 45% of the sample work in self employment. In second generation sample, on average, 19% of children whose mothers were exposed to the war and 17% of children whose fathers were exposed to the war have upper secondary education. On average, children in both samples have around 6 years of schooling.

With regard to bombing intensity in Vietnam, I exploit a unique database assembled by the Defence Security Cooperation Agency (DSCA). The dataset is available from Miguel and Roland (2011).[3] Bombing intensity is reported both at province and district level. In this study, I exploit the bombing intensity at province level. These is done for two reasons. First, there might be bombing externalities across districts with in a province. Second, it is likely that migration across districts and with in a province happened during the war. So to capture these two factors, I

[2]Minnesota Population Center. Integrated Public Use Microdata Series, International: Version 7.1 [dataset]. Minneapolis, MN: IPUMS, 2018. https://doi.org/10.18128/D020.V7.1.

[3]For detailed description of the data, see Miguel and Roland (2011).

Table 4.1: Descriptive statistics of main variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A:Individual characteristics								
	(1) First Generation			(2) Second Gen. Via Mom			(3) Second Gen. Via Dad		
	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD	Obs.
Upper secondary	0.283	0.450	4996350	0.188	0.391	2734567	0.165	0.371	2278241
Years of school	7.928	3.963	4996350	6.210	3.560	2734567	5.953	3.478	2278241
Wage worker	0.283	0.450	4988860						
Self employed	0.452	0.498	4988860						
Born during 1960-1975	0.546	0.498	4996350						
Total bombing per km^2 $\times$ Born during 1960-1975	14.710	34.646	4996350						
Year of birth	1973.905	7.732	4996350	1994.151	4.761	2734567	1994.655	4.492	2278241
Male	0.493	0.500	4996350	0.538	0.499	2734567	0.535	0.499	2278241
Kinh	0.810	0.393	4996350	0.765	0.424	2734567	0.756	0.430	2278241
Rural	0.729	0.444	4996350	0.786	0.410	2734567	0.804	0.397	2278241
Head age	43.847	13.190	4996350	43.270	9.431	2734567	42.320	9.303	2278241
Head is male	0.801	0.399	4996350	0.831	0.375	2734567	0.906	0.291	2278241
Panel B: Bombing intensity ad province level geographic variables									
	Mean	SD	Obs.						
Total U.S. bombs, missiles, rockets	140657.041	260473.793	61						
Total U.S. bombs, missiles, rockets per km^2	28.628	49.747	61						
Population Density 1960-61	228.574	418.209	61						
Av. precipitation (cm)	156.472	30.895	61						
Av. temperature (Celsius)	24.275	1.909	61						
Latitude (°N)	17.953	5.417	61						
Proportion of land area 250500 m	0.124	0.140	61						
Proportion of land area 5001000 m	0.134	0.172	61						
Proportion of land area over 1000 m	0.049	0.097	61						

Source: Vietnam Population and Housing Census (for individual characteristics) and Miguel and Roland (2011) (for bombing data)

focus on province level analysis, which exploits bombing intensity at province level.[4] Panel B in Table 4.1 presents the summary statistics of bombing intensity across 61 provinces of Vietnam. On average, around 140657 bombs, missiles, rockets were dropped across provinces in Vietnam. In this study, I use total U.S. bombs, missiles, rockets per km^2 as proxy for bombing intensity. On average, about 29 bombs, missiles, rockets per km^2 were dropped across provinces in Vietnam. The standard deviation of total U.S. bombs, missiles, rockets per km^2 is 50 bombs, missiles, rockets per km^2, which depicts a significant variation in bombing across provinces.

4.4 Empirical Strategy

The study has two objectives. The first objective is to investigate the effect of early life (including in utero) exposure to bombing Vietnam on education and labour market

[4]Nonetheless, as a robustness analysis, I also discuss results from using district level bombing intensity.

outcomes of adults. The second objective is to explore if the effect of the exposure to bombing transmitted to the second generation.

To estimate the effect of bombing on the first generation outcomes, I follow the following difference-in-difference specification that is estimated using ordinary least square (OLS) (linear probability model (LPM) for binary outcomes) method.

$$Y_{iprt} = \alpha_r + \theta_t + \delta_r t + \lambda \text{Bombs}_{pr} + \beta \text{Bombs}_{pr} \times \text{Born1960-1975}_i + X'_{iprt}\Upsilon + \varepsilon_{iprt}, \quad (4.1)$$

where Y_{iprt} designates the outcome variables for individual i, who live in province p and region r, and born at year t. These include an indicator that takes 1 if individual has upper secondary education and 0 otherwise; years of schooling; an indicator that takes 1 if individual is wage worker and 0 otherwise; and an indicator that takes 1 if individual is self employed and 0 otherwise.[5] Bombs_{pr} is the total U.S. bombs, missiles, rockets per km^2 that were dropped in province p and region r during 1965 to 1975.[6] Born1960-1975_i is an indicator that takes 1 if the individual was born between 1960 to 1975.[7] Individuals born between 1976 to 1986 are given 0 and are considered as a comparison (control) group. For individuals born between 1976 to 1986 (comparison group), $\text{Bombs}_{pr} \times \text{Born1960-1975}_i$ is equal to 0. The strategy also controls for α_r, region fixed effects; θ_t, year of birth fixed effects; and $\delta_r t$, region specific year of birth trends. X'_{iprt} represents household and individual level controls. X'_{iprt} also includes province level war time characteristics. These include: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius); and latitude (^{o}N). The parameter of interest is β. It shows the effect of increase in bombing intensity by 1 unit for

[5]The intuition behind using self employment outcome is individuals affected by the bombing could have less education that in turn leads to joining self employment opportunities which require low skills (Majid, 2015).

[6]There are 8 regions in Vietnam. Across these 8 regions, the sample contains 61 provinces where I have military data and able to merge with the census data.

[7]Even though the war started in 1965, I also included individuals born between 1960 to 1965, because this group were exposed to the war in their early life (between birth to age 5).

individuals born during 1960 to 1975. Standard errors are clustered at the province level and, thus, the error terms are allowed to be correlated with in provinces.

Identification of the causal effect of early life bombing exposure on education and labour market outcomes depends on the assumption that, conditional on province level war time characteristic, birth year and region fixed effects, and also region specific time trends, Bombs_{pr} and $\text{Bombs}_{pr} \times \text{Born1960-1975}_i$ are not correlated with omitted factors that affect the outcome variables. However, the non-random nature of the bombing casts doubt on this assumption. To be more specific, if different provinces bombed less (more) due to different characteristics and if these factors are related with the outcome variables considered here, the OLS estimation above will suffer from omitted variable bias.[8] To address this, I employ instrumental variable strategy. Following Miguel and Roland (2011) and Singhal (2018), I use the distance from each province to the 17^{th} parallel north latitude as the instrument for the total U.S. bombs, missiles, rockets per km^2 that were dropped in province p during 1960 to 1975. The heaviest bombing took place around the 17^{th} parallel. In addition, distance to the 17^{th} parallel is exogenous as it was arbitrarily set as a border between North and South Vietnam through a Cold War negotiations between U.S. and Soviet officials (Miguel and Roland, 2011). In this particular case, the endogenous variables in equation (4.1) are both bombing intensity (Bombs_{pr}) and the interaction of bombing intensity with individual's exposures to bombing ($\text{Bombs}_{pr} \times \text{Born1960-1975}_i$). These are the variables that need to be instrumented. The instruments are distance to the 17^{th} parallel and the interaction between distance to the 17^{th} parallel and exposure to bombing. As a result, we have the following first stage regressions analogues to the OLS regression above:

$$\text{Bombs}_{pr} = \alpha_r + \theta_t + \delta_r t + \beta \text{Distance}_{pr} + \lambda \text{Distance}_{pr} \times \text{Born1960-1975}_i + X'_{iprt} \Upsilon + \varepsilon_{iprt}, \tag{4.2}$$

[8] Bombs_{pr} is endogenous variable. If Bombs_{pr} is endogenous, the composite variable constructed from it ($\text{Bombs}_{pr} \times \text{Born1960-1975}_i$) will also be endogenous.

$$\begin{aligned}\text{Bombs}_{pr} \times \text{Born1960-1975}_i = \alpha_r + \theta_t + \delta_r t + \beta \text{Distance}_{pr} \\ + \lambda \text{Distance}_{pr} \times \text{Born1960-1975}_i + + X'_{iprt} \Upsilon + \varepsilon_{iprt},\end{aligned} \tag{4.3}$$

where Bombs_{pr} is the total U.S. bombs, missiles, rockets per km^2. $\text{Bombs}_{pr} \times \text{Born1960-1975}_i$ is the interaction between the total U.S. bombs, missiles, rockets per km^2 and an indicator that takes 1 if the individual was born between 1960 to 1975. Distance_{pr} and $\text{Distance}_{pr} \times \text{Born1960-1975}_i$ are the instrument variables (distance of the province p from the 17^{th} parallel and its interaction with an indicator that takes 1 if the individual was born between 1960 to 1975). The two equations also controlled for α_r, region fixed effects; θ_t, year of birth fixed effects; $\delta_r t$, region specific year of birth trends; and X'_{ipt}, household, individual and province level characteristics.

To estimate the effect of bombing on the second generation outcomes, I estimate the reduced form relationship between parents' bombing exposure and second generation outcomes. It follows similar difference-in-difference specification to above that is estimated using ordinary least square (OLS) (linear probability model (LPM) for binary outcome) method.

$$\begin{aligned}Y_{iprt} = \alpha_r + \theta_t + \delta_r t + \lambda \text{MomBorn1960-1975}_i + \eta \text{Bombs}_{pr} \\ + \beta \text{Bombs}_{pr} \times \text{MomBorn1960-1975}_i + X'_{iprt} \Upsilon + \varepsilon_{iprt},\end{aligned} \tag{4.4}$$

Equation (4.4) estimates if the effect of mother's exposure to bombing transmitted to her offspring. Y_{iprt} designates the outcome variables for child i, who lives in province p and region r, and born at year t. The outcomes include: an indicator that takes 1 if individual has upper secondary education and 0 otherwise; and years of schooling. The interest independent variable in this case is $\text{Bombs}_{pr} \times \text{MomBorn1960-1975}_i$. It is the interaction of province level bombing intensity with mother's exposure to the war. The regression controls for $\text{MomBorn1960-1975}_i$, an indicator if mother born during 1960 to 1975; Bombs_{pr}, bombing intensity; α_r, region fixed effects; θ_t, year of birth fixed effects; $\delta_p t$, region specific year of birth trends;

and X'_{iprt} household, child and province level characteristics. Similar exercise is also conducted in order to estimate the effect of father's bombing exposure on his children's outcomes. Moreover, similar to the first generation analysis, I also report instrumental variable estimation results. In this case, I instrument bombing intensity at the province level and its interaction with parents' early life bombing exposure by distance to the 17^{th} parallel and its interaction with parents' bombing exposure.

4.5 Results

4.5.1 First Stage Results and Validity of Instruments

There are two endogenous variables in equation (4.1). I report two first stage regressions in Table 4.2. Column (1) reports the relationship between distance from the 17^{th} parallel and bombing intensity at province level controlling for province level characteristics. Column (2) shows, controlling for other factors, the relationship between the interaction of distance from the 17^{th} parallel and indicator whether an individual is born between 1960 to 1975 with the interaction of bombing intensity at province level and indicator of an individual was born between 1960 to 1975. In column (1), distance from the 17^{th} parallel is negatively and strongly correlated with the bombing intensity. This means as one moves away from the 17^{th} parallel, bombing intensity decreases. This makes sense since bombing was the the heaviest around the 17^{th} parallel. In column (2), the interaction of distance from the 17^{th} parallel and indicator whether an individual was born between 1960 to 1975 and interaction of bombing with indicator if individual was born between 1960 to 1975 are also highly correlated. These results are suggestive that the instruments for the two endogenous variables are relevant. The Sanderson-Windmeijer F-test from the first stages are reasonably high (F=12.7 in column (1) and F=12.03 in column (2)). Moreover, the F-stat is even larger when I use district level bombing intensity instead (see, Table C.9 in Appendix C.1).

Furthermore, in the reduced form regression reported in Table 4.3, the instrument for the interest variable (Distance$\times$ born during 1960-1975) is strongly related with

Table 4.2: First stage

	(1) Total bombing per km^2	(2) Total bombing per $km^2\times$ Born during 1960-1975
Distance	-20.95796**	-2.65970
	(8.07040)	(2.92706)
Distance× Born during 1960-1975	-0.45207*	-17.32779***
	(0.23463)	(5.90927)
Sanderson-Windmeijer F-test	12.72	12.03
Observations	4,996,350	4,996,350
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Sanderson-Windmeijer F-test is a modified version of Angrist-Pischke multivariate F test. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). Person weights are used in the regression estimations.

the outcome variables. This evidence together with the results from the first stage regressions suggest that the instruments are fairly strong and relevant. Nonetheless, in Table 4.4, I report Anderson-Rubin p-value. It is the weak instrument robust inference on the endogenous regressors using the Anderson-Rubin Wald test (F-stat version). It tests the null that the coefficients on the endogenous variables are jointly equal to zero. The Anderson-Rubin test in Table 4.4 rejects this null hypothesis for outcomes other than years of schooling. This means bombing intensity and its interaction with indicator of cohort exposed to the war significantly affect these outcomes.

Table 4.3: Reduced Form

	(1) Upper secondary	(2) Years of school	(3) Wage worker	(4) Self emp't
Distance	0.00914	0.08294	-0.01858*	-0.03751***
	(0.00649)	(0.10301)	(0.01028)	(0.01167)
Distance × born during 1960-1975	0.01043***	0.04994	0.00649***	-0.00559***
	(0.00306)	(0.03645)	(0.00220)	(0.00181)
Observations	4,996,350	4,996,350	4,988,860	4,988,860
YOB FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). Person weights are used in the regression estimations.

4.5.2 Main Results

A. First Generation Results

Table 4.4 reports the first generation effect of bombing exposure on education and labour market outcomes estimated from equation (4.1). Panel A reports the effect of exposure to bombing estimated from OLS (DID), while panel B shows the effect of exposure to bombing estimated from instrumental variable (IV-DID) strategies. Column (1) shows bombing exposure effect on the probability of earning upper secondary education; column (2) depicts the effect on years of schooling; and column (3) and column (4) reports the effect on the likelihood of being in wage paying job and engaging in self employment respectively. While the coefficients on total bombing per km^2 tell us any spillover effects of bombing on individuals born after the the war, the coefficients on the interaction of total bombing per km^2 with indicator that individual is born during 1960-1975 identify the difference-in-difference effects of bombing on individuals born during the war (directly exposed to bombing).

In panel A of Table 4.4, it is evident that except for the self employment outcome, exposure to bombing significantly affects all the other outcomes. However, in Panel B, exposure to bombing significantly affects all outcome variables but the years of schooling outcome. In addition, in the IV estimation the magnitudes of the

Table 4.4: Estimated effect of exposure to bombing on first generation

	(1) Upper secondary	(2) Years of school	(3) Wage worker	(4) Self employed
	Panel A: OLS (DID)			
Total bombing per km^2	0.00029	0.00067	0.00110**	-0.00050
	(0.00025)	(0.00229)	(0.00051)	(0.00037)
Total bombing per km^2 × Born during 1960-1975	-0.00027**	-0.00257*	-0.00046**	0.00016
	(0.00013)	(0.00153)	(0.00020)	(0.00015)
Observations	4,996,350	4,996,350	4,988,860	4,988,860
	Panel B: IV(IV-DID)			
Total bombing per km^2	-0.00069	-0.00747	0.00107	0.00205*
	(0.00055)	(0.00638)	(0.00068)	(0.00112)
Total bombing per km^2 × Born during 1960-1975	-0.00061**	-0.00268	-0.00050***	0.00044**
	(0.00026)	(0.00264)	(0.00019)	(0.00021)
Anderson-Rubin P-value	0.0009	0.2336	0.0191	0.0005
Observations	4,996,350	4,996,350	4,988,860	4,988,860
YOB FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the individual is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

coefficients are larger than the OLS. Exposure decreases the probability of getting upper secondary education; and the likelihood of an individual to be a wage worker. However, it increases the likelihood of being self employed worker. Specifically, in column (1) of panel B, for individuals born between 1960 to 1970, an increase in the total U.S. bombs, missiles, rockets per km^2 by 1 decreases the probability of having upper secondary education by 0.0006 percentage points. An average of 29 and a standard deviation of 50 U.S. bombs, missiles, rockets per km^2 were dropped during the war across provinces in Vietnam (see, Table 4.1). This means for individuals born between 1960 to 1970, going from 0 bombs to 50 bombs (a standard deviation increase), decreases the chance to get upper secondary education by 0.031

(= 50*0.0006). This is 11% of the average value of upper secondary education. In column (3) for individuals born between 1960 to 1970, an increase in the total U.S. bombs, missiles, rockets per km^2 by 1 decreases the probability of an individual to be a wage worker by 0.0005. A standard deviation change in bombing could decrease the likelihood of to be a wage worker by 2.5 percentage points (=50*0.0005) which is equivalent to 9% of the average value. Lastly, for individuals born between 1960 to 1970, an increase in the total U.S. bombs, missiles, rockets per km^2 by 1 increases the probability of an individual being self employed by 0.00044. A standard deviation change in bombing could increase the likelihood of working in self employment by 2.2 percentage points (=50*0.00044) which is equivalent to 5% of the average value. Similarly, Akresh et al. (2017) find that exposure to the Biafran war in Nigeria reduces the probability of completing primary and secondary school especially for women.

B. Second Generation Results

In Table 4.5, I report the effects of mother's exposure to bombing at early life on her children's schooling outcomes. Panel A reports the results from OLS estimations, while panel B shows results obtained from estimations using instrumental variable method (IV). Column (1) shows result on the probability of having upper secondary education, while column (2) reports the result on years of schooling. The coefficients on total bombing per km^2 show any spillover effects of bombing on children of individuals born after the the war. The coefficients on the interaction of total bombing per km^2 with indicator if mom born during 1960-1975 (coefficients of interest) identify the difference-in-difference effects of bombing on children of mothers born during the war. Both in panel A and panel B, none of the coefficients are significant. The adverse effect of mother's exposure to bombing is not transmitted to her children's schooling outcomes.

Table 4.6 shows the effect of the effects of father's exposure on his children education outcomes. Similar to the previous analysis, Panel A reports the results from OLS estimations, while panel B shows results obtained from estimations using instrumental variable method (IV). In this case too, none of the coefficients are

Table 4.5: Estimated effect of mothers' exposure to bombing on second generation

	(1) Upper secondary	(2) Years of school
	Panel A: OLS (DID)	
Total bombing per km^2	0.00005	0.00000
	(0.00007)	(0.00067)
Total bombing per km^2 $\times$ Mom born during 1960-1975	0.00001	0.00013
	(0.00007)	(0.00056)
Observations	2,734,567	2,734,567
	Panel B: IV (IV-DID)	
Total bombing per km^2	0.00008	0.00143
	(0.00022)	(0.00239)
Total bombing per km^2 $\times$ Mom born during 1960-1975	-0.00010	-0.00058
	(0.00011)	(0.00088)
Observations	2,734,567	2,734,567
Dummy Mother born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

significant. Children born from fathers who were exposed to bombing are not systematically different from their counterparts (children born from fathers who were not exposed to bombing at early life). The take away from the second generation analysis is that the impacts of exposure to bombing have not been transmitted to the second generation.

Table 4.6: Estimated effect of fathers' exposure to bombing on second generation

	(1) Upper secondary	(2) Years of school
	Panel A: OLS (DID)	
Total bombing per km^2	0.00001	-0.00002
	(0.00009)	(0.00068)
Total bombing per km^2 × Dad born during 1960-1975	0.00005	0.00030
	(0.00007)	(0.00054)
Observations	2,278,241	2,278,241
	Panel B: IV (IV-DID)	
Total bombing per km^2	0.00011	0.00200
	(0.00022)	(0.00247)
Total bombing per km^2 × Dad born during 1960-1975	-0.00005	-0.00030
	(0.00013)	(0.00100)
Observations	2,278,241	2,278,241
Dummy Father born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

4.6 Discussion

It is worth exploring why there is no significant effect of bombing on the second generation. Importantly, why is that US bombing significantly impacts individuals exposed at early life but not their offspring? One plausible reason could be compensatory state investment. If the government distribute more investment to provinces that were highly affected by the bombing, one can expect the adverse effect of exposure to bombing be diminished to the extent it could be averted. Figure 4.1 shows the government's investment distribution to more heavily and less heavily bombed provinces over time. It shows the per capita investment in millions of 1985 Dong.[9] It is apparent that investment to both groups of provinces increased overtime. Immediately after the war ended (between 1976 to 1981), government investment flow towards both more heavily and less heavily bombed provinces was low. However, after 1981, the investment directed to these group of provinces shows an increasing trend. More importantly, after 1981, more investment is directed to more heavily bombed provinces compared to less heavily bombed ones. This distribution of state expenditures could have a compensating effect and, as a result, the effect of the exposure could have been weakened or faded away. For the first generation sample, however, there are reasons this might not be the case. Firstly, these individuals were exposed to bombing at early life (in utero to age 5). This is a sensitive and critical period to be exposed to shocks. The effects of exposure to shocks irreversibly persist to adulthood. Secondly, as Figure 4.1 indicates the compensating investment came to in effect after sometime. At this stage it may be hard to reverse course. Yet, it should also be noted that the effect on the first generation could have been even worse had it not been for these investments by the government. However, the delayed compensating investment could have come just in time for the second generation. That may be the reason for no significant effect of bombing on the second generation.

[9]Per capita state investment in the period 1976-85 for more heavily bombed and less heavily bombed provinces is constructed as follows. First, for the period of 1976-85, I constructed the sum of investment flow to less heavily bombed provinces (total bombing per km^2 <12.81 (the median)) and more heavily bombed provinces (total bombing per km^2 >or equal to 12.81 (the median)). Similar computation is done for population size using the 1985 population size across provinces (province population data is incomplete for other years than 1985). Second, the per capita investment is computed for each group of provinces as investment in each period divided by population size in 1985.

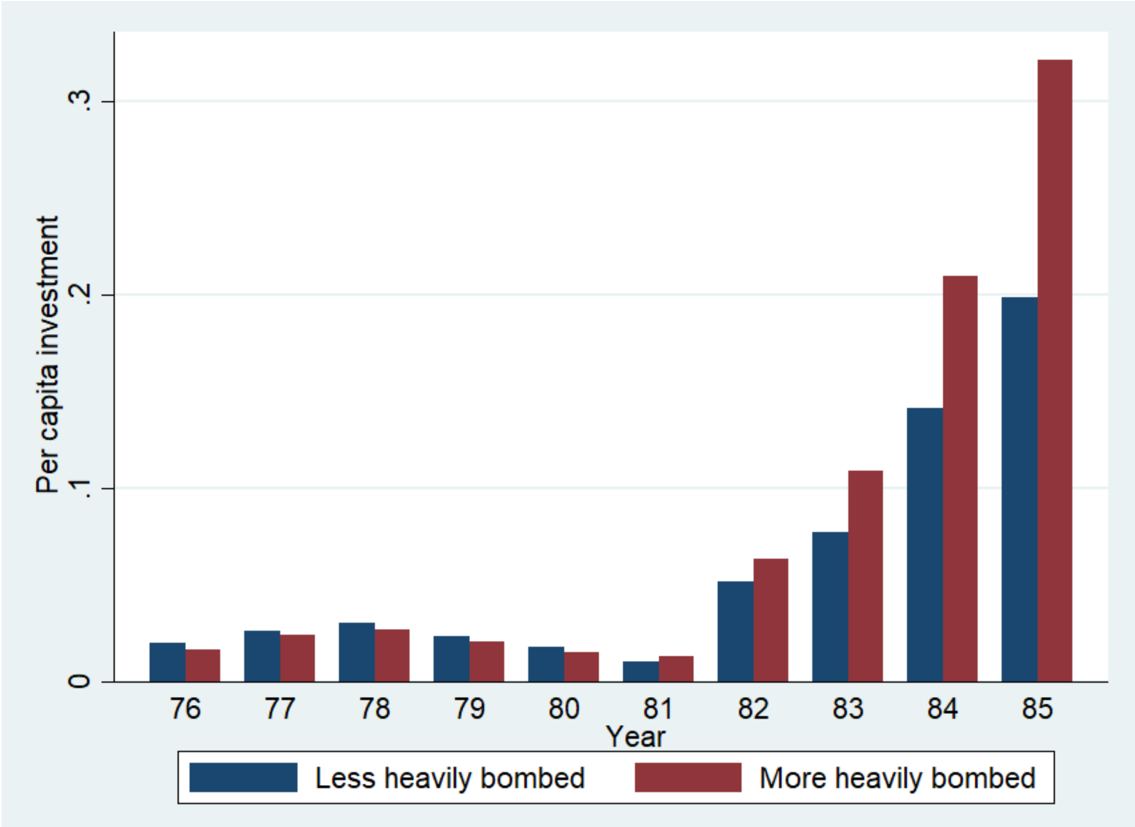

FIGURE 4.1. Per capita government investment flow to more heavily bombed and to less heavily bombed provinces of Vietnam, 1976-85

Source: Author's calculations using data from Miguel and Roland (2011)

4.7 Heterogeneity Analysis

In this section I discuss if there is heterogeneity in the effects of exposure to bombing. Specifically, the section shows if the effect of exposure is different for men and women. Firstly, mortality selection could drive the gender imbalance effect. Guided by the medical literature that boys in utero are more fragile than girls (Eriksson et al., 2010; Kraemer, 2000), empirical studies document that during negative shocks boys die in utero which is manifested by the sex ratio skewed to girls (Almond et al., 2010; Dagnelie et al., 2018). Not to mention that boys also face higher risk of death during infancy (Mu and Zhang, 2011). The weak boys died off at the young age means the survived men are the healthiest and the strongest. Strong negative effect of the shock on women is expected as result. Moreover, there might be gender bias in parental investment. Empirical studies find evidences of son preference that leads to gender bias parental investment in different Asian countries (see for example, Barcellos et al. (2014) in India, Maccini and Yang (2009) in Indonesia; and Mu and Zhang (2011) in China). This would further exacerbate the adverse effect observed on female outcomes. In Vietnam, though, there seems to be no evidence of gender bias parental investment (Haughton and Haughton, 1997). Secondly, if selection is weak and gender bias investment is non-existent, strong negative effect on men's outcome might be expected due to scaring effect which is strong for boys. To test for these competing forces in Vietnam, I re-estimate the effect of bombing separately on men and female samples. Table C.1 in Appendix C.1 shows the gender imbalances results for the first generation sample, while Table C.2 and Table C.3 in Appendix C.1 report the effects on the second generation sample estimated using IV. For the first generation sample, other than for self employment outcome, in which stronger significant effect is found for female, there seems to be no systematic significant difference between impacts of bombing on men and women. Similar no significant difference between male and female is also found for the second generation outcomes.

4.8 Robustness Checks

Several factors could be challenges to interpret the baseline results as the true effects of bombing exposure on education and labour market outcomes of adults. These include mortality, fertility and migration selections. Moreover, some provinces may have been targeted by the use of herbicide like agent orange. This could be a confounding factor as it may also affect the long-term health development of the targeted sub population. All these factors may threaten the causal effects of the baseline results. In the next sections, I discus how robust the benchmark results are in dealing with these identification problems.

4.8.1 Selection

Mortality selection. The first concern comes from mortality selection. If foetuses and infants that are at the lowest margin of health distribution died in utero and as infant, the surviving sample may comprise of the strongest group of individuals. The effect of the shock may appear minimal, yet the actual effect may have been greater than estimated. This selection may not be a big issue as it implies the baseline results may have underestimated the true effects of the bombing. Despite this, I test if there is potential mortality selection. I estimate if exposure to bombing significantly reduced the cohort size. If the weakest children were indeed died at a young age, the data should show that through a lower cohort size that are affected by bombing. In addition, I also estimate the effect of bombing on the probability of an individual in the sample is male. Boys in utero or infants lived through shocks face lower probability of survival. Thus, the likelihood one finds an individual will be a male will be weak if mortality selection is in effect. Table C.4 in Appendix C.1 report these results. In column (1), bombing does not seem to reduce cohort size. In column (2), there is no significant relationship between the bombing and the chance that an individual in the sample is a male. So, mortality selection is not a major concern.

Fertility selection. A more serious concern is fertility selection. Mothers may delay having children during war times and decide to have children during non-war times. In this case, the baseline effects may be overestimated especially if mothers with poorer attributes (like less education) have decided to have children during war times. To test for this, in Table C.4 in Appendix C.1, I estimate if exposure to bombing significantly reduced the cohort size. A significant negative effect means, mothers may have delayed having babies during the war. Column (1) shows no evidence of that. So, fertility selection may not be a major issue.

Migration selection. The other concern is related to migration selection. In the data I only observe place of residence but not place of birth. If exposure is determined based on the individual's current place of residence and households (children) moved from place of birth, there is a chance that early life exposure to war would be incorrectly assigned. The US military data on bombing intensity was reported both at district and province level. In this paper, the determination of exposure is based on province of residence. Assuming that migration may occur between districts but with in a province, I focus on province level measurement of exposure. So, I argue that migration selection is minimal due to how the exposure is constructed from the outset.

4.8.2 Agent Orange as an Alternative Mechanism

During the war, greater than 70 million liters of military herbicide were sprayed in Vietnam (Do, 2009). Exposure to these chemicals like agent orange may impact the long-term development of health thereby education and labour market outcomes. Moreover, Miguel and Roland (2011) suggest that the use of such herbicide may be correlated with bombing intensity. As a robustness, similar to Singhal (2018), I run the analysis again by excluding provinces that were highly targeted by the chemicals.[10] Table C.5 in Appendix C.1 shows results for the first generation obtained from IV; Table C.6 and Table C.7 in Appendix C.1 report the IV results of the second generation outcomes. The results show that the benchmark results are pretty much unaltered by the exclusion of those provinces.

4.8.3 Other Robustness Checks

The analysis so far employed province level total bombing per km^2 as a proxy of bombing intensity. In this section, I discuss if the results are robust in using district level bombing intensity.[11] Table C.8 in Appendix C.1 presents the IV results on the first generation, while Table C.10 and C.11 in Appendix C.1 report the IV results on the second generation.[12] The results are very similar to the province level results. Furthermore, using district level bombing intensity variation, in Table C.12, Table C.13, and Table C.14, I report results from regressions that include district level soil types as additional geographic controls. The tables show that controlling for the soil categories do not alter our main results.

[10] Provinces with sprayed area as a percentage of total area is greater than 10% are dropped from the analysis. This information is obtained from (US-Vietnam Dialogue Group On Agent Orange/Dioxin, 2012).

[11] The sample comprises of 566 districts of Vietnam.

[12] Table C.9 in Appendix C.1 reports the first stage and reduced form results of this analysis.

4.9 Conclusions

In this study I investigate the first and second generation impacts of bombing Vietnam. Exploiting Vietnam Population and Housing Census and military data on bombing intensity at province level, and applying difference-in-difference in combination with instrumental variable methods, I study the long-term effects of early life exposure to bombing on education and labour market outcomes. I find early life exposure to bombing significantly and adversely affects education and labour market outcomes of adults who experienced the shock directly. Nonetheless, these adverse effects of the bombing are not transmitted to the second generation.

No access to primary and secondary school, early childhood malnutrition and maternal stress could be the mechanisms driving the adverse effect of bombing on the first generation.

I show that compensatory state investment that reached the second generation on time might explain the lack of transmission of the effects of shock to the second generation. However, the effect of bombing on the first generation couldn't be averted as a result of late compensatory state investments. Nonetheless, one can argue that the effect of the shock on the first generation may have been minimized by these compensatory state investments.

From policy perspective, the lesson from this study is that policy makers need to design appropriate distributional and compensatory investment policies to prevent the long-term effect of early life shocks. More importantly, these policies need to be implemented before it is too late. In that regard, unconditional transfer programs during conflicts targeting vulnerable households may help mitigate the adverse effect of the shock on children (the most vulnerable part of the population). Ecker et al. (2019), for instance, document that unconditional cash transfers during the time of the conflict in Yemen mitigate the adverse effect of the shock on children nutrition. Moreover, post conflict, policies focusing on building infrastructures (such as schools) and conditional transfer programs (transfers that are contingent to children's attendance to school) may help affected individuals catch up.

One of the limitations of the study is related to the mechanism used to explain why the study finds significant effects on the first generation but not on the second generation. One reason could be compensatory state investment. Studies such as Adhvaryu et al. (2018) document that investments directed to those affected by adverse shocks at early life can mitigate the effect of the shock and help individuals to catch up. In this study, I show that the government's investment distribution to more heavily bombed provinces came to effect late for the first generation, but it may have reached the second generation at critical period. However, it should be noted that this discussion is based on descriptive analysis not rigorous causal analysis. As a result, it should only be thought as suggestive evidence.

Another limitation is related to migration selection. The data lacks information related to place of birth. As a result, early life exposure to war may be incorrectly assigned if households move out of their place of birth due to the war. To minimize this problem, the analysis focuses on province level exposure assuming migration happened within provinces and between districts. However, it should be noted that the concern of migration selection may not be full avoided.

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Appendix C.1 Chapter 4 Appendix

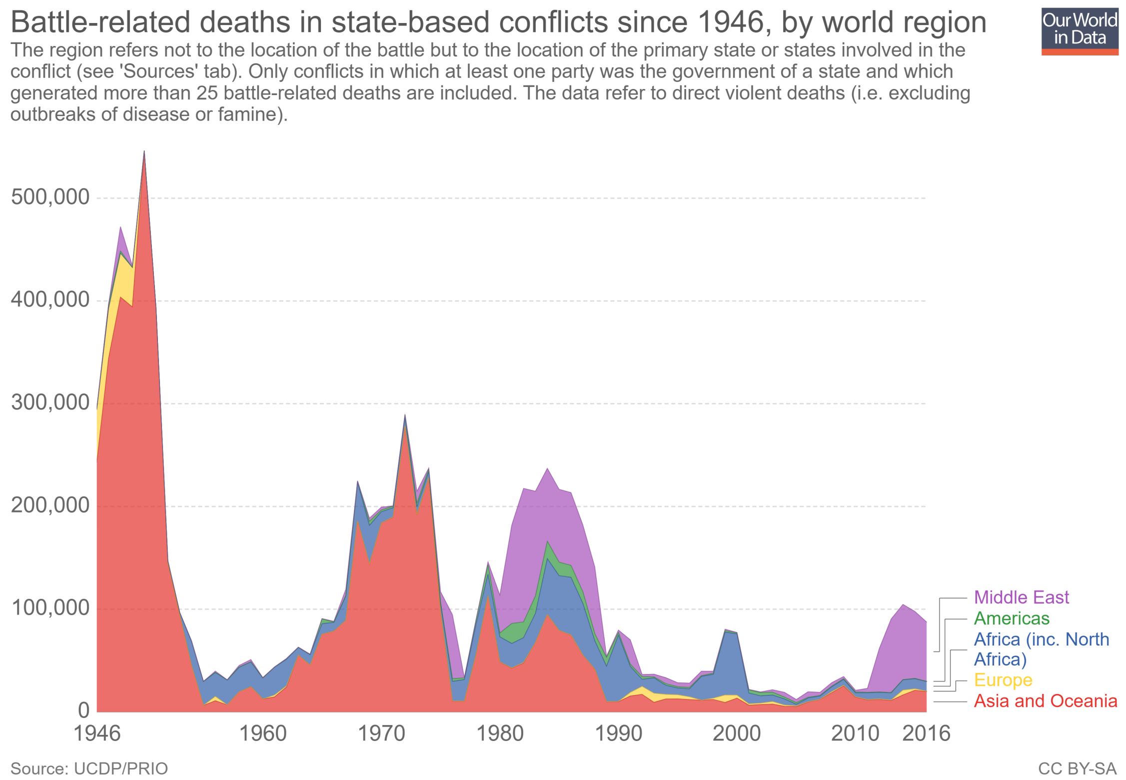

FIGURE C.1. Battle-related deaths in state-based conflicts since 1946, by world region

Source: Max Max Roser (2018)

Table C.1: Estimated effect of exposure to bombing on first generation, gender imbalance

	IV (IV-DID)							
	Male				Female			
	(1) Upper secondary	(2) Years of school	(3) Wage worker	(4) Self employed	(5) Upper secondary	(6) Years of school	(7) Wage worker	(8) Self employed
Total bombing per km^2	-0.00071	-0.00663	0.00137*	0.00000	-0.00068	-0.00828	0.00076	0.00412**
	(0.00055)	(0.00585)	(0.00080)	(0.00053)	(0.00057)	(0.00708)	(0.00063)	(0.00200)
Total bombing per km^2 × born during 1960-1975	-0.00053*	-0.00245	-0.00052***	0.00016	-0.00068***	-0.00298	-0.00049**	0.00069**
	(0.00028)	(0.00242)	(0.00019)	(0.00018)	(0.00026)	(0.00300)	(0.00021)	(0.00030)
Observations	2,463,555	2,463,555	2,460,015	2,460,015	2,532,795	2,532,795	2,528,845	2,528,845
YOB FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

Table C.2: Estimated effect of mothers' exposure to bombing on second generation, gender imbalance

	IV (IV-DID)			
	Male		Female	
	(1) Upper secondary	(2) Years of school	(3) Upper secondary	(4) Years of school
Total bombing per km^2	0.00001	0.00130	0.00017	0.00178
	(0.00022)	(0.00223)	(0.00023)	(0.00268)
Total bombing per km^2 × Born during 1960-1975	-0.00010	-0.00104	-0.00009	-0.00017
	(0.00011)	(0.00095)	(0.00011)	(0.00093)
Observations	1,472,174	1,472,174	1,262,393	1,262,393
Dummy Mother born during war	Yes	Yes	Yes	Yes
YOB FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). Person weights are used in the regression estimations.

Table C.3: Estimated effect of fathers' exposure to bombing on second generation, gender imbalance

	IV (IV-DID)			
	Male		Female	
	(1) Upper secondary	(2) Years of school	(3) Upper secondary	(4) Years of school
Total bombing per km^2	-0.00001	0.00136	0.00025	0.00298
	(0.00022)	(0.00230)	(0.00023)	(0.00275)
Total bombing per km^2 × Born during 1960-1975	0.00000	-0.00025	-0.00011	-0.00053
	(0.00013)	(0.00102)	(0.00013)	(0.00106)
Observations	1,218,267	1,218,267	1,059,974	1,059,974
Dummy Father born during war	Yes	Yes	Yes	Yes
YOB FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). Person weights are used in the regression estimations.

Table C.4: Selection checks

	(1) Cohort size	(2) Male
Total bombing per km^2	-6.55299**	-0.00000*
	(3.10343)	(0.00000)
Total bombing per km^2 × Born during 1960-1975	1.12569	0.00000
	(0.92942)	(0.00000)
Observations	1,647	4,996,350
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C.5: Estimated effect of exposure to bombing on first generation, excluding provinces targeted by agent orange

	IV (IV-DID)			
	(1) Upper secondary	(2) Years of school	(3) Wage worker	(4) Self employed
	Panel A: Born during war			
Total bombing per km^2	-0.00077	-0.00361	0.00058	0.00250*
	(0.00077)	(0.00823)	(0.00067)	(0.00131)
Total bombing per km^2 × Born during 1960-1975	-0.00109**	-0.00119	-0.00067***	0.00088***
	(0.00047)	(0.00401)	(0.00025)	(0.00030)
Observations	3,887,362	3,887,362	3,881,835	3,881,835
YOB FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

Table C.6: Estimated effect of mothers' exposure to bombing on second generation, excluding provinces targeted by agent orange

	IV (IV-DID)	
	(1)	(2)
	Upper secondary	Years of school
Total bombing per km^2	-0.00041	0.00032
	(0.00036)	(0.00371)
Total bombing per km^2 $\times$ Mom born during 1960-1975	0.00008	0.00152
	(0.00023)	(0.00182)
Observations	2,200,822	2,200,822
Dummy Mother born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

Table C.7: Estimated effect of fathers' exposure to bombing on second generation, excluding provinces targeted by agent orange

	IV (IV-DID)	
	(1)	(2)
	Upper secondary	Years of school
Total bombing per km^2	-0.00036	0.00118
	(0.00040)	(0.00414)
Total bombing per km^2 $\times$ Dad born during 1960-1975	0.00016	0.00184
	(0.00030)	(0.00227)
Observations	1,848,388	1,848,388
Dummy Father born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the province level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes province level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

Table C.8: Estimated effect of exposure to bombing on first generation, district level bombing

	Panel B: IV(DID-IV)			
	(1) Upper secondary	(2) Years of school	(3) Wage worker	(4) Self employed
Total bombing per km^2	-0.00077***	-0.00851***	0.00066**	0.00138***
	(0.00028)	(0.00311)	(0.00030)	(0.00035)
Total bombing per km^2 $\times$ Born during 1960-1975	-0.00045***	-0.00156	-0.00041***	0.00029**
	(0.00013)	(0.00118)	(0.00011)	(0.00012)
Anderson-Rubin P-value	0.0000	0.0001	0.0000	0.0000
Observations	4,986,510	4,986,510	4,979,036	4,979,036
YOB FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the district level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the individual is ether from rural or urban areas; household age; and dummy for household head gender. It also includes district level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). Person weights are used in the regression estimations.

Table C.9: First stage and Reduced form, district level bombing

	First stage		Reduced form			
	(1) Total bombing per km^2	(2) Total bombing per km^2 $\times$ born during 1960-1975	(3) Upper secondary	(4) Years of school	(5) Wage worker	(6) Self employed
Distance	-26.82901***	-5.13638***	0.01320***	0.11556*	-0.01490***	-0.03333***
	(4.50300)	(1.40212)	(0.00418)	(0.06150)	(0.00534)	(0.00549)
Distance$\times$ Born during 1960-1975	-1.51987**	-19.69368***	0.00954***	0.04238***	0.00644***	-0.00492***
	(0.65330)	(3.13834)	(0.00144)	(0.01611)	(0.00126)	(0.00132)
Sanderson-Windmeijer F-test	44.48	42.32				
Observations	4,986,510	4,986,510	4,986,510	4,986,510	4,979,036	4,979,036
YOB FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean (SD) Dep. var	31.10 (65.5)	17.31(52.1)	0.28(0.45)	7.9(3.96)	0.28 (0.45)	0.45(0.49)

Robust standard errors (clustered at the district level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes district level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). Person weights are used in the regression estimations.

Table C.10: Estimated effect of mothers' exposure to bombing on second generation, district level bombing

	Panel B: IV (IV-DID)	
	(1)	(2)
	Upper secondary	Years of school
Total bombing per km^2	-0.00006	-0.00003
	(0.00008)	(0.00098)
Total bombing per km^2 $\times$ Mom born during 1960-1975	-0.00004	-0.00034
	(0.00006)	(0.00045)
Observations	2,729,331	2,729,331
Dummy Mother born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the district level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes district level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

Table C.11: Estimated effect of fathers' exposure to bombing on second generation, district level bombing

	Panel B: IV (IV-DID)	
	(1)	(2)
	Upper secondary	Years of school
Total bombing per km^2	-0.00003	0.00052
	(0.00009)	(0.00109)
Total bombing per km^2 $\times$ Mom born during 1960-1975	0.00000	-0.00004
	(0.00006)	(0.00049)
Observations	2,273,580	2,273,580
Dummy Mother born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the district level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes district level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (°N). Person weights are used in the regression estimations.

Table C.12: Estimated effect of exposure to bombing on first generation, district level bombing and controlling for soil types

	Panel B: IV(DID-IV)			
	(1)	(2)	(3)	(4)
	Upper secondary	Years of school	Wage worker	Self employed
Total bombing per km^2	-0.00075***	-0.00896***	0.00056*	0.00156***
	(0.00029)	(0.00306)	(0.00032)	(0.00039)
Total bombing per km^2				
× Born during 1960-1975	-0.00047***	-0.00179	-0.00042***	0.00031***
	(0.00014)	(0.00119)	(0.00011)	(0.00012)
Observations	4,986,510	4,986,510	4,979,036	4,979,036
YOB FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Region specific time trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Robust standard errors (clustered at the district level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the individual is ether from rural or urban areas; household age; and dummy for household head gender. It also includes district level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). The regressions also control for district soil type controls (which include the proportion of district land in 18 different soil categories). Person weights are used in the regression estimations.

Table C.13: Estimated effect of mothers' exposure to bombing on second generation, district level bombing and controlling for soil types

	Panel B: IV (IV-DID)	
	(1)	(2)
	Upper secondary	Years of school
Total bombing per km^2	-0.00011	-0.00057
	(0.00009)	(0.00093)
Total bombing per km^2		
× Mom born during 1960-1975	-0.00004	-0.00036
	(0.00006)	(0.00048)
Observations	2,729,331	2,729,331
Dummy Mother born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the district level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes district level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). The regressions also control for district soil type controls (which include the proportion of district land in 18 different soil categories). Person weights are used in the regression estimations.

Table C.14: Estimated effect of fathers' exposure to bombing on second generation, district level bombing and controlling for soil types

	Panel B: IV (IV-DID)	
	(1)	(2)
	Upper secondary	Years of school
Total bombing per km^2	-0.00007	0.00012
	(0.00009)	(0.00105)
Total bombing per km^2		
$\times$ Mom born during 1960-1975	-0.00000	-0.00010
	(0.00007)	(0.00055)
Observations	2,273,580	2,273,580
Dummy Mother born during war	Yes	Yes
YOB FE	Yes	Yes
Region FE	Yes	Yes
Region specific time trends	Yes	Yes
Controls	Yes	Yes

Robust standard errors (clustered at the district level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Controls include (X): dummies for gender, ethnicity and if the child is ether from rural or urban areas; household age; and dummy for household head gender. It also includes district level war time characteristics such as: altitude (proportion of land area 250-500m; proportion of land area 500-1000m; proportion of land area over 1000m, where the omitted altitude category is 0-250m); average precipitation (cm); average temperature (celsius), and latitude (^{o}N). The regressions also control for district soil type controls (which include the proportion of district land in 18 different soil categories). Person weights are used in the regression estimations.