Universities of Milan and Pavia

Doctoral Thesis

Empirical Essays on Health Economics in Africa

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Co-authorship statement

This thesis consists of two main empirical chapters. However, there is an introductory chapter that tries to link up the two empirical issues investigated in the two chapters by providing a motivation for undertaking such studies. The structure of this thesis report are as follows:

Introduction

Chapter 1: Does reducing the length of school cycles affect women's fertility and child health? Quasi experimental evidence from Ghana.

Supervised by Prof. Massimiliano Bratti.

Chapter 2: Heat waves and child health in sub-Saharan Africa.

Supervised by Prof. Massimiliano Bratti.

The second chapter is the result of a joint project with Massimiliano Bratti and Simone Russo. Although we continuously interacted in the development of the project, I did most of the empirical analysis and the writing in this current version.

Summary

This thesis presents empirical evidence on two thematic but related health economics issues for countries in sub-Saharan Africa–a region which trails in many of the world's economic and social indicators. Primarily, investments in human capital–education and health–stimulate opportunities needed for the growth and enhancement of a skilled and healthy labour force. On the one hand, education enables young people to acquire skills to take up high quality jobs and embrace new opportunities needed to boost economic growth. On the other hand, investments in the health systems by way of strengthening and improving the capacities would help address the health needs of children and adults in the region. Subsequently, children can grow into healthy and better adults for whom their contribution to future economic development can be guaranteed.

As a result, adverse health shocks in early childhood can have harmful adult consequences particularly in developing countries because of limited mitigating health infrastructures. Improving child health therefore is critical for the development of economies and one potential policy instrument which has been identified is maternal education. However, estimating the effect of maternal education on child health without accounting for the potential endogeneity issues that plague the relationship would not be able to uncover the causal effect. Therefore, the first chapter of the thesis is devoted to estimating the effect of reducing the length of education cycles of mothers on their fertility and children's health by using a comprehensive schooling reform as an instrument for education in Ghana. In 1987, the Ghana government undertook a comprehensive reform in the education sector primarily aimed at ad-

dressing the increasing inequities particularly between secondary school and middle school by ensuring that all children of school age had access to higher education. As a consequence, pre-tertiary education duration was reduced from 17 years to 12 years. Specifically, middle school duration was shortened from 4 years to 3 years whereas secondary school duration was shortened from 7 years to 3 years. The argument is that, given the general call to increase female education as a critical policy instrument, could the reduction be detrimental to child health outcomes?

Much as maternal education can serve as an instrument to improve child health outcomes, certain exogenous events such as climatic shocks can affect children even before they are born. As such, early life access to nutrition has long-run effects on health and well-being. When exposed to shocks during early life, it affects the fetal programming and consequently lead to undesirable outcomes during adulthood. Thus, the second chapter of the thesis is focused on testing whether extreme heat waves *in utero* and during early childhood causally affect child health outcomes.

The conclusions emanating from the analyses of the two papers are as follows: in the first chapter, I find that focusing on women with equivalent educational attainment/qualification for which the educational reform ultimately affected school cycles' duration, there was a sharp decline in the years of schooling for the affected women. Despite the discontinuous change in schooling induced by the policy, I find that maternal years of schooling (whether basic or secondary) have no effect on fertility and children's low birth weight, and long- and short-term malnutrition. However, there appears to be some evidence of a positive effect in terms of reducing the probability of child mortality for women with basic education.

In the second paper, we find negative effects of heat shocks experienced *in utero* on children's birth and early child health outcomes and lack of mitigation strategies in SSA.

Introduction

Health and education can be viewed as investments in human capital which lead to a higher future standard of living (Schultz, 1999). The health of the population can therefore be used as a measure of wealth and social development. Also, it is widely accepted that education is a leading instrument for promoting economic growth. It is therefore not surprising that Africa has been touted as developing since its population experiences lower levels of both education and health compared to other regions of the world. These suggest that investing in health and educational systems is necessary for improving Africa's standard of living and to accelerate economic development and growth. This eventually would contribute to saving millions of lives, prevent long-life disabilities, promote good health and well-being which can move countries to closer to achieving some of the Sustainable Development Goals (SDGs).

Intimately, child health is an important health policy issue due to its short- and long-term consequences on both individuals and society. In particular, the extent to which a society is able to provide proper nutrition for its members is an important indicator, and child health is usually used a sign of the society's development level. Many studies have examined and confirmed that early childhood nutritional conditions have long lasting effects. For instance, stunted children have lower cognitive capacity which inhibits their ability to learn and consequently affect their long-term abilities. Early childhood malnutrition has also been found to result in higher risk of chronic adult diseases (Matte et al., 1999; Almond and Currie, 2011*b*), and hence interventions during early life can have long lasting benefits (Case and Paxson, 2002).

A number of factors affect child health, but maternal education has been regarded as the most important as public policy on developing countries has focused on women's education (World Bank, 1993; Knodel and Jones, 1996). One of the theoretical mechanisms through which maternal education can affect child health is by changing the household budget constraint or by influencing mothers to have healthier behaviours (Currie and Moretti, 2003). Educated women are more likely to be higher earners which implies a direct effect through the budget constraint while indirectly it is likely to increase through assortative mating (Behrman and Rosenzweig, 2002). For instance, education may improve the healthy behaviours of mothers by reducing the probability of smoking and reducing fertility.

The correlation between maternal education and investment in child quality might reflect the omission of factors such as family background or the rate of impatience of the mother. Considering the latter for instance, women who put less weight on the future benefits of education tend to have lower educational attainment and may be mostly likely be "careless" mothers. Consequently, several studies in trying to uncover the causal effect of education on child quality have used instrumental variable estimation techniques. Different sources of exogenous variations in education have been exploited including college openings (Currie and Moretti, 2003), schooling expansion (Breierova and Duflo, 2004; Grépin and Bharadwaj, 2015; Güneş, 2015), compulsory schooling reforms (Lindeboom et al., 2009; McCrary and Royer, 2011) and school closures (Zhang, 2014) among others.

Considering the social and economic returns of education, development agencies have generally placed emphasis on primary and secondary education for all as a means of improving economic growth and mitigating poverty aside other indirect benefits. As a consequence, countries have variously embarked on educational reforms particularly at the lower level to help achieve some intended goals of increasing access and ensuring quality. Ghana is no exception to educational restructuring as it is seen as a role model for many African countries in the provision of free basic education, with enrolment (84.1%) and gender parity (1.02) rates amongst the highest in Sub-Saharan Africa (UNICEF, 2013). As several studies have distinctively exploited schooling reforms to estimating the causal relationship between maternal education and child quality, and considering the fact that there is a paucity of evidence as far as Ghana is concerned, the first chapter of this thesis is devoted to finding out whether the reduction in school cycles affect women's fertility and child health outcomes. In so doing, the chapter seeks to exploit a comprehensive reform that took place in Ghana in 1987 which reduced pre-tertiary school duration from 17 to 12 years in an attempt to overhaul the entire educational sector (Akyeampong, 2007). This reform was necessitated due to a myriad of challenges including competition, selection and choice which had gained root in primary and middle school education and had resulted in limited access to secondary education, especially for children from disadvantaged and poor households (Addae-Mensah et al., 1973). Primarily, the reform was to ensure that all children of school age had access to a higher level of general academic training as a way to address the inequity between secondary school and middle school. One cardinal question that emanates from this restructuring process is that, could the reduction in years of schooling at both levels of education be pernicious to child health, taken into account the fact that there is a general call of increasing female education as an important policy tool to achieve better child outcomes and fertility? Therefore, the first chapter of this study asks the following empirical question: does reducing the length of school cycle affect women's fertility and child health? This study is quite distinctive as it focuses on the effect of shortening other than increasing length of educational cycles as used in other studies.

Aside from education of the mother that can quintessentially affect the quality of children, other factors such as climatic conditions can affect children's health which will have a long-lasting effect. Research on the climate change effects on health has also become increasingly topical in recent times. With rising mean temperatures across the globe with more extreme events such as heat waves, floods and droughts, the implications for developing countries are highly distinctive particularly in Africa (Collier et al., 2008; Barros et al., 2014). The last 50 to 100 years has seen near surface temperatures increased by 0.5°C or more over most parts of Africa (Hulme et al., 2001; Stern et al., 2011; Nicholson et al., 2013). It is also projected that climate change will accelerate in the coming decades, with temperatures increasing at approximately 0.2°C per decade (Word Health Organisation, 2007). Allowing concentrations of all greenhouse gases and aerosols to be kept constant at year 2000 levels, it is expected that a further global warming of approximately 0.1°C per decade would be realised. Further, the African continent is largely a recipient of extreme heat waves relative to other continents and it is anticipated to be excessively strained, although it contributed little to the cause (Russo et al., 2014, 2016). Evidence on the impact of climate change on health risks globally is fast increasing. Of particular importance is the effect of climate change on disease burdens particularly related to childhood, such as malnutrition, diarrhoea and malaria. As such the effect of climate change on the vulnerable population such as children, those living in low-lying areas, river delta plains, poor communities, regions with weaker public health etc is crucially a global public health concern.

Also, poor environmental conditions due to climate change *in utero* can pose significant threats during later-life. The extant literature has documented that, children endowed with poor health as a result of poor nutrition *in utero* are more likely to have lower cognitive abilities, lower educational attainment, and worse health outcomes as adults compared to their counterparts without such endowments (Currie and Hyson, 1999; Behrman and Rosenzweig, 2004; Almond, 2006). The response to changes in these initial endowments by parental behaviour is important since that latter can aggravate or lessen the effects of *in utero* shocks. The question therefore of how parental behaviour responds to shifts in initial endowments has been a long standing theoretical debate. On one strand of the debate, Becker and Tomes (1976) argue that parents are more likely to adopt reinforcing strategies if low initial endowments in children lead to lower returns on investment. On the other strand, Behrman et al. (1982) argue that when sibling inequality and welfare matter to parents, they would most likely invest more human capital in the less-endowed child. The question, thus as to how parental behaviour responds to shifts in the initial endowments of children is an empirical one.

The second chapter of this thesis therefore focuses on how prenatal and early life exposure to heat waves affect birth outcomes of children for sub-Saharan Africa. It is believed to be physically strenuous for pregnant women who are excessively exposed to heat which could harm the development of fetuses. In other words, maternal heat stress during pregnancy has negative effects on infant health (Deschênes et al., 2009). Like the first chapter, estimating the causal impact of *in utero* and early childhood conditions on later life outcomes is fraught with the challenge of finding an exogenous variation in such conditions. Economists have utilised natural experiments in identifying exogenous variation in environmental conditions exploiting certain events. For example, events such as famine (Kannisto et al., 1997; Lindeboom et al., 2010; Portrait et al., 2011; Xu et al., 2016), Ramadan fasting (Almond and Mazumder, 2011; Van Ewijk, 2011; Majid, 2015), epidemics (Almond, 2006; Banerjee et al., 2010; Neelsen and Stratmann, 2012), government intervention (Field et al., 2009; Lucas, 2010) and extreme weather shocks (Maccini and Yang, 2009; Skoufias and Vinha, 2012; Grace et al., 2015; Rocha and Soares, 2015; Wilde et al., 2017; Shah and Steinberg, 2017) among others have been used to estimate the causal effect of *in utero* or early childhood exposure to these conditions on later-life outcomes. Therefore this chapter uses an exogenous variation in extreme temperatures to examine the effect of *in utero* and early childhood exposure to heat wave on the health outcomes of children under 5 years in sub-Saharan Africa.

The conclusions emanating from the analyses of the two chapters are as follows: in the first chapter, I find that focusing on women with equivalent educational attainment for the educational reform ultimately affected school cycles' duration, there was a sharp decline in the years of schooling for the affected women. Despite the discontinuous change in schooling induced by the policy, I find that maternal years of schooling (whether basic or secondary) have no effect on fertility and children's low birth weight, and long- and short-term malnutrition. However, there appears to be some evidence of a positive effect in terms of reducing the probability of child mortality for women with basic education.

In the second paper, we demonstrate demonstrates the negative effects of heat waves on children's birth and early childhood health outcomes (birth weight, low birth weight, Weight-for-Age and Height-for-Age Z-scores, undernutrition, severe undernutrition, stunting, severe stunting, anemia) and the lack of mitigation strategies in SSA.

Chapter 1

Does Reducing the Length of School Cycles Affect Women's Fertility and Child Health? *Quasi-Experimental Evidence from Ghana*

Abstract

This paper exploits the exogenous variation in maternal years of schooling as a result of a comprehensive educational reform that reduced the length of school cycles in Ghana to identify the effect of female education on fertility and child health. I focus on women with equivalent educational attainment for which the reform ultimately affected school cycles' durations to identify these effects. I find a sharp decline of 0.71 and 2.05 years for the affected women in basic and secondary school, respectively. Despite this discontinuous change in schooling induced by the policy, I find that maternal years of schooling (whether basic or secondary) have no effect on fertility and children's low birth weight, and long and short term malnutrition. However, there appears to be some evidence of a positive effect in terms of reducing the probability of child mortality for women with basic education.

JEL codes: I12 I21 J13 N37 O12

Keywords: Child health, Fertility, Education cycle, Quasi-experiment, Ghana

1.1 Introduction

Child health is fundamentally important in determining the development of society which is why the just-ended Millennium Development Goals set its fourth mission at reducing child mortality. Although huge strides have been made to reduce child mortality as one of the Millennium Development Goals (MDGs), yet more than 6 million children still die before their fifth birthday every year (UNDP, 2016). As a consequence, the Sustainable Development Goals (SDGs) have sought to avoid these deaths through education, immunization campaigns, prevention and treatment and sexual and reproductive healthcare. Thus, SDG 3 has been set to promote good health and well-being for all, and primarily to end epidemics of AIDS, tuberculosis, malaria and other communicable diseases by 2030 (UNDP, 2016).

Health in early life may be a more significant determinant of adult outcomes in developing countries as documented by a growing body of the literature (Almond and Currie, 2011*b*; Currie, 2011; Currie and Vogl, 2013) and it is determined by many factors including parental education, family income and access to health services or a combination of them. As a consequence, child health and for that matter child mortality are often used as indicators of social development (Zachary et al., 2013) and pose a serious challenge in developing countries. Adverse health shocks in early childhood may have some scathing consequences in adult outcomes particularly in developing countries since they are somewhat constrained to remediate the problem due to the presence and interaction of multiple shocks and the limited mitigating health infrastructures (Currie and Vogl, 2013). In the presence of these limited infrastructures, one potential policy instrument that could play a significant role in improving child health is maternal education.

An intimate issue to child health that has received a considerable amount of attention over the years in developing countries is fertility. Like child health, female education has been an essential component of fertility analysis. As some themes have emerged in the literature on female education-fertility relationship, it becomes unclear as to what level of education is necessary to yield the greatest impact. For instance, Cochrane (1979) conceives that a small amount of education in least literate societies might initially increase fertility whiles Hermalin and Mason (1980) find that the effect of education on individual level fertility is actually country-specific.

It is widely evident therefore that education is a significant determinant of child health and fertility. Conceptually, the channels linking maternal education to child health and fertility have been emphasized by Becker (1960), Mincer (1963), Becker and Lewis (1973) and Mosley and Chen (1984). It is argued that, education guides individuals to alter their attitudes from traditional beliefs towards contemporary ways of using health care services. Also, formal education may transfer health related information and enhance healthy behaviour by mothers (Glewwe, 1999; Frost et al., 2005). Moreover, education empowers women and improves their autonomy (Caldwell, 1979; Caldwell et al., 1983) and bargaining power in decision making in the household, particularly with regard to preferences over quantity as opposed to fewer quality children. It also improves their ability to adhere to basic health care information, fertility options and healthy pregnancy behaviours due to the improvement in their literacy and numeracy skills (Grossman, 1972).

Much of the relationship between maternal education and child health (or mortality) is well known both in developed and developing countries. On one strand of the literature, a strong negative association between maternal education and child mortality is well documented (Caldwell, 1979; Bicego and Boerma, 1990; Hatt and Waters, 2006; Gakidou et al., 2010). The findings have in effect informed policies geared towards large investments in educating young girls. Various channels have been identified through which female schooling contributes to the improvement of child health. An important channel through which maternal education is associated with child health outcomes which studies have found is socioeconomic factors (Frost et al., 2005; Keats, 2014; Alemayehu Azeze and Huang, 2014; Grépin and Bharadwaj, 2015). Other studies (Glewwe, 1999; Frost et al., 2005; Aslam and Kingdon, 2012; Alemayehu Azeze and Huang, 2014) have identified maternal health knowledge as the channel, place of residence (Bicego and Boerma, 1990; Desai and Alva, 1998), modern attitudes (Frost et al., 2005), and personal illness control (Desai and Alva, 1998; Basu and Stephenson, 2005). Furthermore, Currie and Moretti (2003) find that maternal education tend to decrease the probability of low birth weights and preterm birth in the United States.

A compelling way of formulating and designing effective policy instruments to improve child health is through the conduct of scientific research aimed at quantifying the influence of the different channels that link maternal education to child health. In developing countries (particularly Sub-Saharan Africa), however, a dearth of research examining this causal relationship exists. For instance, Osili and Long (2008) use a primary schooling reform in Nigeria and find a positive relationship between education and birth reduction. Glewwe (1999) identifies mothers' health knowledge to be the crucial skill for raising child health in Morocco. Alemayehu Azeze and Huang (2014) analyse the effect of maternal education and its pathways on chronic (long-run) and acute (short-run) malnutrition in Ethiopia and find that education works through socioeconomic status, maternal health knowledge and family planning and reproductive behaviour. On the fertility side, recent evidence using natural experiments suggest a large causal effect of education (Black et al., 2008; Leon et al., 2004; Cygan-Rehm and Maeder, 2013), albeit other studies have found counter evidence (Oreopoulos et al., 2006).

The effect of maternal education on child health and fertility in Ghana has been scantily examined. Few studies (e.g., Bour, 2003; Gyimah, 2003; Appoh and Krekling, 2005; Nketiah-Amponsah et al., 2016; Benefo and Schultz, 1996) have investigated the relationship between maternal education and child health and fertility in Ghana. However, these studies use simple regression models to examine the relationship between mothers' educational level and child health, after controlling for mother, child, family, and community level background variables. In other words, the associations so found in these works are contentious as it is unclear whether they represent causal relationships. Though, these studies found the expected negative correlation between these variables, such a result could be due to omitted variables such as mothers' ability (Griliches, 1977), community infrastructure, taste, values and family resources. Apparently, the omission of these confounding factors may spuriously lead to a positive relationship since they influence both education and outcomes in the same direction. Stated alternately, treating maternal education as exogenous and thus assuming there are no unobserved confounders does not allow the estimation of the causal effect of maternal education on outcomes.

To circumvent the unobserved confounding problem, it is imperative to use an exogenous source of variation in education which is not contemporaneously correlated with child health outcomes. To the best of my knowledge, no studies have been conducted on Ghana that have credibly addressed the estimation of the causal effect of education on child health and fertility. In this study therefore, I present new evidence on the effect of female education on fertility and child health in Ghana using a comprehensive schooling reform as an instrument for education. In particular, I exploit the fact that years of schooling and for that matter levels of education an individual enrols and/or completes are discontinuous function of birth year. In 1987, the Ghana government undertook a comprehensive reform in the education sector primarily aimed at addressing the increasing inequities particularly between secondary school and middle school by ensuring that all children of school age had access to higher education. As a consequence, pre-tertiary education duration was reduced from 17 years to 12 years. Specifically, middle school duration was shortened from 4 years to 3 years whereas secondary school duration was shortened from 7 years to 3 years. To recompense the shortfalls in the lower levels of education, university education was also increased from 3 years to 4 years. As a consequence of this policy, the number of years spent in school and the qualification so attained depended on individual's birth year.

Unlike past literature, this study focuses on the effect of shortening the length of educational cycles. Stated alternately, a distinctive feature of this study is that I exploit a quasi-experimental approach of reducing the human capital corresponding to school cycles (years of schooling), whiles most studies focus on increases in human capital investment (e.g., increasing compulsory school leaving age). One staggering question is that, could the reduction in the years of schooling at both levels (middle and secondary) be detrimental to child health, considering the general call for increasing female education as a critical policy instrument to achieve better child health outcomes? Stated alternately and more specifically, would a 4 (7) year long basic (secondary) schooling lead to higher investments in child quality than a 3 (3) year long basic (secondary) schooling? Thus, assuming individuals born around the same time are similar in potentially unobserved variables, then differences in education at motherhood for women born near the reform year are exogenous. The thrust of the identification strategy is to compare fertility and child health outcomes for mothers born just before and after the implementation of the reform and with equivalent schooling qualification and to relate the magnitude of the differences to the discontinuities in years of schooling. More tersely, the paper attempts to:

(i) Establish whether a strong causal relationship exists between maternal years of schooling, fertility and child health, taking into account the level of education of the mother.

(ii) Examine the important pathways through which maternal education affects children's health.

To carry out the estimation, I used data from the Ghana Demographic and Health Survey (GDHS) and age-specific exposure to the reform. The results suggest that the reform reduced the average years of schooling by about 0.71 (about 7.2%) and 1.07 (about 9%) for women with basic school and secondary school qualifications respectively. I do not find evidence of an increase in maternal years of schooling, be it at the basic or secondary level having an effect on child health, as measured by long- and short-term malnutrition (Height-for-Age and Weight-for-Height Z scores), low birth weight and child mortality and also on fertility. Further, I examined the pathways that might explain the non-evidential relationship between maternal schooling and child health and find no evidence that one more year of schooling improves labour market outcomes or health seeking behaviour such as antenatal care and place of delivery.

The rest of the paper is organized as follows. Section 1.2 reviews the pertinent literature both on the theoretical and empirical relationship between education, fertility and child health. Section 1.3 presents the background of the educational reform and discusses the determination of eligibility status. Section 1.4 describes the data and the empirical strategy. The estimated results are presented in Section 1.5. Section 1.6 presents the robustness checks while section 1.7 presents the heterogeneous effects of the reform. Section 1.8 concludes the paper.

1.2 Related Literature

1.2.1 Theoretical Background

The theoretical foundation for the relationship between education, fertility and child health can be found in the influential works by Becker (1960, 1965, 1981) and subsequently surveyed by Grossman (2006). Becker (1960) developed a standard microeconomic model of fertility which predicts an ambiguous effect of education on fertility, albeit the former increases a woman's permanent income (i.e., education affecting fertility through the labour market). Essentially, the model predicts an unclear effect of an exogenous increase in a woman's education on fertility since it depends on the relative strength of the income and substitution effects. On the one hand, the substitution effect operates as a result of increase in earnings which in turn increases the opportunity cost of leaving the labour market to rear children (Becker, 1965; Willis, 1973). Thus, a woman's inclination to have fewer children is logically strong. On the other hand, higher earnings implies greater propensity for families to afford more children (Becker et al., 1960). The net effect therefore depends on which of the two effects overwhelms the other. If the household's preference is in favour of higher quality other than quantity, then substitution effect outweighs the opposite income effect (Becker and Lewis, 1973; Becker, 1981). As a consequence, parents trade off quantity of children for quality by investing more in each child. Quality could be measured by expenditure per child and also in terms of their healthiness.

In addition to the income channel, the literature has also emphasized on a number of potential channels through which education might affect fertility and child health. Grossman (2006) examined the causal effects of education and the pathways through which it affects nonmarket outcomes through a survey of empirical evidence, whiles relying greatly on Becker (1960, 1965) theory of fertility, time allocation, family, rational addiction and tastes. However, he finds no unambiguous evidence on the mechanisms through which education affects child's health. He, however, predicts that increased parental education increases children's health and cognitive development. Further, mechanism such as assortative mating suggests that, a woman's education causally relates to her partner's education (Behrman and Rosenzweig, 2005), as an exogenous increase in the former affects her permanent income through a partner-related multiplier effect. Black et al. (2008) identified yet another important mechanism through which education might affect fertility which they termed as a pure "incarceration effect". That is to say, being in school essentially confines and restrains a woman from entering into motherhood. Nevertheless, the delay into motherhood can only be transitory which does not necessarily affect completed fertility (Lappegård and Rønsen, 2005). Another important mechanism is knowledge about contraception or reproductive health (Grossman, 1972; Rosenzweig and Schultz, 1989).

1.2.2 Empirical Literature

In spite of the consensus amongst authors from a theoretical standpoint regarding the effect of maternal education on fertility and child health, the empirical literature still remains contentious. The identification strategy adopted in this study is related to a number of studies that employs instrumental variable (IV) techniques to estimate the causal effect of schooling on health and/or fertility. Studies such as Berger and Leigh (1989), Sander (1995*a*,*b*) and Leigh and Dhir (1997) are among the earliest studies to employ IV techniques to estimate the causal impact of schooling on adult's health. A number of studies in recent times have used similar strategies to examine the effects of amount of schooling on an individual's health. Such studies include Arendt (2005), Lleras-Muney (2005), Silles (2009), Oreopoulos et al. (2006), Silles (2011) among others.

There are, however, a scattering number of studies that use IV techniques to study the effects of parental education on child health or on the complementary outcome of fertility. While some of these studies have documented a significant effect of parental schooling on child health, others have found little or no evidence. Regarding education-fertility relationship, there is an expansive documentation of a negative association between female education and fertility, notwithstanding the non-establishment of a causal link due to potential reverse causality and selection on unobservables. The underlying assumption is that instruments are correlated with schooling but uncorrelated with time preference and other confounders. These studies offer inconclusive findings. There are a lot of studies (e.g., Frongillo et al., 1997; Appoh and Krekling, 2005) on education-health and education-fertility relationship, but I particularly focus on the review of studies that used natural experiments to identify the said relationship which is demonstrated in Table A1.

Currie and Moretti (2003) investigate the causal effect of maternal education on birth weight among US white women using college openings in a woman's county in her seventeenth year as an instrument for maternal education. They find that higher maternal education improves birth weight and gestational age. Further, they find important pathways through which maternal education might ultimately improve birth outcomes such as increases in the probability that a new mother is married, reduction in parity, increasing use of prenatal care and reduction in smoking. Their results suggest that for a period of four decades (1950s to 1980s), increase in maternal education accounts for 12% of the 6 percentage point decline in the incidence of low birth weight.

Using the 1995 intercensal survey of Indonesia and school data from the Sekolah

Dasar Program, Breierova and Duflo (2004) investigate the impact of parental education on fertility and child mortality. The authors find that the school program positively affects parental education and negatively affects fertility. Also parental education was found to significantly reduce child mortality. The determination of whether mothers' education matters more than fathers' was quite negligible. Chou et al. (2010) exploit the extension of compulsory education from 6 to 9 years in Taiwan to construct an instrument for schooling to estimate the causal effects of parental schooling on infant birth outcomes and find that parental schooling does matter for favourable infant health outcomes which translates into saving almost 1 infant life in 1,000 life births. The authors conclude that mothers' education, however, matter more than fathers', though no pathway investigation was attempted in this study.

Lindeboom et al. (2009) also use compulsory schooling reform in 1947 in the United Kingdom, which raised the minimum age for leaving school from 14 to 15 years old, to investigate the causal effect of parental schooling on child health. The authors used different measures of health at birth and at ages 7, 11, and 16 and find little evidence that schooling has any effect on child health. The result was regardless of the inclusion or otherwise of maternal or paternal schooling in the regression.

McCrary and Royer (2011) use age-at-school entry policies to identify the effects of mother's education on fertility and infant health while focusing on women born just before and just after the school entry date in Texas and California in the United States. They find that school entry policies affect female education and the quality of a woman's mate and have generally small, but possibly heterogeneous effects on fertility and infant health. The authors, however, argue that school entry policies manipulate primarily the education of young women at risk of dropping out of school.

Maïga (2011) also exploits a sudden change in education policy in Burkina Faso as a source of exogenous variation in education to estimate the impact of mother's education on child health and nutrition. The results show that mother's education significantly and positively affects child's weight-for-height (WHZ) but no effect on HAZ. The author identifies per capita household expenditures as the plausible pathway through which mother's education affects child's health. Further, she observes threshold effects and indicates that the largest impacts of mother's years of education is 13 years of education for WHZ and 12 years for HAZ.

Zhang (2014) similar to McCrary and Royer (2011) and Lindeboom et al. (2009) finds no effect of maternal high school education induced by school closures in China on prematurity, low birth weight, neonatal mortality and infant mortality. The author, however, documents a sharp decline in high school completion for women at age 17 years and 9 months by the first quarter in the first closer year by 22.5%. Additionally, there was no evidence of a woman's exposure to the closures on her selection into motherhood and maternal age at first birth. The author thus concluded that the estimated effects on infant health are not confounded by fertility choices.

Grépin and Bharadwaj (2015) use expanded access to secondary schools and age-specific exposure in Zimbabwe as an instrument for maternal education to estimate the impact of increased maternal secondary education on child mortality. They find that children born to mothers who likely benefited from the policy were about 21% less likely to die than those born to the unaffected cohorts. They examine a number of pathways including late marriage, sexual debut and first birth, as well as better economic opportunities as the mechanisms through which education affects child mortality. However, they find little evidence supporting other channels through which education might affect child mortality.

Rawlings (2015) exploits compulsory schooling law which extended schooling from 6 to 9 years in 1986 in China to investigate the impact of parental education on child health. The author finds that it is maternal, other than paternal education that matters most for child health. However, she finds differences in the effect according to sex of the child. An additional year of maternal education raises boys height-for-age (HAZ) by 0.163 standard deviations, whilst the effect on girls height is nil. This effect was found to be driven mostly by the rural sample, where an additional year of maternal schooling raises boys HAZ by 0.228 standard deviations and lowers the probability of a boy being classified as stunted by 6.6% points. Again, she finds that parental education appears to have little effect on weight-for-age (WAZ) of children.

Güneş (2015) also explores variations across cohorts induced by the timing of compulsory schooling law and variations across provinces by the intensity of additional classrooms constructed in the mother's birth provinces as instruments to examine the effect of maternal education on child health in Turkey. He finds that mother's primary school completion improves infant health measured by very low birth weight, and child health measured by HAZ and WAZ after controlling for many potential confounders. The author identifies some channels through which education operates to affect child health. He finds that maternal education leads to earlier preventive care initiation, reduces smoking, reduces fertility and increases age at first birth.

Makate and Makate (2016) use universal primary school program in Malawi which saw the elimination of tuition fees across all primary school grades as an instrument to investigate the causal effect of mother's school on child mortality. The results suggest that one additional year in school translates to a 3.22 percentage point reduction in mortality for infants and a 6.48 percent reduction for children under age five years. For mothers younger than 19 years, mortality was reduced by 5.95 percentage points. They further identified prenatal care usage, fertility behavioural changes as possible potential channels.

Turning to the relationship between maternal education and fertility, the evidence shows that countries with relatively higher levels of fertility generally find negative effects of education on fertility (see, e.g., Osili and Long, 2008; Lavy and Zablotsky, 2011). Osili and Long (2008) used a nationwide program in 1976 that provided universal primary education in Nigeria by exploiting differences in the program exposure by region and age as instrument to test the causal relationship between education and fertility. Using Nigerian Demographic and Health Survey, they find that increasing female education by one year reduces fertility by 0.26

births. Lavy and Zablotsky (2011) exploit the abrupt end of the military rule which restricted the mobility of Arabs in Israel until the mid-1960's to study the effect of mothers' education on fertility. They find that increased maternal education results in sharp decline in completed fertility. Also it significantly and positively correlates with several potential mechanisms such as reduction in the desired number of children, better knowledge and higher probability of using contraceptives, less religiosity among others.

Braakmann (2011) exploits changes in the minimum school leaving age from 15 to 16 in the UK in 1972 and employed a fuzzy regression discontinuity design to estimate the effect of education on fertility. He finds that for cohorts who had to stay an additional year at school increased the number of their children by 0.27 while those who were shoved into obtaining a qualification experienced 0.85 increase in the number of children. He concluded that for a large part of these changes, it was caused by individuals who had two or more children rather than by those who are deciding for or against any children.

Tequame and Tirivayi (2015) is among the few studies done in Africa which uses an increase in the supply of higher education in Ethiopia to identify the effects of education on fertility. Using age-specific discontinuity in the exposure to higher education reform, the authors find that education lowers fertility by 15% and increases the likelihood for never giving birth by 25%. They further identified positive assortative mating, postponement of sexual activity, marriage and motherhood as the likely mechanisms through which education affects fertility. However, they find no evidence of the effect of female education on labour market participation.[1]

In summary, the conclusion from a majority of the studies reviewed is that schooling causes better child health and/or lower fertility, and only a handful find no or little effect. The inconclusiveness of the findings from the previous studies apparently offers an avenue to obviate some of the nuances that are left in the extant literature. Though the present study comes close to a number of these studies, it

[1]For extensive review of literature on parental schooling on child health, particularly in developing countries, see Strauss and Thomas (1995) and Grossman (2006).

differs principally on one ground: the educational reform that took place in Ghana was a comprehensive and a wholesale package that affected almost every level of the education ladder and that compared with previous studies, the reform exploited in this study reduced years of schooling and not an increase.

1.3 Background and Treatment Status

1.3.1 The 1987 Education Reform

Ghana has been a role model for many African countries in the provision of free basic education, with enrolment (84.1%) and gender parity (1.02) rates amongst the highest in Sub-Saharan Africa (UNICEF, 2013).[2] Possibly, this achievement is in adherence to the goal of the Jomtien World Conference on Education for All in 1990 that free universal basic education as the most important intervention for reducing poverty (Avenstrup et al., 2004).

The foundation of the current educational structure in Ghana emerges from the 1987 Educational reform which generally sought to 'overhaul' the entire educational sector and particularly shorten pre-tertiary duration from 17 years to 12 years (Akyeampong, 2007). Prior to 1987, pre-tertiary education in Ghana spanned 17 years comprising 6 years of Primary School, 4 years of Middle School, 5 years of Secondary education leading to the award of School Certificate/General Certificate of Education - Ordinary Level (SC/GCE 'O' Level) and 2 years of Sixth-Form leading to the award of SC/GCE Advanced ('A') Level. Post reform structure of pre-tertiary education comprises of 6 years of Primary School, 3 years of Junior Secondary School (JSS) leading to the Basic Education Certificate Examination (BECE), 3 years of Senior Secondary School (SSS) leading to the Senior Secondary School Certificate Examination (SSSCE).[3] However, a myriad of challenges includ-

[2]Gender parity index in education helps in explaining how participation in and opportunities for schooling compare for females and males. An index of value less than 1 means favourable for males; an index of value greater 1 indicates that it is favourable for females whereas a value equal to 1 indicates that parity has been achieved.

[3]A minority from well-off backgrounds were doing 13 years of pre-tertiary education. Put it

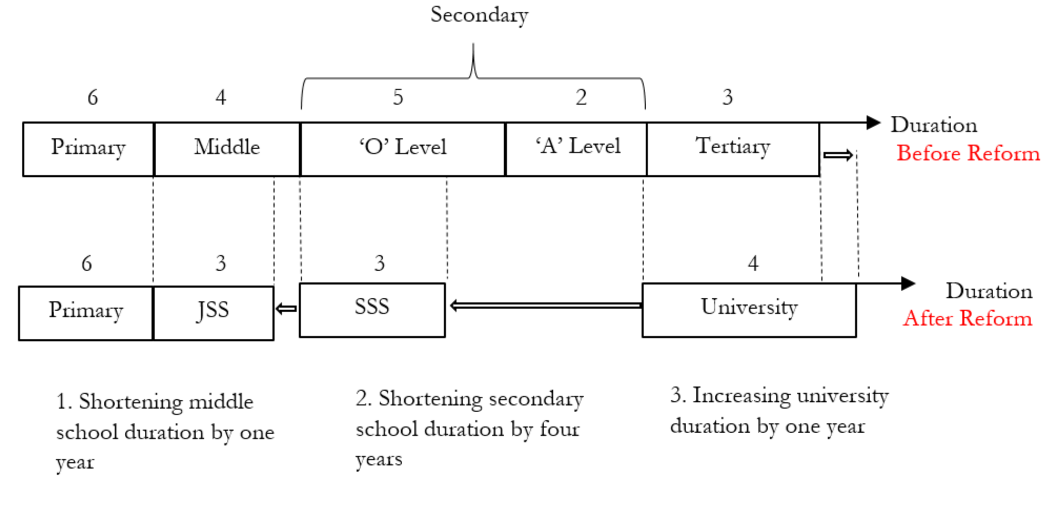
ing competition, selection and choice which had gained root in primary and middle school education resulted in limited access to secondary education, especially for children from disadvantaged and poor households (Addae-Mensah et al., 1973). By "1985, 30 percent of secondary entrants were from private primary schools, most of the rest coming from the fourth year of middle school" (World Bank, 2004*a*, p. 7).

As part of the World Bank's Economic Recovery Programme in Ghana after experiencing a dreadful economic decline in 1983, the then government in an attempt to improve access, quality and educational infrastructure turned to the World Bank to reform Basic Education (Donge et al., 2002; World Bank, 2004*a*). As a consequence, in 1987 the Ghana government abolished the middle schools (4 years), replaced it with 3 years junior secondary and reduced senior secondary from 7 to 3 years. The new system combined primary and junior secondary education to become basic education with a comprehensive improvement in the curriculum. Unlike the defaced Middle school which was primarily grammar education geared towards preparation for secondary school, the reformed Junior Secondary School (JSS) and Senior Secondary School (SSS) curriculum was designed to prepare the majority of children whose formal education terminated either at JSS or SSS for the world of work, and the rest, for further education (Akyeampong, 2007). The primary aim of the reform was, however, to ensure that all children of school going age had access to a higher level of general academic training as a way to address the inequity between secondary school and middle school. Furthermore, to compensate for the shorter school structure, higher education was lengthened from three to four years (minimum) for an honours bachelor's degree (Manuh et al., 2007, p. 43). In addition, the reform introduced the Community Day Secondary School concept which sought to de-emphasise the existing boarding system ostensibly to make secondary education more affordable and accessible to the rural poor (Ministry of Education, 2002). Figure 1.1 summarizes the structure of the educational system in Ghana

differently, students who passed common entrance examination (because they came from well-off families who could afford private primary schools) skipped 4 years of middle school education and enter directly into secondary school.

before and after the implementation of the reform.

Figure 1.1: Structure of education before and after the 1987 reform



Source: Author's own

By September 1987, the conversion of all Middle Schools into Junior Secondary Schools (JSS) had completed. First year pupils in the Middle School automatically became the pioneers in the JSS system. Subsequently, pupils in the upper years in the Middle School phased out yearly until the last batch took their final examination in 1990 (Osei, 2001). Put differently, in 1987 all seventh grade pupils who otherwise would have been admitted into the first year of the traditional middle school were instead admitted as pioneers in the new JSS system. These cohorts sat for the first BECE in 1990, the same year when the seniors of the old middle school system took their last examination. Thus, the first batch of new JSS and the last batch of the old middle school formed the pioneers of the new SSS system who sat for the first SSSCE in 1993 and in 1994/95 academic year, the first cohort of the reform was eligible for university admission. These were the same cohorts that started the new 4 year university system. Furthermore, by June 1996, a transition from the British 'O'-Level and 'A'-Level system had been completed, when the last cohorts took their final A-level examination.

From the foregoing, three distinct groups of individuals can be identified regarding their exposure and eligibility to the reform. As stated before, since the reform was a wholesale restructuring of the educational system, it is clear that an individual's exposure depends on her level of education as well as her age at the time of the implementation. These distinct groups are: (1) women who were old enough at the time of the reform to be affected by the restructuring at any level of education. These individuals could well be those who enrolled in the old secondary school system for which the 3 year university system was still binding for them; (2) women who were young enough to be affected by the 6-3-3-4 system (see Figure 1.1) and (3) women who at the time of the reform were slightly older and hence could not be affected by the JSS system but rather the SSS system. These individuals could have enrolled on a 6-4-3-4 system.

The reform led to a significant improvement in the infrastructure base of basic education which had deteriorated during the economic decline of the 1970s and 1980s. Overall, the number of basic schools increased from 12,997 in 1980 to 18,374 in 2000. At the same time, attendance and completion rates improved for children. The reform also significantly improved enrolment rates and consequently narrowed the gap between boys and girls (World Bank, 2004*b*). In 1996, however, the government introduced the 'free compulsory universal basic education' (FCUBE) to address the quality concerns in basic education which had emanated from the 1987 reform, albeit the latter significantly improved access.

1.3.2 Determination of eligibility status

To determine whether a mother was affected by the education reform, the age of the mother is used. A number of studies using schooling reform to identify to the causal effect of education on child health (Currie and Moretti, 2003; Breierova and Duflo, 2004; Lindeboom et al., 2009; Chou et al., 2010; McCrary and Royer, 2011; Royer, 2004), wages (Duflo, 2001), child mortality (Grépin and Bharadwaj, 2015) and fertility (Osili and Long, 2008) have used similar approach. However, as stated before, the reform was a comprehensive package at virtually all levels of education in the country and that at the time of the implementation, different categories of individuals could be affected. In other words, since almost every level of education was affected by the reform, it was possible for some individuals to be affected wholly by the reform and some partially (see Figure 1.1). Thus, to identify individuals for whom the reform affected at the middle school level, I categorized individuals according to their highest educational level. In other words, I considered individuals who have enrolled and/or completed basic education[4] or equivalent and those who have secondary school qualification respectively in the dataset.[5]

The implementation of the reform took effect in 1987 so individuals starting basic education will be affected.[6] Given that primary school starting age is 6 years in Ghana, then individuals aged between 6 to 12 years in 1987 forms the treated cohort. Those who already were in the middle school and beyond could potentially be used as the control group. However, due to the likelihood of late enrolment and grade repetitions in the primary school, I use age group 15 to 21 as the control group. Figure C8 provides evidence of late entry into primary school as the average age for females is 7.25 years. Thus, this group is likely to be in the upper middle school or even completed at the time of implementation of the reform. For this reason, I do not consider women who were exactly 13 or 14 years old in 1987 in the main estimation and are hence considered partially treated but include them in some specifications to test sensitivity of the results.

Given these equivalent levels of education, I present evidence of the effects of the policy on years of schooling for all women in the sample in an attempt to identify the age for which individuals were eligible for exposure. This idea is premised on the

[4]The reform redefined basic school to include the lower secondary school. Thus basic school used in the study is 6 years of primary school plus 3 years of junior secondary school. It is equivalent to middle school in the old system.

[5]I found some inconsistencies in the dataset and hence such observations were dropped. For instance, it is untenable to have individuals aged 10 years or less in 1987 with middle school qualification. Again, individuals who were too old as at 1987 could not have completed with the new JSS system.

[6]Basic education at the time of the implementation of the reform comprised of 6 years of primary school and 3 years of junior secondary school. Thus basic school in the new and old system has 9 and 10 years of education respectively.

fact that, if years of education matter for child health and fertility outcomes, then there should be a negative effect on health and fertility, conditional on achieving a given level of education before and after the reform and on birth cohort trends.

1.4 Data and Empirical Framework

1.4.1 Data

I used data from three rounds of the Ghana Demographic and Health Surveys (GDHS) conducted in 2003, 2008 and 2014. The Demographic and Health Surveys (DHS) are national household surveys that measure the health, socioeconomic and most importantly anthropometric indicators which emphasize maternal and child health in developing countries. The data are reliable source of information on child mortality across developing countries in terms of comparability and coverage (GSS and Macro, 2009). The sample for the DHS is selected from a two-stage stratified design: in the first stage, enumeration areas or clusters are randomly sampled from census files and in the second stage, a fixed number of households are randomly selected from the list of households within each enumeration area.

For the three surveys used in this study, I extracted data from all women who were between the ages of 6 and 21 years in 1987, that is, all women who were born between 1968 and 1981 in any of the surveys. More precisely, women who were aged 22-35 in the 2003 survey, 27-40 in the 2008 survey and 33-46 in the 2014 survey. The justification for the choice of these survey years is that, since the reform was implemented in 1987 it was necessary to select surveys some years after so as to allow for an overall assessment of the fertility and child health outcomes of these women.[7] The sample of interest in this study was those with equivalent of middle school/JSS.[8]. Pooling across the surveys, the sample consists of 1871 women. This

[7] The first survey conducted on Ghana was in 1988, a year after the implementation of the reform. Again since the survey interviews women who are aged between 15-49, it implies that using surveys so close to the reform year would hugely undermine the assessment of individuals particularly within the treated cohorts.

[8] Analyses of women with secondary school qualification are shifted to the appendix

sample excludes women who were aged 13 or 14 years in 1987 because they are considered to be partially treated. The average years of education attained by women in the sample is 9.39 years. On average, women gave birth to their first child at age 21.24. About 90% of women in the sample have ever married.

To measure child health, I used child anthropometric measurements, birth weight and child mortality. The children in the sample used in the study were younger than 5 years old at the time of the surveys (i.e., 59 months or younger). With regards to the anthropometric measures, I used height-for-age and weight-for-height z-scores to capture respectively long- and short-term malnutrition (Thomas et al., 1991).[9] Information on HAZ and WHZ are reliable because child height and weight are measured by trained nurses during the interviews and data collection. Another measure of child health is low birth weight which is measured as infants weight less than 2500 grams.[10] The average age in months (imputed) is 29.74 and 53% of the children are boys. On the anthropometric outcomes, I further explored the prevalences of stunting (HAZ $<$ -2) and wasting (WHZ $<$ -2) which are 18% and 5% respectively. When HAZ (WHZ) is between -3 and -2, the child is said to be moderately stunted (wasted) and this constitutes 13% and 5% respectively. Finally, the child is severely stunted (wasted) when HAZ $<$ -3 (WHZ $<$ -3) and in the data 5% and 2% respectively fall into this these categories. With the two other child outcomes, 8% of the children have birth weight lower than 2500g while 5% died as the time of the surveys.

Basically, education is measured using three variables in the DHS. These variables are coded as: V106 representing highest education level attended, V107 representing years of education completed at a given level in V106 and V133 represents education in single years. V106 is a standardized variable which provides the level of education categorized under: No education, Primary, Secondary, and Higher.

[9] Anthropometric z-scores are calculated using the 2006 WHO child growth standards for children aged 0-5 years of age by their sex. Stata code (”zscore06”) for calculation of scores was obtained from De Onis (2006).

[10] See International Statistical Classification of Diseases and Related Health Problems Codes (World Health Organisation) for the full list.

Regarding variable V107, the base is all respondents except those answering "No education" or with missing data for V106 (i.e., V106=0 & V106=9). Given these two variables (V106 and V107), education in single years (V133) is constructed in the DHS as follows:[11]

In order to examine the pathways through which maternal education can affect child health, data on outcomes related to fertility, health seeking behaviours of mothers and economic outcomes are collected. Data on health services for children are gleaned from the children (KR) DHS recode files. The maternal health seeking behaviours analysed are generated by creating dichotomous variables: antenatal care and delivery (assisted or in a health facility) are coded 1 if the woman attended 8 or more visits; obtained services from doctors, trained nurses, or trained midwives; and delivered in a health facility and 0 otherwise. Finally, using responses to questions about vaccinations, I constructed a measure to indicate whether or not children were fully immunized. Table A2 summarizes the sample description of the data.

1.4.2 Identification Strategy

To determine the effect of maternal education on fertility and child health outcomes, I estimate the following:

$$y_{ijk} = \alpha_0 + \alpha_1 s_{ijk} + x'_{ijk}\Theta + \varepsilon_{ijk} \tag{1.1}$$

where y_{ijk} is the outcome of interest to mother i for child j of birth cohort k; s_{ijk} is the years of schooling for child $j's$ mother; x'_{ijk} is a vector of predetermined variables

[11]

$$\begin{aligned} V106 &=> V133 \\ 0 &=> 0 \\ 1 &=> V107 \\ 2 &=> V107 + x \\ 3 &=> V107 + y \\ 9 &=> 99 \end{aligned}$$

where x is years of complete primary school and y is years of complete primary and secondary education. In the mapping, the left hand side values refer to V106 and the right hand side values refer to V133 including religion, ethnicity, survey fixed effects (which implicitly controls for age), and region fixed effects and a dummy variable which takes on the value of 1 if an individual lives in an urban area and 0 if she lives in a rural area. The key parameter in this specification is α_1 and estimating it using ordinary least squares (OLS) is challenging due to spurious correlation. Stated differently, interpreting α_1 as the causal effect of education on child outcome may be misleading since the estimate may suffer from reverse causality, omitted variable bias or even measurement errors in education. For instance, career aspirations and the level of impatience of mothers can jointly affect their educational decision and fertility and consequently on their children's health. Further, women with less education lack the resources to invest in their own health and hence more likely to have sick children.

To circumvent these problems, I use an exogenous shift in mother's education induced by the comprehensive educational reform implemented in 1987. The strategy exploits the implementation of the reduction in basic school duration from 10 to 9 years and secondary school duration from 17 years to 12 years to estimate the impact of years of schooling on child health outcomes and the underlying mechanisms. The approach subtly utilizes a regression discontinuity where I compare women who were 6 to 12 years with 15 to 21 years at the time of the reform when there was an abrupt reduction in the years of schooling at the basic and secondary levels.

To estimate the effect of reducing years of education at the different levels, I specify the following first stage regression:

$$
\begin{aligned}
s_{ik} = & \alpha + \beta_1 T_i + \beta_2 (T_i \times (Age87_i - c)) + \beta_3 ((1 - T_i) \times (Age87_i - c)) \\
& + x'_{ik}\theta + \varepsilon_{ik}
\end{aligned}
\tag{1.2}
$$

where s_{ik} is the years of schooling for individual i. T_i is the treatment indicator which denotes whether the individual is affected by the reform in 1987, i.e., it is a dummy variable taking on the value of 1 if age in 1987 is less than or equal to 12 and 0 otherwise. c is the cut-off age in 1987. ε_{ik} represents the error term. The parameter of interest is β_1, which captures the average causal effect in the length of school cycles (basic and secondary) on years of schooling.

The identification strategy restricts the samples to two categories of individuals who have respectively attained basic school and secondary school levels of education, and as a consequence separate regressions are ran based on the specification in equations 1.1 and 1.2. This identification therefore deviates subtly from previous identification strategies since the latter mostly use reforms which seek to increase years of compulsory schooling. The rationale for this identification is that, unlike most studies, the reform in Ghana involved non-compulsory schooling and hence the effect of the reform is uncertain. Essentially, the reduction in basic school duration may have induced some individuals to demand the same level of education and others to proceed to get secondary or even higher level of education. Thus, for those who stop at the same level before and after the reform, the latter unambiguously reduces years of schooling. By contrast, for individuals who may have decided to get further schooling after the reform since the reform shortened the previous cycle, this entails more years of schooling. Therefore, this may create a non-monotonicity of the instrument in the sense that the effect of the reform may be ambiguous. Thus, focusing on individuals with the same qualification preserves the monotonicity of the instrument, since the reform entails a reduction in years of schooling for everyone.

There are other considerations to be kept in mind. First, expanding access to education implies that individuals with lower ability may enter school after the reform which potentially may confound with outcomes. Thus, two cohorts may have different levels of ability, and the comparison would be plagued by a potential ability bias since there was no compulsion on individuals to attend school.[12] This is more relevant for individuals with secondary schooling qualification. Incidentally, if mother's education and ability affect child health in the same direction, then as far as the effect on health and fertility is concerned, it should induce an upper bias. Thus, as a preliminary check, I estimate the effect of the reform on the probability of enrolling in basic schooling to explore the severity of this potential ability bias.

[12] This would not be an issue if the reform on the school length involved compulsory education since schooling is not a choice and thus the level of ability across cohorts would remain constant.

Consequently, the results for women with basic education qualification are used as the baseline while the results from those with secondary qualification are kept in the appendix since the sample size for the latter is also small causing the estimates to be quite noisy. Second, in all specifications, the age polynomial function is interacted with the treatment indicator on either side of the cutoff to allow for a change in slope at the cutoff since the approach is inherently identical to a regression discontinuity approach. Third, it is worth considering that, the farther we move away from the cutoff of 12 on either side, the less comparable the cohorts become. As a consequence, the sample analysis includes pre-eligible cohorts at 15-21 in 1987 as opposed to post-eligible cohorts at 6-12 in 1987. Women who were exactly 13 or 14 years old at the time of the implementation were not used in the main estimation but have been included in some specifications as sensitivity test.

1.5 Estimated Results

I first present the results of the impact of the policy on educational attainment. Second, I examine the instrumental variable estimation of the maternal education on child health. Third, I present the effect of the policy of years of schooling of mothers on fertility related outcomes. This is done to examine whether the issue of sample selection is of concern particularly in interpreting the effect of maternal education on child health. Additionally, I present the IV estimates of years of schooling on fertility outcomes as potential pathways through which education affects child health. I further examine additional pathways that link education to child health.

1.5.1 Effect of the policy on maternal education

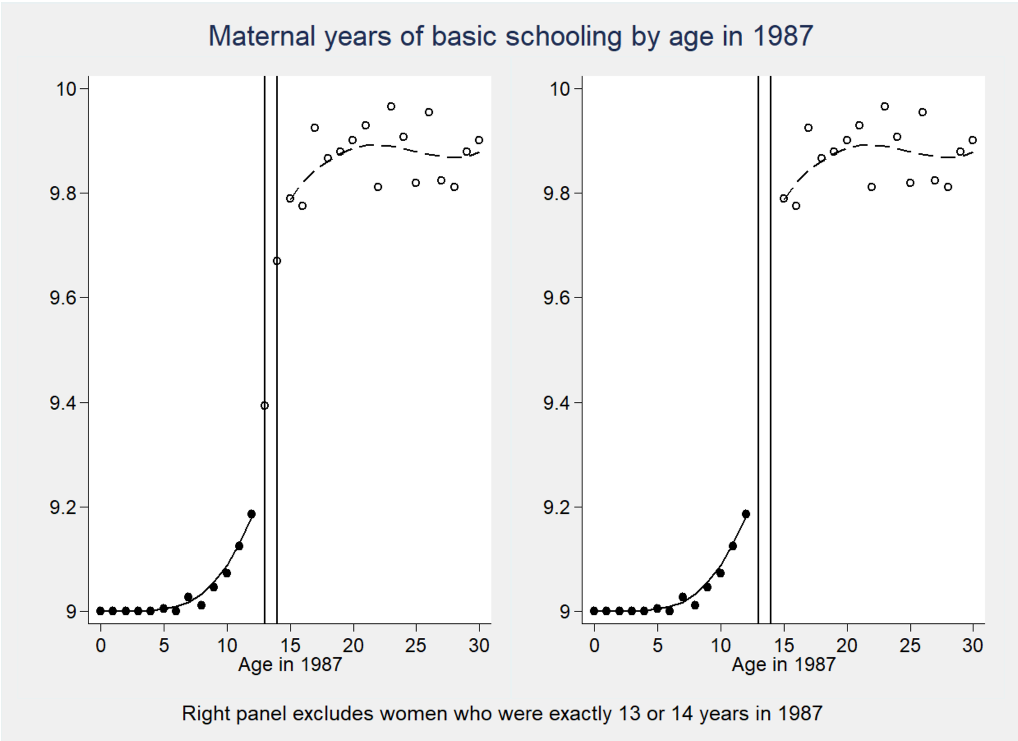
Before presenting the results on the effect of the policy on educational attainment, I estimate the effect of the reform on the probability of enrolling in basic school in order to examine the potential ability bias that might scourge the comparison between the treatment and control groups. The results presented in Table 1.1 indicate that there is no effect of the reform on the probability of attending basic schooling.[13] The implication of the results is that, since the size of the cohort entering basic schooling did not change much, I do not expect a big change in cohorts' unobserved ability levels. As a result, potential ability bias is not a major concern.

Having ruled out the possibility of any potential ability bias, I proceed to examine the effect of the education policy on educational attainment of women. The discussion will be focused on women with basic education qualification, albeit I present results for women with secondary education qualification in the appendix because of small sample size. I begin with a graphical illustration of the relationship between schooling and birth year in Figure 1.2. The figure includes all women aged 0 to 30 years in 1987 and not just those in the main sample used in the estimation. As pointed out earlier, women who were exactly 13 or 14 years old are considered partially treated and are represented by the two vertical lines in each figure. Open circles to the right of the vertical lines are considered the control group and solid circles to the left are considered the treatment group. Two key features should be noted in the figure. First, the discontinuity is seemingly sharp at the time of the policy. This is because the reform reduced the number of years of education at that level by one year. Second, considering the sample of secondary school leavers, the post-exposure trends appear to be stable consistent with expectation while pre-exposure trends seem irregular and noisy. The discontinuity in years of schooling amongst this sample is surprisingly not sharp and visible which is inconsistent with the precept of the reform which sought to reduce the number of years at that level by 4 years. Since there were huge inequities particularly at this level of education, fewer women could attend secondary school before the reform, a possible reason for the noisy trends amongst these cohorts (See Figure C9). In all, the relative discontinuities on either side of the cutoff for the respective women sample reinforce the conclusion that the discontinuous change in years of education is as a consequence

[13]The sample includes all individuals aged between 6 and 21 in 1987 but excluding women who were 13 or 14 regardless of their level of education qualification.

of the policy.

Figure 1.2: Discontinuity in years of basic school



Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted. Notes: Individuals lower than age 13 in 1987 are subjected to the reform. Ages 13 and 14 are considered to be partially treated.

Table 1.1: Reform effect on the probability of enrolling in basic school

	(1)	(2)
Indicator for age less than 13 in 1987	0.115***	−0.016
	(0.039)	(0.025)
Mean of dependent variable	0.42	0.42
R^2	0.013	0.353
Observation	6919	6919
Controls	*No*	*Yes*

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index and linear slopes on either side of the age 12. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Stated in another way, the discontinuities so observed are directly attributable to the education restructuring policy that took place in 1987. It is worth noting also

Table 1.2: Effect of the policy on maternal years of basic education

	(1)	(2)
Panel A: Main sample		
Indicator for age less than 13 in 1987	-0.801^{***}	-0.710^{***}
	(0.019)	(0.026)
Mean of dependent variable	9.86	9.86
R-squared	0.643	0.656
Observations	1871	1871
Panel B: Including ages 13 & 14		
Indicator for age less than 13 in 1987	-0.709^{***}	-0.617^{***}
	(0.030)	(0.035)
Mean of dependent variable	9.77	9.77
R-squared	0.510	0.524
Observations	2143	2143
Controls	*No*	*Yes*

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index and linear slopes on either side of the age 12. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

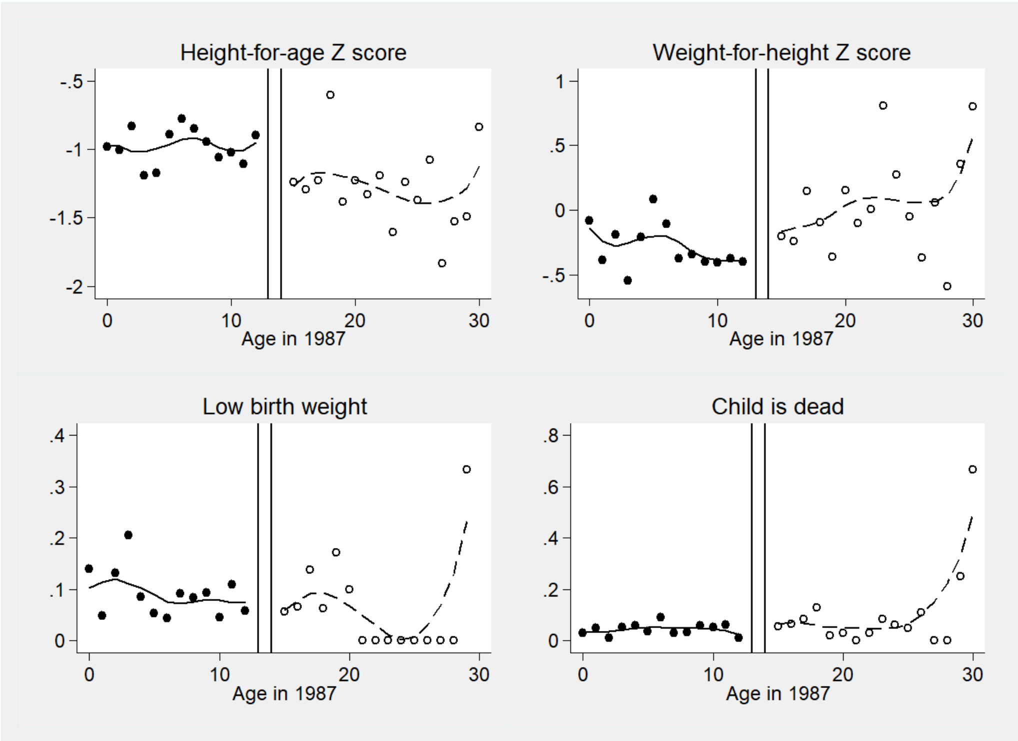
that the figure gives an apparent direction regarding the choice of the polynomial appropriate to fit the cohort trend. Since the figure includes all women, it appears the pre-exposure trend is quadratic. Howbeit, when considering the window around the cutoff, the trend looks linear. Hence, I use linear trend for the main specification but use quadratic trends as a sensitivity check.

Table 1.2 summarizes the discontinuity point estimates for women with basic educational attainment (See Table A3 for estimations across different specified samples and also for the women with secondary school qualification). Column (1) reports the baseline estimates of the first stage without any covariates. I find a decline in the years of basic schooling by 0.71 induced by the policy of educational restructuring. The results are robust to including other exogenous covariates, providing further evidence for the exclusion restriction. The results suggest that, exposure to the reform reduces years of basic schooling by 0.71. The average number of years of basic schooling in the control sample is 9.77, implying that the policy reduced years of basic schooling by approximately 7.3 percentage points. Moreover, the exposure to the reform reduces years of secondary schooling by 2.05 which translates into approximately 14.6 percentage points reduction given the average years of secondary schooling in the baseline sample as 14.05. Including women with ages 13 or 14 in 1987 reduced the impact to -1.91 since these women were only treated partially consistent with the findings by Grépin and Bharadwaj (2015). The first stage estimates also vary with place of residence as the effect is stronger in rural areas for basic school leavers. The magnitude of the effect is stronger among urban dwellers relative to their rural counterparts for those with secondary school attainment. I also include a quadratic trend whiles controlling for all the covariates and the results are robust to these specifications (See Table A3).

1.5.2 Effect of maternal education on child health outcomes

This subsection presents the reduced-form effects of the reform on HAZ, WHZ, the incidence of low birth weight and child mortality. The anthropometric measures are used to capture long-term and short-term malnutrition respectively whiles low birth weight has been widely cited as an indicator for poor child health (Almond et al., 2005). Before presenting the parametric estimates of the effect of the policy on child health outcomes, Figure 1.3 shows the reduced form graphs on HAZ and WHZ scores, low birth weight as well as child mortality . If more years of schooling improves child health, then there should be generally incidence of poor health outcomes among the post-exposure cohorts since the policy reduced their years of schooling. Nonetheless, the trends as shown in the figure are generally noisy and do not show clear discontinuities. The trends in WHZ jumps marginally, albeit might not be statistically indistinguishable from zero. Also, greater proportion of mothers in the pre-exposure cohorts have higher incidences of better WHZ than their post-exposure cohorts. A similar trend is observed for low birth weight outcome which shows a seemingly better outcome for pre-exposure cohorts. However, there appears to be high incidence of child deaths amongst the pre-exposure cohorts compared with their post-exposure cohorts. It is thus unclear as to the effect of the reform on child health outcomes as far as reduction in the years of basic schooling is concerned.

Figure 1.3: Impact of the reform on child health outcomes



Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted.

Maternal education as documented extensively in the literature is positively associated with child health. I pool all levels of education to examine the association between years of education or level of education on child health to get a sense of the features of the data and its similarity with other studies. Table A4 presents the results and generally corroborate the findings of the expected association. As evident in the table, at least some level of mother's education improves HAZ and WHZ whiles no education deteriorates them. Also higher levels of mother's education decreases the probability of child mortality, albeit not significant whiles no education significantly increases the probability of child mortality.

Now focusing on the core sample of the study, Table 1.3 reports the estimation results of the effects of maternal education on child health outcomes. Panel A reports the OLS estimates of the effect of years of mother's education and reveals that an additional year of schooling reduces the probability of low birth weight

Table 1.3: OLS, reduced form and IV estimation of child health outcomes

	(1) HAZ	(2) WHZ	(3) LBW	(4) Death
		Panel A: OLS estimation		
Years of education	−0.274	0.138	−0.002	−0.004
	(0.191)	(0.124)	(0.027)	(0.017)
Mean of dep. variable	−1.21	−0.13	0.09	0.06
Observations	903	901	733	1249
		Panel B: Reduced form		
Treatment indicator	0.157	−0.209	0.009	−0.064**
	(0.170)	(0.147)	(0.059)	(0.025)
Mean of dep. variable	−1.02	−0.28	0.08	0.05
Observations	903	901	733	1249
		Panel C: IV estimation		
Years of education	−0.087	0.305	−0.025	−0.101**
	(0.260)	(0.212)	(0.075)	(0.043)
Mean of dep. variable	−1.07	−0.25	0.08	0.05
First stage F statistic	52.73	50.95	222.03	53.83
Observations	903	901	733	1249

Notes: Clustered (Mother*Age in 1987) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects, child's sex and mother's age at birth and husband's level of education. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and improves long term malnutrition. Furthermore an additional year of mother's education negligibly decreases the probability of child mortality and worsen short term malnutrition. These effects are, however, indistinct from zero.

Turning to the reduced form estimates on child health outcomes (See panel B), the results indicate statistically insignificant effect of the policy on all the outcomes except child mortality. A mother with basic schooling who was exposed to the policy of shortening school years significantly reduces her child's probability of dying by about 0.06. It increases the probability of low birth weight by 0.01 while it increases short term malnutrition by 0.16 standard deviation.

The instrumental variable estimates in Panel C of Table 1.3 aims at providing a better understanding of the reduced form effects and also since instrumental variable regressions are preferable because they purge against potential bias arising from unobserved factors that may correlate with the regressor of interest (i.e., years of education). I use discrete treatment of mother's exposure to the policy as an instrument for her education. The IV estimates generally appear larger than the OLS estimates. A possible reason could be the random measurement error or the presence of omitted variables associated with the OLS estimation. For instance, factors such as household characteristics that influence the outcome variables positively or negatively correlate with maternal schooling causes the downward bias with the OLS estimates.

The results indicate that an additional year of schooling decreases HAZ by 0.9 standard deviation and increases WHZ by 0.31 standard deviation, though not significant. Also, an additional year of schooling reduces the probability of low birth weight by 0.03 and increases the probability of child mortality by 0.10 at a significant level of 5%. These estimates are large relative to their sample averages which provides evidence on large effects of maternal education in reducing the risk of child mortality and low birth weight and improving anthropometric measures of children.

I further examine whether the effect of maternal schooling vary across the gender of the child for which the results are presented in Table 1.4. The IV estimates indicate that the impact of maternal years of education is greatest for boys as far as the anthropometric measures are concerned. An additional year of schooling decreases short-term malnutrition whiles it increases long-term malnutrition. However, regarding child mortality, decreases the probability of girls dying by 0.2. The effect of maternal education being strong for boys in this study corroborates the findings by Rawlings (2015) in China.

I conduct a number of robustness checks on the results for child health outcomes. The results are presented in Table A6 in the appendix. First, since the preferred specification excludes women who were exactly 13 or 14 in 1987, I test whether the inclusion of these women changes the results. Second, I test whether tightening the sample to women who aged 8-19 in 1987 could alter the effects on child health outcomes. Third, though the study settled on surveys quite farther away from the implementation of the policy, schooling could still delay the age at first birth for some women. Consequently, I also test by excluding data from the 2003 survey. Fourth, I test whether non-linear age trends in the data affects the results. Finally, I test by including the product of birth year and survey year fixed effects. The findings are seemingly robust to these alternative specifications.

In sum, the results of the effect of the policy on child health surprisingly contradict many of the results already found in the literature (e.g., Maïga (2011); Chou et al. (2010); Currie and Moretti (2003)). However, the findings lend support to the study by McCrary and Royer (2011). Similar to McCrary and Royer (2011); Zhang (2014), lack of large sample around the cutoff could potentially explain the weak statistical power of the results. When child mortality is considered as the outcome, the results corroborate the findings by Grépin and Bharadwaj (2015); Makate and Makate (2016).

Table 1.4: Disaggregated effect of maternal education on child health outcomes

	(1) HAZ	(2) WHZ	(3) LBW	(4) Death
I: Boys				
	Panel A: OLS estimation			
Years of education	−0.069	−0.112	0.035	0.001
	(0.250)	(0.149)	(0.045)	(0.027)
Mean of dep. variable	−1.21	−0.13	0.09	0.06
Observations	485	484	383	666
	Panel B: IV estimation			
Years of education	-0.605^{**}	0.526^{**}	−0.007	−0.059
	(0.280)	(0.255)	(0.085)	(0.065)
Observation	485	484	383	666
First stage F-statistic	72.40	68.94	368.17	130.36
II. Girls				
	Panel C: OLS estimation			
Years of education	0.327	−0.239	−0.046	−0.023
	(0.248)	(0.162)	(0.042)	(0.025)
Mean of dep. variable	−1.21	−0.13	0.09	0.06
Observations	418	417	350	583
	Panel D: IV estimation			
Years of education	0.435	0.012	−0.085	-0.177^{**}
	(0.546)	(0.597)	(0.084)	(0.073)
Observations	418	417	350	583
First stage F-statistic	30.22	27.86	29.51	13.94

Notes: Clustered (Mother*Age in 1987) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.5.3 Discussion of the IV estimates

Given a cursory glance at Tables 1.3 and 1.4, it appears the IV estimates are larger than the OLS estimates. A plausible explanation for the downward bias of the OLS estimates is the random measurement error in maternal education or potential omitted variables. For instance, factors such as family characteristics that affect the outcome variables negatively correlates with maternal schooling would bias the OLS estimates downwards. Also, since the sample restricts individuals with equivalent of middle school certification, the IV estimates may reflect the effect of maternal education for a subset of the population where having basic school education had a particularly large impact on the outcome variables, that is, mothers who would not have obtained basic education in the absence of the education restructuring.

It is worth noting that the educational reforms in Ghana differed substantially from many of the past natural experiments investigated that had compulsatory elements, since reduction in school cycles for both levels of education was still voluntary. As a result, the IV estimates might reflect local average treatment effects (LATE) among a sub-population of compliers. To state it differently, compulsory schooling and reduction in education cycles will capture different LATE. Significantly, compulsion would imply that children with lowest marginal returns to schooling are now driven by law to attend school. However, in the case of Ghana, the children on the margin might be those who do not continue to secondary school after the reform, i.e., relatively low ability individuals. If mother's education and ability are complementary inputs into child health, then these returns may be relatively low. Contrarily, if the two are substitutes, it may happen that giving low ability mothers more education produces higher returns on average compared to the whole population. As a result, depending on the assumptions about how the reform affects schooling returns and costs, then the reduction in the duration of school cycles would capture different LATE.

Compliers are those for whom the numbers of years of schooling decreases, i.e., those who do not decide to continue into SSS to achieve the same number of years of schooling before the reform, e.g. to enrol and complete one year of SSS in order to have 4 years like in the old middle school regime. Whiles it is seemingly impossible to distinctively identify compliers, the magnitude of the IV estimates can conceivably be examined within the context of sub-populations that consists of compliers. However, the identification strategy used in this study somehow swamps the difficulty in identifying compliers since the sampled women have the same qualifications. That is to say that, under a certain sense, by cutting the sample so that it mostly includes compliers by definition.

The reform eligibility of the women as stated is used as the instrument. The effect of the reform is assumed to affect school length and not the quality of schooling. The robustness of the first stage results (with and without controls) provides further evidence of the exclusion restriction. It is noteworthy to mention that the reduced form estimates are valid even without the exclusion restriction. The causal estimates for the alternative outcomes are valid particularly for the basic school sample. However, interpreting the results of the secondary school sample should be done with a considerable degree of admonition because of the small sample properties which weakens the instruments.

1.5.4 Education and fertility

Two issues are considered in this subsection. First, the issue of sample selection problem is addressed and second I examine the effect of education on fertility. Thus, analyses on fertility are done first to examine the severity or otherwise of sample selection problem as well as used as a mechanism through which maternal education can affect child health. To exemplify the issue of sample selection, I first estimate the reduced form effects of a woman's reform eligibility on her selection into motherhood and age at first birth.This is done in the light of averting the complications that might result in interpreting the effect of maternal education on infant health. Figure 1.4 provides the graphical presentation of the relationship between birth cohorts and some fertility outcomes. In the figure, the vertical line represents the oldest cohorts (born in 1975) and aged 12 years in 1987 exposed to the reform. Regarding the proportion of mothers, each point represents the proportion of mothers observed in each cell by birth year. Thus, to the left of the vertical line are those mothers who were unaffected by the reform whiles those to the right were affected. If indeed the policy affects a woman's decision to become a mother, there should be a break in the trends at age 12 in 1987. However, the plot reveals a rather smooth trend. Thus, there is no evidence that women on either side of the age cutoff have different probabilities of becoming mothers. A similar trend is observed when age at first birth is considered. Again, there is no visual evidence of a discontinuity as a consequence of the implementation of the policy. Thus, this finding seems to suggest that endogenous sample selection induced by the reform is not a big concern. Table 1.5 presents the estimated effects of the policy on a woman's selection into motherhood and her age at first birth under age 18.[14] The point estimate is -0.038 and statistically insignificant, thus suggesting that women on either side of the cutoff have similar probabilities of becoming mothers. Column (2) also presents the effect of the reform on the probability of shifting maternal age at birth to under 18 and the estimate is statistically insignificant. Thus, the issue of endogenous sample selection caused by one's exposure to the reform causes less concern. A similar result is obtained for the sample of women with secondary education qualification (See Table C2), albeit the signs in the latter are positive but not significant.

Following Royer (2004) and Zhang (2014), I explore further whether the policy has any effect on maternal age at first birth by estimating the reduced form for each maternal age at first birth from 12 to 40 for the sample of women with basic education and 13 to 36 for those with secondary education. The results reported in Table A5 show no strong evidence that mothers exposed to the reform have higher probability of having their first child before the age of 18. The policy, however, increases the probability of having first birth at age 23 by 6.1 percentage points which is considered as a safe age to give birth and hence, unlikely to have an injurious effect on child health. I find negligible decrease at age 16 but is significant at 10 percent level. The decrease in the probability of giving birth at age 16 could be beneficial to child health. On this account, if a woman's exposure to the reform has a damaging effect on child health, it cannot be attributed to the effect to changes in maternal age at first birth induced by the reform.

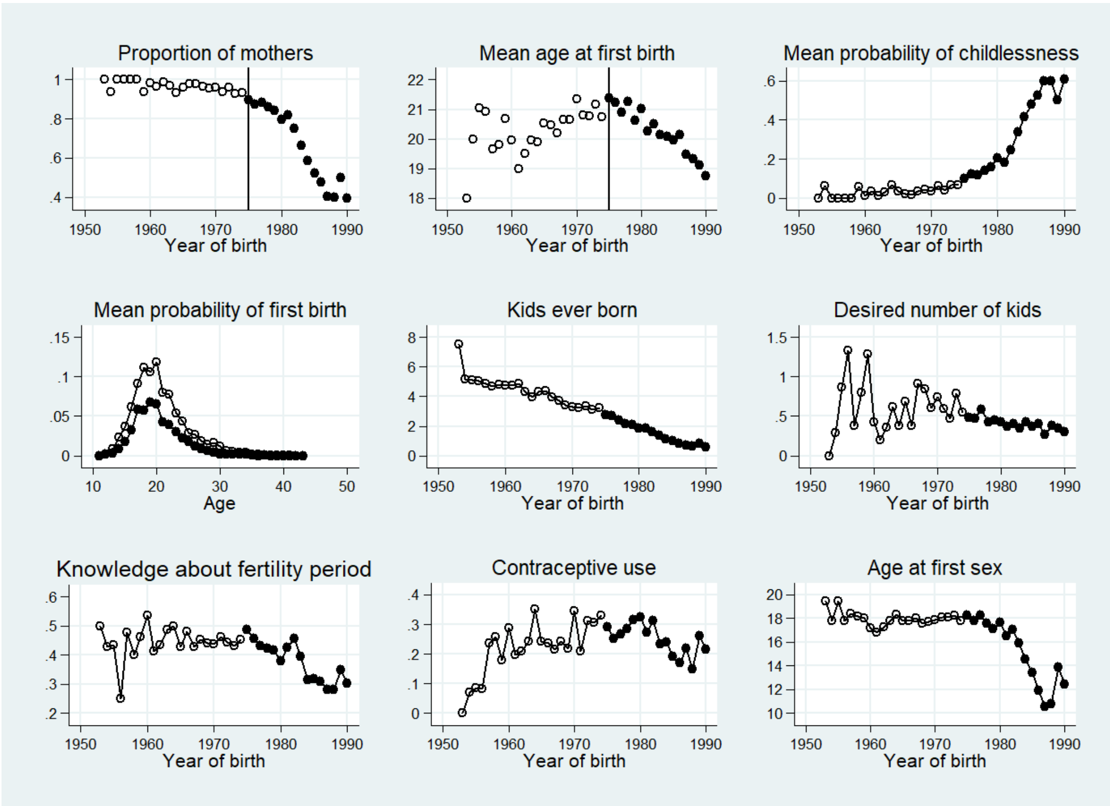
[14] The choice of age 18 is based on the finding by Royer (2004) that mothers who give birth before age 18 are more likely to give pre-term babies.

Table 1.5: Sample selection into motherhood and teenage pregnancy

	(1) Motherhood	(2) First birth<18
Treatment status	−0.038	−0.070
	(0.032)	(0.050)
Mean of dependent variable	0.96	0.17
Observations	1871	1871

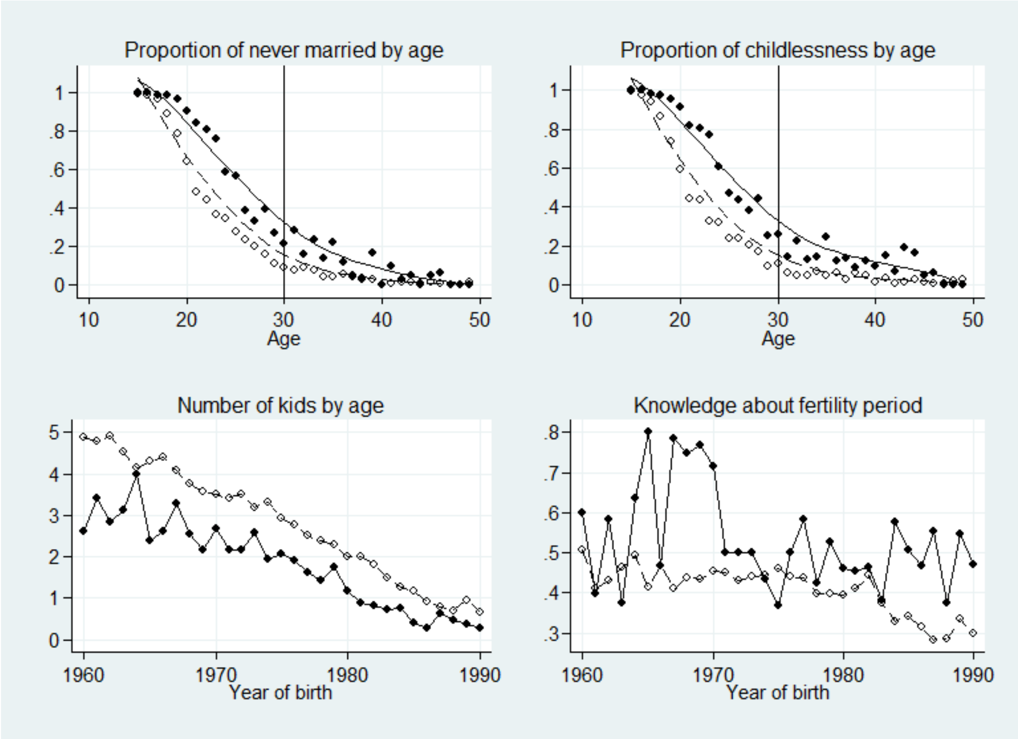
Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1.4: Observed motherhood, age at first birth and other fertility related outcomes



Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted. Hollow circles (thick circles) represent control (treated) cohorts.

Figure 1.5: Fertility outcomes by age and education level



Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted. The solid lines and circles represent secondary school sample while dashed lines and hollow circles represent basic school sample. The upper panel solid and dashed lines are coefficients from local polynomial regression.

Further graphical analyses on other fertility outcomes by reform status are done since the effect of education could manifest in these outcomes. Figure 1.4 shows a generally increasing incidence of the probability of becoming childless for the affected cohorts and a seemingly stable incidence for the unaffected cohorts. In terms of the probability of women having their first child at a given age, the plot shows a similar trend for both categories of women, albeit the control cohorts have slightly higher incidences. The incidence is even greater for the sample of women with basic education attainment compared with women with secondary education attainment. This makes sense because if these women discontinue their education at that level, it increases their likelihood of being exposed to sexual activities. Again, the fact that the control cohorts show higher incidences than their treated counterparts is in alignment with expectation since they used more years in their education cycle. Overall, however, these women are more likely to have their first child beyond age 18 and tapers off thereafter quite swiftly.

Figure 1.4 also shows that women affected by the policy surprisingly have fewer children compared with the unaffected cohorts. However, the trend does not look jumpy which seems to suggest that the two categories of women are similar. In terms of desired fertility, contraceptive use and knowledge about ovulatory cycle, the women seem to have similar outcomes. Regarding their exposure to sex, the treated cohorts seem to have their sexual debut at a rather earlier age.

When compared across educational qualification, the sample of secondary school leavers have slightly fewer children, an outcome which is theoretically logical. Generally, the pre-exposure cohorts seem to have had their sexual debut at a much older age than the affected cohorts. However, around the cutoff, the outcome seems to be similar amongst the two groups. In terms of contraception and knowledge about fertility period, the two groups appear similar.

Moreover, I present graphical evidence of average number of birth, knowledge of ovulatory cycle, childlessness and spouseless by cohorts according to the educational levels used in this study. Figure 1.5 shows that the average total number of births is a decreasing function of age regardless of educational level, although it is lower for women with higher education. Women with secondary education have greater knowledge about their fertility period compared with basic school counterparts. I also analyse the proportion of women who neither gave birth nor married by their level of education and clearly there is a gap (though not so wide) between women with higher level of education and those with basic education. Thus, women with secondary school attainment are proportionately higher in terms of their proclivity of remaining childless or spouseless.

One conclusion emanating from the graphical analyses is that, women with different levels of education apparently have different fertility outcomes. Comparing women with basic education to those with secondary education reveals yet some differences in their fertility outcomes. What remains puzzling is whether these differences are significant and in particular, whether it matters for the number of years of school one spends to complete a particular level/cycle to achieve the intended effects. To solve this conundrum, I present the reduced form estimates of the effects of reducing the number of years of basic and secondary school on these fertility outcomes by focusing on women who were affected (unaffected) by the policy during its implementation period.

Tables 1.6 presents the reduced form estimates of the effect of the policy on a number of fertility related outcomes. The regressions utilise the age of the woman in 1987 as the forcing variable. I used five measures of fertility in addition to the ones provided in Table 1.5. For mothers with basic education, the policy has no significant effect on all the outcomes. When women with equivalent of secondary school were considered, the expected outcome was achieved as the affected cohorts significantly reduces their age at first birth by about 3 years compared with the unaffected cohorts. The latter achievement is not surprising as the policy sought to reduce years of secondary education from 7 to 3 years and hence women with more years of schoolings are theoretically expected to have children at a much later date (See Table C3). Put differently, attending higher education and in particular spending more years at the level decreases the opportunities to enter marriage and/or give birth. This phenomenon is what Black et al. (2008) refer to as the "incarceration effect".

In sum, I find little evidence that the policy affects fertility outcomes. This lack of concrete evidence of the impact of reducing years of schooling on these fertility outcomes somewhat limits the causal role education might play in a woman's fertility planning, at least in the case of Ghana. This finding corroborates the study by McCrary and Royer (2011) who find no effect of school entry policies on fertility outcomes in the United States. Thus, it is not educational attainment per se that determines sexual activity of women as it did not show impact on outcomes such as knowledge of fertility period (ovulatory cycle). As argued by McCrary and Royer (2011), lack of statistical impact of the policy on fertility is indicative of a seemingly comparable selected sample, and hence can be used to study the effect of education on child health without sample selection corrections.

Table 1.6: Effect of the reform on fertility

	(1) Number of children	(2) Childlessness	(3) Age at firstbirth	(4) Fertility period	(5) Desired fertility
Treatment status	-0.035	0.040	0.174	-0.028	0.218
	(0.238)	(0.033)	(0.518)	(0.054)	(0.184)
Mean dep. variable	3.55	0.05	21.12	0.47	0.56
Observations	1851	1851	1632	1848	1851

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.5.5 Pathways linking education to child health

Maternal education might indirectly affect child health through its effect on a mother's behaviour. Thus, to better understand the lack of evidence of maternal schooling on child health, it is incumbent to examine the pathways through which maternal education can influence child health. I consider a number of pathways grouped under maternal health seeking behaviours, spousal quality, economic outcomes and fertility outcomes. Table 1.7 reports the IV estimates of effects of maternal education on these outcomes. The first set of mechanisms that I explore is the impact of education on maternal health-seeking behaviour on behalf of her child. For the sample of women with basic education, the effect of education on these outcomes is not different from zero except significantly increasing the probability of being fully immunized.

I also explore the potential role of labour market participation or economic outcomes as a mechanism for educational impacts on child health. The DHS data do not provide comprehensive information on wage and work history of respondents. However, I utilise an information on the likelihood of a woman being in the labour market which is based on the responses to the work status 12 months preceding the time of the survey.

Table 1.7: IV estimates of potential mechanisms

	A: Health seeking behaviour				B: Economic Outcomes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Institutional delivery	Received 8+ prenatal care	Delivery by health professionals	Fully immunized	Woman working	Urban location	High wealth index dummy	Low wealth index dummy
Years of education	0.005	−0.024	−0.003	−0.210**	0.127	0.219*	0.098	−0.051
	(0.116)	(0.133)	(0.129)	(0.100)	(0.084)	(0.115)	(0.130)	(0.114)
Observations	1351	1019	1355	1358	1830	1830	1830	1830
First stage F-stat	7.74	8.84	7.75	7.82	39.36	39.36	39.36	39.36
	C: Husband's outcomes				D: Fertility related outcomes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Husband working	Age gap	White-collar job	Education gap	Number of children	Age at first birth	Fertility period	Desired fertility
Years of education	−0.001	1.171	−0.054	−0.846	−0.063	−0.583	0.222*	−0.294
	(0.025)	(1.553)	(0.087)	(1.039)	(0.400)	(1.116)	(0.131)	(0.441)
Observations	1755	1473	1755	1755	1808	1720	1805	1808
First stage F-stat	38.28	33.38	38.28	38.28	38.10	38.07	38.06	38.10

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Again, I define two dummy variables: which I coded 1 each for being in the lowest wealth quintile and highest wealth quintile. No coefficient is statistically significant when economic outcomes are considered. As the results are as expected in terms of significance, they may not be unanticipated particularly in the case of Ghana which is beleaguered with some considerable labour market constraints.

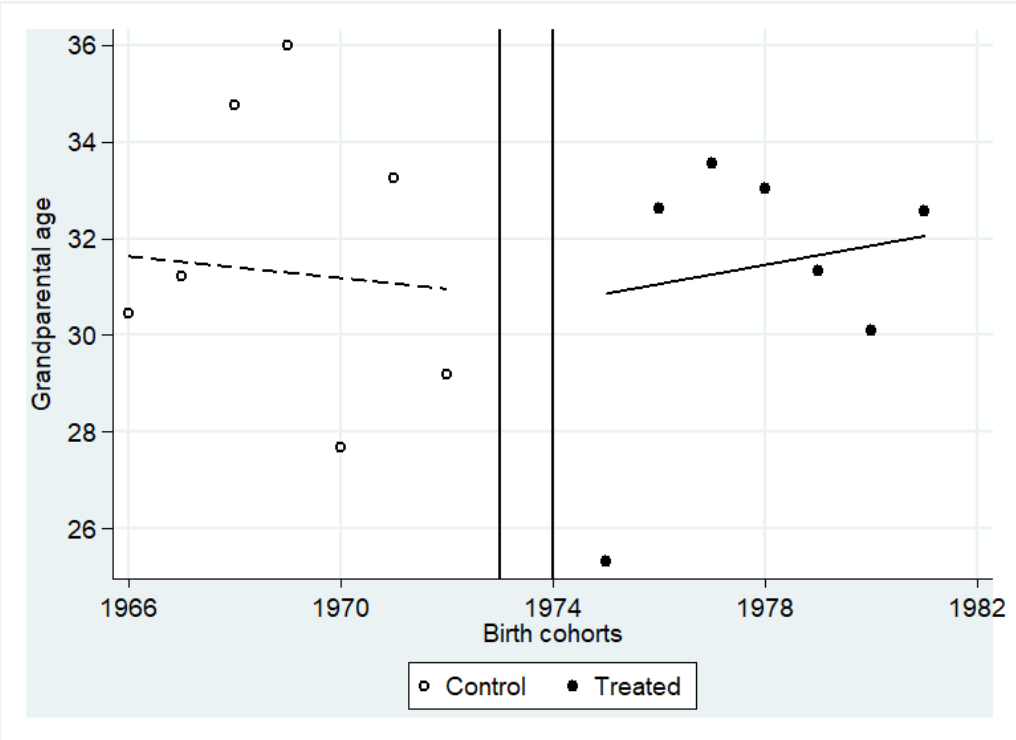
Moreover, I explore the effect of education on the quality of marriage (defined here as the education of the spouse), as well-educated women would likely marry well-educated and wealthier men. Given that the average age gap between couples in the sample is about 6 years, the likelihood that the reform affected only maternal education is high, thereby allowing for the separate role men play in affecting indirectly the outcomes of interest. Estimates shown in panel C show a similar labour market performance as their partners, albeit they are small and statistically indistinct from zero. Again, education increases the age gap with partners for women with basic level of education, but statistically insignificant.

The final set of mechanisms I explore is the impact of the reform to maternal education on the fertility and sexual behaviour of women. Again, I find no evidence of increased schooling on any of the outcomes for women with basic education. However, there is some muted evidence of increased knowledge about fertility period.

1.6 Robustness

In this section, I provide an evidence that women who are on either side of the cutoff (age 12 in 1987) are similar along predetermined dimensions. Essentially, the validity of the identification strategy of the effects of maternal education rests on the assumption that women born before and after the cutoff date have similar predetermined characteristics. This assumption can be tested by examining the continuity of predetermined characteristics (i.e., baseline traits) in the day of birth of the sampled women. If there were other negative selections on women at age 12 in 1987, it would be implausible to attribute the drop in school years to solely the effect of the policy. Regrettably, information on such predetermined characteristics is limited in the DHS data. However, I observe a small sub-sample of 332 women who are daughters of household heads, that is, these individuals are grandparents of children in the sample.[15] Thus, the age of these parents are used to generate their ages at the births of the women used in the study. As revealed in Figure 1.6, I find little evidence of any discontinuity in the grandparental age at birth.

Figure 1.6: Robustness: discontinuity in grandparental age at birth

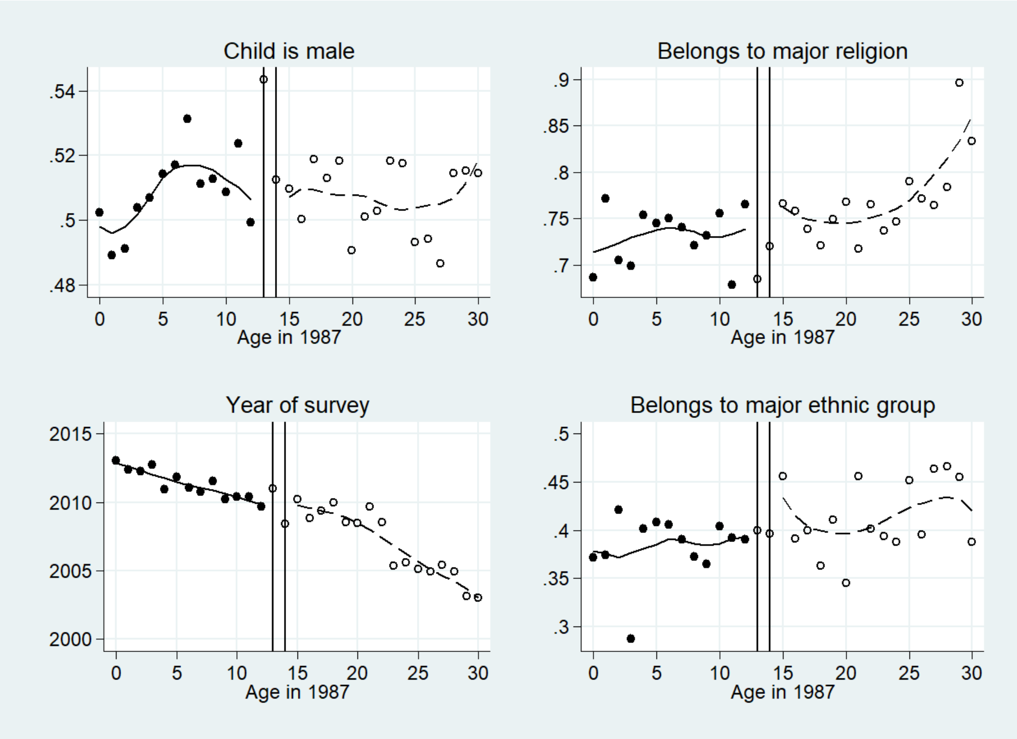


Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted.

Again, I test the smoothness of covariates assumption as frequently done in the regression discontinuity literature. However, the limitation of the DHS to collect data on rich set of predetermined variables makes it quite impossible for almost any outcome to be affected by the reduction in length of school cycles. Figure 1.7 shows that religion of the mother, sex of child, ethnicity of the mother and the year of survey are quite smooth around the cutoff. Thus, these covariates are not affected around the point of discontinuity.

[15] For this test, I did not split the sample according to the level of education of mothers since the data by so doing would be so scanty. This is justified since the age cutoff is the same these samples of women.

Figure 1.7: Robustness: smoothness of covariates



Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted.

1.7 Treatment Heterogeneity

The effect of the educational restructuring policy on schooling could conceivably be heterogeneous across regions. As the reform intended basically to address the inequities between secondary school and middle school, it sought to ensure that all children of school going age had access to higher levels of education. Consequently, the construction of schools (private and public) particularly in regions which hitherto had no such schools could generate a significant positive shock to individuals living in those regions. This is because, accessibility would be hugely improved and in case where schools are closer to the inhabitants, the cost of attending school would be immensely reduced, especially so when a larger chunk of the cost elements had been absorbed by the government. Nonetheless, I expect that less densely populated regions are more likely to experience increase in cost of accessing education due to higher travel cost which consequently can result in higher dropouts. I test this hypothesis by dividing the regions according to the pre-existing education level since school quality and not only length of cycles might have changed.[16] Following Zhang (2014), I measured pre-existing level of education as the proportion of mothers who had completed basic education among the control group at ages 13-20. By this, I divided the regions into two: that is, those that fall above and below the median pre-existing level of education.[17]

Table 1.8: Heterogeneous effects of the reform

	Pre-existing school levels			
	(1)	(2)	(3)	(4)
	Basic school		Secondary school	
	Below median	Above median	Below median	Above median
Years of education	-0.245^{***}	-0.364^{***}	-1.659^{***}	-1.698^{***}
	(0.088)	(0.067)	(0.509)	(0.384)
Observations	416	1414	126	233
Mean of dep. variable	9.41	9.03	12.74	12.74
HAZ	0.660	-0.139	2.183	0.396
	(0.442)	(0.214)	(2.045)	(0.350)
Observations	273	814	57	125
Mean of dep. variable	-1.12	-1.12	-0.72	-0.72
WHZ	0.080	-0.219^{***}	-2.501^{**}	0.212
	(0.324)	(0.069)	(1.046)	(0.231)
Observations	274	809	56	125
Mean of dep. variable	-0.14	-0.25	-0.06	-0.06
Low birth weight	0.147	-0.048	-0.149	0.071
	(0.095)	(0.056)	(0.216)	(0.075)
Observations	161	710	69	143
Mean of dep. variable	0.10	0.10	0.07	0.07
Child mortality	-0.040	-0.046^{***}	-0.267^{**}	-0.018
	(0.041)	(0.017)	(0.114)	(0.039)
Observations	217	1117	91	177
Mean of dep. variable	0.06	0.06	0.04	0.04

Notes: Clustered (Mother*Age in 1987) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[16] For instance, a high increase in enrolment or increase in the number of schools may have reduced pupil-teacher ratio. If enrolment did not increase, this would increase school quality.

[17] Using this measurement, Ashanti, Western, Greater Accra, Eastern and Brong Ahafo regions were found to be above the median of 12.78%, whereas Central, Volta, Northern, Upper East and Upper West were below the median.

The reform reduced significantly years of education of education for regions below and above the median educational levels, albeit with varying degrees. For instance, it reduced years of schooling by around 13% for the sample of secondary school women below and above the median level. Regarding the basic school women, it reduced it by 2.6% and 4% respectively for women below and above the median educational level (see Table 1.8). Regarding the health outcomes, the results show no evidence of a decrease in years of schooling on HAZ and low birth weight. On child mortality, the effect is stronger for women with basic education above the median education level. On the other hand, it is stronger for women with secondary education below the median level.

Again, the relationship between maternal education and child health could be nonlinear, in a sense that, the effect will depend on the level of education affected by the reform. The health returns of education has been found to be greatest amongst the least educated. The reform exploited in this study, though was comprehensive, did not affect the lower level of education from kindergarten to primary where the effect as suggested might have been greatest. Nonetheless, considering the level of education used in this study where the reform primarily targeted, I still suspect that the estimates so obtained should be relatively larger compared with studies that used interventions at the highest level of education.

1.8 Conclusion

An esteemed policy instrument that has been identified to improve health outcomes of children is increasing female education. Previous literature have tried to uncover the causal effect of female education on child or infant health outcomes using different research designs and estimation techniques. As a result, various pathways linking education to child health have been identified. The literature that use exogenous variations in education to uncover the causal relationship focus on interventions targeted at a specific level of the educational hierarchy.

Building on the previous literature, this study seeks to answer the question: does reducing the length of school cycles affect a woman's fertility and child health? In essence, this study is one of the few papers to study the effect of maternal education on child health in a developing country by exploiting a comprehensive educational reform that took place in Ghana in 1987 to identify exogenous effects. By comparing women with equivalent educational attainment and also according to their reform eligibility, I find that the reform led to a decline in years of schooling by 0.71 and 2.05 respectively for women with basic and secondary school respectively. Notwithstanding this discontinuous change in schooling induced by the reform, I find that one more year of maternal schooling has no effect on fertility, low birth weight, child's long and short term malnutrition. However, there appears to be some evidence of a positive effect in terms of reducing the probability of child mortality for women with basic education. The results seem to contradict most of the findings from the previous literature and corroborate only a few, partly because I do not have many terms of comparison as to the quasi experiments of reducing human capital investment. In addition, I find that a woman's eligibility to the reform on her selection into motherhood and maternal age at first birth is inconsequential. Thus the estimated effects of education on child health so achieved are not confounded by fertility choices. This provides some credence that the individual groups of women born near the time of the reform are similar. This point was ratified by providing evidence that there are similarities on the predetermined characteristics of these women.

One fundamental conclusion emanating from this study is that, years spent in attaining a particular educational cycle (particularly basic education) for women do not really matter as far as their fertility and health of their children are concerned, at least for the case of Ghana. A possible explanation could be that the "incarceration effect" is low, or that the returns to the same level of education did not change, i.e., potential issues with quality of schooling.

Acknowledgements

Comments from participants at the IV LEER Workshop on Education Economics (Leuven), in particular Madhuri Agarwal and Debra Shepherd are gratefully acknowledged. I also acknowledge Karen Grépin for sharing her do file at the initial stages which immensely helped me in the data management. I am also grateful to seminar participants at the Department of Economics, Management and Quantitative Methods at the University of Milan. Special thanks to Lucia Corno and Daniela Vuri for the extensive comments they gave on earlier version of this paper. The usual disclaimers apply.

Table A1: Empirical evidence on the effect of education on child health and fertility

Author(s)	Type of reform exploited	Education level affected	Outcomes considered	Observed outcome	Country
Currie and Moretti (2003)	College Openings	College	Birth weight & Gestational age	Positive	U.S.A
Breierova and Duflo (2004)	School construction	Primary	Child mortality & Number of children	Negative No effect	Indonesia
Lindeboom et al. (2009)	Increase minimum school leaving age	Secondary	Birth weight	Negative	Great Britain
Maïga (2011)	School construction	Primary	WHZ & HAZ	Positive	Burkina Faso
McCrary and Royer (2011)	Age at school entry	Kindergarten	Low birth weight Prematurity & Infant death	No effect	U.S.A
Zhang (2014)	High school closures	Secondary	Low birth weight, prematurity, neonatal & infant mortality	No effect	China
Güneş (2015)	Compulsory school law	Primary	Low birthweight, HAZ & WAZ	Positive	Turkey
Grépin and Bharadwaj (2015)	Secondary school expansion	Secondary	Child mortality	Negative	Zimbabwe
Rawlings (2015)	Compulsory schooling law	JSS	HAZ & WAZ	Positive (Boys) No effect (Girls)	China
Makate and Makate (2016)	Tuition fee elimination	Primary	Child mortality	Negative	Malawi
Osili and Long (2008)	Universal primary education	Primary	Number of children	Negative	Nigeria
Black et al. (2008)	Compulsory schooling law	Primary	Age at first birth	Positive	U.S.A & Norway
Fort (2009)	Compulsory schooling law	JHS	Number of children	No effect	Italy
Lavy and Zablotsky (2011)	Military rule & restricted access	Primary	Number of children	Negative	Israel
Braakman (2011)	Minimum school leaving age	Secondary	Number of children	Positive	Great Britain

Table A2: Sample description

	Overall			***Treatment (6-12)***			***Control (15-21)***		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
Panel A: Characteristics of woman									
Years of education	9.39	0.49	1871	9.06	0.24	1105	9.86	0.35	766
Age in 1987	12.46	4.84	1871	8.78	1.94	1105	17.76	2.01	766
Mother birth year	1974.54	4.84	1871	1978.22	1.94	1105	1969.24	2.01	766
Ever married	0.90	0.30	1871	0.85	0.36	1105	0.97	0.17	766
Age at first birth	21.24	4.11	1652	21.31	3.92	920	21.16	4.34	732
Panel B: Child health outcomes & Child characteristics									
Child is male	0.53	0.50	1277	0.53	0.50	921	0.53	0.50	356
Child's age (in months)	29.74	16.94	1277	29.33	17.09	921	30.82	16.53	356
Height-for-age z-score (HAZ)	-1.03	1.51	926	-0.95	1.50	645	-1.20	1.53	281
Stunting	0.18	0.38	1277	0.16	0.37	921	0.22	0.41	356
Moderate stunting	0.13	0.33	1277	0.12	0.32	921	0.14	0.35	356
Severe stunting	0.05	0.22	1277	0.04	0.19	921	0.08	0.27	356
Weight-for-height z-score (WHZ)	-0.28	1.28	924	-0.34	1.26	645	-0.13	1.31	279
Wasting	0.05	0.23	1277	0.05	0.22	921	0.06	0.24	356
Moderate wasting	0.05	0.22	1277	0.05	0.22	921	0.06	0.23	356
Severe wasting	0.02	0.12	1277	0.02	0.13	921	0.01	0.11	356
Low birth weight (Birth weight ¡2500g)	0.08	0.27	747	0.08	0.27	538	0.09	0.28	209
Child is dead	0.05	0.22	1277	0.05	0.22	921	0.06	0.24	356
Panel C: Health behaviours of mothers									
Receipt of 8+ prenatal visits	0.37	0.48	960	0.36	0.48	680	0.40	0.49	280
Delivery in a health facility	0.76	0.43	1277	0.77	0.42	921	0.76	0.43	356
Delivery assistance by health professionals	0.78	0.42	1275	0.78	0.41	920	0.76	0.43	355
Child is fully immunized	0.56	0.50	1277	0.55	0.50	921	0.58	0.49	356

Notes: Sample does not include women who were exactly 13 or 14 in 1987.

Appendix

1.A Additional Tables

Table A3: Effect of the policy on maternal years of education

	(1)	(2)	(3)	(4)
	Years of basic education		Years of sec. education	
Full sample				
Treatment status			−2.152***	−2.046***
			(0.144)	(0.135)
Mean of dep. variable			14.05	14.05
R^2			0.534	0.583
Observations			445	445
Including ages 13 &14				
Treatment status			−2.030***	−1.908***
			(0.117)	(0.117)
Mean of dep. variable			13.93	13.93
R^2			0.453	0.503
Observations			496	496
Rural sample				
Treatment status	−0.785***	−0.745***	−2.307***	−2.206***
	(0.026)	(0.029)	(0.280)	(0.282)
R^2	0.627	0.636	0.585	0.715
Observations	747	747	95	95
Urban sample				
Treatment status	−0.713***	−0.591***	−2.110***	−2.070***
	(0.035)	(0.042)	(0.167)	(0.166)
R^2	0.511	0.535	0.520	0.574
Observations	1291	1291	350	350
Polynomial				
Treatment status	−0.709***	−0.352***	−2.152***	−2.048***
	(0.030)	(0.050)	(0.144)	(0.342)
R^2	0.510	0.567	0.534	0.581
Observations	2143	2143	445	445
Control	*No*	*Yes*	*No*	*Yes*

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index and linear slopes on either side of the age 12. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: OLS estimation of the effect of education on child health

	(1) HAZ	(2) WHZ	(3) Low birth weight	(4) Child death
	Panel A: Years of education			
Years of education	0.007*	0.000	0.001	−0.001
	(0.004)	(0.003)	(0.001)	(0.001)
	Panel B: Education Qualification			
Primary	0.147**	0.002	−0.008	0.007
	(0.066)	(0.048)	(0.016)	(0.007)
JSS/Middle	0.143**	0.004	0.001	−0.008
	(0.058)	(0.052)	(0.020)	(0.008)
Secondary	0.353***	0.034	0.002	−0.010
	(0.095)	(0.078)	(0.023)	(0.012)
Higher	0.489***	0.296***	−0.010	−0.012
	(0.130)	(0.097)	(0.023)	(0.015)
No/Kindergarten	−2.105***	−0.353***	0.140***	0.058***
	(0.158)	(0.126)	(0.036)	(0.019)
Observations	8330	8262	5507	12718

Notes: Clustered (Mother*Age in 1987) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects, child's sex and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Maternal age at first birth

	(1)	(2)	(3)	(4)
	Basic school		Secondary school	
Age	Coefficient	P-value	Coefficient	P-value
12	0.006	0.183		
13	0.008	0.184	0.013	0.354
14	−0.017	0.196	−0.015	0.675
15	−0.020	0.313	−0.001	0.924
16	−0.049*	0.080	0.011	0.591
17	0.004	0.915	−0.008	0.850
18	−0.033	0.379	0.078	0.114
19	0.020	0.551	0.073**	0.033
20	0.046	0.315	0.062	0.353
21	0.015	0.696	0.063	0.482
22	0.028	0.524	0.100	0.130
23	0.061**	0.049	0.003	0.976
24	−0.039	0.183	−0.015	0.795
25	0.003	0.897	−0.003	0.969
26	−0.013	0.641	−0.074	0.324
27	0.011	0.559	−0.082	0.436
28	−0.018	0.182	−0.099	0.104
29	0.016	0.259	−0.017	0.757
30	−0.016	0.212	−0.060	0.206
31	−0.009	0.304	0.023	0.635
32	−0.004	0.713	−0.084	0.287
33	0.007	0.426	−0.046	0.298
34	0.006	0.566	0.072**	0.018
35	−0.001	0.783	0.035	0.246
36	−0.006	0.329	−0.028	0.329
37	0.003	0.317		
38	−0.007	0.233		
39	−0.002	0.345		
40	−0.001	0.474		
Sample mean		21.24		24.07
Observations		1652		320

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Maternal education and child health outcomes: robustness

	(1) HAZ	(2) WHZ	(3) Low birth weight	(4) Child death
	Including age 13 and 14 in 1987			
Years of education	0.031	0.672**	−0.006	0.104*
	(0.415)	(0.262)	(0.124)	(0.062)
Observations	1051	1046	844	1445
First stage F-statistic	11.15	11.50	6.89	12.66
	Restricting analysis to ages 8-19 (inclusive in 1987)			
Years of education	−0.315	0.360	−0.144	0.034
	(0.455)	(0.301)	(0.090)	(0.080)
Observations	650	649	515	897
First stage F-statistic	33.16	31.62	206.92	34.83
	Excluding 2003 DHS			
Years of education	0.866	0.877	−0.081	0.126
	(2.206)	(1.114)	(0.252)	(0.140)
Observations	543	539	561	841
First stage F-statistic	1.55	1.77	2.36	3.37
	Including 2nd order polynomial			
Years of education	−2.332	3.758	−0.099	−0.226
	(6.531)	(9.902)	(0.563)	(0.724)
Observations	981	977	798	1358
First stage F-statistic	0.17	0.16	2.67	0.22
	Including birth × survey year fixed effects			
Years of education	−0.067	−1.426	−0.529	−0.847
	(2.636)	(2.245)	(0.660)	(0.657)
Mean of dep. variable				
Observations	981.00	977.00	798.00	1358.00
First stage F-statistic	1.15	1.33	3.60	2.43

Notes: Clustered (Mother*Age in 1987) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 (inclusive) in 1987 (except where noted) but excluding women who were 13 or 14 (except where noted). All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects, child's sex and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.B Secondary school sample results

Table C1: Effect of secondary education on child health outcomes

	(1) HAZ	(2) WHZ	(3) Low birth weight	(4) Child death
		Panel A: OLS estimation		
Years of education	−0.032	−0.104	−0.028	0.012
	(0.145)	(0.162)	(0.026)	(0.017)
Mean of dep. variable	−0.66	−0.14	0.07	0.04
Observations	157	155	193	237
		Panel B: Reduced form		
Treatment indicator	1.162**	−0.083	0.014	−0.110
	(0.563)	(0.738)	(0.125)	(0.076)
Mean of dep. variable	−0.66	−0.14	0.07	0.04
Observations	157	155	193	237
		Panel C: IV estimation		
Years of education	0.387	−0.491	−0.161**	−0.066
	(0.380)	(0.361)	(0.063)	(0.052)
Mean of dep. variable	−0.66	−0.14	0.07	0.04
First stage F statistic	17.88	17.43	6.15	8.76
Observations	157	155	193	237

Notes: Clustered (Mother*Age in 1987) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects, child's sex and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Sample selection into motherhood and teenage pregnancy: Secondary

	(1) Motherhood	(2) First birth < 18
Treatment status	0.069	0.019
	(0.108)	(0.054)
Mean of dependent variable	0.87	0.07
Observations	445	445

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Effect of the reform on fertility: secondary

	(1) Number of children	(2) Childlessness	(3) Age at firstbirth	(4) Fertility period	(5) Desired fertility
Treatment status	0.406	−0.064	−2.806**	0.013	0.131
	(0.398)	(0.108)	(1.314)	(0.123)	(0.267)
Mean dep. variable	2.39	0.13	24.38	0.60	0.55
Observations	441	441	316	439	441

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C4: Mechanisms-IV Estimation: Secondary

	A: Health seeking behaviour				B: Economic Outcomes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Institutional delivery	Received 8+ prenatal care	Delivery by health professionals	Fully immunized	Woman working	Urban location	High wealth index dummy	Low wealth index dummy
Years of education	0.102	−0.389*	0.157***	−0.011	0.109	0.077	0.030	0.082
	(0.075)	(0.202)	(0.058)	(0.052)	(0.085)	(0.109)	(0.082)	(0.050)
Observations	235	181	237	237	399	399	399	399
First stage F-stat	8.82	8.23	8.76	8.76	16.73	16.73	16.73	16.73
	C: Husband's outcomes				D: Fertility related outcomes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Husband working	Age gap	White-collar job	Education gap	Number of children	Age at firth birth	Fertility period	Desired fertility
Years of education	0.006	1.838	0.243	2.470*	−0.981*	1.935	0.041	0.738*
	(0.008)	(1.947)	(0.194)	(1.426)	(0.510)	(1.539)	(0.163)	(0.421)
Observations	286	253	286	286	398	272	397	398
First stage F-stat	8.16	10.87	8.16	8.16	9.26	7.72	9.26	9.26

Notes: Clustered (region*birth year) standard errors are in parentheses. Estimates are obtained from women who were between the ages of 6 and 21 in 1987 but excluding women who were 13 or 14. All specifications include controls for urban and rural location, region fixed effects, survey fixed effects, religion fixed effects, ethnicity fixed effects, wealth index, linear slopes on either side of the age 12, child birth year fixed effects and mother's age at birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.C Figures

Figure C8: Average age for entering primary one

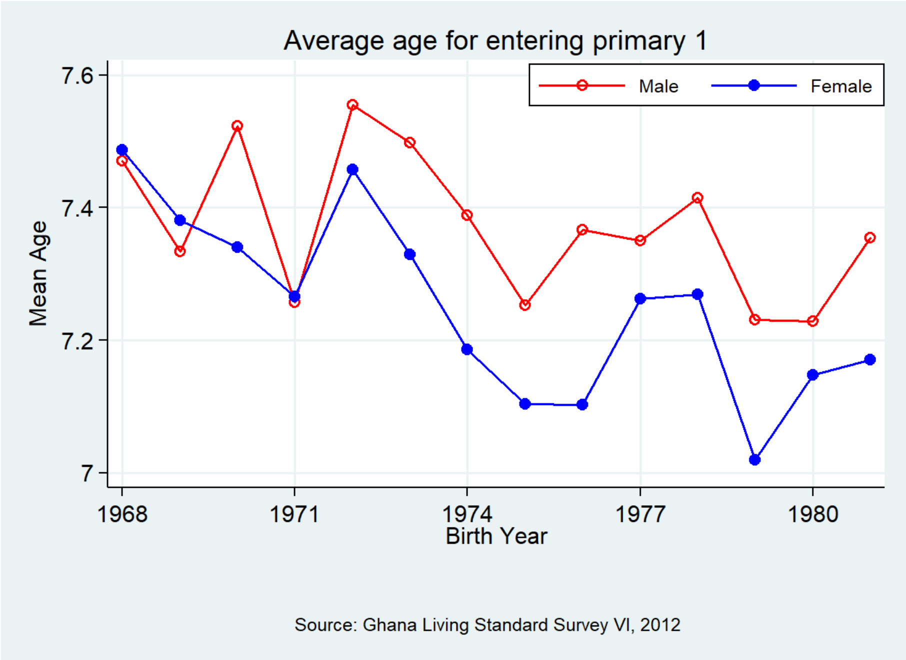
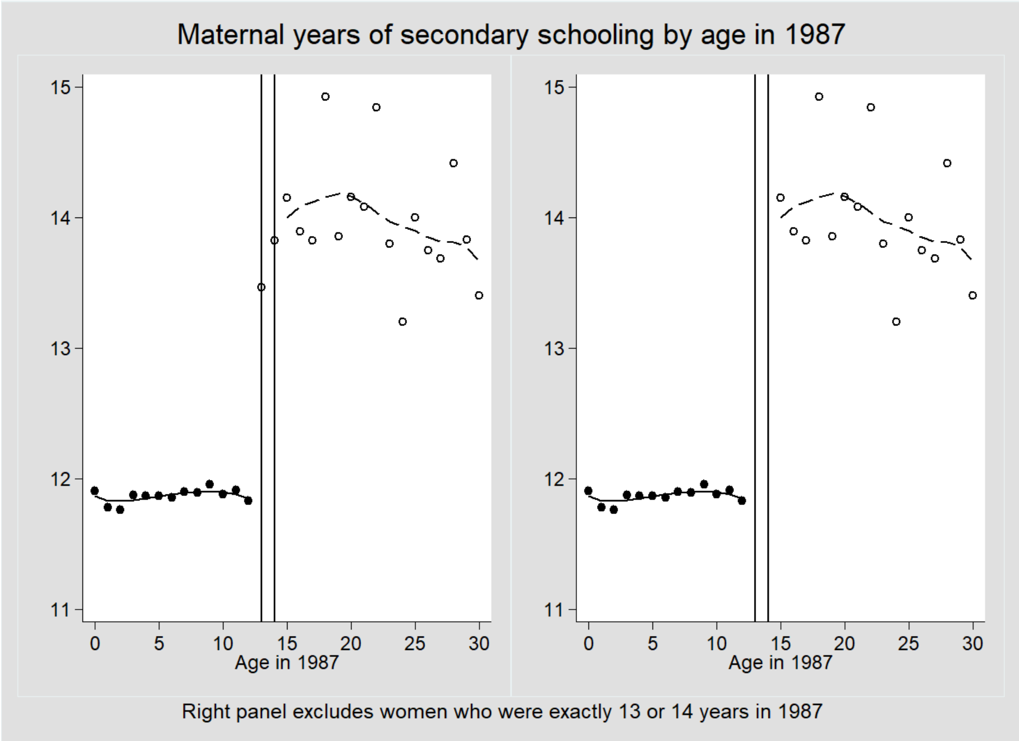
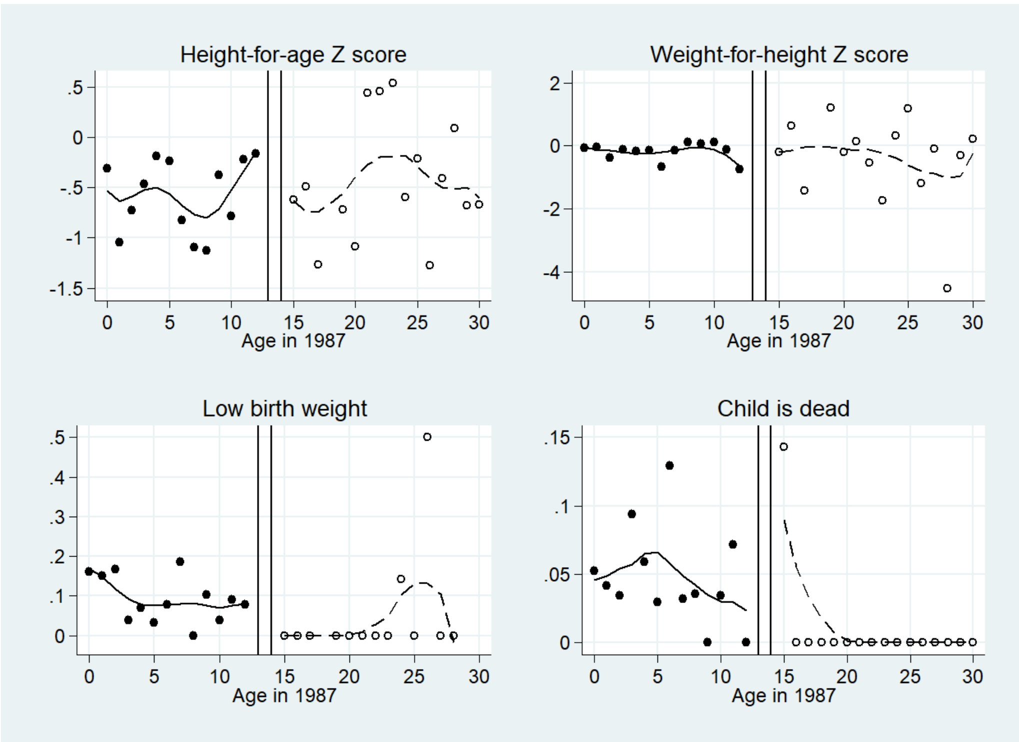


Figure C9: Discontinuity in years of secondary school



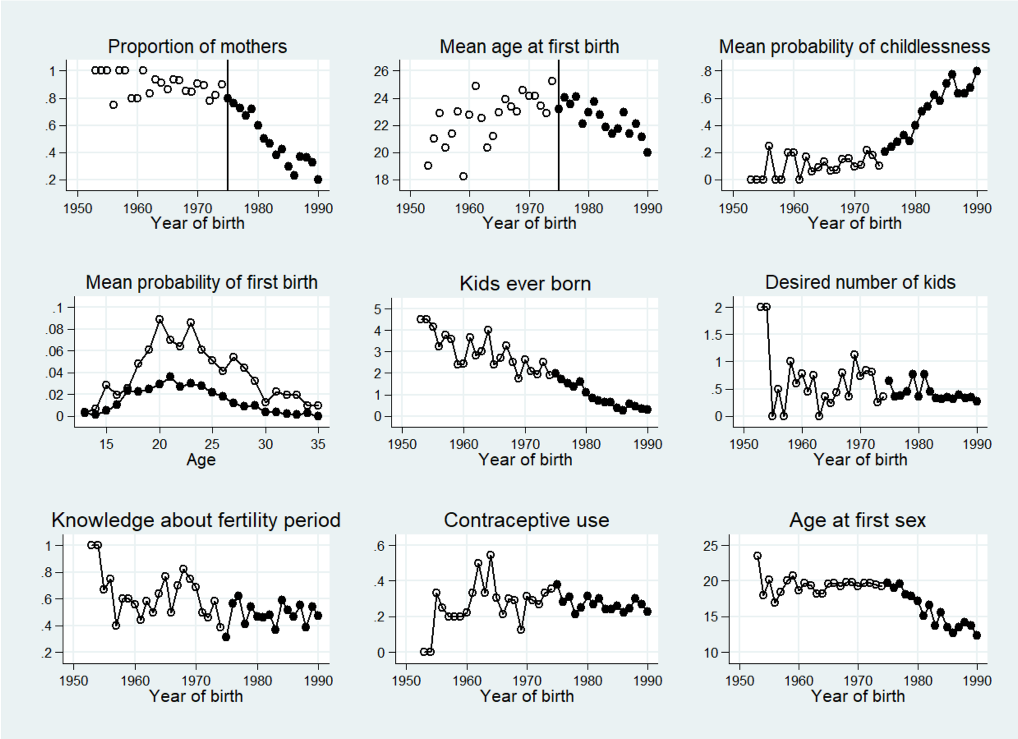
Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted. Notes: Individuals lower than age 13 in 1987 are subjected to the reform. Ages 13 and 14 are considered to be partially treated.

Figure C10: Impact of the reform on child health outcomes



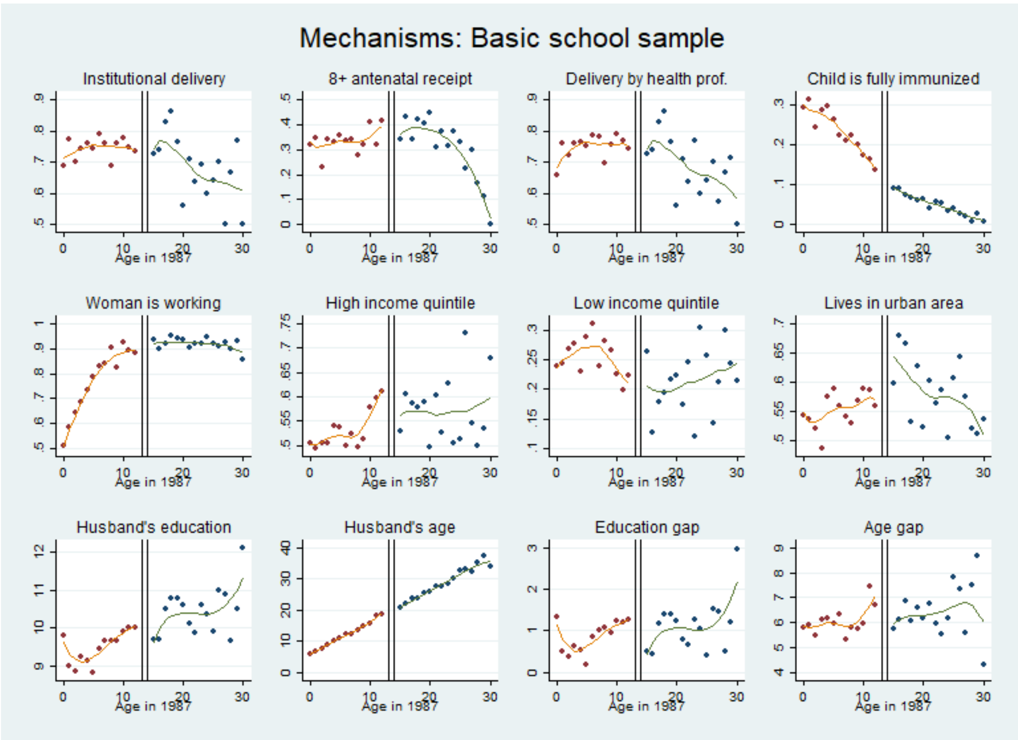
Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted.

Figure C11: Fertility outcomes for the secondary school sample



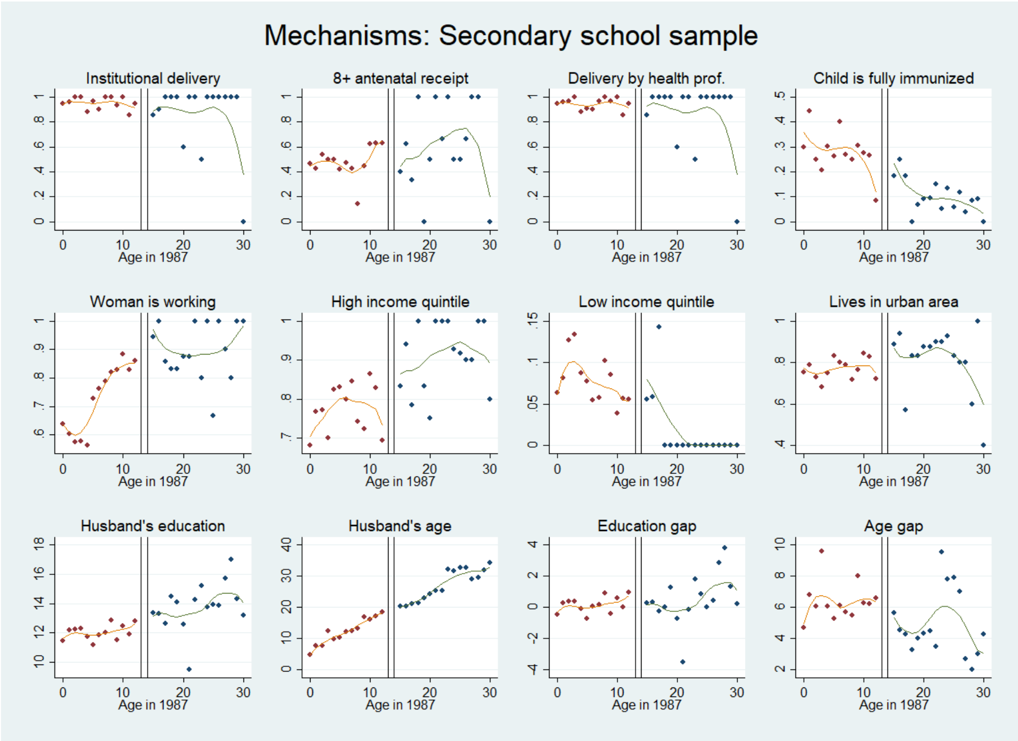
Source: GDHS 2003, 2008, 2014; own calculations. Note: Plots are unweighted. Hollow circles (thick circles) represent control (treated) cohorts.

Figure C12: Impact of reform on pathway variables-basic school sample



Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted.

Figure C13: Impact of reform on pathway variables-secondary school sample



Source: GDHS 2003, 2008, 2014; own calculations. Notes: Plots are unweighted.

Chapter 2

Heat waves and Child Health in sub-Saharan Africa

Abstract

This paper investigates the consequences of in utero exposure to extremely hot temperatures on child health in Sub-Saharan Africa (SSA) using a novel indicator of heat waves (Heat Wave Magnitude Index daily). Using several geo-referenced waves of the Demographic and Health Surveys merged with gridded data on the presence of heat waves and their magnitude since the 1980s, our analysis demonstrates the negative effects of heat waves on children's birth and early childhood health outcomes (birth weight, low birth weight, weight-for-age and height-for-age z-scores, undernutrition, severe undernutrition, stunting, severe stunting, anemia). Potential mitigation strategies in the SSA context are investigated.

JEL codes: I14 I15 Q54 J13

Keywords: Heat waves, child health, sub-Saharan Africa

2.1 Introduction

A growing body of research has analyzed the effects of various fetal shocks on children's outcomes both at birth and later in life. This paper builds on Barker's fetal origins hypothesis (Barker, 1990, 1995) which posits that, access to nutrition in early life has long-run effects on individual health and well-being. In other words, some chronic health conditions can be tracked to the course of fetal development. Early life shocks such as famines, epidemics, recessions affect fetal programming and consequently lead to lower cognitive attainment, earlier morbidities and lower life expectancy for cohorts born during these periods.[1]

Among the several shocks that children may experience in utero, of particular importance are weather-related shocks, which have become increasingly topical in recent times. Indeed, in the coming decades, with global warming and increasing global mean temperatures, heat waves (periods of excessively hot weather), will become longer, hotter and more frequent than in the present climate (Russo et al., 2014).[2]

In this respect, Africa is one of the most vulnerable continents to climate extremes due to its extreme exposure and low adaptive capacity (Barros et al., 2014). At the same time Africa is the continent where the occurrence of extreme heat waves is expected to be more likely than in other continents (Russo et al., 2014, 2016) and it is liable to bear the brunt of the effects of extreme weather shocks, albeit it has contributed little to its cause. The last 50 to 100 years has seen near surface temperatures increased by 0.5°C or more over most parts of Africa (Hulme et al., 2001; Stern et al., 2011; Nicholson et al., 2013). Within the period 1989 to 2009, northern

[1] For example, events such as famine (Kannisto et al., 1997; Lindeboom et al., 2010; Portrait et al., 2011; Xu et al., 2016), Ramadan fasting (Almond and Mazumder, 2011; Van Ewijk, 2011; Majid, 2015), epidemics (Almond, 2006; Banerjee et al., 2010; Neelsen and Stratmann, 2012), government interventions (Field et al., 2009; Lucas, 2010) and extreme weather shocks (Maccini and Yang, 2009; Skoufias and Vinha, 2012; Grace et al., 2015; Rocha and Soares, 2015; Wilde et al., 2017; Shah and Steinberg, 2017) experienced in utero have been found to affect children's outcomes. Economists have investigated how prenatal shocks and other conditions affect not only health later on in life, but also a range of other outcomes including test scores, educational attainment, income and socioeconomic status in general (Almond and Currie, 2011*a*,*c*).

[2] A more precise definition of heat waves is given in Section 2.2.

Africa and north-western Sahara experienced 40 to 50 hot days per year (Vizy and Cook, 2012). There is also a further projection of an increased number of hot days in the coming decades, particularly in the northern part of Africa (Patricola and Cook, 2010; Vizy and Cook, 2012). Hence, in the upcoming decades, the occurrence of heat waves could have a strong impact on human mortality and crop production in Africa (Russo et al., 2016).

In this paper, we focus on the potential short- to medium-term health effects (e.g., at birth or during childhood) of heat waves experienced while *in utero* using the Demographic and Health Surveys (DHS) data. Understanding how weather-related shocks impair children's health is key, especially to develop the strategies needed to mitigate the potential long term consequences of such shocks. However, evidence on the effects of heat shocks on child health for the African continent is still lacking. The few existing studies either provide evidence only on single countries (e.g. Mulmi et al., 2016*a*) or on one or few proxies of child health — such as birth weight (e.g. Grace et al., 2015), or on relatively rare and extreme events such as child mortality (e.g. Kudamatsu et al., 2016; Wilde et al., 2017).

In the current paper, we seek to improve our knowledge of the effects of extreme temperature on child health (i) by focusing on sub-Saharan Africa (SSA), one of the world's regions whose population is most at risk of suffering the negative impact of rising temperatures, both because of the incidence of the phenomenon and the lack of coping mechanisms; (ii) by investigating the effect of heat shocks on several health outcomes including birth weight, low birth weight, height-for-age, weight-for-age, stunting, underweight, and mortality; (iii) by carrying out an effect heterogeneity analysis to investigate which groups (i.e., socio-economic groups and gender) are more exposed to the health damages of heat waves; (iv) by reporting extensive evidence on the availability and effectiveness of mitigation strategies in the SSA context (e.g., housing type, water and sanitation, electricity and refrigeration, urban residence, pre-natal health care and geographical mobility).

In order to address potential omitted variables biases owing to unobservable characteristics which may make some regions/periods of the year more likely to be hit by heat waves but also to provide worse conditions for a healthy child development, we leverage presumably exogenous variation in a child's exposure to extreme temperatures while in utero by exploiting within-district-month of birth differences in the incidence of heat waves across years of birth. In so doing, we compare two children that were born in the same district, for instance Gushiegu district in the Northern region of Ghana, in the same month, say November, but in two different years, one in which that district was affected by heat waves in the 9 months before the mother gave birth and one in which that same district was not affected by such shocks. The strategy of including district-month of birth fixed effects has been already followed in the literature (e.g., Wilde et al., 2017; Kumar et al., 2016) because it allows to control for time-invariant district-month of birth unobservables. For instance, women who decide to conceive children in given months of the year (e.g., winter) may systematically differ from those that conceive in other months (e.g., summer) on characteristics that correlate with child health, and this selection may be district specific (e.g. be stronger in hotter than in colder districts). District-month of births fixed effects allow to address potential selective fertility issues. To further reduce potential concerns related with selective migration and fertility, our preferred empirical specification also includes district-specific time trends, which capture long-term trends in temperatures observed at the district level and that may be more predictable by the population.

We find that, exposure to heat waves while in utero is detrimental to child health. Heat waves experienced by the child in the first month of gestation are particularly damaging in terms of birth weight (-22 grams) and probability of low birth weight (0.9 percentage point). Exposure to heat waves during pregnancy reduces weight-for-age by 0.039, and increases the probability of being underweight, severely underweight, and severely stunted by 1, 1.2 and 1.5 percentage points, respectively. By contrast, we do not find evidence of exposure to heat waves increasing either in-utero or post-birth child mortality.

Our results demonstrate that the effects are larger for shocks experienced during the first trimester for post-birth weight related health measures and during the third trimester of pregnancy, for weight related health measures. Furthermore, the effect of heat waves experienced in the first five years after birth is similar in magnitude and direction of the effects while in utero.

Sons are more affected than daughters by in-utero exposure to heat waves. This is consistent with male fetuses being less resistant and suffering from the legacy of heat waves well after birth (*scarring effect*). The effect heterogeneity analysis also points to the fact that children of low educated mothers (i.e. at most primary schooling), are more likely to disproportionally bear the health burden of heat waves. The analysis of the potential mitigation mechanisms demonstrates that access to improved water and sanitation has a protective effect on weight-for-age and reduces the probability of being stunted, probably by sheltering individuals from infectious diseases, while improved housing reduces the risk of low birth weight suggesting that heat stress may be particularly harmful for this outcome. By contrast, we do not find any protecting role for living in urban areas, having access to electricity and refrigeration, geographical mobility, or for compensating investments in child health under the form of pre-natal care.

The rest of the paper is structured as follows. Section 2.2 describes the data sources and the main features of the dependent and independent variables used in the empirical analysis. Section 2.3 explains the empirical strategy. The main results are presented in Section 2.4 while some selectivity and measurement error issues are dicussed in Section 2.5. Sections 2.6 and 2.7 report some robustness checks and an analysis of effect heterogeneity, respectively. The relevance of potential strategies to cope with heat waves in the SSA context is discussed in Section 2.8. Section 2.9 draws conclusions.

2.2 Data

We pool comprehensive information on mothers and their children, and geo-referenced data on weather variables to construct a dataset for 24 Sub-Saharan African countries over the period 1990 through to 2015. Appendix A provides detailed information on the countries and the survey waves used in the empirical analysis. The type and sources of data used are explained in the following sections.

2.2.1 Weather shocks

Calculation procedure of the Heat Wave Magnitude Index daily (HWMId)

Daily maximum temperature and precipitation data from the ERA-Interim (ERA-I) reanalysis from the European Centre for medium-range weather forecasts (Dee et al., 2011) are used to gather data on heat waves and droughts in the past and present climate in Africa. The dataset has a 6-hourly time resolution and is available from January 1979 to January 2016. The ERA-I is based on a T255 resolution (0.7^o, 79 km). The magnitude of heat waves and drought events[3] are estimated at a seasonal scale by means of the Heat Wave Magnitude Index daily (HWMId), following Russo et al. (2015) and Russo et al. (2016).

In order not to split heat waves that start in a 3-month block and end in the consecutive 4th month not included in the block, the HWMId is calculated for 12 moving blocks of 3 months: January–February–March, February–March–April, ..., December–January–February (see Russo et al., 2016). Following Russo et al. (2016), a heat wave is defined as *a period of at least three consecutive days with maximum temperature (T_{max}) above the daily threshold for the reference period 1981–2010.* The threshold is defined as the 90th percentile of daily maxima, centered on a 31-day window. The magnitude of a heat wave is calculated by summing up the magnitude

[3] Although precipitations and droughts are not the main focus of this paper, we include them in some regression models in the robustness checks.

of each hot day comprising a heat wave with daily magnitude (M_d) at each grid point calculated as described in Russo et al. (2015) as follows:[4]

$$M_d(T_d) = \begin{cases} \frac{T_d - T_{30y25p}}{T_{30y75p} - T_{30y25p}} & \text{if } T_d > T_{30y25p} \\ 0 & \text{if } T_d \leq T_{30y25p} \end{cases} \tag{2.1}$$

where T_d is the daily maximum temperature on the d day of the heat wave. T_{30y25p} and T_{30y75p} are respectively the 25th and 75th percentiles (i.e., first and third quartiles) of the time series composed of 30 year annual T_{max} within the reference period 1981–2010. The slope of $M_d(T_d)$ depends on T_{30y75p} and T_{30y25p}, which in turn are location specific.

The units in which the magnitude of heat waves is expressed have an immediate interpretation. The denominator of the M_d function is the interquartile range (IQR) of the 30 yearly T_{max} within the reference period (1981-2010), and represents a non-parametric measure of the variability of the time series composed of the annual T_{max} in the same period. Thus temperature anomalies are defined according to the temperature variability in each location. If a day of a heat wave has a temperature value T_d equal to T_{30y75p}, its magnitude will be equal to one, if T_d exceeds the 25th percentile of T_{max} by twice the IQR, the heat wave magnitude is two, and so on so forth.

The HWMId is preferred to other indexes for since it enables to compare heat waves occurring in different regions and in different years. In particular the HWMId has the desirable property of merging several climate-related measurements of heat events (i.e., duration and intensity) into a single number. Also, it has the advantage of defining the severity of the heat events on the basis of global thresholds, which no longer depends on the region (Russo et al., 2014, 2015; Zampieri et al., 2016).

[4] The HWMId calculation has been carried out using the HWMId function recently included in the R package called "extRemes" (Gilleland and Katz, 2016).

Calculation procedure of the Standardized Precipitation Index (SPI)

The Standardized Precipitation Index (SPI) is calculated as follows. The SPI uses monthly precipitation aggregates at various time scales (1, 3, 6, 12, 18, and 24 months, etc.) ideally for a continuous period of 30 years. For illustrative purposes, we use a 3-month scale, where the precipitation accumulation of month $j-2$ to month j is summed up and attributed to month j. This implies that the first two months of the data time series are missing. The next step follows a normalization procedure, in which an appropriate probability density function is first fitted to the long-term time series of the aggregated precipitation. The fitted function is then used to calculate the cumulative distribution of the data points, which are finally transformed into standardized normal variates. This process is repeated for all needed time scales. Once the relationship of probability to precipitation is established from the historical records, the probability of any observed precipitation data point is calculated and used along with an estimate of the inverse normal to calculate the precipitation deviation for a normally distributed probability density with a mean of zero and unit standard deviation. This value is the SPI for the particular precipitation data point (McKee et al., 1993).

Child exposure to weather shocks

To define a measure of a child's exposure to heat waves, we carry out the following steps. First, we follow Russo et al. (2014) to classify heat waves according to the HWMId. Geographical areas with HWMId values of zero are considered "not exposed" to heat waves. In other words, values equal to zero imply that there is no heat wave shock. Contrarily, areas with positive values are considered "exposed" to heat waves, albeit to different degrees. Thus, we first create a binary indicator for whether or not a district-month-year was hit by a heat wave, that is, whether or not the corresponding HWMId is positive or zero. Second, we create additional binary indicators reflecting the severity of exposure measured by the HWMId which are defined following Russo et al. (2016) as: $(0,3]$, $(3,6]$, $(6,9]$, and $(9+)$. Having defined these variables, we then simply construct indicator variables for heat shocks overlapping with the period of pregnancy. Namely, we create an aggregate dichomotomous indicator for whether a child was exposed to a heat shock (and dummies for its intensity) in utero and dichotomous indicators for the intensity of the shocks.

As for precipitations, following the literature, positive rainfall shocks are defined with SPI between +1 and +2 standard deviations with drought defined as less than -1 standard deviations (see McKee et al., 1993, for more details). Also in this case a child is exposed if the mother experiences these shocks during gestation.

2.2.2 Demographic and Health Surveys (DHS)

Our empirical analysis is based on the Demographic and Health Surveys (DHS), which gather representative household level data providing detailed information on health and demographic characteristics of populations living in developing countries across the world. The DHS data are also spatially referenced and they provide latitude and longitude values of their sampled clusters.[5] These clusters are randomly spatially displaced up to 10km (5km) from their actual urban (rural) locations. This is intended to preserve anonymity of the respondents. Additionally, the DHS collect detailed micro-level information about maternal characteristics and provides retrospective information about the health of infants and children.

Initially, we used every DHS between 1990 and 2016 for which the Global Positioning System (GPS) datasets are available, for a total of 84 surveys across 31 countries. The GPS datasets provides information on geographical coordinates of latitudes and longitudes of each DHS cluster in the sample. Using DHS unique cluster identifiers, we matched the surveys to their respective shape files. We further used the geo-referenced DHS surveys and matched them to the shape files of their second administrative regions (hereinafter referred to as *districts*). Using QGIS geospatial software and the district shape files, we overlaid the gridded SPI

[5] A cluster in the DHS by definition is a geographic location that represents the centroid of the area where the respondent resides.

and HWMId datasets onto the districts in the DHS to generate a monthly panel of weather variables corresponding to each district. Since the SPI and HWMId data come in a raster form, we calculated the mean and the maximum values of the pixels of the raster layers that were within each polygon which generates output columns in the vector layer. We dropped from the sample observations for which no HWMId data were available. We further dropped from the sample children born from multiple births (duplets, triplets, etc.) since twinning is commonly related with a lower weight at birth (Kramer, 1987). We also excluded children whose anthropometric measures are considered biologically implausible (Mei and Grummer-Strawn, 2007).[6] Finally, we excluded all children whose birth weights were observed to be above 6,500 g. or below 500 g., as these are beyond the normal range according to the medical literature (Doubilet et al., 1997). The final dataset uses 60 surveys covering the period 1990 and 2015 for 24 Sub-Saharan African countries (see Table A1 in the Appendix for the complete list).[7]

Anthropometric measures

The DHS collects objective measures of *weight* and *height* by qualified and trained nurses during the interviews and data collection for all children in the sample are comparable across all the surveys. This is important since self-reported height, for instance, has been found to exhibit downward bias (Macgregor et al., 2006). We construct two anthropometric z-scores, namely weight-for-age z-score (WAZ) and height-for-Age Z-score (HAZ) using the 2006 WHO child growth standards for all children aged 0-5 years of age by their sex.[8] Additionally, we construct binary variables *stunting* and *severe stunting* based on WHO definitions: equal to *one* if the

[6] Based on 2006 World Health Organisation's new cut-off value, the following measures are considered biologically implausible: if a child's Height-for-Age Z-score (HAZ) is below -6 or above + 6, weight-for-age Z-score (WAZ) is below -6 or above +5, weight-for-height Z-score (WHZ) is below -5 or above +5, or body-mass-index Z-score (BMIZ) is below -5 or above +5.

[7] For some countries, there was no information on heat wave magnitude index daily for the entire period, whereas it was missing for some years for other countries. For instance, we lost Senegal entirely from the sample since it had no information on HWMId for every year considered. For other countries like Ghana, some observations with missing HWMId were dropped from some survey years.

[8] The Stata code `zcore06` (De Onis, 2006) was used to carry out the computation.

child's HAZ falls below -2 (-3) standard deviations from the median Height-for-Age. Similarly, we construct two additional binary variables *underweight* and *severe underweight* equal to *one* if the child's WAZ falls below -2 (-3) standard deviations from the median Weight-for-Age (World Health Organisation, 2015).[9] The use of these measures is consistent with our goal of assessing the impact of early exposures to heat waves as they generally reflect inadequate nutritional intakes during the early stages of child development. These developments usually occur before age two and have long-lasting effects once they are established.[10]

Other health variables

We complement the anthropometric measures with other health-related measures. Recent waves of DHS gather blood samples of children during the data collection. Hemoglobin levels adjusted for cluster altitude are used to create a categorical variable for anemia levels (not anemic, mild, moderate and severe) based on the WHO standards. Based on this classification, we construct a binary variable which is equal to one if a child is reported to have at least mild anemia and zero if he/she is not anemic. Anemia is usually a measure of poor nutrition (i.e., iron deficiency) and poor health (World Health Organisation, 2008). We also employ information on birth weight and incidence of low birth weight among newborns as an additional measure of child health (at birth). Again, we use the WHO definition of low birth weight (i.e., birth weight less than 2,500 g.).

2.2.3 Descriptive statistics

Table 2.1 presents the sample description of the variables used in the empirical analysis. The mean WAZ is -0.97 with approximately 20% and 7% of the children having a Z-score of less than -2 and -3 standard deviations, respectively. Thus, in our

[9] Standard equipments and methods are used in measuring children's weight and height. For example, children younger than 24 months are measured lying down, while standing height is measured for children aged 24 months and older.

[10] Indeed, it is quite unlikely to regain the height lost during this period nor achieve a normal body weight for most children.

sub-Saharan Africa child sample, approximately one-third are suffering from some form of malnutrition. Similarly, HAZ average is around -1.47, with approximately 38% and 18% of the children having a z-score of below -2 and -3 standard deviations, respectively, meaning that about half of the children are shorter than normal. The average birth weight of a child is 3.2 kg. The data also show that about 59% of children suffer from at least mild anemia. Half of the children in the sample are boys while the average age of children is about 28 months. 47% of the mothers are poor. Literate mothers constitute about 63% of the sample while 28% live in urban areas. On average, a woman in the sample is 29 years old and mothers' average age at birth for the children whose outcomes are analyzed is 27 years. About 51% of the districts have experienced heat shocks over the period 1979 and 2016 but with varying intensities. For instance, only a paltry 3% of the sampled districts have been hit by heat waves beyond the magnitude of 9 while about 32% have been exposed to magnitudes in the range of 0 and 3. About 58% of the sample have access to improved water while 31% have access to improved sanitation. The data also show that 23% of the sampled households have access to electricity. Finally, 42%, 47% and 53% respectively have access to improved floor, wall and roofing materials.

Table 2.1: Sample description of the variables

Variable	Obs	Mean	Std. Dev.	Min	Max
A: Outcomes					
Weight-for-age z score (WAZ)	260203	-0.97	1.36	-6	5
Undernutrition (WAZ < -2)	260203	0.20		0	100
Severe undernutrition (WAZ < -3)	260203	0.07		0	100
Height-for-age z score (HAZ)	260203	-1.47	1.81	-6	6
Stunting (HAZ < -2)	260203	0.38		0	100
Severe stunting (HAZ < -3)	260203	0.18		0	100
Anemia ($<$ 11 g./dL)	89847	0.59		0	100
Birth weight (g.)	115431	3235.53	653.75	500	6500
Low birth weight ($<$ 2500 g.)	115431	0.08		0	100
B: Heat waves					
Heat wave magnitude index daily (HWMId)	260203	1.57	2.73	0	32.35
$HWMId = 0$	260203	0.49		0	1
$0 < HWMId \leq 3$	260203	0.32		0	1
$3 < HWMId \leq 6$	260203	0.12		0	1
$6 < HWMId \leq 9$	260203	0.04		0	1
$HWMId > 9$	260203	0.03		0	1
C: Other controls					
Mother's age at birth	260203	26.97	6.66	15	45
Mother's current age	260203	28.87	6.81	15	49
Mother is literate	260203	0.63		0	1
No education	260203	0.37		0	1
Primary education	260203	0.39		0	1
Secondary education	260203	0.21		0	1
Higher education	260203	0.03		0	1
Mother is Christian	260203	0.62		0	1
Lives in an urban area	260203	0.28		0	1
Poor	208629	0.47		0	1
Child's age (in months)	260203	27.79	17.11	0	59
Child is male	260203	0.50		0	1
Child's birth order	260203	3.70	2.42	1	18
Child's birth year	260203	2005.85	6.64	1985	2016
Improved water	260203	0.58		0	1
Improved sanitation	260203	0.31		0	1
Improved floor material	250865	0.42		0	1
Improved wall material	191436	0.47		0	1
Improved roofing material	193714	0.53		0	1
Electricity	260203	0.23		0	1

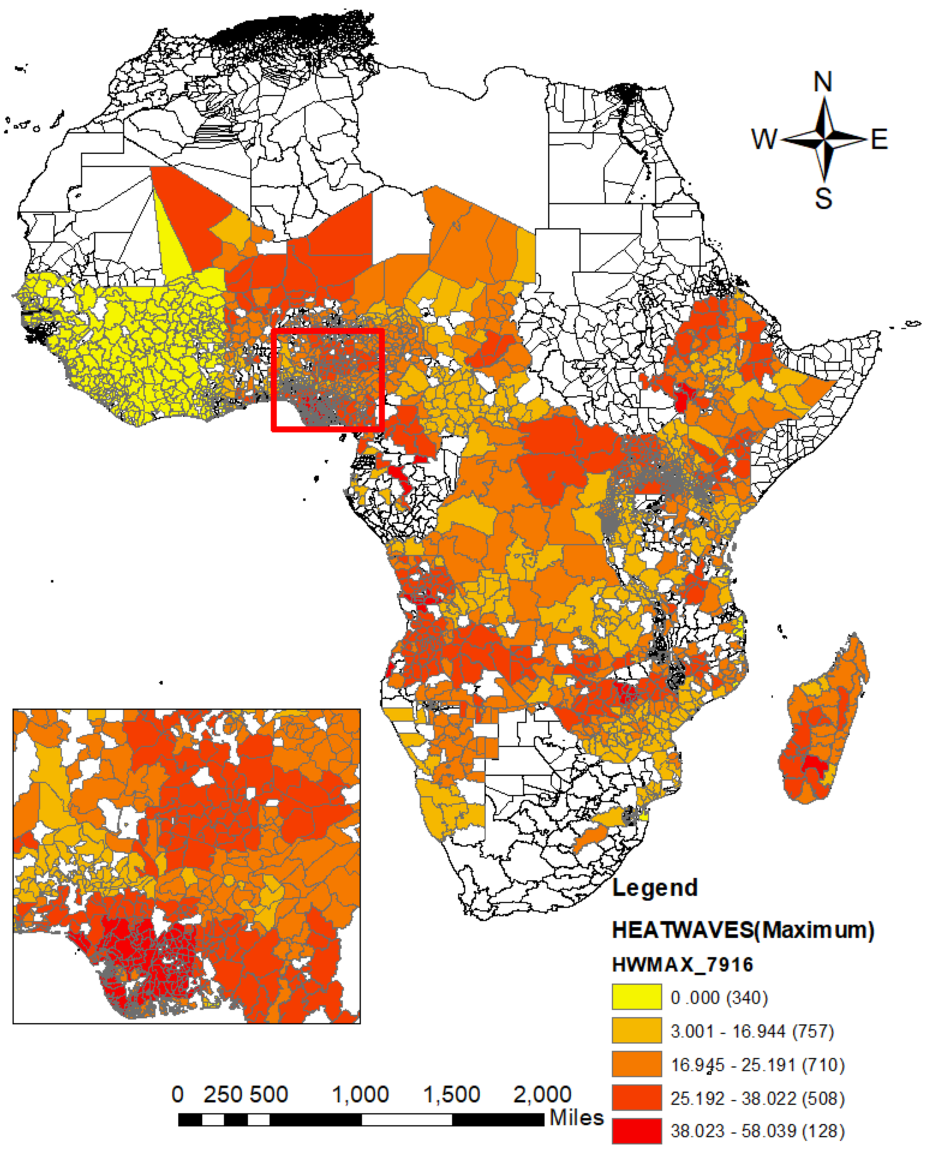
Notes: Authors' calculations from the Demographic and Health Surveys (DHS Program, USAID). Poor is defined as the household in the lowest wealth index category. The number of observations differs according to the availability of the different variables in the waves used in our analysis.

2.2.4 Climate variability in Sub-Saharan Africa

In this section, we present the distribution of climate conditions in Sub-Saharan Africa over the period 1979 to 2016. We first show the distribution of heat waves across districts according to their level of intensity. There is marked variation in climatic conditions in SSA over the period 1979 and 2016. Figure 2.1 presents the map of the distribution of heat waves for the period under analysis and indicates significant exposure of many districts to heat waves. As the figure the great majority of the districts have been exposed to heat shocks during the period of analysis, and therefore variation in most of the districts can be leveraged to identify the causal effects of heat waves.

We also plot the proportion of districts exposed to extreme weather events between 1979 and 2016 in Figures 2.2a and 2.2b. The top panel illustrates the trends in the districts when there is no heat wave and when there is an extreme heat wave (i.e., when HWMId is above 3). The bottom panel on the other hand uses the data from the SPI to categorize districts according to those that experienced droughts (SPI below -1) and floods (SPI above +1). The trends show that extreme weather anomalies occur frequently but quite unpredictably. Generally, significant proportions of districts do experience heat waves particularly in periods after 1996. This suggests that there has been an increase in the probability of observing heat waves across many districts in SSA which corroborates the information on the map in Figure 2.1. The percentage of districts that experienced droughts within the period is everywhere less than those that experienced floods. In other words, districts are more likely to experience floods than droughts.

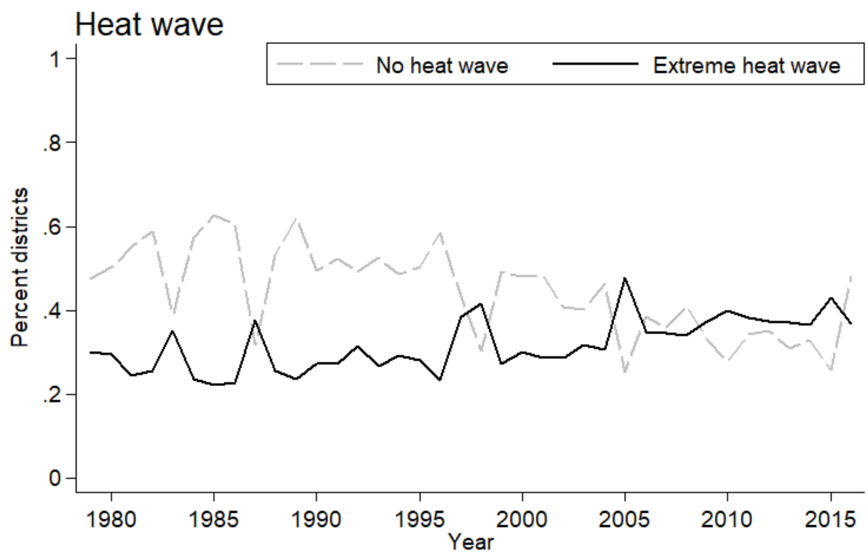
Figure 2.1: Maximum heat waves magnitudes by district (1979–2016)



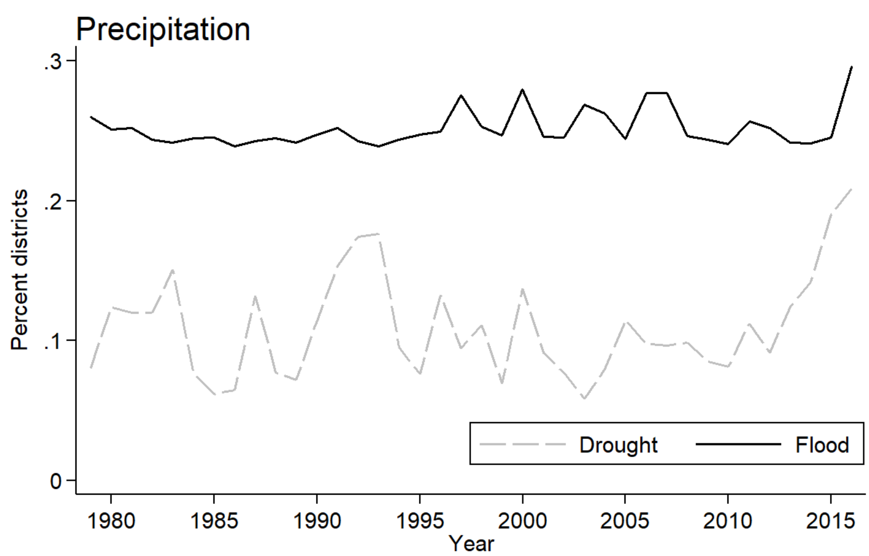
Notes: The figure shows the maximum heat wave magnitude index computed according to Russo et al. (2014) for SSA. Areas depicted in white either do not belong to SSA or are excluded from the analysis owing to heat waves data unavailability.

Figure 2.2: Extreme heat shocks and precipitations in Sub-Saharan Africa

(a)



(b)



Notes: Figure (a) shows the percentage of districts in SSA countries with no heat shock and with extreme heat stress for the period 1979-2016. Heat wave magnitude index daily (HWMId) of zero means no heat wave and values above 3 indicate extreme heat waves. Figure (b) shows extreme precipitation shocks for the same period. Droughts and floods are defined as SPI below -1 and above +2 standard deviations, respectively. Source: Authors' own calculations based on the Global Administrative Areas (GADM) version 2.6, Heat Wave Magnitude Index (Russo et al., 2014) and Standardized Precipitation Index (McKee et al., 1993, 1995).

2.3 Empirical strategy

Our empirical approach exploits variation in exposure to heat waves within a given district-month cell across years while in utero or during early childhood. We estimate the effect of prenatal exposure to heat shocks on early child health using the following model:[11]

$$h_{i,c,d,m,y,t} = \theta_{c,y} + \gamma_{d,m} + \beta T_{d,m,y} + \psi \mathbf{X}_{i,c,d,m,y,t} + \phi Trend_{d,y} + \varepsilon_{i,c,d,m,y,t}, \qquad (2.2)$$

where $h_{i,c,d,m,y,t}$ is the health outcome measured in the survey t for the child born in country c in district d in month m and in year y; $\theta_{c,y}$ is a country-year of birth fixed effect and $\gamma_{d,m}$ a district-month of birth fixed effect. Our regressor of interest is $T_{d,m,y}$, which is an aggregate indicator for exposure to heat shocks for the entire period of gestation.[12] Additionally, we include a vector of child, maternal and household characteristics $\mathbf{X}_i$, which are reported in the tables' footnotes. $Trend_{d,t}$ is a district-specific linear time trend and $\varepsilon_{i,c,d,m,y,t}$ an idiosyncratic error term.

Our strategy compares health outcomes of children pre-natally and post-natally exposed to unusually hot temperatures against outcomes of children who are not exposed to such heat shocks. The key independent variable is "exposure to heat wave" during gestation. Our identifying assumption is that the occurrence of a heat wave shock is a quasi-random event within a district over time, and thus, the assignment of exposure to a heat wave in utero and during early childhood is also *as good as random*. To address potential omitted variable bias related to the fact that children born in given countries, years and months may be more likely to be exposed to heat waves but also to unobserved factors affecting their future health outcomes, we include in the model country-year and district-month fixed effects. They control, among other things, for business cycle effects and month (or season) of birth effects, which have been found to affect children's outcomes (see, for

[11] See Wilde et al. (2017) or Molina and Saldarriaga (2017) for a similar specification.

[12] When we consider post-birth outcomes, we include $POST_{i,c,d,m,y,t}$ to indicate the number of heat waves experienced during the first fifty nine months after birth as an additional regressor.

instance, Dehejia and Lleras-Muney, 2004; Buckles and Hungerman, 2013; Bozzoli and Quintana-Domeque, 2014). To put it simply, the kind of between-individual variation that we leverage is that between children born in the same district and month, but in two different years, one in which a heat wave took place in the nine months preceding the month of child birth and another in which there was no such event. District specific linear trends help rule out the possibility that treated and control children were already on differential trajectories in their outcome variables, i.e., change in the outcome that would have happened even in the absence of the treatment which is picked up by β in our specification. They also help to net out predictable trends in weather extremes at the district level. We cluster standard errors at the district level since exposure is invariant within districts in each single year and there is possible correlation of errors within districts of the same country (Pepper, 2002).[13]

For small countries, it is possible that heat waves may hit all districts in a given year and month which implies that there is no variation across districts, that is, the deviation of our weather shock variable from the country-year mean is zero. To check that there is significant unexplained variation in heat waves across districts, we run a regression of heat waves on country-year fixed effects. Since the R^2 from this regression is relatively low ($R^2 = 0.161$), we adopt the specification of equation (2.2) and include country-year fixed effects in the model.[14]

Our analysis is further developed by disaggregating exposure according to the gestational period (i.e., trimester of pregnancy) in order to identify in which gestational periods the effects are concentrated. Thus, we estimate a variant of equation (2.2) as follows:

$$h_{i,c,d,m,y,t} = \theta_{c,y} + \gamma_{d,m} + \sum_{j=1}^{3} \beta_j T^j_{d,m,y} + \psi \mathbf{X}_{i,c,d,m,y,t} + \phi Trend_{d,y} + \varepsilon_{i,c,d,m,y,t}, \quad (2.3)$$

where $T^j_{d,m,y}$ is the indicator for exposure to heat shock for each trimester j.

[13] Clustering by district addresses correlation across DHS clusters within district.

[14] The complete regression output is available upon request.

Identifying the exact trimester of pregnancy would be ideal if we could count forward from the day of conception. Regrettably, information on pre-term births and last day of menstruation of mothers are lacking in the DHS, which prevents the exact calculation of the full length of pregnancy and the precise identification of trimesters of gestation in the data. Thus, assuming all pregnancies last 9 months, we calculate the trimester of pregnancy by counting backward from the child's month of birth.[15]

As we anticipated, equation (2.2) is also estimated using bin dichotomous variables for heat waves of different intensity following the categorization in Russo et al. (2016).[16]

2.4 Main results

To estimate the average impact of heat shocks on child health outcomes, we estimate equations (2.2) and (2.3). In addition to standardized anthropometric measures for children between 0 to 59 months of age as our measure of health, we also consider three other health outcomes: birth weight, low birth weight and anemia.

Table 2.2 presents the baseline results of in utero heat wave exposure on the health outcomes indicated in headings of columns (1) – (9) based on the estimation of equation (2.2). Panel A presents the results by considering the exposure to heat waves for the entire pregnancy period regardless of the trimester of gestation. Panel B segments the analysis by considering exposure according to trimester of pregnancy. The coefficients and standard errors of the binary outcomes have been multiplied by 100 and hence their effects are measured in percentage points. Results indicate that heat wave exposure while in utero significantly reduces WAZ by 0.04 standard deviations, which translates into a decrease in WAZ of approximately 4% given the

[15] We test the sensitivity of our results by decomposing in utero shocks into the first, second-to-eighth and ninth months while controlling for the month before conception and a possible tenth month of gestation in Section 2.6. This is intended to assess the potential bias induced by measurement error.

[16] We do not do the same for equation (2.3), i.e., we do not allow for heat waves of different intensities to have differential effects by trimester of gestation, since the estimates suffer from a problem of small cell sizes.

average WAZ for the sample of individuals whose districts were not hit by heat shocks is -1.04. Further, we find statistically significant increases in the likelihood of being born underweight and severely underweight for children exposed to heat waves by approximately 1.01 and 1.22 percentage points, respectively. Relative to the average of the baseline underweight and severely underweight rates of 21.62% and 7.23%, respectively, the effect is quite substantial, namely at 4.7% and 16.9%, respectively. Exposure to heat waves also significantly increases the likelihood of being severely stunted by 1.5 percentage points. Relative to the baseline sample mean, the effect is about 8.3%. We further find a significant positive effect of heat waves on the probability of low birth weight, of approximately 0.9 percent point which translates into a 11.4% increase in the likelihood of having a low birth weight. We do not find any effect of in utero exposure to heat waves on the other health outcomes considered in the analysis.

Allowing for heterogeneous effects according to gestational period in Panel B of Table 2.2, we find that the negative effects on WAZ are concentrated in the third trimester, while the negative effects on what is generally considered as a longer term health outcome, namely HAZ, is concentrated in the first trimester of gestation. Results are generally in line with those in panel A, although a significant *negative* effect on the probability of suffering from anemia is estimated for the second trimester. The negative (positive) effects of heat waves on BW (LBW) appear to be concentrated in the third trimester of gestation, but are generally statistically insignificant at conventional levels.

The analysis that uses HWMId categorized by intensity does not add much insights, probably because it suffers from a lack of power (as we already observed in Table 2.1 just a small share of the sample is affected by heat waves with very high HWMId). Table 2.3 only shows statistically significant positive effects for heat waves in the $(3;6]$ range of magnitude on severe stunting, and *negative* effects of heat waves in the $(6;9]$ range on anemia.

Next, we complement the analysis by considering the effects of post-birth shocks

Table 2.2: Effects of heat waves during pregnancy on child health outcomes

Dependent variable:	(1) WAZ	(2) WAZ< -2	(3) WAZ< -3	(4) HAZ	(5) HAZ< -2	(6) HAZ< -3	(7) Anemia	(8) BW	(9) LBW
Panel A: Effect of heat waves exposure at any time during pregnancy									
Exposure	-0.039**	1.006*	1.224***	-0.021	0.593	1.457**	-1.775	-10.108	0.890*
	(0.020)	(0.586)	(0.443)	(0.027)	(0.695)	(0.582)	(1.087)	(17.932)	(0.455)
Observations[a]	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.162	0.131	0.219	0.186	0.157	0.249	0.189	0.142
Mean dependent variable	-1.04	21.62%	7.23%	-1.476	38.42%	17.63%	62.69%	3220	7.83%
Panel B: Effect of heat waves exposure by trimester									
Exposure trimester 1	0.001	0.174	0.165	-0.011	0.746**	0.554**	0.053	2.309	0.095
	(0.012)	(0.360)	(0.200)	(0.014)	(0.330)	(0.280)	(0.481)	(7.025)	(0.275)
Exposure trimester 2	-0.011	0.165	0.228	-0.027*	0.414	0.366	-1.329**	-5.585	0.189
	(0.014)	(0.326)	(0.253)	(0.015)	(0.367)	(0.248)	(0.559)	(7.300)	(0.246)
Exposure trimester 3	-0.030***	0.739**	0.362*	-0.003	-0.026	0.487	-0.256	-12.458	0.443
	(0.010)	(0.306)	(0.199)	(0.014)	(0.385)	(0.296)	(0.615)	(9.334)	(0.293)
Observations[a]	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.162	0.131	0.219	0.186	0.157	0.249	0.189	0.142
Mean dependent variable	-1.04	21.62%	7.23%	-1.476	38.42%	17.63%	62.69%	3220	7.83%
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns 2, 3, 5, 6, 7 and 9 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, education level of mother, child's birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. Exposure is an aggregate indicator which is equal to one if a child was exposed to a heat wave shock in either of the nine months preceding birth. BW is birth weight and LBW is low birth weight. * Significant at 10% level; ** at 5%; *** at 1%. The mean of the dependent variable is calculated at the baseline (i.e., HWMId=0). The number of observations varies across columns depending on the availability of the health outcomes.

Table 2.3: Effect of heat waves during pregnancy on child health outcomes, by HWMId level

Dependent variable:	(1) WAZ	(2) WAZ< -2	(3) WAZ< -3	(4) HAZ	(5) HAZ< -2	(6) HAZ< -3	(7) Anemia	(8) BW	(9) LBW
Heat wave magnitude index daily range:									
$(0, 3]$	0.010	-0.073	0.288	-0.000	-0.010	0.341	-0.552	-6.095	0.514
	(0.016)	(0.484)	(0.264)	(0.020)	(0.494)	(0.375)	(0.815)	(12.922)	(0.400)
$(3, 6]$	-0.003	0.244	-0.043	0.003	0.242	0.577**	-0.700	-0.466	-0.097
	(0.012)	(0.346)	(0.197)	(0.016)	(0.418)	(0.282)	(0.622)	(6.776)	(0.232)
$(6, 9]$	-0.024	0.334	0.113	0.000	0.016	-0.226	-1.232*	-6.659	0.145
	(0.016)	(0.460)	(0.388)	(0.020)	(0.448)	(0.325)	(0.668)	(6.427)	(0.259)
> 9	-0.011	0.473	-0.172	-0.004	0.022	-0.105	-0.095	9.244	-0.379
	(0.021)	(0.607)	(0.360)	(0.024)	(0.540)	(0.531)	(0.930)	(8.681)	(0.344)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.162	0.131	0.219	0.186	0.157	0.249	0.189	0.142
Mean of dependent variable	-1.04	21.62%	7.23%	-1.476	38.42%	17.63%	62.69%	3220	7.83%
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns 2, 3, 5, 6, 7 and 9 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, education level of mother, child's birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. Exposure is divided by intervals of magnitude of HWMId. BW is birth weight and LBW is low birth weight. * Significant at 10% level; ** at 5%; *** at 1%. The mean of the dependent variable is calculated at the baseline (i.e., HWMId=0). The number of observations varies across columns depending on the availability of the health outcomes.

in addition to in utero exposure. Table 2.4 reports the estimated effects. We define post-birth shocks as the *number* of heat waves experienced by a child 59 months after birth attributed to individuals according to district of birth. The signs of the estimated coefficients are all consistent with expectations. The coefficient estimates of *in utero* shocks are very similar to those in Table 2.2. Moreover, we find that post-birth exposure to heat waves has negative effects on both WAZ and HAZ. Although these results are indicative of the potential importance of post-birth shocks, it is more difficult to give to these estimates a causal interpretation since households are more likely to change their locations because of heat shocks when a longer time span is considered.[17]

2.5 Selectivity and measurement issues

There are several reasons why our identification strategy may lead to upper or lower bound estimates of the effect of interest.

First, in order for a child to be observed in our estimation sample s/he must survive to a heat wave. However, a child may die during pregnancy or after pregnancy, and the probability of dying may be affected by in-utero exposure to heat waves (i.e., non-random selection). For this reason, in Section 2.5.1 we report evidence of fetal selection and child mortality, which can be considered as two extreme health outcomes.

Second, parents that decide to *conceive* during a heat wave may be a non-random sample of the population according to children's potential health outcomes. For instance, they may be negatively selected (lower educated and/or poor) causing a spurious correlation between heat waves exposure and children's health outcomes. Some evidence on fertility behaviour and selection into parenthood is presented in Section 2.5.2.

Finally we report some robustness checks that address potential measurement

[17] We also estimated the effect of post-birth shocks by intensity of heat waves. We find that the effects are stronger at higher intensities. The results are available upon request.

Table 2.4: Effect of heat wave exposure in utero and after birth on child health outcomes (Under 5 years)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	WAZ	WAZ< -2	WAZ< -3	HAZ	HAZ< -2	HAZ< -3	Anemia
In utero exposure	-0.0303	0.7148	1.0524**	-0.0134	0.3944	1.2943**	-1.7332
	(0.020)	(0.593)	(0.440)	(0.027)	(0.686)	(0.579)	(1.065)
Post birth exposure	-0.0005***	0.0178***	0.0105***	-0.0005***	0.0121***	0.0099***	-0.0029
	(0.000)	(0.002)	(0.001)	(0.000)	(0.002)	(0.002)	(0.007)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103
R-squared	0.212	0.163	0.131	0.219	0.186	0.158	0.249
Mean of dependent variable	-1.04	21.62%	7.23%	-1.476	38.42%	17.63%	62.69%
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns other than 1 and 4 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, education level of mother, child's birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban), maternal age at birth. In utero is an indicator which is equal to one if a child was exposed to heat wave in any of the nine months preceding birth. Post is the number of heat wave shocks experienced by a child 59 months after birth. The mean of the dependent variable is presented for the control group for which shocks in-utero and 59 months after birth are both zero. The number of observations varies across columns depending on the availability of the health outcomes. * Significant at 10% level; ** at 5%; *** at 1%.

error concerns related to household migration: some households may migrate because of a heat wave. In this case, attribution of exposure to heat shocks according to district of child birth may be inaccurate. Although geographical mobility is not probably the best strategy to cope with heat waves, especially for pregnant women and given the long travel times prevailing in Africa (Linard et al., 2012) and the geographical spread of heat waves,[18] we report in Section 2.5.3 the estimates on the sample of "stayers", i.e., women who were living in the district of the child birth at least two years before delivery.

2.5.1 *Fetal selection and post-birth child mortality*

Our estimates of the causal effect of heat waves exposure during pregnancy on child health outcomes may be affected by in utero selection (i.e., the probability that the child dies in the womb). Stronger and healthier fetuses are more likely to survive the entire duration of pregnancy, while weaker ones are likely to die because of heat wave shocks.

While it is difficult to measure spontaneous abortions, especially when they occur in the first weeks after conception, we make an effort to test for fetal selection by regressing an indicator for terminated pregnancy on heat waves around the *time of conception* and socioeconomic status of the mother.[19] Terminated pregnancy is an indicator which takes a value of one if the pregnancy is terminated before birth for any reason (either voluntary or not) and zero otherwise. The estimates are reported in Table 2.5 and do not indicate any effect of heat waves on fetal selection. The same conclusions is reached when we allow for heterogeneous effects by socio-economic status, in columns (4) and (5). Interestingly, columns from (2) to (5) point to a higher incidence of pregnancy termination for low socio-economic status, i.e. low-educated or poor, women. If we consider voluntary terminations, this may seem a bit counter-intuitive, as children could be 'normal goods'. Yet, it is consistent

[18] Indeed, heat waves are less geographical concentrated than precipitation shocks (Dell et al., 2014).

[19] This can be done because for terminated pregnancies the DHS provides month of termination.

with the quantity-quality tradeoff (Becker and Lewis, 1973) or the finding reported in the literature that abortion too may be a 'normal good' (**??**).[20]

Table 2.5: Heat waves and terminated pregnancy

	(1) Terminate	(2) Terminate	(3) Terminate	(4) Terminate	(5) Terminate
Exposure	-0.0009	-0.0037	0.0161	-0.0887	0.0778
	(0.167)	(0.166)	(0.167)	(0.233)	(0.193)
Poor		-1.141***		-1.241***	
		(0.263)		(0.322)	
Primary			-2.374***		-2.267***
			(0.315)		(0.375)
Exposure × Poor				0.188	
				(0.325)	
Exposure × Primary					-0.204
					(0.339)
Observations	311503	311503	311503	311503	311503
R^2	0.034	0.034	0.035	0.034	0.035
Country-Year of pregnancy FE	Yes	Yes	Yes	Yes	Yes
District-month-of pregnancy FE	Yes	Yes	Yes	Yes	Yes
District linear trends	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the district level are in parentheses.T he dependent variable is an indicator variable which takes the value of one if the woman reports that a pregnancy was terminated before birth for any reason. Primary indicates that the mother has no more than primary education. Poor indicates that the mother belongs to the lowest quintile of the wealth distribution. All regressions control for mother's age at pregnancy. All the coefficients in Columns (1) through (5) are scaled by 100 to make them more readable. The sample here is significantly larger than the sample used in the baseline regressions because in the latter we included only children who are alive at the time of the survey. To test for fetal loss and mortality, we include the sample of children who were not alive at the time of the survey. * Significant at 10% level; ** at 5%; *** at 1%.

We further test the fetal selection hypothesis by examining the effect of heat waves on the sex ratio in the data. According to the literature, heat wave shocks during pregnancy may affect the probability that the child is female since female fetuses are more resistant, i.e., *culling* effect (Catalano et al., 2005; Catalano and Bruckner, 2006; Catalano et al., 2006; Liu et al., 2014). To investigate this effect, we regress a dummy variable for whether the child is female on heat waves exposure at conception. The results are reported in columns (1) and (2) of Table 2.6. Column

[20] Low-educated and low-income women, for instance, have less access to 'safe abortions'.

(1) shows the estimates for the full sample of children while column (2) restricts the sample to children who are less than 2 years at the time of the survey, in order to address potential biases related to differential post-birth mortality. The results indicate that exposure to heat waves *at conception* is not significantly related to the sex ratio, confirming our previous results of absence of strong in-utero selection effects. All in all, our analysis suggests that the sample of children born alive is not a selected sample of children with respect to heat wave exposure.[21]

Table 2.6: Heat waves and child gender

	(1) Female	(2) Female
Exposure	0.360 (0.233)	0.405 (0.422)
Observations	260203	115558
R^2	0.084	0.140
District-Month of birth FE	Yes	Yes
Country-Year of birth FE	Yes	Yes
District linear trend	Yes	Yes

Notes: Robust standard errors clustered at the district level are in parentheses. The dependent variable is an indicator variable which takes a value of one if the individual is female and zero otherwise. In column (2) the sample is restricted to children who are less than two years old at the time of the interview. Controls include mother literacy, mother's educational level, religion (indicator for Christianity), place of residence (indicator for urban), and wealth status (indicator for poor). * Significant at 10% level; ** at 5%; *** at 1%.

We complement the analysis by examining post-birth selection through infant mortality. We do so by regressing the probability of a child dying at various months after birth conditional on being born alive on our main variable of interest (in-utero exposure to heat waves). Table 2.7 reports the results on heat waves exposure and infant mortality. Although the coefficient estimates are all positive, we do no find any clear evidence that heat wave exposure *during gestation* affects mortality of children after they are born. Our findings are consistent with the evidence from SSA reported in Wilde et al. (2017), which however focuses on heat shocks at conception

[21] This results contrasts with Wilde et al. (2017)that find effects on the sex ratio and *negative* effects of heat shocks on the probability to terminate a pregnancy. In addition to using a different sample and a different indicator of heat waves, Wilde et al. do not include district linear trends. All these factors may explain the different results.

and not during gestation.[22]

Table 2.7: Heat waves and child mortality at various ages

	(1) Death 0-12	(2) Death 13-24	(3) Death 25-36	(4) Death 37-48	(5) Death 49-60
Exposure	0.628	0.387	0.239	0.350	0.385
	(0.399)	(0.343)	(0.312)	(0.324)	(0.386)
Observations	76796	147565	218010	289619	353428
R^2	0.167	0.105	0.082	0.070	0.062
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes
District linear trends	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the district level are in parentheses. In column (1), the sample contains children who were born less than 12 months before the interview and the dependent variable is an indicator variable which takes a value of one if the child is dead at the time of the interview. In column (2), the sample contains children who were born 12 to 24 months before the interview and the dependent variable is an indicator variable which takes a value of one if the child died between ages 12 and 24 months. A similar procedure is followed for the month ranges 25-36, 37-48 and 49-60 in columns (3), (4) and (5), respectively. Control variables include mother literacy, education level of mother, place of residence (an indicator for urban), religion (an indicator for Christianity), wealth status (an indicator for poor) and child's sex. * Significant at 10% level; ** at 5%; *** at 1%.

2.5.2 *Selection on fertility choices*

Heat waves may influence women's proclivity to become pregnant. Intense heat stress may reduce sexual activity for both men and women (Lam and Miron, 1996; Barreca et al., 2014; Wilde et al., 2017) and could be unevenly large for individuals who do not have the resources to invest in domestic appliances (e.g., air conditioning) needed to attenuate its effect. Accordingly, women with lower socioeconomic status may be more likely to experience sharp declines in fertility in the immediate periods after intense and protracted heat waves. This could imply a correlation between parental characteristics and extreme temperatures during pregnancy. Another possible theoretical explanation of the link between heat waves and parental characteristics is the effect of the former on the physiological capability of parents

[22] However, using 50 retrospective fertility surveys from 28 countries and global meteorological reanalysis data for 1957-2002, Kudamatsu et al. (2016) report that infants born in unusually long spells of malarious weather in low malaria transmission areas have a higher mortality.

to procreate. For instance, extreme heat waves could reduce the quality of male sperms or affect the ovulation cycle of females negatively.

Sexual activity

We test whether sexual activity during periods of extreme heat waves is higher or lower, and if it is associated with socioeconomic status. We create an indicator variable which is equal to one if a woman was sexually active in the *four weeks preceding the survey* in our data. We then regress this variable on heat waves, socioeconomic characteristics of the mother and their interaction. We use two indicators of socioeconomic status for women: an indicator variable for whether a woman is in the lowest quintile of the wealth distribution, and another indicator for women with no more than primary education. We estimate the model by including district-survey month FEs and country-survey year FEs, and district linear trends. We report the results in Table 2.8. The estimates suggest that sexual activity increases with exposure to heat waves in the month preceding the survey, but there is no significant interaction with mother's socio-economic status (columns (4) and (5)). Thus, in this section, we do not find any evidence of either positive or negative selection into conception during heat waves.

Parental characteristics

A further way of testing the interplay between selection into parenthood and exposure to heat waves is to run models in which indicator variables for parental characteristics are regressed on the indicator for heat wave exposure.

Table 2.9 shows some evidence of *negative selection* into parenthood according to heat waves exposure during pregnancy. Panel A indeed suggests that compared to periods in which heat waves are not experienced, mothers are more likely to be low educated (around 4.7 percentage points) and poor (around 4.5 percentage points).

Table 2.8: Heat waves and sexual activity

	(1) Sexually active	(2) Sexually active	(3) Sexually active	(4) Sexually active	(5) Sexually active
Exposure	1.103**	1.105**	1.118**	0.525	1.230**
	(0.547)	(0.548)	(0.546)	(0.970)	(0.588)
Primary		-0.172		-0.316	
		(0.920)		(0.863)	
Poor			-3.302***		-3.243***
			(0.680)		(0.660)
Exposure×Primary				0.764	
				(0.994)	
Exposure×Poor					-0.292
					(0.891)
Observations	265810	265810	265810	265810	265810
R^2	0.260	0.260	0.260	0.260	0.260
District-Month of Survey FE	Yes	Yes	Yes	Yes	Yes
Country-Year of Survey FE	Yes	Yes	Yes	Yes	Yes
District linear trends	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the district level are in parentheses. The dependent variable is an indicator variable which takes the value one if the respondent was sexually active in the four weeks preceding the interview. The value is multiplied by 100 so the coefficients represent percentage points changes. Exposure is an indicator which is equal to one if a child was exposed to a heat wave shock in the four weeks preceding the survey and zero otherwise. 'Primary' indicates that the mother has no more than primary education. 'Poor' indicates that the mother belongs to the lowest quintile of the wealth distribution. * Significant at 10% ; ** at 5%; *** at 1% level.

Since this result may be related to the selection determined by experiencing heat waves around the time of conception, e.g. highly educated women may be more likely to use contraception during a heat wave, in Panel B we focus on heat waves excluding the first month of gestation. However, the results are very similar.

This evidence could be explained in two ways: 1) high socio-economic status mothers may be more likely to avoid heat waves, for instance by migrating towards districts not affected by heat shocks. This would produce a lower percentage of highly educated women to be classified as being 'exposed' based on the district of child birth in periods in which that district is hit by a heat wave (some evidence on selective migration is reported in the next section); 2) high socio-economic status mothers may more likely to interrupt a pregnancy (either voluntary or involuntary) when they are affected by a heat shock. However, the results reported in Table 2.5 are not consistent with this second explanation.

Table 2.9: Heat waves and parental characteristics

	(1) Primary	(2) Poor
Panel A: Whole pregnancy period		
Exposure	4.700***	4.509**
	(1.267)	(1.378)
Observations	260203	260203
R^2	0.264	0.258
Panel B: Shocks in months 2-9 after conception		
Exposure	4.147***	3.878**
	(1.207)	(1.273)
Observations	260203	260203
R^2	0.264	0.258
Panel C: By bins of heat wave in the month of conception		
$(0,3]$	1.392	0.909
	(0.904)	(1.046)
$(3,6]$	1.312	1.821**
	(0.722)	(0.821)
	(0.615)	(0.705)
$(6,9]$	1.449**	1.623
	(0.617)	(0.833)
> 9	0.280	0.980
	(1.185)	(1.451)
Observations	260203	260203
R^2	0.264	0.258
District-Month of Birth FE	Yes	Yes
Country-Year of Birth FE	Yes	Yes
District linear trend	Yes	Yes

Notes: Robust standard errors clustered at the district level are in parentheses. Primary is indicator for at most primary education of the mother. Poor indicates that the mother belongs to the lowest quintile of the wealth distribution. All the coefficients in Columns (1) and (2) are scaled by 100 to make them more readable. * Significant at 10% ; ** at 5%; *** at 1% level.

2.5.3 *Migration and measurement error*

Another potential source of bias in our estimates may be due to selective migration, that is, migration after conception but before birth. If a household migrates to another district after child conception but before child birth, it would be inaccurate to assume that the current district of residence is the same as the district during pregnancy. Thus, households who move from 'harsh' weather districts to districts with a more 'favourable' climate are more likely to be healthier than their counterparts who are exposed longer to heat waves and as a consequence the estimated coefficients may suffer from measurement error bias. Stated differently, if mothers move between countries and/or districts, assigning heat wave exposure to children born to such mothers might be incorrect. We therefore define non-migrant mothers those who report having lived in the district at least two years before the child is born ("stayers", hereafter). This ensures that mothers are not migrating while they are pregnant and hence we can correctly assign each child with its corresponding exposure to heat waves. For ease of comparison with the baseline results, the baseline results are reported in Panel A of Table 2.10. Panel B presents the analysis for "stayers". The signs of the coefficients remain unchanged and also the magnitudes are quite similar. They are slightly larger in line with the idea that the baseline estimates may suffer from a downward bias.

2.6 Robustness checks

We perform a sensitivity analysis by controlling for precipitation shocks during pregnancy and after birth in order to check for the possibility that the results are driven by precipitation rather than heat waves. The estimated coefficients on heat waves in panel B of Table 2.10 are not sensitive to the inclusion of precipitation shocks, in terms of both statistical significance and magnitude.

We further check the sensitivity of our results to splitting in utero exposure to heat shocks in the first month, second to eighth month, and in the ninth month

Table 2.10: Selective migration

Dependent variable:	(1) WAZ	(2) WAZ< -2	(3) WAZ< -3	(4) HAZ	(5) HAZ< -2	(6) HAZ< -3	(7) Anemia	(8) BW	(9) LBW
Panel A: Baseline									
Exposure	-0.039**	1.006*	1.224***	-0.021	0.593	1.457**	-1.775	-10.108	0.890*
	(0.020)	(0.586)	(0.443)	(0.027)	(0.695)	(0.582)	(1.087)	(17.932)	(0.455)
Panel B: Including only 'stayers' (i.e. non-migrants)									
Exposure	-0.043*	1.016	1.302**	-0.015	0.644	1.510**	-1.408	-17.724	0.937*
	(0.023)	(0.702)	(0.533)	(0.028)	(0.781)	(0.637)	(1.032)	(20.601)	(0.492)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.162	0.131	0.219	0.186	0.157	0.249	0.189	0.142
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns 2, 3, 5, 6, 7 and 9 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban), maternal age at birth. Exposure is an aggregate indicator which is equal to one if a child was exposure to a heat wave shock in either of the nine months preceding birth. All regressions include district specific time trends. BW is birth weight and LBW is low birth weight. * Significant at 10% level; ** at 5%; *** at 1%.

Table 2.11: Robustness checks: Precipitations, splitting exposure by month and controlling for neigbouring months around gestation

Dependent variable:	(1) WAZ	(2) WAZ< -2	(3) WAZ< -3	(4) HAZ	(5) HAZ< -2	(6) HAZ< -3	(7) Anemia	(8) BW	(9) LBW
Panel A: Baseline									
Exposure	-0.039**	1.006*	1.224***	-0.021	0.593	1.457**	-1.775	-10.108	0.890*
	(0.020)	(0.586)	(0.443)	(0.027)	(0.695)	(0.582)	(1.087)	(17.932)	(0.455)
Panel B: Controlling for precipitation									
Exposure	-0.040**	1.019*	1.225***	-0.021	0.612	1.481**	-1.867*	-10.345	0.906*
	(0.020)	(0.593)	(0.451)	(0.027)	(0.699)	(0.590)	(1.107)	(17.913)	(0.461)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.162	0.131	0.219	0.186	0.157	0.249	0.189	0.142
Panel C: Decomposing exposure by month									
1 month before gestation	0.006	0.008	0.056	0.009	0.002	0.108	-0.206	0.989	0.069
	(0.008)	(0.249)	(0.139)	(0.010)	(0.283)	(0.246)	(0.525)	(5.986)	(0.240)
Month 1	-0.050**	1.267*	0.842*	-0.025	0.406	0.776	0.519	-21.897***	0.888***
	(0.021)	(0.651)	(0.449)	(0.020)	(0.508)	(0.498)	(0.563)	(8.318)	(0.277)
Months 2-8	-0.022	0.613	0.654**	-0.041*	1.076**	1.439***	-1.455*	10.660	0.133
	(0.018)	(0.541)	(0.321)	(0.021)	(0.537)	(0.405)	(0.875)	(13.106)	(0.356)
Month 9	0.002	-0.095	-0.044	0.004	0.225	-0.186	-0.306	-6.333	-0.037
	(0.008)	(0.214)	(0.148)	(0.010)	(0.243)	(0.224)	(0.454)	(5.609)	(0.241)
Month 10	0.013	-0.254	-0.230	0.035***	-0.637**	-0.558**	-0.350	-3.870	-0.139
	(0.008)	(0.240)	(0.199)	(0.013)	(0.321)	(0.227)	(0.474)	(5.696)	(0.238)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.162	0.131	0.219	0.186	0.157	0.249	0.190	0.142
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns 2, 3, 5, 6 and 9 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. Exposure is an aggregate indicator which is equal to one if a child was exposure to a heat wave shock in either of the nine months preceding birth. BW is birth weight and LBW is low birth weight. * Significant at 10% level; ** at 5%; *** at 1%.

of gestation. We also control for shocks in the month before pregnancy and a possible tenth month of gestation. The idea is that, shocks in the 'central' months of gestation should be less affected by measurement error related to pre-term or post-term births, contrary to the initial and final months. This check also addresses the concern that our 'exposure' variable is based on month and not on day of birth, which is not available in the data. So even in the case of a 'regular' (i.e., nine months) pregnancy, the month before conception and the month after delivery, based on the period of pregnancy computed from the survey, may be associated with child health outcomes. The results are reported in panel C of Table 2.11. The estimated effects for the 'central months' are robust to the inclusion of of the first and last month of gestation, and the first month before conception and after birth. The first month of gestation appears to be especially important for birth weight (−22 grams) and the probability of low birth weight (0.89 percentage point). Unlike in our baseline estimates in Table 2.2, both effects are very precisely estimated and statistically significant at the 1% level. This points to the fact that heat waves around conception may be more important for child birth weight than shocks experienced in other months of gestation. Our results are in line wit those in Grace et al. (2015) that documents a negative effect of heat shocks (number of days above 100F) on birth weight, especially concentrated in the first and second trimester of gestation and in countries in the Agropastoralist, Pastoralist, Urban, Fishers and Irrigated category, while no significant effect is found for the Agriculturalist countries' group.[23]. On the contrary, both Deschênes et al. (2009) in the US context and Andalón et al. (2016) for Colombia find that the negative effects of hot temperatures on birth weight are concentrated in the second and third trimesters of gestation.

Curiously, exposure to heat waves in the month after the normal duration of gestation turns out to be *positively* related with HAZ.

[23]Country groups were defined by the authors on the basis of the 'livelihood zone'.

2.7 Heterogeneous effects

In this section, we present evidence of heterogeneous effects of exposure to heat shocks in utero on child health outcomes. Tables 2.12 and 2.13 show the differential effects of heat shocks on health outcomes when our sample is apportioned according to the mother's characteristics (education and wealth of the household) and the sex of the child, respectively. In Table 2.12, the estimated effect of heat shock is stronger for mothers with no more than primary education for all the outcomes. With the exception of HAZ, stunting and anemia, all coefficients are statistically significant in the sample of mothers with no more than primary education. On the other hand, heat waves exhibit no significant effect on the sample of mothers with higher education (i.e., at least secondary) on all the outcomes, although the signs are in line with the theoretical predictions. Although the coefficients are not estimated precisely enough to make them statistically different (see the t-statistics in the table), these findings points to the fact that highly educated mothers may be more likely to adopt coping strategies that allow them to attenuate or avoid the negative effects of heat waves on children's health.

Panel B reports the heterogeneous effects according to the wealth quintile to which the mother belongs. In this case results are more mixed. Indeed, children of poor mothers seems to be more likely to be underweight or severely stunted when hit by heat waves, while children or rich mothers are more likely to be born underweight.

Effect heterogeneity by child gender in analyzed in Table 2.13. The point estimates are larger for boys in terms of both economic magnitude and statistical significance. For instance, exposure to heat waves significantly reduces WAZ, increases the probability of being underweight and severely underweight, and raises the likelihood of being stunted and severely stunted for boys. These findings are consistent with the results on the absence of fetal selection (Section 2.5.1) and hint to a *scarring effect*. Indeed, should only the strongest male fetuses survive to heat waves, we should expect much smaller gender differences. The same result suggest

Table 2.12: Heterogeneous effect of heat shocks by maternal characteristics: sample split estimates

Dependent variable:	(1) WAZ	(2) WAZ< -2	(3) WAZ< -3	(4) HAZ	(5) HAZ< -2	(6) HAZ< -3	(7) Anemia	(8) BW	(9) LBW
Panel A: At most primary education									
Exposure	-0.048**	1.273*	1.205***	-0.022	1.205	1.757***	-1.615	-25.496	1.142**
	(0.021)	(0.660)	(0.434)	(0.029)	(0.787)	(0.614)	(1.259)	(17.915)	(0.562)
Panel B: At least secondary education									
Exposure	-0.004	0.636	0.360	0.013	0.222	0.372	0.523	4.811	-0.562
	(0.029)	(0.902)	(0.497)	(0.037)	(1.097)	(0.815)	(2.241)	(18.648)	(0.703)
t-statistics Primary=Secondary	1.050	0.050	0.034	0.002	0.052	0.267	2.001	0.304	3.739
p-value	0.307	0.824	0.854	0.969	0.820	0.606	0.159	0.582	0.054
Panel C: Poor									
Exposure	-0.062*	1.886*	0.982	0.007	1.679	1.837**	-0.435	-11.720	-0.245
	(0.032)	(1.055)	(0.734)	(0.048)	(1.133)	(0.909)	(1.295)	(34.800)	(1.105)
Panel D: Rich									
Exposure	-0.026	0.814	0.937**	-0.013	0.492	1.068*	-3.130**	-17.233	0.859*
	(0.020)	(0.616)	(0.376)	(0.024)	(0.715)	(0.553)	(1.468)	(13.544)	(0.457)
t-statistics Rich=Poor	1.606	0.228	0.297	0.659	0.206	0.010	0.409	0.061	3.122
p-value	0.206	0.633	0.586	0.418	0.650	0.921	0.524	0.805	0.079
District-Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Binary outcomes are multiplied by 100, so that coefficients are in units of percentage points. Control variables include mother literacy, birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. Exposure is an aggregate indicator which is equal to one if a child was exposed to a heat wave shock in either of the nine months preceding birth. BW is birth weight and LBW is low birth weight. t-statistics tests the equality between the different categories. * Significant at 10% level; ** at 5%; *** at 1%.

Table 2.13: Heterogeneous effect of heat shocks by child gender: sample split estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	WAZ	WAZ< -2	WAZ< -3	HAZ	HAZ< -2	HAZ< -3	Anemia	BW	LBW
				Panel A: Girl					
Exposure	-0.023	0.462	0.644	0.012	-0.130	0.517	-1.112	-4.620	0.616
	(0.024)	(0.755)	(0.418)	(0.029)	(0.887)	(0.651)	(1.204)	(19.709)	(0.682)
				Panel B: Boy					
Exposure	-0.038*	1.387**	1.117**	-0.016	1.612**	1.798***	-1.515	-15.651	0.122
	(0.023)	(0.681)	(0.495)	(0.029)	(0.800)	(0.573)	(1.488)	(17.159)	(0.638)
t-statistics Girl=Boy	1.332	0.547	1.308	0.598	1.057	0.764	0.086	0.407	0.138
p-value	0.250	0.460	0.254	0.440	0.305	0.383	0.769	0.524	0.710
District-Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Binary outcomes are multiplied by 100, so that coefficients are in units of percentage points. Control variables include mother literacy, birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. Exposure is an aggregate indicator which is equal to one if a child was exposed to a heat wave shock in either of the nine months preceding birth. BW is birth weight and LBW is low birth weight. t-statistics tests the equality between the different categories. * Significant at 10% level; ** at 5%; *** at 1%.

that parents do not engage in differential compensating behavior for boys and girls, or such differential investments are not large enough to cancel the effects of heat waves. However, also in this case, the null hypothesis of equal coefficients by gender cannot be rejected.

2.8 Mitigation

In this section, we investigate whether there is evidence in our data that households in the SSA context can adopt strategies to *mitigate* the negative effects of heat waves on child health. This analysis also indirectly provides hints on the mechanisms that may be behind the health damaging effect of heat waves. Indeed, although heat waves are likely to directly impact on individual health through heat stress, some effects may be mediated by 'macro' factors such as food availability or the spread of infectious diseases.

2.8.1 *Infectious diseases: access to improved water and saniation*

As one potential mediating factor for the negative effects of heat waves may be the prevalence of infectious diseases, we test whether the availability of improved water and sanitation mitigate the negative effects of heat waves. We classify water sources as improved water following the World Bank definition: piped water into dwelling, piped into yard/plot, piped to neighbour's house, borehole with pump, protected well, protected spring and bottled water. We also define improved sanitation as a flush toilet, septic tank, piped sewer system, flush to open pit, compositing toilet, a ventilated improved pit latrine, a pit latrine with a slab. A household is classified as having improved toilet for instance if the toilet is used only by members of one household and if the facility used by the household separates waste from human contact (WHO and UNICEF, 2014). Limited access to improved water and sanitation potentially exposes individuals to fatal diseases. We built an aggregate indicator for whether the household has access to improved water or improved sanitation (WS)

and interact it with exposure to heat waves. The results are reported in Table 2.14 from which we find some limited evidence of a protecting effect of improved water and sanitation in terms of reduced WAZ and the probability of being stunted.

2.8.2 *Improved housing*

We test another potential mitigating strategy by assessing the effect of heat wave exposure interacted with having improved housing and building materials. In our data, housing characteristics are classified as natural, rudimentary and finished. In the analysis, we define natural and rudimentary roofing and wall types as 'unimproved', while only natural flooring is considered as 'unimproved'. Based on these categorizations, we define improved housing as having improved floor, wall, and roof construction, while unimproved housing is a composite of unimproved floor, wall and roof construction (Florey and Taylor, 2016).[24] The results are presented in Table 2.15 and show a significant interaction effect on the probability of low birth weight. Namely, access to improved housing appears to cancel the positive effect of heat waves on the probability that the exposed child is born underweight. Hence, heat stress may be particularly damaging in terms of child birth weight.

2.8.3 *Place of residence*

Food scarcity is an important determinant of both short- and long-term children's health outcomes (Miller, 2017), and heat waves may negatively affect food availability (Zampieri et al., 2017). Mulmi et al. (2016*b*), for instance, reports evidence from Nepal that access to food markets may attenuate the negative effects of climate shocks. The underlying idea is that heat waves may negatively impact food security especially of households that consume out of their own production, or of very local production. In this respect, living in cities (i.e. urban residence) allows households

[24]Improved floor is categorized as having a rudimentary (i.e., tablets, mat, adobe) or finished floor (i.e., parquet, carpet, cement, bricks). Improved wall is categorized as having a finished wall (i.e., covered adobe, bricks, cement blocks, wood planks). Improved roof is categorized as having a finished roof (i.e., metal, wood, ceramic tiles, cement, roofing shingles).

Table 2.14: Mitigation: access to improved water and sanitation

Dependent variable:	(1) WAZ	(2) WAZ< -2	(3) WAZ< -3	(4) HAZ	(5) HAZ< -2	(6) HAZ< -3	(7) Anemia	(8) BW	(9) LBW
Exposure	-0.036*	0.699	1.041**	-0.010	0.479	1.229**	-1.715	-10.313	0.888*
	(0.019)	(0.591)	(0.435)	(0.025)	(0.689)	(0.565)	(1.110)	(18.164)	(0.487)
Improved Water (W)	0.050***	-1.302***	-0.544***	0.087***	-2.448***	-1.741***	-1.859**	-16.740*	-0.175
	(0.013)	(0.398)	(0.188)	(0.017)	(0.417)	(0.351)	(0.715)	(9.122)	(0.274)
Improved Sanitation (S)	0.095***	-2.145***	-0.609*	0.132***	-2.919***	-1.464***	-3.248***	7.357	-0.188
	(0.016)	(0.531)	(0.310)	(0.023)	(0.653)	(0.528)	(0.878)	(11.357)	(0.414)
Exposure $\times$ WS	0.053***	-0.124	0.062	0.023	-1.295**	-0.275	-0.066	11.669	-0.507
	(0.016)	(0.479)	(0.249)	(0.023)	(0.517)	(0.436)	(0.860)	(12.477)	(0.474)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.161	0.130	0.222	0.188	0.158	0.250	0.197	0.144
District-Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns other than 1 and 8 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. BW is birth weight and LBW is low birth weight. Exposure is an aggregate indicator which is one if a child was exposed to heat wave shocks in any of the nine months preceding birth. WS is an aggregate indicator which is equal to 1 if the household has access to improved water or sanitation and 0 otherwise. * Significant at 10% level; ** at 5%; *** at 1%.

Table 2.15: Mitigation: access to improved housing

Dependent variable:	WAZ	WAZ< -2	WAZ< -3	HAZ	HAZ< -2	HAZ< -3	Anemia	BW	LBW
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure	-0.025	1.118	1.034*	0.022	0.520	0.949	-1.316	-22.706	1.445**
	(0.029)	(0.894)	(0.582)	(0.034)	(1.018)	(0.847)	(1.402)	(31.414)	(0.616)
Improved housing	0.214***	-4.943***	-2.182***	0.296***	-6.455***	-3.450***	-4.138**	-30.123	1.242
	(0.048)	(1.090)	(0.669)	(0.051)	(1.534)	(1.039)	(2.025)	(33.710)	(0.783)
Exposure×Improved housing	0.019	-0.067	-0.299	-0.040	-0.781	-1.056	0.614	44.641	-2.226***
	(0.050)	(1.143)	(0.722)	(0.048)	(1.415)	(1.058)	(2.003)	(33.027)	(0.810)
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	187,218	187,218	187,218	187,218	187,218	187,218	78,442	90,232	90,232
R-squared	0.212	0.164	0.140	0.216	0.189	0.159	0.266	0.210	0.161

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns other than 1 and 8 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. BW is birth weight and LBW is low birth weight. 'Improved housing' is an indicator equal to 1 if the house has improved floor, wall and roof construction. * Significant at 10% level; ** at 5%; *** at 1%.

to diversify food sources, through access to food markets, and may help mitigate the negative effects of heat waves. In our data we do not have good indicators of food availability during gestation and we use an interaction term between heat waves and a dichotomous indicator for urban residence to test for heterogeneous effects of heat shocks by urban/rural residence.[25]

The results are reported in panel A of Table 2.16. Although it is evident that children of urban households fare better in terms of health, the table does not show any evidence of differential effects of heat waves by households' urban/rural residence status. We interpret this finding as urban residence not offering more opportunity of mitigation of the negative effects of heat waves on child health than rural residence, at last in SSA, and as indirect evidence of food insecurity not being the main mediating factor for these negative effects.[26]

2.8.4 *Electricity and refrigeration*

Access to electricity and refrigeration may provide additional mitigation strategies. Electricity may allow for access to air conditioning while refrigeration allows to preserve foods and beverages, and reduce the risk of being exposed to infectious diseases. We define electricity as a composite indicator which takes the value of one if the household has access to electricity and has a refrigerator and zero otherwise.

Panel B of Table 2.16 reports that, having access to electricity and a refrigerator is positively associated with better children's health outcomes, however their availability does not seem to significantly attenuate the negative effects of heat waves. This may be partly explained by the limited access to electricity in the SSA sample (23% on average).

[25] We also carried out other tests of the food insecurity channel, by estimating differential effects of heat waves according to the according to the agricultural season of *conception*, as in Wilde et al. (2017) but we generally found similar effects for the rainy and the dry season. Moreover, in our case, it is much harder to proxy for food insecurity since we are interested in the effect of heat wave exposure for the whole gestation (not only around conception), given the delays that characterize the time in which the shock materializes and the time in which its effects are observed in food markets. These results are available upon request.

[26] Consistent with this interpretation, Zampieri et al. (2017) reports that, in contrast to common perception, water excess affects wheat production more than drought in several countries.

Table 2.16: Mitigation: Heat waves urban residence and access to electricity and refrigeration

Dependent variable:	WAZ	WAZ< -2	WAZ< -3	HAZ	HAZ< -2	HAZ< -3	Anemia	BW	LBW
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Heat wave interaction with urban place of residence									
Exposure	-0.034	0.564	1.040**	-0.005	0.310	1.433**	-1.174	-12.794	0.644
	(0.022)	(0.662)	(0.490)	(0.030)	(0.797)	(0.690)	(1.049)	(21.481)	(0.628)
Urban	0.274***	-6.352***	-2.305***	0.387***	-9.405***	-5.202***	-1.737	-3.653	-0.817
	(0.037)	(1.055)	(0.622)	(0.042)	(1.158)	(0.835)	(2.981)	(24.457)	(0.759)
Exposure × urban	-0.016	1.517	0.636	-0.046	0.825	0.006	-1.990	8.857	0.471
	(0.035)	(1.050)	(0.614)	(0.046)	(1.164)	(0.845)	(2.806)	(23.889)	(0.795)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.214	0.161	0.130	0.223	0.188	0.158	0.249	0.197	0.144
Panel B: Heat wave interaction with electricity access and refrigerator									
Exposure	-0.039*	0.863	1.190***	-0.014	0.456	1.439**	-2.025*	-11.794	1.033**
	(0.020)	(0.606)	(0.442)	(0.029)	(0.711)	(0.602)	(1.122)	(18.569)	(0.465)
Electricity	0.304***	-5.372***	-1.234**	0.419***	-9.605***	-3.448***	-8.392***	6.705	0.710
	(0.065)	(1.160)	(0.574)	(0.075)	(1.701)	(0.911)	(2.314)	(31.084)	(0.970)
Exposure×electricity	0.012	1.427	0.353	-0.038	0.712	-0.190	4.170*	18.242	-1.170
	(0.068)	(1.353)	(0.609)	(0.081)	(1.796)	(0.959)	(2.425)	(30.334)	(1.001)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.214	0.161	0.130	0.223	0.188	0.158	0.249	0.197	0.144
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns other than 1 and 8 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. BW is birth weight and LBW is low birth weight. 'Electricity' is a composite indicator which is equal to 1 if the household has access to electricity and refrigerator and 0 otherwise. 'Urban' is an indicator which is equal to 1 if the household resides in an urban area and 0 otherwise. * Significant at 10% level; ** at 5%; *** at 1%.

2.8.5 *Health seeking behaviour during pregnancy*

Carrillo et al. (2016) show that in Colombia mothers that are affected during gestation by heat shocks engage more intensively in health-seeking behaviour for their children. We test the same hypothesis in the SSA context.

We test the effect of a child's exposure to heat waves while *in-utero* on the number of antenatal visits, institutional delivery and medical assistance at child birth. We define medical assistance during delivery as an indicator which is equal to 1 if the assistance came from a doctor, nurse or community health assistant and 0 otherwise. Regarding, institutional delivery, we define it an indicator which is equal to 1 when delivery took place in hospital or any approved medical centre and 0 otherwise. Antenatal check-ups is the number of visits to the hospital for medical care during pregnancy Results are presented in Table 2.17 and indicate that exposure to *in-utero* shocks *decreases* the probability of institutional delivery by 0.3 percentage point. The effect on the probability of medical assistance at child birth is about the same. Exposure also decreases the number of prenatal visits by approximately 0.8 times. Hence, unlike Carrillo et al. (2016), we not find evidence of compensatory behaviour while our results are consistent with those in Molina and Saldarriaga (2017) which reports negative effects of heat shocks on the probability of medical assistance at child birth in the Andean region.

Table 2.17: Mechanisms: Health care during pregnancy

Dependent variable	(1) Prenatal check-ups	(2) Institutional delivery	(3) Medical assistance at birth
Exposure in utero	-0.827***	-0.029**	-0.030***
	(0.304)	(0.011)	(0.011)
Observations	183,625	260,203	260,203
R-squared	0.207	0.389	0.401
District-month of birth FE	Yes	Yes	Yes
Country-year of birth FE	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. Exposure is an indicator which is equal to 1 if a child experience heat wave shocks in any of the months during gestation and 0 otherwise. The dependent variable in column (1) is the number of prenatal check-ups. Institutional delivery is an indicator if a mother delivered in a formal health institutional. Medical assistance is an indicator equal to 1 if mother received assistance from a professional personal. The sample size in column (1) is smaller than the sample size in other columns because the number of pre-natal check-ups is only available for the lastborn child in the DHS datasets. Control variables include sex of child, mother literacy, religion (indicator for Christianity), place of residence (indicator for urban), maternal age at birth and survey year fixed effect. * Significant at 10% level; ** at 5%; *** at 1%.

2.8.6 *Selective migration*

We do not have information on geographical mobility of mothers during pregnancy, but only after child birth. Hence, in this section we report some evidence on the latter. The idea is that if we find evidence of post-birth mobility away from regions affected by heat shocks, then the same may happen during pregnancy.

To understand whether treated mothers are more likely to migrate after child birth if they were hit by a shock during pregnancy, we estimate a model in which the dependent variable is an indicator which is 1 if the mother currently resides in a different place with respect to that of child birth, and 0 otherwise. The results reported in Table 2.18 indicate that exposed mothers are *less* likely to be in a different location than that of delivery. In column (1) exposure is included without interactions, while columns (2) and (3) include interactions with mother's education and wealth status. The results in column (1) suggest that mothers hit by a

Table 2.18: Effect of heat wave exposure on the probability of mother's migration

	(1) Mover	(2) Mover	(3) Mover	(4) Mover	(5) Mover
Exposure inut2	-0.020***	-0.020***	-0.019***	-0.023***	-0.019
	(0.007)	(0.007)	(0.007)	(0.007)	(0.012)
Poor		-0.024***		-0.036***	
		(0.005)		(0.011)	
Primary			-0.027***		-0.027
			(0.005)		(0.016)
Exposure × Poor				0.0128	
				(0.011)	
Exposure × Primary					-0.0003
					(0.015)
Observations	260203	260203	260203	260203	260203
R^2	0.503	0.503	0.503	0.503	0.503
District-month of birth FE	Yes	Yes	Yes	Yes	Yes
Country-year of birth FE	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level are in parentheses. The dependent variable is an indicator for whether a mother residing in a different place with respect to the birth of the child. Controls includes child's sex, age of child (in months), religion (an indicator for Christianity) and place of residence (an indicator for urban residence). 'Primary' indicates that the mother has no more than primary education. 'Poor' indicates that the mother belongs to the lowest quintile of the wealth distribution. * Significant at 10% level; ** at 5%; *** at 1%.

heat wave during pregnancy are 2 percentage points less likely to move. Columns (2)-(3) demonstrate that low socio-economic status mothers are less likely to move. Columns (4) and (5) that there is not differential effect of heat waves on mobility by either mother's education or wealth status. All in all, we do not find any evidence of selective migration associated with heat waves. The probability of changes district actually falls around a heat wave. This is consistent with geographical mobility not being a viable coping in the SSA context. Yet this evidence is at odds with that in Section 2.5.2 that children's of low socio-economic status mothers are disproportionally affected by heat waves, although as we mentioned, owing of lack of data we are not able to provide evidence on mobility during pregnancy.

2.9 Concluding remarks

Climate change is impacting on several aspects of human activity and it is likely to affect human health. This paper is an attempt to add to the extant literature on the adverse effects of increasing temperatures on health conditions of children in Africa. We use several geo-referenced waves of the Demographic and Health Surveys for 24 sub-Saharan countries over the period 1990 to 2015 merged with gridded monthly data on the presence of heat waves and their magnitudes since the 1980s to investigate the effects of heat shocks during gestation on children's health outcomes at birth and in their first five years of age.

Our analysis demonstrates the negative effects of heat shocks experienced in utero on children's birth and early childhood outcomes. In-utero exposure to heat waves increases the probability of low birth weight by 0.9 percentage points. But the effects of heat shocks are not limited to the short term, i.e. around child birth, but they persist in infancy. Indeed, children who are exposed to heat waves in utero have a 0.039 lower weight-for-age, and higher probabilities of being underweight, severely underweight, and severely stunted (of +1, +1.2, +1.5 percentage points, respectively) and the probability of being severaly stunted by 1.5 percentage points. The analysis of effect heterogeneity indicates stronger effects for boys than for daughters, consistent with the evidence that male fetuses are less resistant to environmental shocks (*scarring effect*), and stronger effects for low educated or less wealthy mothers, consistent with higher socio-economic status allowing for better coping mechanisms.

When we seek to assess the relevance of a number of potential coping mechanisms in the SSA context, we find evidence of a protective role of access to improved housing materials on short term outcomes (i.e. low birth weight) and of access to improved water and sanitation on medium-term outcomes, namely WAZ and the probability of being stunted.

Our study contributes to the extant literature in a number of ways. First, by focusing on SSA we recognize the importance of a region which is both susceptible to extreme temperatures and lacks adequate adaptation strategies. Second, we employ a number of health outcomes (both short- and long-term) that might be affected by heat wave exposure while past studies concentrate on only one or few outcomes, such as mortality or birth weight. Third, we adopt in our empirical analysis a new indicator of heat waves (Russo et al., 2015), which has been empirically validated (e.g. by crossing it with media coverage of heat episodes), overcomes some of the limitations of indicators that have been used in the past literature. Finally, we report a thorough analysis of the potential mitigating strategies that may help reduce the negative effects of heat waves on babies' and infants' health in Africa.

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.1 Appendix: Additional tables

Table A1: List of surveys in the DHS

Country	Waves	Number of regions
Angola	2015	18
Benin	1996, 2001, 2011-12	12
Burkina Faso	1993, 1999, 2003, 2010	3
Burundi	2010,	5
Cameroon	1991, 2004, 2011	10
Chad	2014	20
Congo D.R	2007, 2013-14	11
Gabon	2012	8
Ghana	1993, 1998, 2003, 2008, 2014	10
Kenya	2003, 2008-09, 2014	8
Madagascar	1997	6
Malawi	2000, 2010, 2015	8
Mali	1996, 2001, 2006, 2012	9
Mozambique	2011	11
Namibia	2000, 2006-07, 2013	13
Niger	1992, 1998	8
Nigeria	1990, 2003, 2008, 2013	6
Rwanda	2005, 2014-15	5
Swaziland	2006-07	4
Tanzania	1999, 2010, 2015-16	28
Togo	1998, 2013-14	5
Uganda	2000-01, 2006, 2011	10
Zambia	2007, 2013-14	10
Zimbabwe	1999, 2005-06, 2010-11, 2015	10
Total	60	238

Notes: This table reports the list of country and DHS waves included in our empirical analysis.

Table A2: Effect of in utero heat wave exposure on child health outcomes, by alternative HWMId categorization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	WAZ	WAZ< -2	WAZ< -3	HAZ	HAZ< -2	HAZ< -3	Anemia	BW	LBW
Heat wave index range:									
$(0, 3]$	0.014	-0.140	0.260	0.001	-0.022	0.361	-0.505	-5.842	0.490
	(0.009)	(0.267)	(0.172)	(0.012)	(0.318)	(0.258)	(0.611)	(7.176)	(0.305)
> 3	-0.029***	0.668***	0.270**	-0.006	0.336	0.437**	-0.882**	-3.599	0.167
	(0.007)	(0.199)	(0.128)	(0.009)	(0.237)	(0.192)	(0.399)	(4.963)	(0.211)
District-Month of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.212	0.162	0.131	0.219	0.186	0.157	0.249	0.189	0.142
Mean of dependent variable	-1.04	21.62%	7.23%	-1.476	38.42%	17.63%	62.69%	3220	7.83%

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns 2, 3, 5, 6, 7 and 9 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include sex of child, mother literacy, education level of mother, child's birth order, age of child in months, religion (indicator for Christianity), place of residence (indicator for urban residence), maternal age at birth. Exposure is an aggregate indicator which is equal to one if a child was exposure to a heat wave shock in either of the nine months preceding birth. All regressions include district specific time trends. BW is birth weight and LBW is low birth weight. * Significant at 10% level; ** at 5%; *** at 1%. The mean of the dependent variable is calculated at the baseline (i.e., HWMId=0). [a]5606 singleton observations were dropped thus causing the discrepancy between the counts from the summary statistics and the regression observations.

Table A3: Effects of heat waves during pregnancy on child health outcomes

	(1) WAZ	(2) WAZ< -2	(3) WAZ< -3	(4) HAZ	(5) HAZ< -2	(6) HAZ< -3	(7) Anemia	(8) BW	(9) LBW
Exposure	-0.039**	1.006*	1.224***	-0.021	0.593	1.457**	-1.775	-10.108	0.890*
	(0.020)	(0.586)	(0.443)	(0.027)	(0.695)	(0.582)	(1.087)	(17.932)	(0.455)
Literate	0.715***	-13.008***	-5.056***	0.793***	-19.344***	-10.482***	-13.554***	16.924	-2.322***
	(0.037)	(0.544)	(0.356)	(0.046)	(0.982)	(0.574)	(1.803)	(16.204)	(0.560)
Primary	-0.552***	7.595***	2.033***	-0.664***	15.122***	6.287***	10.119***	14.103	1.449***
	(0.036)	(0.549)	(0.293)	(0.036)	(0.805)	(0.476)	(1.822)	(14.882)	(0.484)
Secondary	-0.365***	3.782***	0.798***	-0.429***	8.569***	2.229***	6.780***	4.624	0.659
	(0.031)	(0.573)	(0.279)	(0.030)	(0.677)	(0.410)	(1.724)	(12.707)	(0.447)
Christian	0.248***	-6.345***	-3.258***	0.133***	-3.695***	-3.126***	-2.430***	37.572***	-0.663*
	(0.033)	(0.956)	(0.529)	(0.036)	(0.885)	(0.702)	(0.740)	(12.501)	(0.385)
Urban	0.148***	-2.665***	-0.851***	0.230***	-5.333***	-3.218***	-1.233	-10.896	0.276
	(0.018)	(0.501)	(0.292)	(0.021)	(0.559)	(0.479)	(0.824)	(9.189)	(0.276)
Poor	-0.180***	4.186***	1.801***	-0.177***	5.477***	3.614***	3.859***	-11.644	0.664**
	(0.013)	(0.385)	(0.286)	(0.018)	(0.510)	(0.410)	(0.619)	(9.148)	(0.298)
Age at birth	0.006***	-0.078***	-0.030***	0.012***	-0.262***	-0.153***	-0.154***	7.124***	-0.202***
	(0.001)	(0.018)	(0.009)	(0.001)	(0.023)	(0.018)	(0.032)	(0.433)	(0.019)
Child's age	-0.017***	0.203***	0.038**	-0.025***	0.353***	0.148***	-0.622***	0.951**	0.002
	(0.001)	(0.053)	(0.018)	(0.002)	(0.048)	(0.039)	(0.092)	(0.407)	(0.016)
Male	-0.115***	3.179***	1.397***	-0.180***	5.082***	3.493***	2.924***	112.344***	-2.122***
	(0.011)	(0.217)	(0.265)	(0.008)	(0.254)	(0.174)	(0.382)	(4.253)	(0.198)
Observations	260,203	260,203	260,203	260,203	260,203	260,203	88,103	112,242	112,242
R-squared	0.216	0.163	0.131	0.224	0.190	0.159	0.250	0.194	0.144
Mean	-1.036	21.62	7.225	-1.476	38.42	17.63	62.69	3220	7.828

Notes: Robust standard errors clustered at district level are in parentheses. Outcomes in columns 2, 3, 5, 6, 7 and 9 are multiplied by 100, so that coefficients are in units of percentage points. Control variables include child's birth order. Exposure is an aggregate indicator which is equal to one if a child was exposure to a heat wave shock in either of the nine months preceding birth. All regressions include district specific time trends. BW is birth weight and LBW is low birth weight. * Significant at 10% level; ** at 5%; *** at 1%. The mean of the dependent variable is calculated at the baseline (i.e., HWMId=0). [a]5606 singleton observations were dropped thus causing the discrepancy between the counts from the summary statistics and the regression observations. The number of observations varies across columns depending on the availability of the health outcomes.

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