ESSAYS IN APPLIED MICROECONOMICS

A Dissertation

by

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ABSTRACT

This dissertation examines three essays on factors affecting human capital development, using quasi-experimental research designs.

The first essay "Exposure to Negative Shocks and Child Development: Evidence from Boko Haram Attacks" examines the impacts of exposure to negative shocks on early childhood development.Growing evidence shows that exposure to violent attacks during early childhood impairs the physical development of children. In this paper, I show that these effects extend to psychological development. By exploiting exogenous variation in the location and timing of Boko Haram attacks in Nigeria and new measures of physical and psychological development, I estimate the effects of exposure to violence on child development. Children exposed to terror attacks are 0.35 SDs shorter and lag in cognition by 0.18 SDs. The deficits are largest in children exposed to violence at younger ages. Mediation analysis shows that 6% of the effect on height is mediated by nutrition and parental investments can explain 14% of the effect on psychological development. This research, therefore, highlights areas in which interventions in early childhood can lessen the adverse impacts of negative shocks.

The second essay "Intergenerational Transmission of Human Capital: Effects of Maternal Education on Child Education" presents evidence on the effects of parental education in improving the education of their children. Research shows that parental education is a good predictor of children's educational outcomes. However, little is known about the underlying mechanisms through which the effects are transmitted. In this paper, I estimate the intergenerational effects of maternal education on child education. To identify effects, I exploit the timing and geographical intensity of Nigeria's 1976 educational reform, one of Africa's largest school construction projects. One extra year of maternal education increases grade-for-age by 13 percent, the probability of children completing primary school by 22 percent, and attending secondary school by 29 percent. I find that the effects are particularly pronounced for girls. The findings are robust across different specifications and validity

tests. These results are not simply due to improved access to education for children whose mothers benefited from the program, as children of slightly older mothers in the same region are less educated. I also find that the improved outcomes are not driven by better labor market opportunities for the mother or changes in fertility outcomes. Instead, improved living conditions, increased involvement in decisions relating to the child's education and health, as well as having a more educated father are important channels through which maternal education matters for children's schooling.

In the third essay "Reassessing the Effects of Education on Fertility", I study how education affects fertility for women with low levels of human capital. Conventional wisdom suggests that reduced fertility could imply "better quality" children and higher survival rates for women and children. However, can education be a driver for reducing fertility rates in developing countries? To estimate the causal effect of education on fertility, I exploit the timing and geographical intensity of Nigeria's 1976 educational reform. I find no effects on total fertility and the number of children born before the age of 25, but the number of children born before the age of 18 decreases by 0.2 births. An analysis of the underlying mechanisms shows that the effects are driven by women getting married and having their first birth at an older age. The results also indicate that more educated women are more likely to use modern contraceptives and marry more educated men.

DEDICATION

To Christopher, Joshua, ZoeGrace Ige and every child deserving a successful future.

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1. INTRODUCTION

The aim of this dissertation is to understand factors that affect the human capital formation of children from economically disadvantaged backgrounds. Human capital formation is an important predictor of future outcomes and policies and events that affect the education and health of children impact their longrun success. In this dissertation, I study the causal impacts of the exposure to a negative event on early childhood development, as well as the effect of an educational policy on education and fertility. The studies examined in this dissertation employ quasi-experimental approaches to provide evidence useful for policy.

Section 2 studies the causal impacts of exposure to negative shocks on the physical and cognitive development of children. As a type of exposure to negative shocks, I focus on the Boko Haram insurgency in Nigeria. Many countries and regions are increasingly experiencing different forms of violence and conflicts in forms political instability, intra-communal clashes, organized crime and international terrorist groups. According to the United Nations, in 2016, more countries experienced violent conflict than at any point in almost 30 years and according to the World Bank, up to two thirds of the world's extreme poor could live in fragility, conflict and violent settings. With rising violence occurrences, the costs of violence is also rising both in monetary and non-monetary terms. Violent attacks have severe and devastating social and economic impacts on the population and economy. Even more worrisome is the impact of violence on vulnerable populations, especially children. Exposure to negative events impacts the human capital formation of children which ultimately affects their human capital outcomes in the future. It is therefore important to understand the impact on child development during formative years and highlight areas for intervention that could lessen the negative impact of violence. The main results show that children exposed to the Boko Haram insurgency lag on their physical and cognitive development compared to children who were not exposed by 0.35 and 0.18 standard deviations, respectively. Areas for intervention highlighted in this study include improved nutrition and increased parental investments and reinforce the need for optimal investments at early stages of development than at later stages.

Section 3 examines how parental education is an important factor in the human capital formation of children. There are returns to education, which implies education by itself has value and studies that focus on the pecuniary benefits to education might underestimate the effects since there are positive spillovers that might have not been captured. One of such is more educated parents having more educated children. According to UNESCO (2019), about 258 million children,adolescents and youth were out of school in 2018, representing one-sixth of the global population of this age group. With education being a driver for reducing poverty and improving health, and gender equality, governments and development organizations are increasing investments in education. However, the benefits from these investments may not be fully realized if children are not attending school. Educating women is often considered an important tool for improving child outcomes from infancy to adolescence. The findings show that children of more educated mothers are more likely to be on track in school by 4.3 percentage points,more likely to complete primary school by 4.7 percentage points, more likely to attend secondary school by 4.7 percentage points, and the effects are larger in girls. Findings from this study hold important implications for education and anti-poverty policies.

Section 4 considers the effects of education on fertility outcomes. Africa has the highest fertility rate and her share of global population is projected to grow from 16% in 2015 to 25% in 2050. High fertility rates is often associated with low economic development, poverty, and low human capital outcomes. Not only does high fertility impact women's health, it also affects the "quality" of her children as lower birth rates divert scarce capital towards savings and investments in growth-enhancing activities. The goal of this essay is to examine if education is effective in reducing fertility rates. The results indicate that education does not affect total fertility but the timing of fertility. I find that more educated mothers have 0.2 fewer births than less educated women before the age of 18. Possible channels for the delay in birth timing include older ages at marriage and first birth, as well as an increase in the use of modern contraceptives.

2. EXPOSURE TO NEGATIVE SHOCKS AND CHILD DEVELOPMENT: EVIDENCE FROM BOKO HARAM ATTACKS

2.1 Introduction

Early childhood is an important period for different aspects of development, and children exposed to adverse conditions are often disadvantaged (Baker-Henningham & López Bóo, 2010; Gertler et al., 2014; Grantham-McGregor et al., 2007; Hoddinott, Maluccio, Behrman, Flores & Martorell, 2008; Macours, Schady & Vakis, 2008). For example, Grantham-McGregor et al. (2007) show that disadvantaged children who do not reach their full development potential are less productive as adults since they average fewer years of schooling and test scores. While growing evidence shows that exposure to violent attacks during early childhood impairs physical development, the evidence on psychological development is limited. Therefore, in this paper, I study how exposure to the Boko Haram terror attacks in Nigeria affects different aspects of child development.

Many countries and regions are increasingly experiencing different forms of violence including intra-communal conflicts, political oppositions and terrorist attacks. Also, as the climate is changing, different agricultural communities are experiencing shocks to productivity which can lead to tension across groups and increase the risk of violence. Violence results in trauma, fear, stress, food insecurity, income losses, forced displacement, sexual and gender-based violence and vulnerability to infectious diseases. Besides, violent political attacks have severe and devastating social and economic impacts on the population and economy. For example, the Boko Haram violence is estimated to cost $9 billion in infrastructural damage and $8.3 billion in output losses as of 2016. In addition to hurting a country's economy by worsening economic inequalities, violent attacks can have other, less-obvious effects on the economy by negatively impacting child development, via exposure and maternal stress (Camacho, 2008). Bertoni, Di Maio, Molini and Nisticò (2018) find that exposure to the

Boko Haram violence reduced the probability of primary school enrollment and educational attainment. Overall, both direct and indirect consequences of violence can affect the human capital formation of children as past skills affect future skills (Cunha & Heckman, 2007).

Estimating the causal effect of violence exposure is difficult since violent attacks are often not random. The challenge is to find a source of variation that will be uncorrelated with other factors that affect child development. Furthermore, children are especially vulnerable during violent conflicts, and this impairs their physical development (Akresh, Caruso & Thirumurthy, 2014; Akresh, Lucchetti & Thirumurthy, 2012; Bundervoet, Verwimp & Akresh, 2009; Di Maio & Nandi, 2013; Minoiu & Shemyakina, 2014).[1] Yet, the effects of violence exposure on other aspects of development remain an open question. Furthermore, there is limited evidence on the pathways through which children are negatively affected by violence. Exploring the potential mechanisms is crucial because it holds implications for recovery, transition and resilience policies. To address these concerns, I exploit the timing and location of Boko Haram attacks in Nigeria as a source of exogenous variation in violence exposure. Using a dataset that collects information on different measures of child development and home environment, I estimate the effect of exposure to violence not on only physical development, but also cognitive and motor skills development. I am also able to uncover factors that mediate the effects of violence.

A common measure of physical growth and nutritional deprivation in children is their height. A report by the World Health Organization (WHO) shows that women who had stunted growth as children are more likely to have smaller pelvis and are at greater risk of

[1]Other studies on negative shocks include the Cote d'Ivoire violent conflict- Minoiu and Shemyakina (2014), Eritrean-Ethiopian violent conflict, ((Akresh et al., 2012)), German war, (Akbulut-Yuksel & Yuksel, 2017), Nigerian Biafran violent conflict (Akresh, Bhalotra, Leone & Osili, 2017), Palestine violent conflict (Mansour & Rees, 2012) Ethiopian famine (Tafere et al., 2016), psychological stress caused by death of a parent (Black, Devereux & Salvanes, 2016), extreme weather shocks (Aguilar & Vicarelli, 2011; Currie & Rossin-Slater, 2015; Rosales-Rueda, 2014), exposure to pollution Currie and Neidell (2005), maternal stress (Black et al., 2016; Camacho, 2008), flu endemic (Almond, 2006). The studies most closely related to mine are Dunn (2018) and Ekhator and Asfaw (2018). They analyze the effects of the Boko Haram crisis on weight and height and find that exposure to the crisis reduces child weight but has no effect on height (Ekhator & Asfaw, 2018). To test the validity of my data and strategy, I estimate the effects of violence on measures previously studied in Ekhator and Asfaw (2018). I find similar results on weight (see Appendix Table A.19).

giving birth to children with low birth weight and worse economic and health outcomes as adults. According to the World Bank, on average countries lose 7 percent of their per capita GDP because they did not eliminate stunting when their current workers were children. The WHO also reports that children who suffer from retarded growth as a result of poor diet or recurrent infections tend to be at greater risk for illness and death, and are also more likely to have delayed mental development, poor school performance, and reduced intellectual capacity.[2] Good nutrition, protection and stimulation from talk, play and responsive attention from caregivers help develop the brain and neural process of children (UNICEF, 2018). As shown in Grantham-McGregor et al. (2007), children who experience poor cognitive, motor and socio-emotional development have lower school achievement. In an intervention in Guatemala, boys under the age of 3 who received nutritious food (atole) had higher wages as adults, than those who did not receive atole (Hoddinott et al., 2008). Chang, Walker, Grantham-McGregor and Powell (2010) also report that stunting is associated with fine motor abilities and children with lower scores on fine motor skills measures are at greater risk for lower IQ and student achievement. Overall, these interventions point to deficits in cognition as having lasting impacts.

I rely on the exposure to Boko Haram attacks in Nigeria as an exogenous variation in exposure to negative shocks. Boko Haram is a terror and extremist group in the Northeastern Nigeria who oppose western influence. The group launched its first major attack in Nigeria in 2009 and since then have carried out numerous violent attacks especially in the northeast. Their operations include suicide bombing, razing villages, vandalism, looting of properties, and kidnapping UNCHR (2017), such as the kidnapping of 276 Chibok school girls that captured international attention in 2014. More recently in 2018, 110 girls were abducted from Dapchi, a city in northeastern Nigeria.

The data used in this study are from nationally representative surveys which include the UNICEF's multiple indicator cluster surveys and the demographic and health surveys.

[2]See Bozzoli, Deaton, Quintana-Domeque et al. (2008) for detailed discussion on stunting.

The surveys provide individual and household level characteristics including child health and development measures and are combined with information on the timing and location of terror attacks from the Armed Conflict Location and Events Data (ACLED) project. I rely on variation in the timing and location of the attacks to estimate the causal effect of early exposure to negative shocks on child outcomes (intent-to-treat effects). These sources of variation allow me to apply a generalized difference-in-differences identification strategy that assumes violent states would have experienced changes in child outcomes similar to the non-violent states in the absence of the Boko Haram violence. The first source of variation is from the timing of birth of children (and when they are sampled) and the second source of variation is the geographical location of violent attacks. Specifically, I use data before and after the attacks to estimate changes in child outcomes within violent states using the non-violent states as the control group.[3] To measure a closer level of proximity to attack areas, I use GPS information available from the DHS datasets.

The results show that exposure to the violence reduces height-for-age-z-scores by 0.35 standard deviations and cognition by 0.18 standard deviations. Using GPS information on a subset of the sample, I find that living within 5 km radius of an attack location reduces height-for-age-z-scores by 0.17 SDs.[4] Similar to Rosales-Rueda (2014) and using an alternative model specification, I find suggestive evidence that exposure during the third trimester *in utero* negatively affects height, and exposure during the first trimester affects cognitive development. I conduct a battery of robustness checks to validate the identification strategy. The parallel trends assumption is supported by the absence of pre-trend in the data. I also show that the results are not driven by selective fertility or compositional change. However,

[3]The measures on cognitive and motor development in the data were collected after the attacks started in most states. Therefore, I do not have pre-data and can not test for pre-trends in cognition. Therefore, the results on cognition should be interpreted with caution. However, since height and cognition are associated and the results are similar to other studies, I argue that the parallel trend should also hold for cognition and perform different robustness tests to rule out that the results are driven by selection.

[4]The estimated effects obtained from the spatial analysis is an underestimate of the true effects because of the measurement error in the GPS coordinates. In the DHS dataset, coordinates are displaced to protect the identity of respondents. Also, this analysis is only done for physical development because the DHS datasets used do not contain measures of cognitive development.

I perform a bounding exercise on the estimates. Finally, the results are robust to bounding as in Lee (2009), different specifications, falsification tests and the inclusion of household controls. The robustness of the results to these tests confirm that the incidence of violence is uncorrelated with other determinants of child outcomes.

To understand the mechanisms through which political violence affects child development, I exploit the availability of parental investment and home environment variables. Violence exposure reduced the probability of children eating nutritious foods, time parents spent with children, and increased vulnerability to infections and diseases. Poor nutrition is the largest mediator for the effect of violence on height; it reduced the effect of violence by 6%. Material investment and parental time investment reduced the proportion of the variance in violence by 14% and 10%, respectively. Cunha, Heckman and Schennach (2010) estimate the importance of early parental investment in children for future outcomes. They find that for disadvantaged population, it is optimal to invest more in the early stages of childhood rather than at later stages.[5] These findings reinforce the importance of identifying potential mechanisms because it sheds light on areas for recovery that can lessen the intensity of the shock.

The evidence shown in this paper fits into two strands of literature. One is the effects of early exposure to shocks on child outcomes and the other is the non-monetary cost of violence. However, this study makes several contributions to the literature. First, to my knowledge, this is the first paper to examine the effect of the Boko Haram violence not only on physical development but also on other aspects of child development such as cognitive and motor skills. Furthermore, the result on height is supported using GPS data for analysis. Second, I go beyond estimating the average impact on child outcomes to estimate the effect on the timing of exposure to violence. Finally, I explore new mechanisms through which terror attacks affect child development and are relevant for recovery policies.

The paper proceeds as follows: Section 2 provides background information on the Boko

[5]Similar results are shown in Chang et al. (2010); Gertler et al. (2014); Grantham-McGregor et al. (2007); Hoddinott et al. (2008); Macours et al. (2008).

Haram violence. Section 3 discusses the data and empirical strategy. Section 4 presents the results while section 5 shows robustness checks. In section 6, I show results from the mediation analysis and conclude in section 7.

2.2 Background Information

2.2.1 Country Overview

Nigeria is the most populous country in Africa with an estimated population of over 190 million. The World Bank classifies Nigeria as a lower middle-income country, with a gross national income per capita of $2,028 in 2018 and a life expectancy at birth of 54 in 2017.[6] Nigeria is home to over 250 ethnic groups with distinct languages and religious practices. The Northerners (Hausas and Fulanis) are predominantly Muslims while those in the south (Ibos and Yorubas)are predominantly Christians and more than half of the Nigerian population are in the north.

2.2.2 Boko Haram Attacks in Nigeria

Boko Haram literally means Western education is forbidden. Boko is a group of Islamic fundamentalist who opposes western influence and whose aim is to build an Islamic state in the Northeastern part of Nigeria. The group was formed by Mohammed Yusuf in 2002, and after he was killed in 2009, the group has been led by Abubakar Shekau. The group has its headquarters in Borno state in Northeast Nigeria but carry out their attacks in the north. Boko Haram was ranked the world's deadliest terror group by the Global Terrorism Index in 2015. Since July 2009 the group has launched numerous violent attacks in many states in the region. The attacks started in 2009 as shown in Figure A.1. Although their attacks are spread across the north, they are mostly contained in the northeast. Figure A.2 shows the spread of fatalities, with most of the attacks concentrated in the northeastern states.

Boko Haram is responsible for over 27,000 fatalities and destruction of public infrastructure including schools, health centers, hospitals, and markets. For example in Borno state,

[6]Lower middle-income economies have GNI per capita between $1,026 and $3,955.
Source: https://data.worldbank.org/country/nigeria

over 500 schools, 201 health centers, 1,630 water sources, and thousands of houses were destroyed (UNCHR, 2017). Communities where over 80% of the people rely on agriculture as a source of livelihood, have been scarred by violence. The group has launched attacks of varying intensities in about 14 states including the nation's capital and displaced millions of people. Nearly 15 million people have been affected by Boko Haram's attacks. The violence is estimated to have caused $9billion in infrastructural damage in the six northeastern states as of 2016. Although the government and international partners are working to push back on Boko Haram's activities, there is a lot of work to be done to restore the lives of the people.

2.3 Data and Empirical Strategy

2.3.1 Data

I use nationally representatives - the UNICEF Multiple Indicator Cluster Surveys (MICS) and the Nigerian Demographic and Health Survey as my primary data sources (DHS (2003-2013); UNICEF (2007-2016)). The survey rounds were conducted pre-violence (2003, 2007 and 2008) and post violence (2011, 2013 and 2016/17). The survey collects information on children's health and development outcomes, as well as detailed information on individual and household socio-demographic characteristics.

Measure of violence: I use data from ACLED to identify violent states (Raleigh, Linke, Hegre & Karlsen, 2010). ACLED collects information on the dates, actors, types of violence, locations, and fatalities of all reported political violence and protests events across Africa, South Asia, South-east Asia, and the Middle East. I cross-validated the data in ACLED with other media sources to verify the actual occurrence of events.[7] For the main model specification, I define a violent region as a state in the northeastern part of the country (the focal area of the attacks), with at least one reported violent attack caused by Boko Haram in the state (see Table A.1).[8] Other measures of violence include geographical distance to

[7]Three states in the south were identified in the data as having had at least one fatality caused by Boko Haram. However, I compared multiple sources of information and found out that these attacks were not caused by the group. Thus, they are not classified as violent states. I also checked and found that their inclusion does not change my results.

[8]Northeastern states have been the focus of attacks by the terror group and these states have also been

the nearest site of attacks using GPS data and intensity of violent attacks in all states.

I assign a treatment status to a child if he or she is observed in the data the year violence started in the state or any year after the onset of violence. For example, child A from Taraba was observed in the data in 2016 and the first attack in the state was in 2012. Therefore, the child is defined as being exposed to violence. However, child B from the same state was observed in the data in 2011 but since violence did not start in Taraba till 2012, she is not treated as being exposed to violence. In the robustness section, I define an alternative violent region as any state that has at least one fatality caused by Boko Haram between 2009 and 2016. One drawback of the survey data is that there are no finer levels of geography other than states to capture spatial variation in intensity in some of the survey years. Thus, this study is an intent-to-treat analysis of the effect of violence. However, the 2008 and 2013 DHS surveys allow me to use GPS data to the measure distance to locations of Boko Haram attacks. To define exposure while *in-utero*, I use the information on the child's birth date to define exposure to violence.

Height: Height-for-age z-scores (HAZ) are calculated using the child's height (measured in centimeters), age in months, gender, and height for the reference population. The WHO's multi-center growth reference data is used to obtain the median and standard deviation for the reference population. HAZ ranges from -6 to 6. Measuring the height in z-scores allows for comparison with the reference population and is widely used in the child development literature.[9]

Cognitive ability and motor development: The MICS collects information on early childhood development on children between 35-59 months. I construct the cognitive ability index from measures of a child's cognitive ability and motor development. The raw variables are dummies that take on a value of one if the child can recognize numbers one through ten, at least ten alphabets, read at least four simple and popular words and pick up a small object with at least two fingers, respectively and zero otherwise. These measures were only

the focus and priority of assistance and emergency response by the government and humanitarian agencies.

[9]I recalculated the z-scores for each child across all surveys to reflect the 2006 WHO reference standard.

collected in the 2011 and 2016 survey waves. To construct the index, I first define a variable which is the average of the cognitive and motor measures for each child. Averaging the four variables, the maximum score a child can get is one and a minimum of zero. I group children into different age intervals- three months and six months respectively and then standardize each score with respect to the control group of the relevant age band the child falls into.

The measure of cognitive development is similar to some items on the Bayley Scales of Infant and Toddler Development.[10] The scale has different items including fine motor (objects grasping, reaching, object manipulation, etc.), cognition, communication, physical, adaptive and socio-emotional assessments. There are also some similarities between the measure of cognition in this paper with those used in (Cunha et al., 2010) which includes measures of motor-social development at ages 3–4, picture vocabulary at ages 3-4, etc. The similarities with these previous studies provide some validity to using these survey response variables to measure child development. I also tested for internal consistency using the Chronbach's α scale. The cognitive ability index has an $\alpha = 0.75$.

In Table A.18, I compare the height of children 12 to 59 months in Nigeria to the WHO international reference. It shows that Nigerian children are on average, shorter than the international reference group. The descriptive statistics are presented in Table A.2. The average HAZ is -1.5, this indicates that on average, a Nigerian child has a height-for-age-z-score that is 1.5 standard deviations lower than the international reference population. About 83% of children 35-59 months can pick up a small object with at least two figures, while 33% of those children can both recognize ten alphabets or the first ten numbers. About 50% of children are boys and a majority of children, 67%, live in rural areas. Table A.3 shows children exposed to violence lag in their development compared to children not exposed to violence.

[10] The Bayley Scales of Infant and Toddler Development assess the developmental functioning of children.

2.3.2 Empirical Strategy

To measure the intent-to-treat effect of violence exposure, I estimate the following equation:

$$Y_{istr} = \beta_s + \beta_t + \beta_1 \text{Violence exposure}_{str} + \beta_2 X_{istr} + \epsilon_{istr} \tag{2.1}$$

where $\mathrm{Y_{istr}}$ is the height-for-age z-score, or cognitive ability measure of child i, living in state s, born in time t, and surveyed in round r. β_t is the time fixed effects that controls for shocks common to children born within the same year. β_s is the state fixed effects that controls for shocks or specific characteristics common to children residing in the same state. $\mathrm{X_{istr}}$ includes controls for the gender of the child, urban residence, maternal and household characteristics. Each model includes survey round fixed effects. β_1 is the coefficient of interest, it captures the effect of violence exposure on child outcomes. Violence exposure is a dummy variable that takes on the value of one if the child is observed in a state of violence, the year the violence started or after violence started. Since I only observe cognitive outcomes for children sampled after the onset of violence, the analysis breaks down to comparing cognitive outcomes across violent and non-violent states.

The main identification assumption is that, after controlling for time and state fixed effects, as well as predetermined characteristics, the error term is uncorrelated with the presence of violence. The literature on child development shows that the development of cognitive and emotional abilities largely occur during early years and growth rates in the first few years are higher than at other times. According to Currie and Vogl (2013) and Martorell (1999), children under the age of three are most sensitive to negative shocks because of the vulnerability of their immune system. To test if this holds for the study, I run a different model specification to capture the effects of exposure *in utero*. I also examine the timing of exposure during pregnancy. Other specifications test for gender differences and other heterogeneous effects.

While the difference-in-differences approach gives causal estimates, there are threats to identification. First, there might be a selection issue if, before the start of the violence, states affected by the Boko Haram attacks had lower trends in outcomes than non-violent states. To address this, I plot an event study graph to check for evidence of pre-trends. I include leads of treatment and test if the coefficients on the leads are zero and jointly insignificant. Figure A.3 shows the time passage relative to the onset of violence.[11] I find no evidence of pre-treatment trends in height. The coefficients before t=0 are jointly insignificant. This supports the difference-in-differences identification assumption. To further test that the identification assumption holds, I run placebo tests where I restrict the sample to pre-violence years and create fake violence start years.

Second, changes in characteristics or demographics as a result of the violence can confound the estimates. In Table A.4, I show that the predetermined characteristics are mostly balanced between violent and non-violent states, and across violent and non-violent years. The table shows that those affected are mostly Hausa and live in rural areas. This is expected since the northerners are predominantly Hausa and the attacks have mostly been in rural areas. I address the imbalance by controlling for predetermined characteristics in the model specifications.

Third, selective migration or compositional change can bias the results. On one hand, if children who are more likely to be negatively affected by the crisis are moving to non-violent states, then the results will be underestimated. The results will be overestimated if children who have better outcomes and are less likely to be affected by the crisis are moving to the non-violent states. Although the data does not have information on migration history, I construct lower and upper bounds of the estimates to show that the results are not driven by selective migration.

[11] To do this, I create different indicators for exposure to violence. For example, t=-1 takes on one if the child is observed a year before the violence started in the state and zero otherwise, t=0 if the child is observed the year violence started and zero otherwise, etc.

2.4 Results

2.4.1 Height

Table A.5 presents the results for the effects of violence on height as specified in equation 1. The coefficient in column 1 indicates that children exposed to the violence have height-for-age-z-scores that are 0.35 SDs (25% of the baseline) lower than children of the same age not exposed to the violence. The inclusion of controls accounts for any imbalance in characteristics between violent and non-violent states. To capture the intensity of exposure to violence, I estimate the effect of a one percentage point increase in the share of attacks a child is exposed to.[12] The effect is a reduction in height-for-age-z-scores by 0.38 SDs. Using the length of exposure as treatment shows that an extra year of exposure to violence reduces HAZ by 0.08 SDs.

As the literature on child development suggests, children exposed to negative shocks under the age of three are more affected than children exposed at later ages. Currie and Vogl (2013); Martorell (1999) suggest that stunting occurs by the age of two and is hard to reverse. I find that for children to exposed to violence while *in utero*, height-for-age-z-scores reduces by 0.089 SDs compared to those not exposed while in *in utero*. The coefficient on exposure before age three is 0.4 SDs. This is similar to the coefficient in the main model specification, which suggests that the result is driven by children exposed before the age of three. An alternative way to examine physical development is to test for stunting. A child is stunted if the HAZ is less than -2 standard deviations. I find that exposure to violence increases the probability of stunting by 7 percentage points (column 3). Even after controlling for birth-weight, a measure of health stock at birth, in column 4, the coefficient on violence exposure is still statistically significant, although the magnitude reduced to -0.28.

To validate the results from the main specification, I use spatial data from the NDHS to identify the effect of proximity to attack locations on height-for-age. However, one concern of the dataset is that the geo-coordinates are displaced to protect the identity of the survey

[12] Violence intensity is the proportion of attacks in a state a child is exposed to.

respondent. Therefore, the displacement of the coordinates introduces measurement error in the independent variable, which could potentially attenuate the estimates. Therefore, the effects identified using the geo-located dataset may represent lower bounds of the main estimates. I restrict the sample to households in the Northeast and regress height on an indicator variable that represents if a child is living within 5 kms, 10kms, 20 kms, 50 kms and 100 kms of an attack site, respectively. The results are presented in Table A.6. Column 1 shows that children living within 5 km radius of a Boko Haram attack location have 0.19 SDs lower HAZ . In column 2, including time-varying survey cluster characteristics does not change the result (0.17 SDs). There is some effect for children living a 10km radius but it is not statistically significant. As expected, there is no evidence that children living further away from the attack locations- 20 km, 50 km, and 100 km- are affected by violence as shown in columns 4, 5, and 6. The effects are close to zero, of the opposite sign and not statistically significant.

2.4.2 Cognitive Ability

Table A.7 reports the effects for the combined measure of cognition. The results show that children exposed to violence score 0.18 standard deviations lower on the cognitive ability index than those not exposed to violence. Using continuous measures of exposure, the results show that a one percentage point increase in the share of violence exposed to reduces the cognitive ability index by 0.08 SDs and an additional year of exposure reduces the index by 0.03 SDs. I find that children to exposed to violence while *in utero* have a cognition deficit of 0.086 SDs. The coefficient on exposure before the age of three is 0.05 SDs (not statistically significant). From the results presented, children exposed to violence lag on cognitive domain than those in the control group. I also run alternative model specifications which accounts for the growth spurt in child development. In Panel 1 and 2 of Appendix Table A.21, I include age polynomials and alternative age grouping (3 months interval) and the results do not change.

2.4.3 Timing of Exposure

Table A.8 presents the results of the timing of exposure on children's development. The aim of this analysis is to test if differences in the timing of exposure to violence while *in-utero* matters for child development. I run a model specification where I regress children outcomes on indicators for whether a child was exposed in the first, second or third trimester respectively. Medically, as early as six gestational weeks, the neural tube which gives rise to the brain is formed, and at eight weeks the brain and nervous system begins to develop and most human organs are largely formed. The first trimester is therefore an important period for the development of the brain. According to Pardi and Cetin (2006) fetal growth is highest in the third trimester.

The results in column 1 show that exposure to violence in the third trimester impacts height more severely when compared to children not exposed while *in-utero* (-0.29 SDs). On the other hand, children who were exposed to violence in the first trimester have a larger cognition deficit than those not exposed (-0.09 SDs). In columns 3 and 4, I conduct falsification tests by restricting the sample to children who were not exposed to violence while *in-utero* and create placebo exposure measures. I do not find any significant effects for the placebo treated group at the first or third trimester for height and cognition, respectively. The coefficient on third trimester in column 4 is spurious- significant but of the opposite sign. These results are consistent with the findings in Currie, Neidell and Schmieder (2009) and Coneus and Spiess (2012) who find that exposure to carbon monoxide in the third trimester reduces birth-weight. Black, Bütikofer, Devereux and Salvanes (2013) also find that exposure to radiation during weeks 8 to 16 while *in-utero* affects IQ and Rosales-Rueda (2014) show that the effects of exposure to shocks on height is stronger in the third trimester while for cognition is stronger in the first trimester.

2.4.4 Heterogeneous Effects

In Table A.32, I explore heterogeneous effects across different subgroups. First, I test if boys are more affected than girls. Similar to Akresh et al. (2012), Minoiu and Shemyakina (2014), I find no statistical difference between boys and girls across the outcomes of interests. Next, I test if children in rural areas are more affected than those in urban areas. There is a significant difference on the cognitive dimension between children in rural and urban areas- children in urban areas appear to be worse off. The table also shows that the negative effects of violence are larger on the physical development of children from poor households. I used the wealth index to split the sample into poor and non-poor households.[13] To check that the effect of violence is not driven by poverty, I run the same model specification on the different wealth quintiles and find that both poor and non-poor households are affected by the violence (see Appendix Table A.22).

2.4.5 Effects Persistence

An area relevant for policy implication is how the effects of violence on the different outcomes persist or fade-out over time. For example, given that the effect on stunting or height largely occurs by the age of two, we should not expect the effect of violence on height to increase with age. However, we expect the effect on cognitive ability to increase with age. To test for this, I restrict the analysis to older children in the sample and compare three and four-year-olds who were affected by violence while *in utero*. Appendix Table A.23 presents the results where I interact an indicator for whether the child is four years old with whether the child was exposed in utero. Consistent with the prediction, I find that the effects on height do not increase with age (-0.05 SDs, not statistically significant), but gets worse for cognitive development (-0.14 SDs, negative and statistically significant).

[13]The wealth index is a composite measure to wealth. It combines different household characteristics (asset ownership, living and sanitary conditions and type of residence) to create a score and then a ranking. The survey reports that the index does not provide information on absolute poverty, current income or expenditure levels but is a composite measure of a household's cumulative living standard.

2.5 Robustness Checks

2.5.1 Robustness Checks

In panel A of Table A.10, I run a placebo test to check if the effects shown in Table A.5 are capturing pre-existing differences in the outcomes across violent and non-violent states. If this is true, the effects of violence from equation 1 will not be entirely due to the presence of violence. I address this issue by using the 2003, 2007 and 2008 surveys, in which the children should not have been affected by the violence to define a placebo treatment. To do this, I reassigned the start of violence in violent states to earlier periods where there was no violence. The coefficient on the placebo violence measure (-0.07 SDs) shows that the results are not driven by pre-existing differences in height-for-age z-scores across violent and non-violent states, as it is not statistically significant.[14] Clustering standard errors at the state level does not change the results (panel B).[15]

Panel C presents the results for an alternative definition of treatment. Violence exposure is extended to capture the presence of violence not just in the northeast, but in any state that had at least one Boko Haram attack. Since non-northeastern states had relatively few attacks, we expect this coefficient to be smaller than the main specification. The effect is qualitatively similar but reduces in magnitude, -0.25 for height and -0.04 for cognitive ability (not significant). The exclusion of Niger-Delta states from the estimation (Panel D), leaves the results remain unchanged.[16] In Panel E, I interact the cohort variable with predetermined observable characteristics. This allows for children with different baseline characteristics to respond differently to year-to-year shocks. Put differently, it allows observable characteristics to deferentially affect child development each year. The results stay the same -0.37 and -0.17.

Panel F of Table A.10 shows the effects of violence on child outcomes when I include state-cohort fixed effects which controls for any events, policies that are common to children living

[14] I only run this placebo test for height because the other outcomes were not collected in earlier surveys.

[15] Appendix Table A.24 show estimates for alternative clustering levels.

[16] Crude oil-rich states in the Niger-Delta region experienced some form of communal conflict before the onset of Boko Haram violence.

in the same state, and born in the same year. The coefficients on both outcomes increase in magnitude. Including state-specific linear trends allows for both observable and unobservable state characteristics to linearly change over time. With this inclusion, if states were trending differently before the violence started, the coefficient on violence will be attenuated. However, panel G shows that the states were not trending differently before the onset of the violence. Panel H shows the results with sample weights applied. The effects are similar to the main specification.

In Table A.25, I run falsification tests to show that after controlling for other covariates, there are no effects on variables that should not be affected. These include gender and maternal characteristics. Also, the coefficients on the lead indicators of violence in the regression of height-for-age on violence exposure shown in Figure A.3 are statistically insignificant and close to zero. Put together, the results of the robustness tests support the validity of the identifying assumption.

2.5.2 Selective Fertility

One threat to validity is selective fertility. The onset of the violence can affect fertility decisions, hence selection into the sample. Fertility decisions are correlated with maternal characteristics which can affect child outcomes. To address this concern, I first test if the violence affected the total number of children born to a woman. Second, I compare maternal characteristics of women of child-bearing age who could have had children during the violence across violent and non-violent states. I regress maternal characteristics on state and year of birth fixed effects, and a dummy which takes on the value of one if a woman in a violent state had at least a child during the violent period and zero otherwise. The results in Table A.11, show that the number of children born does not differ across violent and non-violent states and characteristics of women who had children in violent states and periods are not systematically different.

2.5.3 Selective Mortality

A second source of bias is selective mortality i.e. there is a selection of children who survive the violence into the sample. If this is true, it suggests that many children died from the violence such that it is only the fittest or strongest that survived and implies a positive selection into the sample (culling effect). To test if this concern is valid, I use a information on the birth history of a woman and regress neonatal, infant and child mortality on violence exposure.[17] In Table A.12, I find no evidence of selective mortality, the estimates are zero. This is consistent with Nwokolo (2014) who finds no effect of the Boko Haram violence on child mortality in Nigeria. Since I find no effects of culling, then the negative effects on child development may reflect the scarring effect.[18] Current evidence suggests that male fetuses are more affected by negative *in-utero* shocks and so more male fetuses might die compared to girls. If this is true for the sample, then there should be an imbalance in the sex-ratio caused by the violence. I examine this by collapsing the data into state-year cells and regress the sex-ratio on an indicator for being exposed to violence. The results do not show any evidence of selective mortality (column 4).

2.5.4 Selective Migration

Finally, a third source of bias is selective migration. Considering that violence is a negative event, it is unlikely that households move from non-violent states to violent states. Therefore, we can rule out this dimension of compositional change. The plausible dimension is households moving from violent to non-violent states. While I do not have a measure of how long a child has lived in the state observed in, except in two prev-violence waves of the DHS surveys, I argue that the results are not biased by selective migration. First, the political divide between the north and south makes it difficult for northerners to move to the

[17]Neonatal mortality is an indicator variable that takes on the value of one if the child died within the first 28 days of life. Infant mortality is an indicator variable that takes on the value of one if the child died before age one. Child mortality is an indicator variable that takes on the value of one if the child died before age five. Only the DHS data and MICS 2016/17 surveys have individual responses for birth history.

[18]See Bozzoli, Deaton and Quintana-Domeque (2009) for more discussion on the culling and scarring effects.

south. Second, a report by the Internally Displaced Monitoring Center (2018), shows that most of the migration from violent areas was done within states and there is a significant level of return migration to areas previously affected.[19] Since I am identifying violence at the state level, this should not affect my results. However, I conduct a bounding exercise to show the results are not driven by selective migration.

Bounding Estimates: To allow for the possibility that households could move from violent to non-violent states, I construct lower and upper bounds of the treatment effects. This is following the bounding method in Padilla-Romo (2016) - a modification of Lee (2009), which assumes that children (households) in violent states who move to non-violent states are those with the best or worst health or development. I reassign treatment status (being in a violent state) for children in non-violent states who are at the extreme in the distribution of outcomes. This implies that children with the best(worst) health or development are moving from violent to non-violent states. Reassigning treatment for the children with best(worst) outcomes and estimating the main model specification gives the lower(upper) bounds of the treatment effects. This exercise implies a monotonicity assumption that children (households) are moving from violent to non-violent states.

To create the bounds, I first estimate the effect of violence on a population proxy for each state-year observed in the data.[20] The population proxy is constructed by dividing the number of children in each state and for a given year by the total number of children observed in that year. The idea is that if violence makes people move, then the violence coefficient would be negative and statistically significant. I then use the coefficient on violence to construct the bounds (see Appendix B.1). To get the lower (upper) bound, I reassign a treatment status to the top (bottom) 0.01% of children in the distribution of the outcomes from non-violent states to violent states, then I re-estimate equation 1.[21] Table A.13 shows the treatment effects and bounds. The bounds are negative and statistically significant.

[19] Source: http://internal-displacement.org/countries/nigeria. Accessed April 23, 2018.

[20] The coefficient on population is -0.0002 with standard error 0.004

[21] I use the coefficient on population to construct this: 0.01%= 1- 1/(1 + 0.5*0.0002)*100.

Since my estimates from the preferred model are similar to the lower bounds (-0.30 and -0.18), the main treatment effects are conservative estimates.

2.6 Potential Pathways

The next step is to examine potential pathways through which the violence affects child outcomes. I attempt to understand how the presence of violence in a state of residence could affect other factors that are inputs in child development. Studies have shown that nutrition, environment and parental investments affect child development. I examine four channels: food consumption; parental engagement, material investment; and health status (morbidity). First, I show the effect of violence on these outcomes, then I perform a mediation analysis to estimate how much of the effect on height and cognitive ability is passed through the intermediate outcomes.[22]

Food consumption- One of the consequences of exposure to negative shocks such as violence, floods, and drought is that there could be a change in diet composition or malnutrition. People may change the content of their meals to cheaper foods or may even reduce how often they eat. I create different food consumption variables which indicate if a child ate nutritious foods the day before being surveyed. We expect malnourished children to be more likely to lag on development outcomes than well-nourished children. Furthermore, since some of the attacks were in form of destruction of market locations, it is possible that affected households had limited access to food markets which could drive up prices in the nearest accessible market.

Table A.14 shows that children who were exposed to the violence ate nutritious foods less. Violence exposure reduced the consumption of grains and cereals by 7 percentage points, and tubers by 3 percentage points. This could indicate that they are switching from nutritious, pricier foods to cheaper, less nutritious alternatives. Children under the age of five are in the rapid growth phase and need nutritious foods to help them grow and develop.

[22]For all the intermediate measures, the mother was asked these questions on time and material investments, food consumption, and morbidity. The data on food consumption is only available for children under 3 years of age while the time investments questions on children aged 3 and 4.

Therefore, a lack of nutritious foods could stunt growth. According to UNICEF (2018), over 450,000 children affected by the crisis may suffer from acute malnutrition and 45,000 children may die in Borno state if they do not receive treatment. Next, I compare changes in the consumer price index for food items across states in the northeast with those in other parts of the country. I find that between 2011, when there was a huge increase in violent attacks, and 2016, CPI increased by 118% in the northeast compared to 106% in other parts of the country, with the highest increase in Borno state- 149%.

Parental investments- Negative shocks can affect parental investments in children which in turn can affect child outcomes. In households where the main source of livelihood is impacted by the violence, parents may now be occupied with ensuring the survival of their household and may reduce their investments in children. A unique feature of the dataset I am using is that it collects information of time and material investment in children.[23] I use polychoric principal component analysis to construct a summary measure of the time investment in children. The items used to construct the index include indicator variables for time the parents or any individual over 14 years in the household spent with the child reading, singing, telling stories, playing, taking the child outside, to name, count or draw objects in the last 3 days before being surveyed. I further separate the time index variable to know the effect of time spent by mothers and fathers separately. The time engagement variable is important because it indicates some form of non-monetary investment in the child. Parents who spend time with their children are more likely to detect abnormalities in their development. The time investment can be affected by the violence- adults have less time to spend with children because they are now occupied with tending to other pressing needs such as working more hours to provide for the family.

Similarly, I construct a material investment index variable (or home quality, as referred to in similar studies) which is a standardized measure made up from a list of items in the house that a child plays with, using polychoric principal component analysis. Items include

[23]This is not available in the DHS surveys.

toys bought from the store, home-made toys, other objects and books. The items could help develop a child's cognitive and motor skills. For example, having toys around a child could help with grasping and fine motor skills development. These measures of parental investment are similar to the measures used in O. Attanasio, Meghir and Nix (2015); Cunha et al. (2010); Rubio-Codina, Attanasio and Grantham-McGregor (2016).[24] Different studies have shown that material and time investments by parents in their children matter for child development including cognitive, fine motor and socio-emotional (O. Attanasio, 2015; O. Attanasio et al., 2015; Rubio-Codina et al., 2016)).[25] Columns 1, 2, 3 and 4 of Table A.15 show that presence of violence leads to less parental investments in terms of time spent with children and resources available for the children to play with. In particular, the time adults spent with children on educational activities reduced by 0.27 SDs, maternal time reduced by 0.25 SDs and paternal time reduced by 0.21 SDs, while material investments reduced by 0.23 SDs.

Health status- I create a variable that indicates if the child was sick with fever or diarrhea in the last two weeks before the survey. Children are vulnerable to changes in living conditions, environment, and diet. Thus, poor hygiene as a result of worse living conditions and exposure to harsh weather could make them sick. Sick children are more likely to grow at a slower pace than healthy children. Column 5 in Table A.15 shows that children who are exposed to violence are 5 percentage points more likely to be affected by fever or diarrhea than children not exposed to the violence.

Table A.39 reports the effect of violence on child outcomes after sequential inclusion of the mediators. Each column represents a different regression. I separate the analysis into 2 different panels since they correspond to different age groups (some information is only available for certain age groups in the data). Appendix Table A.26 shows the results for the test of significance of the indirect effect of each potential mediator. In panel A column 1 of

[24] The materials include toys bought at a store, household objects that a child plays with and home-made toys.

[25] I validate the investment measures to show that they are positively correlated with mothers' education, household wealth and child development.

Table A.39, I present the coefficient on the exposure to violence without controlling for any mediators. Column 2 (step 1) shows the coefficient on violence after including food variables, column 3 (step 2) after controlling for whether a child was sick or not.[26] In Step 1, food has a positive and significant effect on height. With this inclusion, the coefficient on violence reduced by 6%. In Step 2, controlling for food, the evidence shows that morbidity does not mediate the effect of violence on height for children under three.

Panel B reports the effect of violence after including the mediators sequentially for children over the age of two. In Panel B, both maternal and paternal time investments are mediators for violence. The effect of violence on cognitive development reduced by 10%. In step 2, controlling for parental time investment, availability for materials and objects for children to read and play with reduces the effect of violence on cognitive development by 14%. In step 3, the coefficient on violence does not reduce, which implies that that morbidity is not a mediator for violence. Height-for-age is a measure of the child's nutritional status. Finally in step 4 after controlling for other potential mediators, including height-for-age reduces the coefficient on violence by 4%. Controlling for height-for-age shows that violence is mediated through other factors beyond poor growth.

Put together, the mediators seem to go in the right direction. Food matters the most for height. Material investment is the largest mediator of violence on cognitive development. This is in line with the results of O. P. Attanasio et al. (2014). The results also suggest that parental time is important for cognitive development (the coefficient on maternal time is larger than paternal time) and nutrition is also important for cognitive development. Since the data limitation does not allow me to establish a causal relationship between cognitive ability and violence, then causal inferences may not be made on mediation for this variable. To conclude, I find that investment in children is an important channel through which violence could affect child development. Therefore, interventions aimed at improving the quality of material investments in the home, and nutrition can lessen the effects of violence

[26] Panel A corresponds to the DHS survey and MICS 2016/17. These are the waves that have detailed information on food consumption.

exposure on child development. [27]

2.7 Conclusion

In this paper, I analyze the intent-to-treat effect of violence on child development using the Boko Haram attacks in Nigeria as a source of exogenous violence exposure. Boko Haram is a terror and extremist group in the Northeastern Nigeria who oppose western influence. The group launched its first major attack in Nigeria in 2009 and since then have carried out numerous violent attacks especially in the northeast. I use a difference-in-differences identification strategy to identify effects. This method exploits exogenous variation in the timing of attacks across states. Using UNICEF's multiple indicator cluster surveys and the demographic and health survey for Nigeria, I find that children exposed to violence have height-for-age-z-scores that are 0.35 standard deviations lower than children of the same age not exposed to violence. They also lag on cognitive development by 0.18 standard deviations. The results show that there are no gender differences in the effect on height and cognitive ability. The results are robust to different model specifications and robustness checks.

I also examine potential mechanism through which the violence affected child outcomes. The results show that consumption of less nutritious foods, reduced parental engagement, and limited material resources are channels through which violence affects child development. Poor nutrition is the largest mediator for the effect of violence on height. It reduced the effect of violence by 6%. For cognitive development, material investment in the home and parental engagement with children, are the largest mediators for violence. They reduced the proportion of the variance in violence by 14% and 10%, respectively.

Comparing the estimates obtained in this paper to what has been found in the literature, the effects are similar in magnitude and significance. I find a reduction in height-for-age by 0.35 standard deviations. Minoiu and Shemyakina (2014) estimate the impact of the

[27] There are other mechanisms through which violence affects child outcomes which I do not exploit due to data constraints. For example, as recorded by UNCHR (2017), there were many public facilities and infrastructure destroyed during the violence which could have led to less access to basic amenities in the community.

Cote d'Ivoire civil conflict find an effect of 0.34 on children height while Akresh et al. (2012) estimating the Ethiopian violent conflicts find an effect of 0.45 standard deviations. Bundervoet et al. (2009) find that children exposed to the Burundi war were 0.525 standard deviations shorter than the non-affected cohort. For cognitive development, I find that exposure to violence in the first trimester reduces cognition by 0.09 SDs, which is similar to what Rosales-Rueda (2014) found (-0.1 SDs).

Looking beyond the present, shocks to children health have been shown to affect adult outcomes. Alderman, Hoddinott and Kinsey (2006) find that Zimbabwean children exposed to violent conflicts and malnutrition in childhood were 0.049 standard deviations shorter than children not exposed to the shocks. This translated into being 3.4cm shorter in adolescence, having 0.85 years less of schooling and a loss of lifetime earnings of about 14 percent. Akbulut-Yuksel and Yuksel (2017) find similar results. They show that German children exposed to violence in childhood were 2cm shorter in adulthood than those not exposed to violence. These evidence point to the fact that child development has serious implications on adult stature, education, the labor market outcomes.

Therefore, the results presented in this paper have important implications for policy and humanitarian assistance. Since the current evidence suggests that stunting occurs before age three, with little or no catch-up growth in later years. Relief and assistance should focus on children *in utero* and under three, to alleviate the negative effects of the violence before it scars children permanently. Interventions that improve nutrition, parental engagement with children and material investments can also be effective in improving child outcomes.

3. INTERGENERATIONAL TRANSMISSION OF HUMAN CAPITAL: EFFECTS OF MATERNAL EDUCATION ON CHILD EDUCATION

3.1 Introduction

Despite a general increase in global educational attainment over the past decades, there remain high numbers of out-of-school children. This is a major concern for governments and development organizations. According to UNESCO (2019), about 258 million children, adolescents and youth were out of school in 2018, representing one-sixth of the global population of this age group.[1] These numbers suggest that many children may not reach their full potential. Since human capital formation is a good predictor of future outcomes, it is crucial to understand factors that affect the production of human capital, and are relevant in designing effective policies.[2] One factor is parental education, which has been shown is important for increasing children's education. However, the underlying mechanisms of effects are unclear. Therefore, the purpose of this paper is to estimate the causal relationship between maternal and child education and to further understand the underlying mechanisms.

Educating women is often considered an important tool for improving child outcomes from infancy to adolescence. Currie and Moretti (2003) find that maternal education has significant positive effects on infant health. In childhood and adolescence, Carneiro, Meghir and Parey (2013) show that maternal education improved cognitive skills and reduced behavioral problems. Maternal education also influences time use and increases the time mothers spend with their children (Andrabi, Das & Khwaja, 2012). There is also evidence in the psychology literature that education affects parental beliefs and behavior, and accounts for part of children's success. Davis-Kean (2005) note that parents' education can affect child achievements indirectly through stimulating home environments and parents' achievement

[1] Sub-Saharan Africa has the highest rate of out-of-school children (31%) followed by Southern Asia (22%) and Northern Africa and Western Asia (16%).

[2] Future outcomes include adult health, wages, criminal behavior (Clark & Royer, 2013; Heckman, Humphries & Veramendi, 2018; Lochner & Moretti, 2004).

beliefs. Although different studies have also examined the effects of paternal education (Agüero & Ramachandran, 2018; Black, Devereux & Salvanes, 2005; Chevalier, Harmon, O'Sullivan & Walker, 2013; Holmlund, Lindahl & Plug, 2011; Lundborg, Nilsson & Rooth, 2014), only few studies including Agüero and Ramachandran (2018) and Chevalier et al. (2013) find positive significant effects.[3]

In this paper, I use a fuzzy regression discontinuity (RD) design that exploits a natural experiment in Nigeria to estimate the effects of maternal education on child education.[4] Using a quasi-experimental design overcomes the endogeneity bias from naive estimates when I regress child education on maternal education because the latter is correlated with unobservable characteristics, such as family background and ability that may also affect the schooling of the child. In 1976, the Nigerian government implemented the Universal Primary Education (UPE) reform to provide free primary education to six-year-olds starting school. With increased access to schools, primary school enrollment increased from 49% in 1975 to 86% in 1978, and by 1981 had increased by over 7 million with over 16,000 newly constructed schools (Federal Office of Statistics Nigeria, 1984). The timing of the reform provides a source of exogenous variation in parental education. Although the reform varied in intensity across regions, I restrict the sample to children in the highest reform intensity areas.[5]

This methodology allows me to apply a regression discontinuity design and separate the effect of maternal education from the total effect of the reform. Since primary school officially starts at age 6 and only those born in 1970 or later were eligible for the reform, I compare

[3]Generally, the estimates from twin and adoption studies approaches find that paternal education has significant effects on child education (Behrman & Rosenzweig, 2002; Bingley, Christensen & Jensen, 2009; Björklund, Lindahl & Plug, 2006; Plug, 2004).

[4]Nigeria provides a good setting for study owing to the following reasons: 1) High gender differences in educational attainment and literacy-female literacy rate in 2017 was 59% while male literacy rate was 71%; 2) High number of out-of-school children (40% of for girls and 28% for boys).

[5]Since the goal of the government was to achieve 100% primary school enrollment, more schools were constructed in areas that had low pre-reform primary school enrollment rates. I follow Larreguy and Marshall (2017) by constructing a reform intensity variable using the pre-reform primary school enrollment rates across local governments and gender. The highest intensity area is where no one born between 1960 and 1969 had completed primary school.

the outcomes of school-age children whose mothers were born shortly before and after 1970. I first show that the reform created a discrete jump in years of education completed for mothers born in 1970. Using a two-stage least squares method, I instrument for maternal education with the reform eligibility. One caveat for the interpretation of the results is that it applies to children of mothers living in high intensity areas and are born close to 1970 i.e. those whose mothers' education was impacted because of the reform. To estimate the total effects of the reform, I use a difference-in-differences (DID) approach where I exploit variation from the timing and differential intensity of the reform across regions.

The institutional context supports using the RD design. It is difficult to precisely manipulate the running variable (mother's year of birth) since school officially starts at age six so the reform affected women born in 1970 and later.[6] I validate this assumption by testing for bunching of observations at 1970 in the distribution of maternal year of birth and I find no evidence of manipulation or discontinuity in the trend of births. This suggests that the results are not driven by shocks related to changes in the population. Although according to Bray (1981) the reform was announced in 1974, there is no bunching at 1975 (the earliest cohort whose birth might have been timed to benefit from the reform). Also, it is difficult for parents to know the exact locations of where the schools will be built, and is further complicated by parents having to wait six years before their children are enrolled in school. However, since they are always in the treated cohort, the timing of their birth does not affect identification.

Using the demographic and health surveys dataset, the findings indicate that maternal education improves child education. One extra year of maternal schooling increases the probability of children being on track in school by 4.3 percentage points (0.09 SD), the probability of completing primary school by 4.7 percentage points (0.11 SD), and the probability of attending secondary school by 4.7 percentage points (0.13 SD). These effects correspond to 13%, 22% and 29% of baseline, respectively. The results on gender heterogeneity show

[6] The fuzzy RD design allows overage enrollment.

that the positive effects of maternal education are concentrated among girls. The reduced form estimates from the RD and DID approaches are similar, which further validates the main findings of the study. I confirm the smoothness of covariates across the threshold and fake cutoffs did not produce discontinuities in outcomes. Although a bandwidth of 7 years is used for the main specification, the results are robust to shorter bandwidths. All estimates are robust to the inclusion of a variety of controls, kernel functions, and functional forms. In comparison with other studies on developing countries, the magnitude of the total effect on grade-for-age for the full sample (0.08 SD) is larger than Sunder (2018) for India (0.03 SD) and Akresh, Halim and Kleemans (2018) for Indonesia (0.04 SD).[7] The different magnitudes of the effects across different studies provide implications for policy and suggest that the long-run returns to school constructions reforms might be larger in Africa. A plausible explanation for the differences include lower educational attainment in sub-Saharan Africa compared to other regions of the world.

Parental education can directly affect child education or may affect the choice of other inputs that improve child outcomes. Maternal education affects paternal characteristics and wealth, which are inputs in the child's human capital production. By sequentially including each mediator in the regression specification, the analysis reveals that 7% of the effect of maternal education on child outcome is mediated by paternal education, while 9% is mediated by a higher wealth status. While assortative mating accounts for part of the effects of maternal education, I do not find evidence that it totally drives the results.[8] This finding is consistent with Akresh et al. (2018); Carneiro et al. (2013); Cui, Liu and Zhao (2019); Lundborg et al. (2014). Since I do not have a valid instrument for father's education, I cannot estimate the effect of paternal education on child education. However, this is an area

[7] The comparison is with reference to the estimates from the DID approach. Another explanation for the differences in magnitude could be due to comparing the average treatment effects with local average treatment effects (as in Sunder (2018).

[8] Although the reform affected both male and female, accounting for assortative mating requires instrumenting for paternal education which I could not perform because the instrument is weak for fathers. A possible explanation is the high spousal age difference, over 90% of fathers in the sample were born before 1970.

for future research. I also find that more educated mothers are involved in decisions relating to their children's education and health. This finding highlights an important channel as healthy children are more likely to attend school. Regarding other potential mediators, I find no evidence that fertility decisions and labor market conditions mediate the effects of maternal education.

While there is evidence on the causal relationship between maternal and child education, there are still areas for further study and this paper addresses three gaps in the literature.[9] First, the current findings on the relationship are mixed. Carneiro et al. (2013) find that maternal education leads to large improvement in children outcomes in the US. The results in Lundborg et al. (2014) also show that maternal education improved son's skills and health status. Using a change in compulsory schooling in Norway as a source of variation in education, Black et al. (2005) find a small effect of mothers education on son's education and Chevalier et al. (2013) find that parental education and income do not affect children schooling in Britain.

Second, the underlying mechanisms for the influence of maternal education are understudied. In general, the literature on intergenerational spillovers points to assortative mating acting as a mediator for maternal education (Agüero & Ramachandran, 2018; Carneiro et al., 2013; Cui et al., 2019; Lundborg et al., 2014), but the evidence on how labor market conditions, and wealth status (improved living conditions) may transmit the effect of maternal education to children is limited and not conclusive.[10] Third, most of the studies on the causal relationship between maternal and child education have come from developed countries with limited evidence for developing countries. The evidence across these countries might be different since most of the sources of variation exploited for studies in more advanced countries are at the secondary school or college level. This might not be directly

[9]There are studies that examine the effects of maternal education on child health- Chou, Liu, Grossman and Joyce (2010); Currie and Moretti (2003); Keats (2018). However, that is not the focus of this paper.

[10]Agüero and Ramachandran (2018) find that more educated women have fewer children and postpone childbearing. However, Cui et al. (2019) and Andrabi et al. (2012) find no effect on fertility but an increase in labor force participation (Cui et al., 2019).

applicable to developing countries where most of the reforms have taken place at the primary school level. Furthermore, the majority of the world's population and out-of-school children live in developing countries. Lastly, the level of economic development and functioning of institutions vary widely across these countries.

The limited evidence on developing countries show that maternal education improved test scores and time mothers spent with their children (Pakistan - Andrabi et al. (2012)), increased educational attainment (Zimbabwe - Agüero and Ramachandran (2018)), enrollment, test scores, and college aspiration (China - Cui et al. (2019)).[11] However, the source of variation in maternal schooling in this paper is different from Agüero and Ramachandran (2018) and Cui et al. (2019), who exploit variation from secondary school policies. Therefore, we might expect that the results from these studies might be different from what I find in this paper and may not be directly applicable to contexts where students face different sets of constraints to schooling.[12] Given these reasons, researchers have limited understanding of how policies can incorporate the importance of maternal education to influence the educational outcomes of school-age children in developing economies. In that regard, the results from this study is relevant to countries who have implemented similar primary school reforms such as Indonesia, Kenya, Sierra Leone, Uganda etc.

This paper builds on and contributes to the literature on child development by addressing the issues noted above and improves our understanding of intergenerational spillovers. This is one of the few studies to causally identify the effects of maternal education on child education in Africa and identify the long-term benefits of large school policies. Findings from this study hold important implications for educational and anti-poverty policies as results

[11] Akresh et al. (2018); Mazumder, Rosales-Rueda and Triyana (2019); Sunder (2018) examine the effects of parental exposure to school construction reforms on child education. They regress child outcomes on parental exposure to the reform. The concern with presenting only the reduced form effects is that it is difficult to distinguish between parental effects and direct exposure to schools since the availability of schools could directly affect child outcomes or affect other outcomes in the community that can differentially improve the outcomes of the child.

[12] For studies that exploit variation in primary school reforms, the constraint that children might face is mostly unavailability of schools to attend, which is different from constraints that apply to students affected by compulsory schooling laws. Also, the labor market opportunities available to primary school graduates differs for those who complete secondary school.

from causal studies hold different lessons from results on correlational studies. Parental education as an input in children's outcomes can be influenced by policymakers compared to other inputs such as parenting style (Holmlund et al., 2011). This study also contributes to the literature on intergenerational transmission of human capital in both developed and developing countries. Studies on the intergenerational persistence of education are important for understanding intergenerational mobility since the literature suggests that there is a high correlation between parental and child income (Carneiro et al., 2013). Furthermore, government policies that improve living standards such as increase in access to infrastructure facilities can contribute to improving the educational outcomes of children. Finally, I provide evidence to assist policymakers prioritize among alternative potential investments. Back of the envelope calculations show that for the first generation, the reform increased educational attainment by 0.48 SD with an implied cost of 6,614 NGN (in 2010 Naira) or $43.5 per 0.1 SD increase.

The remainder of this paper proceeds as follows: Section 2 provides background information on education in Nigeria. Section 3 discusses the data and empirical strategy. Section 4 presents the main results, Section 5 shows robustness checks and potential mechanism, and Section 6 concludes.

3.2 The Nigerian Education System

3.2.1 Country Overview

Nigeria is the most populous country in Africa with an estimated population of over 190 million. The World Bank categorizes Nigeria as a lower-middle-income country with a Gross National Income (GNI) per capita of $2,028 and a life expectancy at birth of 54 in 2017.[13] Before independence in 1960, Nigeria was divided into three regions: east, north, and west. A mid-western region was created in 1963 and each region retained a substantial measure of self-government (Akinyele, 1996; Babalola, 2016). Subsequently, these regions were divided

[13]Lower middle-income economies have GNI per capita between $1,026 and $3,995.ht-tps://data.worldbank.org/country/nigeria.

into states: 12 states by 1967, 19 states and a federal capital territory (FCT) by 1976, 21 states by 1987, 30 states by 1991 and 36 states by 1996. There are currently 36 states, a capital, and 774 local governments.[14]

The structure of the education system in Nigeria is similar to the systems in most countries in sub-Saharan Africa and many developing countries. The official school starting age is six, although some children start at five. They spend six years in primary school and three years in junior secondary school. The first nine years of school forms the compulsory basic education, although monitoring and compliance are weak. After junior secondary school, students can continue along the academic track to spend three more years in senior secondary schools or can choose vocational or technical training. Children who complete senior secondary school can continue to institutions of higher learning.

3.2.2 The 1976 Universal Primary Education Reform

Before the government implemented educational reforms across the different regions, missionary education was the main source of schooling. Subsequently, different regions in the 1950's implemented free primary education for students. The free education reform resulted in almost doubling enrollment in the western region in 1955 and the eastern region in 1957 (Abernethy, 1969; Csapo, 1983). However, the free education in the Eastern region was restricted to the first two years of primary school by 1961 (Oyelere, 2010). The western region was the forerunner in education and educational imbalances across the different regions became substantial after independence (Osili & Long, 2008). The limitations in educational expansion in the Northern region was primarily due to Islamic religious practices and traditional attitudes towards girls and women (Csapo, 1983; Osili & Long, 2008). These regional differences were amongst the reasons the universal primary school reform was introduced.

Nigeria, a major producer of crude oil and natural gas, experienced an oil boom in 1973 caused by the increase in oil price. The federal government saw the boom as an opportunity

[14]Local governments are responsible for the collection of fees and levies, provision of public works and services, provision of health and social services as well as payment of primary school teachers' salaries (Smith & Owojaiye, 1981).

to invest in education and implemented the UPE reform in 1976 (Csapo, 1983). To show the government's commitment to education, it is stated in the 1977 National Policy on Education that "education will continue to be highly rated in the national development plans, because education is the most important instrument of change as any fundamental change in the intellectual and social outlook of any society has to be preceded by an educational revolution." During the oil boom, a majority of public expenditure was on primary education, transport, steel, construction, and auto assembly (Pinto, 1987).

The UPE reform is a nationwide free primary education reform introduced by the federal government in September 1976. Since primary school commonly starts between ages 5-6, children starting school after 1975, (i.e. those born after 1969) should be eligible for the reform, while those born before 1970 should be too old to benefit from the reform. The federal government disbursed money to states for the construction of schools, classrooms and teacher-training institutions. The reform is considered one of the largest educational reforms in Africa (Bray, 1981; Larreguy & Marshall, 2017). A total of over 700 million NGN ($551M) was disbursed differentially to states for the reform between 1974 and 1979, with larger amounts apportioned to northern states.[15] The government targeted 100% enrollment in class 1 at the beginning of the UPE reform (Federal Ministry of Economic Development and Reconstruction, 1975) and 100% primary school enrollment by 1981 (Csapo, 1983). Since educational attainment varied widely by region, with rural areas and northern states having a less educated population, the introduction of UPE should have larger impacts in these regions. Overall, the structure of the reform provides a natural experiment to analyze the impact of maternal education on child education.

The reform increased school availability across the country. The number of primary schools and classroom increased substantially. 16,246 new schools were constructed and enrollment increased by over 7 million between 1975-1980 (Federal Ministry of Economic Development Reconstruction and Central Planning Office, 1981). Figure A.22 shows that

[15] The dollar equivalent is in 1976 dollars.

many public schools were founded in 1976 which confirms that the UPE reform is a very big policy change. The reform resulted in large increases in primary school enrollment in states with low prior educational attainment, which are areas concentrated in the North. Primary school enrollment increased by 557% in Kano, 442% in Kaduna and 263% in Benue state between 1975-1977 (Csapo, 1983). Primary school gross enrollment for girls increased from 39.87% in 1976 to 99.23% in 1982 (World Bank, 2018).[16]

The reform was associated with many problems despite its achievements. These include a shortage of teaching staff, use of unqualified teachers and poor equipping of schools across all states (Federal Ministry of Economic Development Reconstruction and Central Planning Office, 1981). However, teacher supply and quality improved in all states in the 1980s. The reform ended in 1981 after an unanticipated decline in oil prices and when the federal government handed over the financing of primary schools to state and local governments Csapo (1983). This resulted in lower growth of primary school enrollment. After the federal government ceased to provide grants for teachers, most states except those in the west reintroduced school fees (Larreguy & Marshall, 2017; Osili & Long, 2008). However, primary school enrollment continued to increase beyond the end of the reform which suggests that availability of schools rather than fees was responsible for the increasing trend.

3.3 Data and Empirical Strategy

3.3.1 Data

I use data from the individual-level responses to the Nigerian Demographics and Health Survey (NDHS) for 2003, 2008 and 2013. NDHS is a project of the United States Agency for International Development (USAID) and the Nigerian National Population Commission (DHS, 2003-2013). The NDHS is nationally representative and consists of a broad range of individual and household level characteristics. I use a sample of children whose mothers were born between 1960 and 1980. All children in the analysis are between the ages of 5-17

[16] https://data.worldbank.org/indicator/SE.PRM.ENRR.FE?end=2014locations=NG-ZG-XM-XLstart=1970view=chart

and living with their mother. I discuss issues arising from selection into the sample based on this age group in the robustness section.

Education: Maternal education is the number of completed years of schooling for mothers. This is the main explanatory variable used in the study. I also use other measures of educational attainment: primary school completion, incomplete and complete secondary schooling to check if the reform induced some mothers to have more than primary education. Since the children in the sample are not old enough to have completed their education, I focus on three outcomes that measure human capital accumulation. The first outcome is grade-for-age, which measures a child's progress through school and captures whether a child is on track in school. It is an indicator variable that takes on the value of one if the difference between the child's age and grade is at most six and zero otherwise. The other outcomes are the probability of completing primary school and the probability of ever attending secondary school. The latter outcomes are restricted to children who are at least 12 years old and should have completed primary school.[17]

Cohort: The year of birth determines whether a woman falls into either an old or young cohort. I define the young cohort (Post UPE) as mothers born between 1970 and 1980, that is, those who should be affected by the reform. Since primary school officially starts at the age of six in the country, children starting primary one in 1976 should have been born in 1970. The older cohort are those born before 1970.

Intensity: Federal allocation to states for the UPE varied significantly, with larger amounts disbursed to states with lower school enrollment (northern and eastern states). Although state expenditure is a measure of intensity, it does not capture the actual reform intensity as there were uneven implementation within states (Larreguy & Marshall, 2017).[18] To define a finer level of intensity, I construct a variable following Larreguy and Marshall (2017) that captures the spatial variation of the reform using differences in educational at-

[17]This also includes children who are not yet up to 12 but have completed primary school, most likely due to double promotion.

[18]Missing data on number of actual schools and classrooms constructed in each state does not allow us use this as an alternative measure of intensity.

tainment across local government areas.[19] The intensity measure is the proportion of women born between 1960 and 1969 who had not completed primary education in a local government area (LGA) and ranges between 0 and 1. Zero represents total pre-reform primary enrollment in an LGA while one implies that no woman born between 1960 and 1969 in an LGA completed primary education. Since investments were made by states to reach universal primary school enrollment, more schools were built in areas that had fewer schools. Therefore, the intensity variable captures the difference between actual and potential enrollment. See Figure A.23 and A.24 for geographical variation in intensities. Darker areas on the map reflect higher UPE intensities and within states, UPE intensities are different. [20]

The intensity measure is defined based on current residence since the only information relating to where a mother went to school is how long she has lived in a particular area and is not available for all survey waves. Therefore I assume that area of residence is the same as where mothers attended primary school. I discuss issues relating to migration in Section 3.

Wealth index: This variable is a composite measure of the household's standard of living or economic status. The wealth index is calculated using easy-to-collect data on a household's ownership of selected assets, such as televisions and bicycles; materials used for housing construction; access to electricity and types of water access and sanitation facilities. The index is then classified into quintiles ranging from 1 (poorest) to 5 (richest). A higher wealth index means better living conditions such as better access to water and sanitation facilities, availability of electricity, improved flooring materials. It also includes possession of durable

[19]There are 774 local governments in Nigeria and 651 in the sample. The 2003 survey does not have identification at the LGA level, so I use clusters to define intensity. Clusters are smaller geographical units than LGAs.

[20]While Table A.27 shows the number of schools constructed during the reform (1975-1981) at the state level, it does not capture variation in the intensities of the UPE reform across smaller regions. However, we see from the table that more schools were constructed in areas that had fewer schools available in 1975 (which are predominantly northern states), and this correlates well with the intensity measure (0.49). Furthermore, to show that the intensity variable captures the intensity of the reform, from the data on school founding dates, the correlation between the number of schools opened across local governments and the intensity variable is 0.47. This is similar to Larreguy and Marshall (2017) (0.43). The measure of schools opened is not used in the main specification because founding dates are missing in a nonrandom way.

consumer goods such as radio, television, refrigerator and means of transport. These items are important in easing the lives of people. For example, having a means of transportation can reduce time of travel and increase access to services beyond walking distance. Radio and television are sources of news and information.

Descriptive statistics are presented in Table A.28. The mean age for a child is 10 years and 4 months and 52% of the sample is male. The average education for a child in the sample is 3 years. The average mother is 39 years old and has 4.3 years of education. This points to the fact that we are dealing with women with low levels of education. The average education in the sample is typical for developing countries. According to Barro and Lee (2013), the mean years of schooling for women aged 25 years and above is 4.3 in Nigeria, 4.6 in Bangladesh, 3.2 in India, 5.4 in Kenya, and 5 in Guatemala. About 67% of households live in rural areas.[21]

3.3.2 Empirical Strategy

While I can exploit the interaction of the temporal and spatial variation in the intensity of the reform to examine how maternal exposure to the reform affects the educational outcome of her children, the results will produce the total effects of the program on the second generation (reduced form effects), and will not yield the effect of an additional increase in maternal education. The total effects of the reform will include the effect of the availability and long-term presence of schools that children could attend, parental effects, non-random school construction and other factors that might have changed in the area in response to the reform, all of which can differentially affect child outcomes across the treatment and control groups. Therefore, I cannot use the interaction of the temporal and spatial variation in the reform as an instrument for maternal education because the exclusion restriction is not likely to hold. While the reduced form effect is important, in this subsection I focus on the effect of increasing maternal education by one year and return to the total effects in the next subsection. I use an RD design to estimate the direct effect of maternal education and

[21] This is representative of the country where more than half of the population live in rural areas.

a DID design to estimate the total effects of the program. Using an RD approach will allow me quantify how much of the total effects is explained by the impact of maternal education.

3.3.3 Regression Discontinuity

I use an exogenous variation in schooling from the UPE reform to deal with the endogeneity problem. The identification comes from the UPE reform which provides variation in maternal education that is uncorrelated with the error term. Since the official school starting age is six, girls born after 1969 should benefit from the reform.[22] I restrict the sample to households in the highest intensity areas and use a fuzzy regression discontinuity design (Imbens & Lemieux, 2008; Lee & Lemieux, 2010) to estimate the effect of maternal education on child education outcomes by instrumenting for maternal education with the reform eligibility.[23] The RD design provides a causal approach to estimation compared to merely regressing child schooling on maternal education might yield unreliable estimates. Education is correlated with unobservables such as family background, family income, neighborhood characteristics, and community resources, that may affect the schooling of the child. The ideal experiment is to randomly allow some women to attend school and leave others without access to education, and then compare the outcomes of their children. However, in the absence of such randomization, the RD design provides as-good-as-random variation in maternal education.

The sample is restricted to households living in the highest UPE intensity areas which allows me to argue that children whose mothers are on either side of the threshold are similar and the only difference between them that could affect their educational outcomes is when their mother was born relative to the start of the UPE reform. Put differently, I am implying

[22] According to Bray (1981) and Aderinto (2015), to determine the age of a child in the absence of a birth certificate, the crude but usual method adopted by the government was the “arm over head task”. A child was asked to reach over the head and touch the opposite ear. If the child could not do it the child was considered under age; if the child could “just” do it, the child was considered six years of age and if the child’s hand reached under the ear, the was considered over-age for school entrance.

[23] Since the running variable is discrete, there might be issues relating inference when using standard RD designs. I follow Lee and Card (2008) by choosing a parametric functional form so that I can cluster the standard error on maternal year of birth.

that the children of the older and younger cohort of mothers are exposed to similar direct effects of the UPE reform which could be through children attending the same school their parents attended. And if other factors changed in these areas as a result of the reform, it will affect the control and treatment group children similarly. To show that the intensity areas are similar in other dimensions, I regress geographical area characteristics available in the data on the reform and find no effects of differential area characteristics (see Appendix Table A.41). These characteristics include population, rurality and economic measures.

The main identifying assumption of the RD design is that all determinants of outcomes vary smoothly across the reform eligibility threshold. Put in other words, individuals should not be able to manipulate where they are relative to the cutoff. It is unlikely that individuals can precisely manipulate this because it is difficult for parents of children who were born around the time of the reform to precisely manipulate when their children will be born. The official primary school starting age is six, therefore children born in 1970 and later should be eligible for the reform while those born before 1970 should be ineligible. However, allowing for the possibility of overage enrollment does not alter the identification since I am using a fuzzy RD design. Although the reform was announced at the beginning of the school year in 1974, it commenced in 1976 and this means that the oldest cohort whose parents could have timed their births to benefit from the reform will be born in 1975 and start school in 1981. This does not affect identification because even if they were born later than 1975 they will still have benefited from the reform.

The equation of interest is:

$$Y_{im} = \beta_0 + \beta_1 M_m + \beta_2 X_{im} + \epsilon_{im} \tag{3.1}$$

where the Y is the outcome of interest for child i of mother m. M is mother m's years of education. X is a vector of control variables including observable characteristics that should not significantly affect Y but increase the precision of the estimates. ϵ captures other unobservable factors affecting Y. In the presence of endogeneity in maternal schooling, equation 1 gives the correlation between maternal and child education. In equation 2, I estimate the effect of the reform on maternal education (first stage) and in equation 3, the reduced form effects:

$$M_{im} = \gamma_0 + \gamma_1 T_i + \gamma_2 f(R_{im}) + \gamma_3 T_i \cdot f(R_{im}) + \gamma_4 X_{im} + \mu_{im} \tag{3.2}$$

$$Y_{im} = \delta_0 + \delta_1 T_i + \delta_2 f(R_{im}) + \delta_3 T_i \cdot f(R_{im}) + \delta_4 X_{im} + \epsilon_{im} \tag{3.3}$$

where T is a dummy variable that takes on the value of one if the mother of child i was born in 1970 or later. R represents maternal year of birth for child i but normalized to zero. The running variable is maternal year of birth and the threshold is 1970. f(R) is a function of the running variable and captures the relationship between R and Y. To allow the slope to change on either side of the threshold, I interact T with f(R). The first stage regression in equation 2 examines whether maternal education was affected by the reform, with γ_1 being the effect of the reform on maternal education. δ_1 in equation 3 gives the total effect of the reform on Y. To account for the fact that children of older mothers are older and have more years of education on average, I include dummies for the age of the child.[24]

I employ the two-stage least squares (2SLS) method to identify the effect of maternal education on child outcomes. I instrument for maternal education with T, which describes the fuzzy approach of the RD design. The fuzzy RD design allows for overage enrollment by the cohort born shortly before the reform. I use the 2SLS to identify the local average treatment effect (LATE) for compliers. The LATE is the average effect on compliers near the cutoff. This is analogous to re-weighting the discontinuity in outcomes by the discontinuity in treatment. The LATE may therefore be different from the average treatment effects since it applies to those whose education was influenced by the UPE reform. In the preferred model specification, I model the relationship between R and Y as linear and use triangular

[24] I also run an alternative specification where I exclude the age of the child and the results are unchanged.

kernel weights.[25] Standard errors are clustered at maternal year of birth. To determine the bandwidth for the main specification, I conduct the leave-one-out cross-validation test on the preferred model specification.[26] Plotting the mean absolute error against the different bandwidths, Appendix Figure A.14 shows that except for bandwidth 2, 7 years gives the smallest MAE. Also in Appendix Figure A.15, the first stage estimates become relatively stable after a bandwidth of 7.[27] Given these results, I use a bandwidth of 7 on either side of the cutoff in the preferred model specification. However, in the robustness section, I also present results for alternative bandwidths, kernels, and functional form.

3.3.4 Test of Identification

As previously described, if other determinants of outcome vary discontinuously at the threshold, then the identifying assumption will not hold since I will not be able to attribute the change in outcome to treatment. Also, while it is unlikely that year of birth was manipulated because of the reform, one way to test this assumption is by examining whether there is evidence of bunching around 1970 in the distribution of maternal year of birth. I should observe a smooth distribution and no bunching at the threshold or discontinuity in the trend of births.

Figure A.25 shows the density function for maternal year of birth. While there is no clear jump at 1970, there are other jumps in the distribution which are at multiples of fives. This pattern is common in survey data in developing countries, where we see people rounding up their ages, especially the less educated. Since the survey year intervals are in multiples of five years (2003, 2008, 2013), there is a pattern of people saying they are 30, 35, 40, 45, etc. While these rounding estimates could potentially bias the results, I follow recommendations from Barreca, Lindo and Waddell (2016) to control for heaping. By allowing the non-heaped and

[25] The use of triangular kernels is to assign more weights to observations closer to the threshold. The weight measures the distance in maternal year of birth from 1970. At the threshold, the weight is one, and keeps declining till it reaches zero for observations outside the bandwidth (meaning they are not included in the regression).

[26] However, this test is more suited for continuous running variables.

[27] The estimates are larger and imprecise at smaller bandwidths because the number of clusters shrinks.

heaped data to have different intercepts or slopes and same treatment effects, this approach would remove any bias from the treatment effect. I discuss the results of the test in Section 5. To show that the age distribution in Figure A.25 is a general pattern in the survey, Appendix Figure A.28 presents the distribution of year of birth for women born between 1950 and 1993. There is no evidence of distinct heaping at a point in the data, which provides more evidence that there is no precise manipulation of the running variable or discontinuity in the trend of births. These results further suggest that the results are not driven by shocks related to changes in the population.

In Table A.29, I present evidence to support that other characteristics that could affect Y are smooth across 1970. The characteristics include age, gender, and region of residence (urban-rural). These variables should not be affected by the reform. If this assumption does not hold, then it suggests that there are different types of people across the threshold and perhaps evidence of sorting. I use child characteristics to predict the outcomes of interest and then test if the predicted outcomes vary discontinuously at the threshold. The results are in Table A.29 and shown graphically in Appendix Table A.9. The estimates are zero and not statistically significant. Rather than using all covariates in a single model, in Appendix Table A.42, I focus on the covariates individually, and the results are consistent with Table A.29.

3.3.5 Difference-in-Differences

A policy-relevant implication for this analysis is providing evidence to help policymakers prioritize across different investments and improve effectiveness of education expenditure. In this section, I estimate the effect of school availability on child outcomes. To do this, I employ the identification strategy used by (Larreguy & Marshall, 2017) to exploit the temporal and spatial variation of the UPE reform using year of birth and area of residence.[28] Since the UPE reform affected all eligible students born after 1969, I define the control group as those born before 1970 and the treated cohort as those born after 1969. This forms the first source

[28] This method has also been used by Chou et al. (2010); Duflo (2001); Osili and Long (2008).

of variation. As described in section 3.1, I use variation across LGAs to define the intensity of the reform. Specifically, I use the proportion of women born between 1960 and 1969 in each LGA, who have incomplete primary education to define the intensity variable. The rationale is that since the government's goal was to achieve 100% primary school enrollment, areas where primary school enrollment was low before the reform will have more schools built and have a higher impact of the reform. The spatial intensity of the UPE reform is the second source of variation.

Therefore, the two different sources of variation allow me to identify separately the effect of the UPE reform from the effect of being in a UPE eligible age group and living in a high intensity area. The difference-in-differences assumption implies that in the absence of the UPE reform, the high UPE intensity areas would have continued along the same trend in outcomes. I use a sample of all children whose mothers were born between 1960 and 1980 in all intensity areas. To estimate the effects of the UPE reform on child schooling, I estimate the reduced form regressions specified below:

$$\begin{aligned} Y = \delta_1(PostUPE \cdot Intensity) + \delta_2 Intensity + \delta_3 PostUPE \\ + \delta_4 X + \delta_s + \delta_t + \delta_{st} + \delta_r + \epsilon \end{aligned} \tag{3.4}$$

Where Y represents the different schooling outcomes, δ_1 is the reduced form effects of the reform on children's schooling. I include time-fixed effects (δ_t) to capture trends in education that are not correlated with the reform. The inclusion of state fixed effects (δ_s) absorbs time-invariant characteristics across states. The specification also includes state-specific linear time-trends (δ_{st}) to allow states have differential trends in the pre-period and control for state-specific unobservables correlated with the reform and child outcomes. X contains mother and child demographic characteristics such as gender, age and urban dummies to improve the efficiency of the estimates and δ_r is the survey round fixed effects. In an alternative specification, I interact other government programs implemented in 1976 with the cohort variable. This controls for other programs implemented around the time of the UPE that could have differentially affected the treatment and control groups. Standard errors are clustered at the state level.[29]

Intensity is defined based on current residence, so I assume that area of residence is the same as where mothers attended primary school. The effects I find would be an overestimate if children who are with low academic abilities moved from high intensity areas to low intensity areas. Or if children with high academic abilities moved from low intensity areas to high intensity areas. While there is evidence of migration around regions in the country, I argue that selective migration do not explain the results. First, the Nigerian 2010 Internal Migration Report shows that 75% of the population had not moved from their LGA or state within the last ten years and employment is a major reason for people moving. Also, according to Larreguy and Marshall (2017), 75% of the migration was urban-urban or rural-rural in areas that had similar intensity levels. Second, the common reasons why people move is for marriage and employment reasons. Choosing where to live based on school location is not as common in Nigeria as in many developed countries. The common reason why people move for educational reasons is to attend college or universities. However, for the sub-sample with information on how long a woman has lived in an area, I define a migrant as a woman living in an area where she did not attend primary school, and a non-migrant otherwise. Then in Section 5, I show that the findings for non-migrants are not different from the full sample, which suggests that the effects are not driven by people moving.[30]

Another concern is selection into motherhood which implies selection into the sample. That is, if the UPE reform altered fertility outcomes, then the estimates could be biased

[29] I cluster at the 36 states and 1 FCT that existed in the country when the survey was administered. However, results are similar when I cluster using the 19 states that existed in 1976 or and gain more precision when I at the survey cluster level (see Table A.36).

[30] Only the 2003 and 2008 waves collect information on migration. To test if the reform induced people to move around the time of the reform, I regress an indicator variable for the likelihood of migrating on the reform. The coefficient on the reform is 0.048 with a P-value of 0.213. I also present results for the full sample in Appendix A.45. The results are consistent with the results for the non-migrant sample. Larreguy and Marshall (2017) note that about 77% of respondents in the Harmonized Nigeria Living Standards Survey (HNLSS) had not moved and Osili and Long (2008) find that two-thirds of respondents in the DHS 1999 wave had not moved.

and the results might reflect a quantity-quality trade-off.[31] To address this concern, I regress the total number of children a woman has on the UPE reform and I find no evidence that the reform affected fertility (see Table A.37 and Appendix Table A.40).[32] Another related concern is the selection of children into the sample based on their age. I link the educational data of children under the age of 18 to their mothers' information (most of which are still living with their parents).[33] However, this is not so much of a concern here since children in the sample are of primary and secondary school age and are less likely to leave home before completing secondary school.[34]

3.4 Results

In this section, I start by examining the effect of the reform on maternal education. This represents the first stage analysis and then I explore if increased maternal education as a result of the reform improved children education outcomes.

3.4.1 RD Design: The Effects of UPE on Maternal Education

Figure A.26 shows the effect of the reform on maternal education using the highest intensity area sample. Using the maximum bandwidth of 10 years on either side of the cutoff, the graph shows the average education for each birth cohort using the raw data. Since the earliest cohort to have benefited from the 1976 reform are those born in 1970, there is a jump in educational attainment at 1970. The corresponding regression estimates

[31] For example, if the reform induced more educated women to have fewer (or more) kids, then there will be a change in the sample composition because women who would have otherwise had kids with good (bad) outcomes now have more or fewer kids than they would have had in the absence of the reform. Then the effects I show will be biased and driven by the fact that there are more or fewer kids with good (or bad) outcomes.

[32] To address concerns about the reform affecting the timing of fertility, since I condition on the age of the parent, the parameter of interest will not reflect the effects of fertility timing (Oreopoulos, Page & Stevens, 2006).

[33] Only 0.09% of children are not living in the same house as their parents. Restricting the sample to children living with their parents alleviates concerns that the schools the children attend might differ from those they would have attended if they were not still living with their parents, which might affect their outcomes. Moreover, there is no evidence that children of UPE eligible mothers are more (less) likely to live away from home.

[34] Generally, in Nigeria, most children leave their parents' homes when they leave for college, employment or marriage.

are shown in Table A.30. Being born after 1969 and thus eligible for the reform increased maternal education by 1.3 years (54% of a standard deviation). The F-statistics from the first stage is 218.7, which provides evidence of a strong first-stage relationship.[35] Column 2 of Table A.30 presents the effects without controls and the results are similar to the base specification in column 1. Appendix Figure A.15 shows that the estimates are robust across different bandwidth specifications. The smallest bandwidth of 2 years yields an average effect of 1.4 years while the largest bandwidth of 10 yields an estimate of 1.3.

Having shown that the reform achieved its goal by increasing the average education of women, I now check that the effects I present are not picking up the general increasing trend in education. I conduct different falsification tests following Imbens and Lemieux (2008) to test for jumps at non-discontinuity points. I check for jumps at the median of the sample to the left and right of 1970. Using the sub-sample to the left, I create a 1965 placebo reform and a 1975 placebo reform using the sub-sample to the right of the cutoff. The placebo treatment groups are those born between 1965-1969, and 1975-1980 while the control groups are those born between 1960-1964, and 1970-1974, respectively. If the coefficient presented in column 1 is picking up a general trend in education, then the coefficients in column 3 and 4 should be positive and significant (spurious). However, that is not the case, the coefficients on the placebo reforms are not statistically significant. These test supports the identification that the exogenous change in education is brought about by the UPE reform.

In column 5, I show that the reform increased the probability of completing primary school, which was the goal of the reform. The reform increased the probability of women in the highest intensity region to have at least a primary education by 16 percentage points. There is also evidence that the reform induced some individuals to go beyond primary education (columns 6-7). The probability of having some secondary education increased by 5.8 percentage points and the probability of completing secondary school increased by 3.3

[35]As a validity check to show that the reform only affected areas in need of primary schools, in Appendix Table A.43 column 1, I show that the reform did not affect women living in the lowest intensity areas which are mostly southern areas. In column 2, the effect on women living in median intensity areas is positive but not significant at conventional levels (0.574).

percentage points.[36]

3.4.2 The Effects of Maternal Education on Child Education

Panel A of Table A.31 shows the results from the OLS estimation. Here I regress child schooling on maternal education. Across the three outcomes, all coefficients are positive and statistically significant. As previously discussed, maternal education is endogenous because it is correlated with other characteristics in the error term that also affect child schooling. However, since I have established an exogenous shift in maternal education that is not related to family characteristics or background, I can causally estimate the effect on child education by instrumenting for maternal education with the reform eligibility.

Panel B shows the reduced form estimates. Maternal exposure to the reform increases the probability that a child is on track in school by 5.7 percentage points (16% of baseline, 0.12 SD i.e. 12 percent of the outcome standard deviation). Children whose mothers were exposed to the reform are also 6.8 (32%, 0.17 SD) and 6.9 (43%, 0.19 SD) percentage points more likely to complete primary school and attend secondary school, respectively. The reduced form effects are also presented graphically in Figure A.11, with a clear jump at 1970 for all outcomes. In Panel C, I present the effect of increasing maternal education by one year on the outcomes of interest. The main specification uses a bandwidth of seven and triangular kernel for estimation. Grade-for-age increases by 4.3 percentage points (13%, 0.09 SD), the probability of completing primary school increases by 4.7 percentage points (22%, 0.11 SD) and the probability of attending secondary school increases by 4.7 percentage points (29%, 0.13 SD). The high F-statistics from the first stage across all outcomes provide further evidence to support the identification.[37] Following Anderson (2008), I present the

[36] Odunowo (2019) shows that the reform also improved literacy for women.

[37] These effects are larger than the OLS estimates and are in line with similar studies on intergenerational mobility (Carneiro et al., 2013; Oreopoulos et al., 2006). The ratio of the IV to OLS estimate ranges between 1.3 and 1.6. There are different reasons why this might happen: 1) the two-stage least squares (2SLS) estimate produces the local average treatment effect (LATE) for the group affected by the reform - and in this case, those at the bottom of the educational distribution- and should be higher for this group. 2) The classical measurement error in maternal education bias outweighs the omitted variables bias. See Oreopoulos et al. (2006) for more discussion.

False Discovery Rate (FDR) Adjusted Q-values for the different measures of schooling in the bottom Panel of Table Table A.31. The adjusted Q-values are interpreted similar to p-values and they correct for the increased likelihood of rejecting the null hypothesis when making multiple comparisons. While the Q-values are slightly larger than the p-values, they do not affect the interpretation of the results.

Appendix Figures A.16, A.17, and A.18 show the estimates across different cohort bandwidths. For grade-for-age, the estimates range from 0.039 to 0.058 (0.08-0.012 SD), 0.041 to 0.057 (0.10-0.14 SD) for primary school completion and 0.044 to 0.051 (0.12-0.14 SD) for attending secondary school. In all specifications, the estimates become stable after a bandwidth of 7, which justifies using 7 years on either side of the cutoff as the main specification. In Section 5, I will discuss the potential factors that could mediate the effect of maternal education. One limitation of the RD design is that the estimates are only relevant for the population near the cutoff i.e. women living in the highest intensity areas across the country and born close to 1970. However, I argue that the results can generalize to a wider population since many developing countries have similar universal primary education reforms. Therefore, the results in this study hold important policy implications for countries with similar educational levels and reforms.

3.4.3 Heterogeneous Effects

I test for differences across gender and regions (urban/rural). The results are presented in Table A.32. Panel A shows the effects are larger for girls; the results are statistically significant.[38] There are no statistically significant differences between the outcomes of children living in rural and urban areas.

[38]One concern with observing larger effects for girls might be that the sex-ratio at older ages are imbalanced because girls may be leaving off to get married. Thus, the lower proportion of girls might be driving the results. First, I plot the distribution of the sex-ratio (boys/girls) across different ages. Up until the age 14, the ratio is 0.5 but increases gradually to 0.64 by age 17. Since the there is evidence of a lower proportion of women at older ages, I test if it is not driving the results. Restricting the sample to those younger than 15, I test for heterogeneous effects across gender and find that the effects are still larger for girls. Therefore, changes in sex-ratio do not explain the results.

3.4.4 DID Design: The Effects of UPE on Maternal Education (full sample)

In this section, I discuss the total effects of the reform on child education using the full sample of children and a DID identification strategy. First, I estimate the effect of the reform on maternal education. Figure A.12 is a dynamic difference-in-difference graph showing the reform did not affect mothers born before 1970. Table A.33 provides the estimates on maternal education. Column 1 shows that the reform increased women's education. Specifically, moving from the lowest to highest intensity area increases education by 2.45 years. To put this in context, women living in a local government area with one standard deviation higher level of intensity have on average, one more year of education.[39] To assess the relevance of the UPE reform to maternal education, I test the null hypothesis that the UPE reform is jointly zero. The F- statistics from the first stage is 60.59.

Similar to the RD estimation, I show that the estimate is not reflecting the effects of other government programs implemented around 1976 (column 2). A concern could be that there were other programs implemented by the government around the time UPE was initiated in 1976 that differentially affected the treatment group and increased their educational attainment. If this were true, then the other programs could potentially confound the main estimate. To check for this, I interact the cohort variable with the 1976 state expenditures on health and information and the 1973 state population. The coefficient in column 2 remains unchanged, which provides the support that the estimates are not confounded. I show in column 3 that the estimates are not picking up a general trend in education by restricting the sample to women who were too old to benefit from the reform.[40] While columns 4-6 show that the reform induced some individuals to have more than primary education.

[39] If I use the highest education level attained instead of years of education, the same conclusion holds. The reform induced women to have 0.7 more levels of education, similar to Larreguy and Marshall (2017) who found an effect of 0.6.

[40] These tests also support the identification assumption that in the absence of the reform, changes in education should not differ between the treatment and control group in areas with low and high UPE intensities and addresses concerns on mean reversion or catch-up.

3.4.5 Second Generation Impacts of UPE (total effects of the reform)

Table A.34 provides the reduced form estimates of the UPE reform on child schooling (equation 4). The reduced form estimates show the effects of maternal exposure to the reform on child schooling. Overall, children whose mothers were exposed to the reform have better schooling outcomes than children whose mothers were not exposed to the reform. Children whose mothers were exposed to the UPE reform are 4.2 percentage points more likely to be track in school (8%, significant at 10%) and 6.7 percentage points more likely to complete primary school (12%). They are also 7.3 percentage points more likely to attend secondary school (15%). Figures A.19, A.20, A.21 present the graphical representation of the result. The graphs show a discrete jump in outcomes for children whose mothers were born in 1970 and a continuous increase for children whose mothers were born after 1970.

While the estimates presented have focused on maternal exposure to the reform and education, it is plausible that paternal education plays an important role in the education of the children. Through assortative sorting, we know that men and women of similar educational levels marry each other and since the reform affected both men and women, the coefficient on maternal education and exposure should be interpreted with caution. The effect of maternal education can capture higher wealth status, the direct effect of maternal education and the effect of spousal characteristics. In Section 5 I attempt to disentangle these effects.

3.5 Robustness Checks, Mediating Factors and Discussion

3.5.1 Robustness Checks

One of the main assumptions of the RD strategy is that no other determinants of outcomes are changing at the threshold. This implies that children on either side of the threshold are similar and the inclusion of controls should not change the outcomes. In Panel B of Table A.35, I exclude controls from the main specification and the estimates are similar to the base specification in Panel A. We might also be worried that although the sample is restricted to mothers in the highest intensity region, there might still be systematic differences across children on either side of the cutoff. In Panel C, I include state fixed effects which will compare only children of mothers living within the same state, and I find that the results do not change in a meaningful way. Similarly, Panel D addresses concerns associated with other changes in the state, correlated with the UPE reform, that may differentially affect children in the treatment and control groups. The estimates are mostly unchanged when I control for other government reforms.

Panels E and F show the results using placebo reforms. Restricting the sample to those born before 1971 and assuming the reform happened in 1965, I find no effect on child outcomes in Panel E (the coefficients are not statistically significant- 0.613, 0.509, 0.355). Creating a placebo 1975 reform year and restricting the sample to mothers born between 1970 and 1980, shows no discontinuity at the fake threshold (-0.059, -0.927, -0.485). These results supports the identification that the base specification is not picking up a general trend in education. Finally, the estimates are not sensitive to varying functional forms (Panels G and H). The estimates get larger with higher-order and more flexible polynomials.

The estimates are robust to a uniform kernel specification (Panel A) and varying bandwidths (Panel B – Panel E) of Table A.36. Smaller bandwidths yield estimates similar to the base specification but are more imprecise (the confidence interval overlaps for all bandwidths). Allowing for heteroscedasticity-robust standard errors rather than clustering at maternal year of birth does not change the results (Panel F). Alternative methods of clustering are presented in Appendix Table A.44.[41] As previously discussed, there is a pattern of rounding in the reporting of maternal year of birth in the survey. I follow Barreca et al. (2016) to address this heaping problem by including an indicator for heaped year of birth (Panel G) and in Panel H, I interact the indicator for heaps with the treatment variable. This approach removes the bias by allowing the heaped and non-heaped data to have dif-

[41] Alternative clustering include state and cluster level, two-way clustering (year of birth and state), wild cluster bootstrap (year of birth and state, respectively).

ferent intercepts and slopes.[42] While the magnitude on grade-for-age drops slightly, it does not alter the interpretation of the result.

Lastly, we might be concerned that the results might be driven by sample selection. To assume an extreme scenario of an overestimation implies low ability women born after the reform are migrating selectively from the highest intensity areas to the lowest intensity areas. Although mothers moving from the highest to lowest intensity areas are not identifying any effects, they change the composition of the highest intensity sample. As mentioned in Section 4, I find no effects of selective migration. However restricting the sample to women who completed primary school in the highest intensity areas and women born before 1976 (to limit the risk of selective migration) does not change the results.[43]

3.5.2 Other Robustness Checks

Appendix Table A.45 presents the results that address threats to identification for the DID identification. Panel B controls for other changes in the state that may differentially affect children's schooling. I include state-level health and information expenditures in 1976 and the 1973 state population, all interacted with the cohort variable and the results remain unchanged. Clustering standard errors at the 1976 state level does not affect the magnitude and significance of the estimates in Panel C. Following Abadie, Athey, Imbens and Wooldridge (2017), I cluster the standard errors at the survey cluster level since this is the level at which units in the sample were selected and there are clusters in the population that are not represented in the sample. The estimates in Panel D increase in significance, which indicates that the base specification yields conservative estimates.

The results in Panel E provide additional support for the identification assumption that in the absence of the reform, changes in education should not differ between the treatment and control group, in areas with low and high UPE intensities. Creating a placebo cohort

[42] However, if the treatment effects for the heaped and non-heaped data are different, this method will not recover the unbiased average treatment effect.

[43] Other robustness tests in Appendix Table A.46 show that the results are robust to using a sample of children 6-17 years old, excluding controls for the age of the child, a probit estimation and controls for ethnicity.

similar to what was done for mothers in the previous section, where I restrict the sample to mothers born before 1970, shows no effect of the reform on those too old to benefit. Estimates in Panel F do not include differential trends in the pre-period. I also include state-specific cohort fixed effects in panel G to address concerns that the reform is related to other government reforms that differentially affected mothers born after 1970 and affects their children's schooling. The estimates are larger when I exploit this source of variation; which shows that the results are not driven by other government reforms around 1976.

In Panel H, I allow for overage enrollment or the possibility that the reform induced some mothers to stay in school longer. I exclude children of women born between 1967 and 1969 who might have partially benefited from the UPE. The results are consistent with the main specification estimates. Finally, I address the issues relating to selective migration. The estimates could be biased if there are systematic differences between migrants and non-migrants. To circumvent this bias, my main estimates are restricted to non-migrants. Although I find no evidence that reform caused some people to move, the results for the full sample, which includes migrants and non-migrants are not different from the base specification (Panel I).

3.5.3 Potential Mechanisms of Second-Generation Impacts

Using the sample of households in the highest intensity regions I shed light on the underlying mechanisms or channels through which maternal education improves child outcomes. A mediator has to be affected by maternal education and should affect child outcomes. Therefore, I first test if maternal education affects the set of potential mediators shown to be associated with child development. Then I sequentially include the mediating variables in the regression, quantify the effect of each variable.[44] Though there could be different potential mechanisms, I only focus on a few due to data availability.[45]

[44]A mediator should have a significant causal relationship with maternal education and reduce the effect of maternal education on child outcomes when included in the same regression.

[45]While not ignoring that the inclusion of mediators may be a "bad control" problem, this method provides a simple way to test if the results are driven by potential mediators.

While education can change or increase the value that mothers attach to education, it can also help mothers make better choices to improve children education outcomes. Using the fuzzy RD design applied in the children's analysis, Tables A.37 and A.38 show the effect of maternal education on a set of potential mediators- wealth status, involvement in decisions about the child's education, labor market, and spousal characteristics. Table A.37 presents evidence of assortative mating. Mother's education does not affect whether or not she is living with her partner (column 1). This is not surprising as 97% of women live with their partners. An additional year increase in maternal education increases father's education by 0.92 years (column 2) and reduces the spousal age difference by 0.3 years in column 3 (not significant). The results in column 4 and 5 do not show that maternal education affects the timing of birth and fertility.[46] Column 6 shows that maternal education improves the wealth index.

Women marry men that are on average twelve years older. This is reflective of the marriage market in many developing countries; where couples marry outside their age cohort and polygamy is permitted. The implication for this study is that the reform did not affect the education of spouses as over 90% of spouses were born before 1970. This means that for the cohort of women born between 1963 and 1977, their spouses were born between 1951 and 1965.[47] This partly rules out the hypothesis that the effects on children's outcomes are largely driven by the father affected by the reform or being more educated. Similar to Agüero and Ramachandran (2018), I find that although women do not marry within their age cohort, they marry more educated men, suggesting that "even within a pool of possible partners belonging to a different cohort, educated women choose to marry more educated men."

[46]These effects are conditional on having at least one child since the sample is restricted to mothers. Odunowo (2019) finds no effect on birth spacing. Finding no effects on fertility allows me to rule out the quality-quantity trade-off channel. This implies that women are not focusing their resources on fewer children instead of spreading it across many children.

[47]However, the reform also affected men in the UPE eligible age group. Using a sample of men born between 1960 and 1980 and in the highest intensity areas, I regress men's education on the reform and find that the reform increased education by 0.70 years. I can reject the hypothesis that the effect of the reform on men and women are the same.

Another potential pathway is that changes in labor market outcomes could affect how maternal education improves children outcomes. For instance, if more educated mothers are more likely to work for pay outside the home and earn more, then the increase in resources may be substituted for time spent with children. Andrabi et al. (2012) find no improvement in labor market outcomes for more educated mothers, but they spend more time with their children and this improves children's cognitive outcomes. This could arise because mothers learn in school that schooling requires efforts and they assist their children in their studies. In Table A.38, I find no significant differences in the labor market participation of women affected by the reform (column 1) nor on the probability of being paid for their labor (column 2). This is not surprising since the reform was designed to affect low levels of education. Also conditional on working 84% of women were self-employed and 90% of the working mothers are paid.

While I find that fertility and labor market outcomes do not drive the main results, a possible channel could be that education increases the value parents place on their children's schooling and so are more likely to be involved and concerned about the educational progress of their children. I test if more educated women are more likely to be involved in decisions about their children's education. I find effects of education increasing women's participation in decisions on children's education and health (6.7 and 9.4 percentage points, respectively). This finding is in line with studies that show that healthy children are more likely to attend school. In a companion paper, Odunowo (2019), I find that the reform increased literacy which could suggest that mothers might be helping their children with school assignments especially at the primary school level. Another plausible channel is that education makes individuals more likely to trust the state and send their children to school. Larreguy and Marshall (2017) find that educated Nigerians are more likely to participate in politics and participate in their communities.

Next, to quantify the effect of the mediators, I sequentially include paternal education and wealth index in the regression. Each column of Table A.39 represent a different regression.

For all outcomes, controlling for wealth index in column 2 reduces the magnitude of maternal education by 8-9%. In Panel 1 the magnitude on maternal education reduced from 0.052 in step 0 to 0.048 in step 1, after controlling for wealth index. This accounts for an 8% reduction in the effect of maternal education on grade-for-age.[48] In column 3, paternal education mediates for 2-7% of the effect of maternal education on child schooling. While assortative mating accounts for part of the effects of maternal education, I do not find evidence that it fully explains the results. This finding is consistent with Akresh et al. (2018); Cui et al. (2019); Lundborg et al. (2014). I cannot estimate the causal effect of paternal education on child outcomes because I do not have a valid instrument.[49] Testing the equality of coefficients, I can reject the null hypothesis that paternal and maternal education have the same effects. While it is true that in the presence of assortative mating, the effects of maternal education might also capture the effect of paternal education, the results show that the effects of maternal education are larger than paternal education. Overall, with the inclusion of each mediator, the coefficient on maternal education is still positive and statistically significant and suggests that the results are not totally driven by assortative mating.

3.5.4 Discussion

Effect size: Comparing the total effects on the second generation outcomes with what has been found in studies on similar educational reforms in developing countries, I find a 0.08 SD effect on grade-for-age while Sunder (2018) find an effect of 0.03 SD and Akresh et al. (2018) find an effect of 0.04 SD. The effect I find on primary school completion (0.13 SD) is greater than Akresh et al. (2018) (0.002 SD). The magnitudes of the parameters across different studies provide implications for policy. The long-run effects might be larger in Africa because primary school completion rates and secondary school enrollment rates are

[48] In Panel 1 of Table A.39 the coefficient on maternal education is 0.052 and reduces to 0.048 when wealth index is included, so ((0.052-0.048)/0.052) x 100 = 7.7%

[49] Using the sample of men born between 1963 and 1977 in the highest intensity areas (not spouses of women in the main sample), and applying equations 2 and 3, I find that the reform increased men's years of education by 0.70 years. Furthermore, I find that increasing men's education by one year increases grade-for-age by 2.5 percentage points (t=1.73). However, this magnitude is about half the size of mother's effect. Thus, I can rule out that the effects are totally driven by father's education.

lower in sub-Saharan Africa, 68.75% and 34.4% respectively as of 2017, than in other regions such as south-Asia which have corresponding rates of 95.18% and 59.78%.

Comparing the effect of the reform on the education of both generations, it increased the probability of completing primary school by 0.17 SD for children and 0.56 SD for mothers.[50] The reform increased the probability of attending secondary school by 0.19 SD for children and 0.34 SD for the mothers.[51] While the effect of the reform is larger for mother than children, the results provide evidence on the durability of the policy, since it affected the outcomes of both generation.

In a larger context of school inputs and demand-side interventions that aim to improve the educational outcomes of children, policies that increase school access appear to have large impacts. While it is difficult to directly compare across studies, as only a few of these studies have examined second-generation effects, the estimates on secondary school enrollment for the second generation (4.4 to 5.0 percentage points) are similar to those found in studies on cash transfer and scholarship reforms (3 to 8.7 percentage points), bicycle provision for girls (5.2 percentage points), and school meals (mostly no effect).

Finally, the results in this study contribute to the larger literature that provides evidence to help policymakers prioritize among alternative potential investments, which could be through identifying the cost-effectiveness of alternative policies. Also, within a given budget, reallocation of public expenditure to effective policies can improve outcomes (Glewwe & Muralidharan, 2016). Performing partial back-of-the-envelope calculations, I can show the cost-effectiveness of the policy for the first generation. According to Osili and Long (2008), the government spent about 700 million Naira between 1974 and 1979 on the UPE reform. I use the difference in total enrollment at the beginning and end of the UPE reform as the number of students that benefited from the reform. This implies an average fund per capita

[50] For the first-generation outcome, the effect is similar to other studies on Nigeria Osili and Long (2008) and Larreguy and Marshall (2017)

[51] I do not present results for years of education for the second generation since many of them have not yet completed their schooling.

of about 31,748 NGN (in 2010 Naira) or $209. [52]

Since I find that for the full sample, the UPE increased maternal educational attainment by 0.48 SD, the implied cost is 6,614 NGN (in 2010 Naira or $43.5) per 0.1 SD increase.[53] While this a rough estimate, it might overstate the implied cost since we find positive effects second-generation effects. This means that the implied costs might be lower once we factor in the benefits for the second generation. Notwithstanding, the reform seems to have been effective, given the wide range of outcomes it affected as documented in other studies as well. Put together, the available information and evidence point to school construction interventions as being effective in increasing school enrollment and educational attainment.

3.6 Conclusion

In discussing the intergenerational transmission of education, we generally see that parents with more education have more educated children. However, a particular concern in estimating spillover effects is being able to distinguish between causation and selection and uncover potential mechanisms. Understanding this is important because it can assist policymakers in tackling challenges faced in educating children. In this paper, I estimate the causal effects of a school reform on women's education and the schooling of their children, in a setting with low levels of education. In 1976, the Federal Government of Nigeria implemented the Universal Primary Education (UPE) reform- one of Africa's largest school expansion reforms- which provided free access to primary schools to children. The reform ended in 1981 when the government experienced a shortfall in oil revenues. This provides a natural experiment that allows me to provide reliable estimates.

I use two identification strategies in this paper: fuzzy regression discontinuity approach,

[52]Enrollment in 1975 was 5,950,297 and 15,214,481 in 1981 (difference = 9,264,184). Cost per capita in 1976 Naira: 75.96 NGN = 700,000,000/9,264,184.

[53]Note that this cost does not include recruitment of new teachers and payment of salaries, as well as construction of teacher training centers. See (Glewwe & Muralidharan, 2016) for an estimate of the cost-effectiveness of different interventions. They note that the estimates should be interpreted with caution because many of them do not include administrative costs. Select scholarship programs ($1-14/0.1 SD increase), select conditional cash transfer programs ($77-$138/0.1 SD increase), computer introduction programs ($2-33/0.1 SD increase) and teacher incentive programs ($1/0.1 SD increase).

where I exploit a discontinuity in eligibility for the UPE reform to estimate the effects of maternal education on child schooling, and a difference-in-differences identification strategy, where I exploit intensity in the reform using the pre-reform primary school enrollment rates across local governments and gender, to estimate the total effects of the reform on the second generation outcomes. Using data from the Nigerian Demographic and Health Surveys from 2003 to 2013, I find that the reform increased educational attainment for mothers who were exposed by 1.3 years. A one-year increase in maternal education increases the probability that a child is making normal progress in school by 4.3 percentage points, are 4.7 percentage points more likely to complete primary school and 4.7 percentage points more likely to have some secondary education. These results are robust to different robustness and specification checks. I find that these effects are mediated by having more educated fathers and more wealth. However, the results show that maternal education is the main channel and not outweighed by other mediators, given the data available. Finally, the similarity in results from the two different identification strategies further validates the findings from the study.

The results in this study contribute to a larger literature that provides evidence to help policymakers prioritize among alternative potential investments. This could be through identifying the cost-effectiveness of alternative policies. Policies that improve the quantity and quality of human capital in society contribute to improving the outcomes of the current and future generations and breaking poverty cycles. For example, parents' education as an input in children's outcomes can be influenced by policymakers compared to other inputs such as parenting style (Holmlund et al., 2011). Also, within a given budget, reallocation of public expenditure to effective policies can improve outcomes (Glewwe & Muralidharan, 2016). Performing partial back of the envelope calculations and following Glewwe and Muralidharan (2016), I find that for the first generation, the reform increased educational attainment by 0.48 SDs, with an implied cost of 6,614 NGN (in 2010 Naira or $43.5) per 0.1 SD increase.

4. REASSESSING THE EFFECTS OF EDUCATION ON FERTILITY

4.1 Introduction

Educating women is widely held to be an important tool for improving fertility outcomes. While fertility rates have been declining globally, sub-Saharan Africa faces difficulty in stemming her increasing population. According to the United Nations, the global fertility rate in 2019 was 2.5 live births per woman but 4.5 live births per woman in sub-Saharan Africa. The population of Africa is projected to double by 2050, accounting for 23% of the world's population, an increase from 7% in 1960 (World Bank, 2019). Many studies have shown more educated women tend to have smaller families (Black, Devereux & Salvanes, 2008; Chicoine, 2020; Cygan-Rehm & Maeder, 2013; Keats, 2018; Lavy & Zablotsky, 2011; Osili & Long, 2008). However, there is evidence that contrasts this relationship. Braakmann (2011) and Fort, Schneeweis and Winter-Ebmer (2016) find a positive effect in the UK and continental Europe, respectively, and McCrary and Royer (2011) do not find evidence that education reduces fertility outcomes in the US. Therefore, the goal of this paper is to estimate the causal relationship between education and fertility among women with low levels of education and to understand the underlying mechanisms driving the relationship.

There are different channels through which education can impact fertility. One, education increases a woman's earnings through participation in the labor market (Becker, 1992; Becker & Lewis, 1973). Participation in the labor market increases the opportunity cost of having children, which then leads to a decrease in fertility but of higher quality. The income effect is further strengthened under the presence of assortative mating, as more educated women marry more educated men with higher income. The quality-quantity trade-off suggest that higher income individuals have fewer but quality children (Becker, 1960). Two, education increases knowledge, ability to process information, and access to healthcare facilities that will improve welfare. Education may improve women's bargaining power within a marriage and make them more likely to be involved in decision on reproductive health. For example, more educated women are more aware of family planning methods and are more likely to use contraceptives (Shapiro & Tambashe, 1994).

In this paper, I present evidence on the effect of education on fertility outcomes using a natural experiment in Nigeria for identification. In 1976, the Nigerian government implemented the Universal Primary Education (UPE) reform to provide free primary education to six-year-olds starting school. With increased access to schools, primary school enrollment increased from 49% in 1975 to 86% in 1978, and by 1981 had increased by over 7 million with over 16,000 newly constructed schools (Federal Office of Statistics Nigeria, 1984). The timing of the reform provides a source of exogenous variation in parental education. Although the reform varied in intensity across regions, I restrict the sample to children in the highest reform intensity areas.[1] This methodology allows me to apply a fuzzy regression discontinuity (RD) design since primary school officially starts at age 6 and only those born in 1970 or later were eligible for the reform.[2] Assuming women living in the same areas are similar on non-educated characteristics, differences in years of schooling for women born close to 1970 are exogenous. Thus, by comparing the fertility outcomes of women just before and after 1970, I can estimate the effect of education.

First, I show that the reform increased average years of schooling for women born after 1970. They have 0.8 more years of education than women ineligible to benefit from the reform. Second, the increased schooling does not significantly affect fertility outcomes. I find no effects on the total number of births and number of children born before age 25.[3] However, the number of children born before age 18 decreases by 0.2 births. This could be explained by the incarceration effect, where keeping girls in school reduces the chances of

[1]Since the goal of the government was to achieve 100% primary school enrollment, more schools were constructed in areas that had low pre-reform primary school enrollment rates. I follow Larreguy and Marshall (2017) by constructing a reform intensity variable using the pre-reform primary school enrollment rates across local governments and gender. The highest intensity area is where no one born between 1960 and 1969 had completed primary school.

[2]The fuzzy RD design allows overage enrollment.

[3]Since the women in the sample are still within the childbearing age range, I cannot estimate the effect on completed fertility.

teenage pregnancy Black et al. (2008). All estimates are robust to the inclusion of a variety of controls, kernel functions, and functional forms. Next, I complement the main results with an understanding of the underlying mechanisms. The analysis reveals that women are more likely to delay the age at which they have their first birth and there is suggestive evidence of postponement in first cohabitation or marriage. The results also show that women are more likely to use modern contraceptives, which is an important determinant of reduced fertility. Finally, there is evidence of positive assortative mating, as more educated women have more educated spouses and narrower spousal age gap. [4]

The identification of RD design hinges on an imprecise manipulation of the year of birth. I validate this assumption by testing for bunching of observations at 1970 in the distribution of year of birth and I find no evidence of manipulation or discontinuity in the trend of births. This suggests that the results are not driven by shocks related to changes in the population. Although according to Bray (1981) the reform was announced in 1974, there is no bunching at 1975 (the earliest cohort whose birth might have been timed to benefit from the reform). Also, it is difficult for parents to know the exact locations of where the schools will be built, and is further complicated by parents having to wait six years before their children are enrolled in school. However, since they are always in the treated cohort, the timing of their birth does not affect identification. I also find that the women on either side of the cutoff are similar in terms of predetermined characteristics. One caveat for the interpretation of the results is that it applies to women living in high intensity areas and are born close to 1970 i.e. those whose education was impacted because of the reform. I also present evidence on the full sample using a difference-in-differences strategy, where I instrument for education with the differential intensity of the reform and year of birth.

This study makes important contributions to the literature. First, the evidence in this

[4] The closest paper to this study is the analysis done by Osili and Long (2008) on Nigeria, exploiting the same UPE reform. The measure of fertility examined in the paper is the number of births before age 25. They find an effect of 0.26 fewer births. However, the study does not examine the potential mechanisms explaining the results. Furthermore, I use more granular data at the local government level to capture program intensity rather than using variation at the state level.

paper shows non-pecuniary returns to school construction reforms. Policies that increase access to education increase the educational attainment of individuals and the human capital of subsequent generations. Second, this study presents evidence that education affects the teenage pregnancies which is important for policies that target the prevention of teen pregnancy. That I find no effect on total number of births and number of children born before the age of 25 does not signify that education is not effective along these dimensions. It could however be suggestive that education can be combined with other interventions to reduce total fertility. Third, it uncovers the underlying mechanisms driving the effects. For example, it reinforces the need to educate the population on modern family planning practices.

The remainder of this paper proceeds as follows: Section 2 provides background information on education in Nigeria. Section 3 discusses the data and empirical strategy. Section 4 presents the main results and potential mechanisms, Section 5 shows robustness checks and Section 6 concludes.

4.2 The Nigerian Education System

4.2.1 Country Overview

Nigeria is the most populous country in Africa with an estimated population of over 190 million. The World Bank categorizes Nigeria as a lower-middle-income country with a Gross National Income (GNI) per capita of $2,028 and a life expectancy at birth of 54 in 2017.[5] Before independence in 1960, Nigeria was divided into three regions: east, north, and west. A mid-western region was created in 1963 and each region retained a substantial measure of self-government (Akinyele, 1996; Babalola, 2016). Subsequently, these regions were divided into states: 12 states by 1967, 19 states and a federal capital territory (FCT) by 1976, 21 states by 1987, 30 states by 1991 and 36 states by 1996. There are currently 36 states, a capital, and 774 local governments.[6]

[5] Lower middle-income economies have GNI per capita between $1,026 and $3,995.

[6] Local governments are responsible for the collection of fees and levies, provision of public works and services, provision of health and social services as well as payment of primary school teachers' salaries (Smith

The structure of the education system in Nigeria is similar to the systems in most countries in sub-Saharan Africa and many developing countries. The official school starting age is six, although some children start at five. They spend six years in primary school and three years in junior secondary school. The first nine years of school forms the compulsory basic education, although monitoring and compliance are weak. After junior secondary school, students can continue along the academic track to spend three more years in senior secondary schools or can choose vocational or technical training. Children who complete senior secondary school can continue to institutions of higher learning.

4.2.2 The 1976 Universal Primary Education Reform

Before the government implemented educational reforms across the different regions, missionary education was the main source of schooling. Subsequently, different regions in the 1950's implemented free primary education for students. The free education reform resulted in almost doubling enrollment in the western region in 1955 and the eastern region in 1957 (Abernethy, 1969; Csapo, 1983). However, the free education in the Eastern region was restricted to the first two years of primary school by 1961 (Oyelere, 2010). The western region was the forerunner in education and educational imbalances across the different regions became substantial after independence (Osili & Long, 2008). The limitations in educational expansion in the Northern region was primarily due to Islamic religious practices and traditional attitudes towards girls and women (Csapo, 1983; Osili & Long, 2008). These regional differences were amongst the reasons the universal primary school reform was introduced.

Nigeria, a major producer of crude oil and natural gas, experienced an oil boom in 1973 caused by the increase in oil price. The federal government saw the boom as an opportunity to invest in education and implemented the UPE reform in 1976 (Csapo, 1983). To show the government's commitment to education, it is stated in the 1977 National Policy on Education that "education will continue to be highly rated in the national development plans, because education is the most important instrument of change as any fundamental change in the

& Owojaiye, 1981).

intellectual and social outlook of any society has to be preceded by an educational revolution." During the oil boom, a majority of public expenditure was on primary education, transport, steel, construction, and auto assembly (Pinto, 1987).

The UPE reform is a nationwide free primary education reform introduced by the federal government in September 1976. Since primary school commonly starts between ages 5-6, children starting school after 1975, (i.e. those born after 1969) should be eligible for the reform, while those born before 1970 should be too old to benefit from the reform. The federal government disbursed money to states for the construction of schools, classrooms and teacher-training institutions. The reform is considered one of the largest educational reforms in Africa (Bray, 1981; Larreguy & Marshall, 2017). A total of over 700 million NGN ($551M) was disbursed differentially to states for the reform between 1974 and 1979, with larger amounts apportioned to northern states.[7] The government targeted 100% enrollment in class 1 at the beginning of the UPE reform (Federal Ministry of Economic Development and Reconstruction, 1975) and 100% primary school enrollment by 1981 (Csapo, 1983). Since educational attainment varied widely by region, with rural areas and northern states having a less educated population, the introduction of UPE should have larger impacts in these regions. Overall, the structure of the reform provides a natural experiment to analyze the impact of education on child education.

The reform increased school availability across the country. The number of primary schools and classroom increased substantially. 16,246 new schools were constructed and enrollment increased by over 7 million between 1975-1980 (Federal Ministry of Economic Development Reconstruction and Central Planning Office, 1981). Figure A.22 shows that many public schools were founded in 1976 which confirms that the UPE reform is a very big policy change. The reform resulted in large increases in primary school enrollment in states with low prior educational attainment, which are areas concentrated in the North. Primary school enrollment increased by 557% in Kano, 442% in Kaduna and 263% in Benue state

[7]The dollar equivalent is in 1976 dollars.

between 1975-1977 (Csapo, 1983). Primary school gross enrollment for girls increased from 39.87% in 1976 to 99.23% in 1982 (World Bank, 2018).[8]

The reform was associated with many problems despite its achievements. These include a shortage of teaching staff, use of unqualified teachers and poor equipping of schools across all states (Federal Ministry of Economic Development Reconstruction and Central Planning Office, 1981). However, teacher supply and quality improved in all states in the 1980s. The reform ended in 1981 after an unanticipated decline in oil prices and when the federal government handed over the financing of primary schools to state and local governments Csapo (1983). This resulted in lower growth of primary school enrollment. After the federal government ceased to provide grants for teachers, most states except those in the west reintroduced school fees (Larreguy & Marshall, 2017; Osili & Long, 2008). However, primary school enrollment continued to increase beyond the end of the reform which suggests that availability of schools rather than fees was responsible for the increasing trend.

4.3 Data and Empirical Strategy

4.3.1 Data

The data used in this study comes from the individual-level responses to the Nigerian Demographics and Health Survey (NDHS) (DHS, 2003-2013). NDHS -a project of United States Agency for International Development (USAID)- is a nationally representative survey. I use responses from women between the ages of 23-49 living in the 36 states of the country, including the Federal Capital Territory.

Education: The education variable is measured as the number of completed years of schooling. This is the main explanatory variable used in the study. I create other measures of education such as primary school completion, incomplete secondary school education, secondary school completion and more than secondary school completion to check if the program induced people to have more than primary school education.

[8]https://data.worldbank.org/indicator/SE.PRM.ENRR.FE?end=2014locations=NG-ZG-XM-XLstart=1970view=chart

Outcome variables: I examine three measures of fertility: total number of children ever born, the number of children born before age 25 and 18, respectively.

Cohort variable: The year of birth determines whether a woman falls into either an old or young cohort. I define the young cohort (UPE cohort) as those born between 1970 and 1980, that is, those who should be affected by the reform. Since primary school officially starts at the age of six in the country, children who started primary one in 1976 should have been born in 1970. The older cohort are those born before 1970.

Intensity: Federal allocation to states for the UPE varied significantly, with larger amounts disbursed to states with lower school enrollment (northern and eastern states). Although state expenditure is a measure of intensity, it does not capture the actual reform intensity as there were uneven implementation within states (Larreguy & Marshall, 2017).[9] To define a finer level of intensity, I construct a variable following Larreguy and Marshall (2017) that captures the spatial variation of the reform using differences in educational attainment across local government areas.[10] The intensity measure is the proportion of women born between 1960 and 1969 who had not completed primary education in a local government area (LGA) and ranges between 0 and 1. Zero represents total pre-reform primary enrollment in an LGA while one implies that no woman born between 1960 and 1969 in an LGA completed primary education. Since investments were made by states to reach universal primary school enrollment, more schools were built in areas that had fewer schools. Therefore, the intensity variable captures the difference between actual and potential enrollment. See Figure A.23 and A.24 for geographical variation in intensities. Darker areas on the map reflect higher UPE intensities and within states, UPE intensities are different. [11]

[9]Missing data on number of actual schools and classrooms constructed in each state does not allow us use this as an alternative measure of intensity.

[10]There are 774 local governments in Nigeria and 651 in the sample. The 2003 survey does not have identification at the LGA level, so I use clusters to define intensity. Clusters are smaller geographical units than LGAs.

[11]While Table A.27 shows the number of schools constructed during the reform (1975-1981) at the state level, it does not capture variation in the intensities of the UPE reform across smaller regions. However, we see from the table that more schools were constructed in areas that had fewer schools available in 1975 (which are predominantly northern states), and this correlates well with the intensity measure (0.49). Furthermore, to show that the intensity variable captures the intensity of the reform, from the data on school founding

The intensity measure is defined based on current residence since the only information relating to where a woman went to school is how long she has lived in a particular area and is not available for all survey waves. Therefore I assume that area of residence is the same as where she attended primary school. I discuss issues relating to migration in Section 3.

Control variables: The control variables include ethnicity, whether the woman lives in an urban or rural area and age. Ethnicity is categorized into the four main ethnic groups: Yoruba; Ibo; and Hausa-Fulani, with other ethnic groups being the reference category.

Table A.47 presents the summary statistics. The average years of education for women aged 23 to 49 is 4.8 years. This points to the low level of education in the country. Similar to many countries in sub-Saharan Africa (Angola, Burundi,Niger, Mali, Somalia), the average fertility rate is 5.6 children per woman. In a country with a significant proportion of girls getting married early (44% of girls get married before their 18th birthday), the average number of birth before 18 is 0.5, with a maximum of 5. The average age at first cohabitation or marriage is 18 years and the average age at first birth is 20. 97% of women in the sample are married and 66% are living in rural areas.

4.3.2 Empirical Strategy

I employ two identification strategies to identify the causal impact of education on fertility. One method exploits the interaction of the temporal and spatial variation in the intensity of the reform using a difference-in-difference strategy. Restricting the sample, I employ a second method, the fuzzy regression discontinuity design to estimate the impact of education on the fertility outcomes of women living in the highest treatment intensity areas.

4.3.3 Regression Discontinuity

I use an exogenous variation in schooling from the UPE reform to deal with the endogeneity of education. The identification comes from the UPE reform which provides variation

dates, the correlation between the number of schools opened across local governments and the intensity variable is 0.47. This is similar to Larreguy and Marshall (2017) (0.43). The measure of schools opened is not used in the main specification because founding dates are missing in a nonrandom way.

in education that is uncorrelated with the error term. Since the official school starting age is six, girls born after 1969 should benefit from the reform.[12] I restrict the sample to households in the highest intensity areas and use a fuzzy regression discontinuity design (Imbens & Lemieux, 2008; Lee & Lemieux, 2010) to estimate the effect of education on fertility outcomes by instrumenting for education with the reform eligibility.[13] The RD design provides a causal approach to estimation compared to regressing child schooling on education might yield unreliable estimates. Education is correlated with unobservables such as family background, family income, neighborhood characteristics, and community resources, that may affect the schooling of the child. The ideal experiment is to randomly allow some women to attend school and leave others without access to education, and then compare the outcomes of their children. However, in the absence of such randomization, the RD design provides as-good-as-random variation in education.

The sample is restricted to households living in the highest UPE intensity areas which allows me to argue that women on either side of the threshold are similar and the only difference between them that could affect their fertility outcomes is when they were born relative to the start of the UPE reform. Women who were born before 1970 form the control group because they were too old to benefit from the reform while women born in 1970 and after form the treatment group. To show that the areas are similar in other dimensions, I regress geographical area characteristics available in the data on the reform and find no effects of differential area characteristics (see Appendix Table A.58). These characteristics include population, rurality and economic measures.

The main identifying assumption of the RD design is that all determinants of outcomes vary smoothly across the reform eligibility threshold. Put in other words, individuals should

[12]According to Bray (1981) and Aderinto (2015), to determine the age of a child in the absence of a birth certificate, the crude but usual method adopted by the government was the "arm over head task". A child was asked to reach over the head and touch the opposite ear. If the child could not do it the child was considered under age; if the child could "just" do it, the child was considered six years of age and if the child's hand reached under the ear, the was considered over-age for school entrance.

[13]Since the running variable is discrete, there might be issues relating inference when using standard RD designs. I follow Lee and Card (2008) by choosing a parametric functional form so that I can cluster the standard error on year of birth.

not be able to manipulate where they are relative to the cutoff. It is unlikely that individuals can precisely manipulate this because it is difficult for parents of children who were born around the time of the reform to precisely manipulate when their children will be born. The official primary school starting age is six, therefore children born in 1970 and later should be eligible for the reform while those born before 1970 should be ineligible. However, allowing for the possibility of overage enrollment does not alter the identification since I am using a fuzzy RD design. Although the reform was announced at the beginning of the school year in 1974, it commenced in 1976 and this means that the oldest cohort whose parents could have timed their births to benefit from the reform will be born in 1975 and start school in 1981. This does not affect identification because even if they were born after 1975 they will still have benefited from the reform.

The equation of interest is:

$$Y_i = \beta_0 + \beta_1 Ed_i + \beta_2 X_i + \epsilon_i \tag{4.1}$$

where the Y is the outcome of interest for woman i. Ed is woman i's years of education. X is a vector of control variables including observable characteristics that should not significantly affect Y but increase the precision of the estimates. ϵ captures other unobservable factors affecting Y. In the presence of endogeneity in schooling, equation 1 gives the correlation between education and fertility outcomes. In equation 2, I estimate the effect of the reform on education (first stage) and in equation 3, the reduced form effects:

$$Ed_i = \gamma_0 + \gamma_1 T_i + \gamma_2 f(R_i) + \gamma_3 T_i \cdot f(R_i) + \gamma_4 X_i + \mu_i \tag{4.2}$$

$$Y_i = \delta_0 + \delta_1 T_i + \delta_2 f(R_i) + \delta_3 T_i \cdot f(R_i) + \delta_4 X_i + \epsilon_i \tag{4.3}$$

where T is a dummy variable that takes on the value of one if woman i was born in 1970 or later. R represents year of birth for woman i but normalized to zero. The running variable is year of birth and the threshold is 1970. f(R) is a function of the running variable and captures the relationship between R and Y. To allow the slope to change on either side of the threshold, I interact T with f(R). The first stage regression in equation 2 examines whether education was affected by the reform, with γ_1 being the effect of the reform on education. δ_1 in equation 3 gives the total effect of the reform on Y.

I employ the two-stage least squares (2SLS) method to identify the effect of education on fertility outcomes. I instrument for education with T, which describes the fuzzy approach of the RD design. The fuzzy RD design allows for overage enrollment by the cohort born shortly before the reform. I use the 2SLS to identify the local average treatment effect (LATE) for compliers. The LATE is the average effect on compliers near the cutoff. This is analogous to re-weighting the discontinuity in outcomes by the discontinuity in treatment. The LATE may therefore be different from the average treatment effects since it applies to those whose education was influenced by the UPE reform. In the preferred model specification, I model the relationship between R and Y as quadratic and use triangular kernel weights.[14] Standard errors are clustered at year of birth. To determine the bandwidth for the main specification, I conduct the leave-one-out cross-validation test on the preferred model specification.[15] Plotting the mean absolute error against the different bandwidths, Appendix Figure A.29 shows that the MAE drops at a bandwidth of 7 and becomes the smallest at 10 years. Also in Appendix Figure A.30, the first stage estimates are stable between bandwidths of 5 and 10.[16] Given these results, I use a bandwidth of 10 on either side of the cutoff in the preferred model specification. However, in the robustness section, I also present results for alternative bandwidths, kernels, and functional form.

[14]The use of triangular kernels is to assign more weights to observations closer to the threshold. The weight measures the distance in year of birth from 1970. At the threshold, the weight is one, and keeps declining till it reaches zero for observations outside the bandwidth (meaning they are not included in the regression).

[15]However, this test is more suited for continuous running variables.

[16]The estimates are larger and imprecise at smaller bandwidths because the number of clusters shrinks.

4.3.4 Test of Identification

As previously described, if other determinants of outcome vary discontinuously at the threshold, then the identifying assumption will not hold since I will not be able to attribute the change in outcome to treatment. Also, while it is unlikely that year of birth was manipulated because of the reform, one way to test this assumption is by examining whether there is evidence of bunching around 1970 in the distribution of year of birth. I should observe a smooth distribution and no bunching at the threshold or discontinuity in the trend of births.

Figure A.25 shows the density function for year of birth. While there is no clear jump at 1970, there are other jumps in the distribution which are at multiples of fives. This pattern is common in survey data in developing countries, where we see people rounding up their ages, especially the less educated. Since the survey year intervals are in multiples of five years (2003, 2008, 2013), there is a pattern of people saying they are 30, 35, 40, 45, etc. While these rounding estimates could potentially bias the results, I follow recommendations from Barreca et al. (2016) to control for heaping. By allowing the non-heaped and heaped data to have different intercepts or slopes and same treatment effects, this approach would remove any bias from the treatment effect. I discuss the results of the test in Section 5. To show that the age distribution in Figure A.25 is a general pattern in the survey, Appendix Figure A.28 presents the distribution of year of birth for women born between 1950 and 1993. There is no evidence of distinct heaping at a point in the data, which provides more evidence that there is no precise manipulation of the running variable or discontinuity in the trend of births. These results further suggest that the results are not driven by shocks related to changes in the population.

In Table A.48, I present evidence to support that other characteristics that could affect Y are smooth across 1970. The characteristics include age, region of residence (urban-rural), ethnicity and survey round. These variables should not be affected by the reform. The effect of the reform on these characteristics are not significant, except on ethnicity which is marginally significant at the 10% level. In all model specifications I include these characteristics, however, I also show that the results do not change when I exclude the controls.

4.3.5 Difference-in-Differences

In this section, I use the DiD strategy to estimate the impact of education on fertility using the full sample. To do this, I employ the identification strategy used by (Larreguy & Marshall, 2017) to exploit the temporal and spatial variation of the UPE reform using year of birth and area of residence.[17] Since the UPE reform affected all eligible children born after 1969, I define the control group as those born before 1970 and the treated cohort as those born after 1969. This forms the first source of variation. As described in section 3.1, I use variation across LGAs to define the intensity of the reform. Specifically, I use the proportion of women born between 1960 and 1969 in each LGA, who have incomplete primary education to define the intensity variable. The rationale is that since the government's goal was to achieve 100% primary school enrollment, areas where primary school enrollment was low before the reform will have more schools built and have a higher impact of the reform. The spatial intensity of the UPE reform is the second source of variation.

Therefore, the two different sources of variation allow me to identify separately the effect of the UPE reform from the effect of being in a UPE eligible age group and living in a high intensity area. The difference-in-differences assumption implies that in the absence of the UPE reform, the high UPE intensity areas would have continued along the same trend in outcomes. I use a sample of all women born between 1960 and 1980 in all intensity areas. To estimate the effects of the UPE reform on fertility, I estimate the first stage regression and instrument for education with the reform as specified below:

$$
\begin{aligned}
Ed = \beta_1(PostUPE \cdot Intensity) + \beta_2 Intensity + \beta_3 PostUPE \\
+ \beta_4 X + \beta_s + \beta_t + \delta_{st} + \beta_r + \epsilon
\end{aligned}
\tag{4.4}
$$

[17]This method has also been used by Chou et al. (2010); Duflo (2001); Osili and Long (2008).

Where β_1 gives the effect of the reform on education. I include time-fixed effects (β_t) to capture trends in education that are not correlated with the reform. The inclusion of state fixed effects (β_s) absorbs time-invariant characteristics across states. The specification also includes state-specific linear time-trends (β_{st}) to allow states have differential trends in the pre-period and control for state-specific unobservables correlated with the reform and child outcomes. X contains child demographic characteristics such as age, ethnicity and urban dummies to improve the efficiency of the estimates and β_r is the survey round fixed effects. This equation gives the first stage results.

$$\begin{aligned} Y = \alpha_1(PostUPE \cdot Intensity) + \alpha_2 Intensity + \alpha_3 PostUPE \\ + \alpha_4 X + \delta_s + \alpha_t + \alpha_{st} + \alpha_r + \epsilon \end{aligned} \tag{4.5}$$

Where Y represents the different fertility outcomes, and the equation represents the reduced form effects. To estimate the effects of education on fertility, I scale the reduced form estimates with the first stage estimates. In an alternative specification, I interact other government programs implemented in 1976 with the cohort variable. This controls for other programs implemented around the time of the UPE that could have differentially affected the treatment and control groups. Standard errors are clustered at the state level.[18]

Intensity is defined based on current residence, so I assume that area of residence is the same as where mothers attended primary school. The effects I find would be an overestimate if children who are with low academic abilities moved from high intensity areas to low intensity areas. Or if children with high academic abilities moved from low intensity areas to high intensity areas. While there is evidence of migration around regions in the country, I argue that selective migration do not explain the results. First, the Nigerian 2010 Internal Migration Report shows that 75% of the population had not moved from their LGA or state within the last ten years and employment is a major reason for people moving. Also,

[18]I cluster at the 36 states and 1 FCT that existed in the country when the survey was administered. However, results are similar when I cluster using the 19 states that existed in 1976 (see Table A.54).

according to Larreguy and Marshall (2017), 75% of the migration was urban-urban or rural-rural in areas that had similar intensity levels. Second, the common reasons why people move is for marriage and employment reasons. Choosing where to live based on school location is not as common in Nigeria as in many developed countries. The common reason why people move for educational reasons is to attend college or universities. However, for the sub-sample with information on how long a woman has lived in an area, I define a migrant as a woman living in an area where she did not attend primary school, and a non-migrant otherwise. Then in Section 5, I show that the findings for non-migrants are not different from the full sample, which suggests that the effects are not driven by people moving.[19]

4.4 Results

I present the results in the subsequent sections. The first subsection shows the results using the RD approach and the second subsection shows the results from the DID approach. First, I examine the effect of the reform on education. I find that the reform increased average schooling for women born after the 1970. Second, I find no difference in total number of births and number of children before the age of 25 for those born just before and after 1970. However, women born after 1970 have fewer births before the age of 18. Third, I examine the underlying mechanisms through which education impacts fertility decisions and outcomes. I find that possible channels include delay in age at first birth, contraceptives use, and assortative matching. Finally, I discuss robustness and specification tests.

4.4.1 RD Design: The Effects of UPE on Education

Figure **??** shows the effect of the reform on education using the highest intensity sample. Using the maximum bandwidth of 10 years on either side of the cutoff, the graph shows the average education for each birth cohort using the raw data. Since the earliest cohort to

[19] Only the 2003 and 2008 waves collect information on migration. To test if the reform induced people to move around the time of the reform, I regress an indicator variable for the likelihood of migrating on the reform. The coefficient on the reform is 0.038 with a P-value of 0.334. I also present results for the full sample in Appendix A.60. The results are consistent with the results for the non-migrant sample. Larreguy and Marshall (2017) note that about 77% of respondents in the Harmonized Nigeria Living Standards Survey (HNLSS) had not moved.

have benefited from the 1976 reform are those born in 1970, there is a jump in educational attainment at 1970. The corresponding regression estimates are shown in Table A.49. Being born after 1969 and thus eligible for the reform increased education by 0.8 years (35% of a standard deviation). The F-statistics from the first stage is 26, which provides evidence of a strong first-stage relationship.[20] Column 2 of Table A.49 presents the effects without controls and the results are similar to the base specification in column 1. Appendix Figure A.30 shows that the estimates are robust across different bandwidth specifications. The smallest bandwidth of 3 years yields an average effect of 1.4 years while the largest bandwidth of 10 yields an estimate of 0.8. Therefore, using a bandwidth of 10 gives an underestimate of the effect of the reform.

Having shown that the reform achieved its goal by increasing the average education of women, I now check that the effects I present are not picking up the general increasing trend in education. I conduct different falsification tests following Imbens and Lemieux (2008) to test for jumps at non-discontinuity points. I check for jumps at the median of the sample to the left and right of 1970. Using the sub-sample to the left, I create a 1965 placebo reform and a 1975 placebo reform using the sub-sample to the right of the cutoff. The placebo treatment groups are those born between 1965-1969, and 1975-1980 while the control groups are those born between 1960-1964, and 1970-1974, respectively. If the coefficient presented in column 1 is picking up a general trend in education, then the coefficients in column 3 and 4 should be positive and significant (spurious). However, that is not the case, the coefficients on the placebo reforms are not statistically significant. These test supports the identification that the exogenous change in education is brought about by the UPE reform.

In column 5, I show that the reform increased the probability of completing primary school, which was the goal of the reform. The reform increased the probability of women in the highest intensity region to have at least a primary education by 4.3 percentage points.

[20] As a validity check to show that the reform only affected areas in need of primary schools, in Appendix Table A.59 column 1, I show that the reform did not affect women living in the lowest intensity areas which are mostly southern areas. In column 2, the effect on women living in median intensity areas is positive but not significant at conventional levels.

There is also evidence that the reform induced some individuals to go beyond primary education (columns 6-7). The probability of having some secondary education increased by 2.3 percentage points and the probability of completing secondary school increased by 1.6 percentage points. I also find that women exposed to the reform are more likely to be literates compared to the control group.[21]

4.4.2 The Effects of Education on Fertility Outcomes

Panel A of Table A.50 shows the results from the OLS estimation. Here I regress fertility outcomes on education. Across the three outcomes, all coefficients are negative and statistically significant. As previously discussed, education is endogenous because it is correlated with other characteristics in the error term that also affect fertility. However, since I have established an exogenous shift in education that is not related to family characteristics or background, I can causally estimate the effect on fertility by instrumenting for education with the reform eligibility.

Panel B shows the reduced form estimates. Exposure to the reform does not affect the total number of births, the coefficient is positive but not significant. The effect on the number of births before the age of 25 is negative but not statistically significant. Column 3 however shows that women exposed to the reform have 0.14 fewer births than women who were not exposed to the reform. In Panel C, I present the effect of increasing education by one year on fertility outcomes. The main specification uses a bandwidth of ten and triangular kernel for estimation. There are no effects on total number of births. I am unable to examine the effects on completed fertility because a significant portion of the women are still within the childbearing age. The coefficient on the number of children born before the age of 25 is negative but not statistically significant (0.21). This estimate is slightly smaller than the effects presented in Osili and Long (2008) for Nigeria (0.26). In column 3, I find that increasing education by one year reduces the number of children born before the age of 18 by 0.19 births. This corresponds to a 6.75% decrease. One explanation for an effect

[21] Coefficient is 0.06 with a P-Value of 0.00

at age 18 compared to older ages is the incarceration effect which ends as soon as they are out of school. As shown in Table A.49, women exposed to the reform are 1.6 percentage points more likely to complete secondary school. Secondary school ends at the age of 18, which implies that keeping girls in school keeps them from getting pregnant early. No effects on total births and the timing of births by age 25 seem to suggest that although educated women delay when they have their first births, they still catch up in terms of total number of births. However, it will be informative to see if there is a difference in completed fertility.

Following Anderson (2008), I present the False Discovery Rate (FDR) Adjusted Q-values for the different measures of fertility outcomes in the bottom Panel of Table A.50. The adjusted Q-values are interpreted similar to p-values and they correct for the increased likelihood of rejecting the null hypothesis when making multiple comparisons. While the Q-values are slightly larger than the p-values, they do not affect the interpretation of the results.

Appendix Figures A.31, A.32, and A.33 show the estimates across different cohort bandwidths. The estimates are not significant across all bandwidths for total number of births and children born before 25. For number of children born before 18, the coefficients range from -0.1 to -0.19. In all bandwidths specifications, the estimates are stable up until a bandwidth of 10, which justifies using 10 years on either side of the cutoff as the main specification.

4.4.3 DID Design: The Effects of UPE on Education and Fertility Outcomes (full sample)

In this section, I discuss the effects of the reform on education and fertility using the full sample and a DID identification strategy. First, I estimate the effect of the reform on education. Figure A.27 is a dynamic difference-in-difference graph showing the reform did not affect women born before 1970. Table A.51 provides the estimates on education. Column 1 shows that the reform increased women's education. Specifically, moving from the lowest to highest intensity area increases education by 1.78 years. To put this in context, women living in a local government area with one standard deviation higher level of intensity have on average, 0.7 more years of education. To assess the relevance of the UPE reform to education, I test the null hypothesis that the UPE reform is jointly zero. The F- statistics from the first stage is 28, which suggest that the reform instrument is jointly different from zero.

Similar to the RD estimation, I show that the estimate is not reflecting the effects of other government programs implemented around 1976 (column 2). I show in column 2 that the estimates are not picking up a general trend in education by restricting the sample to women who were too old to benefit from the reform.[22] Column 3 shows the effect of the reform on women that were direct beneficiaries of the reform, that is, those who experienced the full fee elimination program (born between 1970 and 1975). The results are similar to the main estimates which suggests that increasing access to education was the driver for higher enrollment and not necessarily the fees elimination. Columns 4-6 show that the reform induced some individuals to have more than primary education.

Table A.52 provides the OLS, reduced form and 2SLS estimates for the full sample of women. The OLS estimates are negative and statistically significant across all outcomes. However, there are no effects on the reduced form and 2SLS estimates. The coefficient on the number of children born before 18 is negative but not statistically significant.

4.4.4 Potential Mechanisms

Next, I use the sample of households in the highest intensity regions to understand the underlying mechanisms through which education affects fertility outcomes.

Using the fuzzy RD design, Tables A.55 shows the effect of education on a set of potential mediators- age at first birth, first cohabitation, contraceptive use, and assortative matching. Column 1 of Table A.55 provides evidence that women are delaying the age at which they have their first births by 0.6 years on average. There is also suggestive evidence that they are getting married at a slightly older age (not statistically significant). These factors could

[22]These tests also support the identification assumption that in the absence of the reform, changes in education should not differ between the treatment and control group in areas with low and high UPE intensities and addresses concerns on mean reversion or catch-up.

be explained by the fact that as more children have access to education and are kept in school, they are more likely to be kept "out of trouble" through the incarceration effect. On average, they are delaying when they get married till when they are done with secondary school at the age of 18. Next, respondents are asked whether they use modern family planning methods. Column 3 shows that they are more 4 percentage points more likely to use modern contraceptives. Therefore, an important channel in reducing fertility rates is through educating girls and women on family planning methods.

Columns 4 and 5 show evidence of positive assortative matching. This is in line with Breierova and Duflo (2004),Keats (2018), and Lavy and Zablotsky (2011). More educated women are more likely to marry more educated men. The effect is 0.87 years and is statistically significant. There is also evidence that the spousal age gap shrinks as more women are getting more educated. The average spousal age gap in the sample is 12 years but education shrinks the gap by almost two years. These factors point towards women empowerment. It could be evidence that more educated girls and women are choosing their spouses rather than being forced into marriage. Since the reform affected both boys and girls, the effects on changes in fertility timing could be interpreted as being partly driven by the education of the spouse as well.

4.5 Robustness Checks

In this section, I address threats to the validity of the identification assumption. The identification of the effects of education hinges on the assumption that women born before and after the cutoff dates have similar predetermined characteristics.

In Panel B of Table A.53, I exclude controls from the main specification and the estimates are similar to the base specification in Panel A. This suggests that no no other determinants of outcomes are changing at the threshold. In Panel C, I include state fixed effects which compares the outcomes of women living within the same state, and I find that the results do not change in a meaningful way. Including the state fixed-effects addresses concerns relating to systematic differences across women living in different states which also affects the outcomes. Similarly, Panel D addresses concerns associated with other changes in the state, correlated with the UPE reform, that may differentially affect women in the treatment and control groups. The estimates are mostly unchanged when I control for other government reforms implemented around the time of the reform.

Panels E and F show the results using placebo reforms. Restricting the sample to those born before 1971 and assuming the reform happened in 1965, I find no effect on fertility outcomes in Panel E (the coefficients are not statistically significant). Creating a placebo 1975 reform year and restricting the sample to women born between 1970 and 1980, shows no discontinuity at the fake threshold. These results supports the identification that the main specification is not picking up a general trend in education. Finally, the estimates are not sensitive to varying functional forms (Panels G and H). The estimates remain stable with the exclusion of higher order and more flexible polynomials. The estimates are robust to varying bandwidths (Panel A – Panel D) of Table A.54. The estimates gets larger with a uniform kernel specification (Panel E) and allowing for heteroscedasticity-robust standard errors rather than clustering at year of birth does not change the results (Panel F). As previously discussed, there is a pattern of rounding in the reporting of year of birth in the survey. I follow Barreca et al. (2016) to address this heaping problem by including an indicator for heaped year of birth and an interaction of the indicator for heaps with the treatment variable. This approach removes the bias by allowing the heaped and non-heaped data to have different intercepts and slopes.[23] The coefficient on the number of children born before 25 becomes slightly larger and significant but the estimate on number of children before 18 is unchanged.

4.5.1 Other Robustness Checks

Appendix Table A.60 presents the results that address threats to identification for the DID identification. In Panel A, I exclude sample weights from the regression specifications

[23]However, if the treatment effects for the heaped and non-heaped data are different, this method will not recover the unbiased average treatment effect.

and the effects are still not statistically significant. Panel B controls for other changes in the state that may differentially affect fertility outcomes. I include state-level health and information expenditures in 1976 and the 1973 state population, all interacted with the cohort variable and the results remain unchanged. Including state-cohort fixed effects in Panel C to control for state programs or changes within states that differentially affected the UPE cohort does not affect the main estimates. Clustering standard errors at the 1976 state level does not affect the magnitude and significance of the estimates in Panel D. The estimates do not change when I restrict the sample to those born before 1976 (Panel E).

I address the issues relating to selective migration. The estimates could be biased if there are systematic differences between migrants and non-migrants. To circumvent this bias, my main estimates are restricted to non-migrants. Although I find no evidence that reform caused some people to move, the results for the full sample, which includes migrants and non-migrants are not different from the base specification (Panel F). Panel G excludes Lagos, the business capital of the country, to account for the fact that young people might be moving for economic reasons. Estimates in Panel H do not include differential trends in the pre-period. The effects are positive but not statistically significant. Panel I defines the intensity measure using an alternative cohort of women born between 1960-1964 to address the concern of partial treatment by women born between 1965 and 1969. The effects do not change qualitatively. Finally, the results in Panel J provide additional support for the identification assumption that in the absence of the reform, changes in education should not differ between the treatment and control group, in areas with low and high UPE intensities. Creating a placebo cohort similar to what was done for women in the previous section, where I restrict the sample to women born before 1970, shows no effect of the reform on those too old to benefit.

4.6 Conclusion

In this paper, I examine the impact of education in reducing fertility using a natural experiment in Nigeria. In 1976, the Federal Government of Nigeria implemented the Universal Primary Education (UPE) reform which provided free access to primary schools to children. With increased access to schools, primary school enrollment increased from 49% in 1975 to 86% in 1978, and by 1981 had increased by over 7 million with over 16,000 newly constructed schools. The study presents evidence that the reform increased schooling in the country and the increase in schooling led to a delay in the timing of fertility. There is a significant decrease in the number of births before the age of 18. The effect is partly driven by a delay in the age at first birth. The incarceration effect could explain this channel as girls are kept in school beyond primary school and thus less likely to get pregnant. This could explain why I do not see an effect on the number of children born before 25, when girls are no longer required to be in school. There is also evidence that the increased schooling leads to an increase in the use of modern contraceptives as a family planning method. Finally, the presence of positive assortative matching could also explain the delay in timing of births.

The identification strategies used in the study allow me to provide causal estimates. Using the fuzzy regression discontinuity approach, I exploit a discontinuity in eligibility for the UPE reform to estimate the effects of education on fertility outcomes, and a difference-in-differences identification strategy, where I exploit intensity in the reform using the pre-reform primary school enrollment rates across local governments and gender, to estimate the effects of the reform on full sample. The results are robust to a wide range of robustness and falsification tests. The estimates are however specific to the sub-population of women whose education is affected by the UPE reform.

While I find no evidence of reduction in total fertility and number of children born before 25, the results open up a discussion on interventions relevant for policy in reducing the fertility rate in Sub-Saharan Africa. Given the evidence on incarceration effects in the study, the government should enforce compulsory schooling and early marriage laws to reduce the prevalence of teenage pregnancy. The Child Rights Act in Nigeria, which was passed in 2003, sets the age of marriage at 18 years-old. Second, there should be an increase in awareness and access to family planning resources over the media and in curriculum at school. Third, remove barriers to women empowerment by granting easy access to capital such as credit and land. Fourth, address social norms that constrain women's economic opportunities.

5. SUMMARY AND CONCLUSIONS

The three essays in this dissertation examined factors that affect the human capital formation of children. Particularly, how exposure to negative events and an educational reform, respectively, contribute to shaping the longrun success of children. The three main research questions posed in this dissertation were answered using quasi-experimental research designs. In Section 2, I find that children exposed to negative events in formative years lag on their physical and cognitive development. The results of the analysis also show that the timing of exposure matters, as exposure during the first trimester affects cognitive development while exposure during the third trimester affects physical development. In Section 3, I find that parental education matters for child schooling. Children of more educated mothers are more likely to be on track in school, complete primary school and attend secondary school. In Section 4, I find that education affects the timing of birth as more educated women are more likely to delay when they have their first births.

The first essay employs a difference-in-differences strategy while the second and third essays use both a regression discontinuity design and difference-in-differences identification strategy to provide causal estimates. The purpose of estimating causal effects is to separate the effects of the event/reform from other confounding factors that could bias the results. In each section, I provide evidence to support the validity of the identification assumptions. The results presented have important implications for policy. First, given that shocks to child health affect adult outcomes, interventions should focus on improving nutrition and stimulation in disadvantaged children. Second, policies that educate girls hold longrun benefits into the future. We see that increasing mother's education is an important factor in the human capital production of children and provides a tool for breaking poverty cycles. Furthermore, policies that improve access to schools, especially in areas with low educational attainment, hold longrun benefits. Finally, education can affect fertility outcomes by delaying the age women have their first birth and increasing the use of modern contraceptives.

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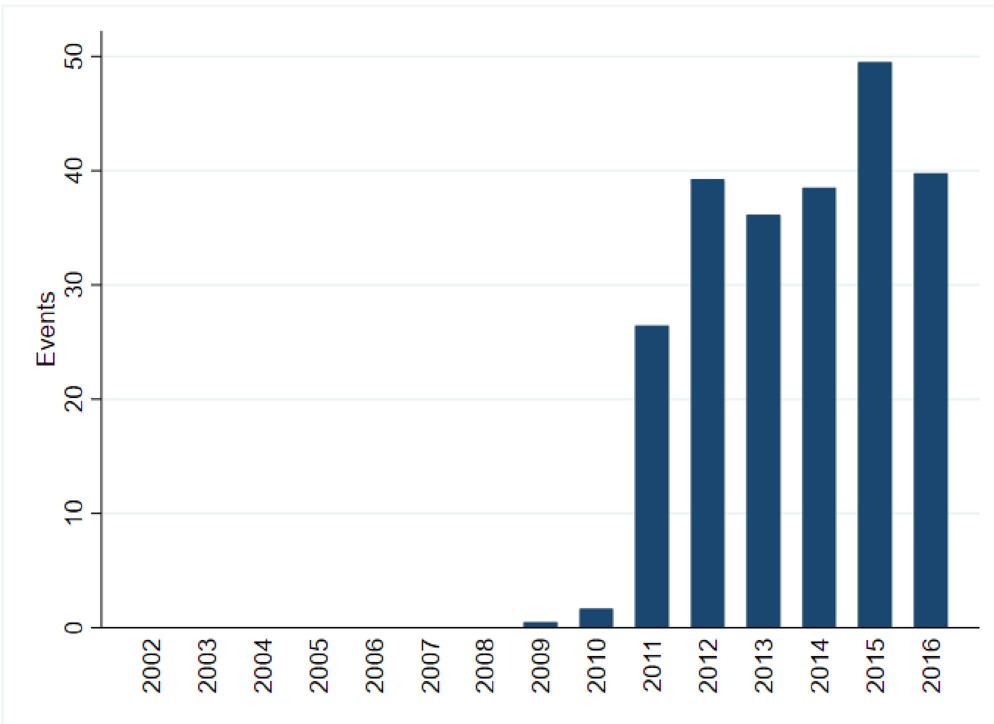
APPENDIX A

FIGURES AND TABLES

A.1 Early Childhood Development

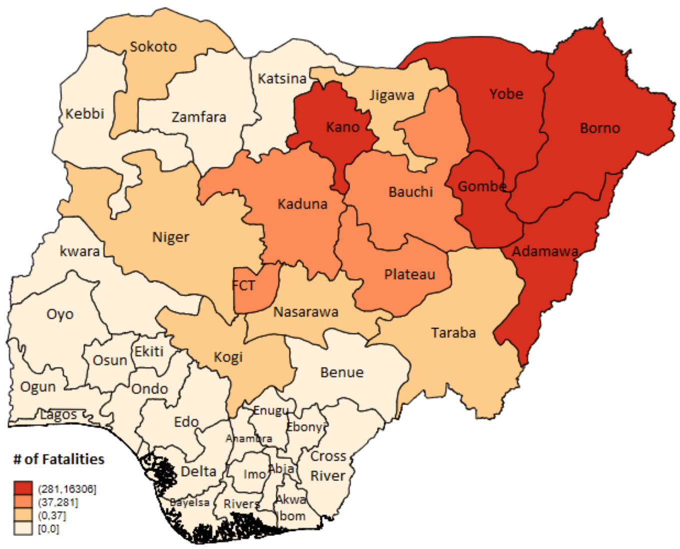
Figure A.1: Timing of Boko Haram attacks: Average events by year of occurrence



Source: ACLED 2017, (Raleigh et. al., 2010)

Notes: Data is from ACLED 2017, (Raleigh et. al., 2010). The figure in Panel A shows the average number of attacks caused by Boko Haram across different years. Events refers to different attacks caused by Boko Haram, whether or not they were fatal.

Figure A.2: Boko Haram attacks (fatalities) by states



Source: ACLED (Raleigh et. al. 2010)

Notes: Data is from ACLED 2017, (Raleigh et. al., 2010). This figure shows the intensity of Boko Haram fatalities across different states in Nigeria. Darker colors indicate higher level of fatalities.

Table A.1: Boko Haram events by states

State	Region	Events	Year started	State	Region	Events	Year started
Kogi	North Central	7	2012	Imo	South East	-	
Plateau	North Central	15	2010	Ebonyi	South East	-	
Benue	North Central	-		Abia	South East	-	
FCT Abuja	North Central	13	2011	Anambra	South East	-	
Nasarawa	North Central	3	2013	Enugu	South East	-	
Niger	North Central	8	2011	Edo	South South	-	
Kwara	North Central	-		Bayelsa	South South	-	
Borno	North East	799	2009	Akwa Ibom	South South	-	
Bauchi	North East	33	2009	Cross River	South South	-	
Taraba	North East	4	2012	Delta	South South	-	
Yobe	North East	153	2010	Rivers	South South	-	
Gombe	North East	25	2011	Osun	South West	-	
Adamawa	North East	101	2011	Oyo	South West	-	
Kaduna	North West	28	2012	Lagos	South West	-	
Kano	North West	65	2009	Ondo	South West	-	
Jigawa	North West	3	2012	Ekiti	South West	-	
Zamfara	North West	-		Ogun	South West	-	
Sokoto	North West	7	2011				
Kebbi	North West	-					
Katsina	North West	-					

Notes: Data is from ACLED, (Raleigh et. al. 2010). *Region* represents the geo-political zones in Nigeria (six in total). *Events* is the number of attacks caused by Boko Haram. *Year started* is the year Boko Haram launched its first attack in the state. Boko Haram attacks have been contained in the North so events for states in the south will be zero.

Table A.2: Summary statistics

Variable	N	Mean	Std. Dev.	Min	Max
Height (cms)	106,578	82.49	13.92	49	130
HAZ	106,578	-1.50	1.90	-6	6
Motor skills	20,568	0.83	0.38	0	1
Recognize alphabets	20,696	0.33	0.47	0	1
Recognize numbers	20,675	0.33	0.47	0	1
Read four words	20,699	0.23	0.42	0	1
Years of exposure	106,578	0.27	0.85	0	5
Age 0-2 exposure	106,578	0.11	0.31	0	1
Exposure to violence	106,578	0.13	0.33	0	1
In-utero exposure	106,578	0.07	0.25	0	1
Events per year	106,578	1.54	6.06	0	84.85
Cognitive ability index	20,798	-0.09	0.98	-1.51	2.02
Urban	106,578	0.33	0.47	0	1
Male	106,578	0.50	0.50	0	1
Age in months	106,578	28.72	17.22	0	59
Ethnicity					
Igbo	105,049	0.12	0.33	0	1
Yoruba	105,049	0.21	0.41	0	1
Other ethnicity	105,049	0.21	0.41	0	1
Hausa	105,049	0.29	0.45	0	1
Religion					
Islam	106,360	0.55	0.50	0	1
Christian	106,578	0.44	0.50	0	1
Traditional	106,360	0.01	0.08	0	1
None	106,360	0.00	0.04	0	1
Mother					
Age	103,128	29.74	6.96	15	49
Level of education	106,568	0.97	0.94	0	3
Head of Household					
Age	106,100	42.65	12.05	15	95
Gender	106,578	0.92	0.27	0	1
Wealth index	106,578	2.75	1.37	1	5

Notes: Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and from the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). *HAZ* is the height-for-age z-score. Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. *Motor skills* is a dummy variable that takes on one if the child is able to pick up an object with at least two fingers. *Recognize alphabets* is a dummy variable that takes on the value of one if the child can recognize at least ten alphabets. *Recognize numbers* is a dummy variable that takes on one if the child can recognize the first ten numbers. *Read four words* is a dummy variable that takes on one if the child can read at least four popular and simple words. *Cognitive ability* is a standardized measure of the four cognitive measures and motor skills. It is standardized with respect to the control group and age in bandwidths of 6 months. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Motor skills and cognition measures are only available in the 2011 and 2016/17 surveys. Wealth Index ranges from poorest (1) to richest (5).

Table A.3: Mean differences in outcomes

	Control	Treated	P-value
Height-for-age (SD)	-1.437	-1.930	0.000
	(1.91)	(1.83)	
Cognitive ability (SD)	-0.002	-0.546	0.000
	(1.00)	(0.69)	

Notes: *Cognitive ability* is a standardized measure of cognition and motor skills. It is standardized with respect to the control group and age, in bandwidths of 6 months. Standard deviation in parenthesis. The third column is the p-value from the equality of means between the treatment and control group for each outcome.

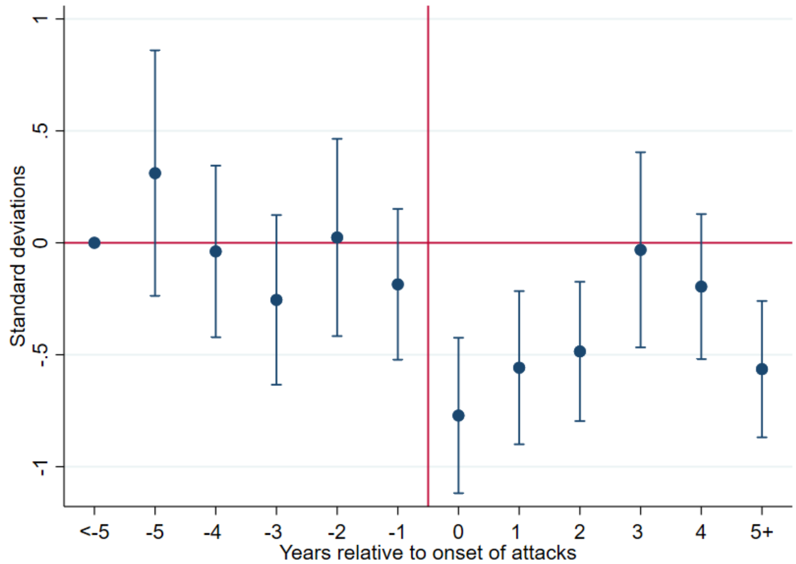
Table A.4: Balancedness of covariates: Effects of violence on predetermined characteristics

	Religion			**Ethnicity**		
	Christian	**Muslim**	**Other**	**Hausa**	**Yoruba**	**Ibo**
Presence of violence	-0.052**	0.045**	0.000	0.187***	-0.066**	-0.001
	(0.022)	(0.022)	(0.002)	(0.021)	(0.028)	(0.003)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	101,017	101,017	101,017	101,017	101,017	101,017

	Urban	**Gender**	**Mother's ed. level**	**Mother's age**	**HH: Age**	**HH: Gender**
Presence of violence	-0.109***	0.008	0.008	-0.113	-0.325	-0.010
	(0.035)	(0.009)	(0.037)	(0.172)	(0.322)	(0.007)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	101,017	101,017	101,017	101,017	101,017	101,017

Notes: *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and from the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2002 and 2017, aged 0- 59 months. Each regression includes state and year of birth fixed effects. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Figure A.3: Effects of violence on children height-for-age



Notes: The figure represents a dynamic difference-in-differences graph. The x-axis is the distance between the year observed in the data and the year violence started in the child's state. The year violence started is normalized to zero. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and from the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). Controls include gender, age, mother and household controls state and year of birth fixed effects. Confidence intervals are at the 95% significance level.

Table A.5: Effects of violence on height-for-age

	Height 1	Height 2	Height 3	Stunting 4
Exposed to Violence	-0.343***	-0.348***	-0.283*	0.071***
	(0.064)	(0.062)	(0.156)	(0.013)
Violence intensity		-0.325***		
(Share of total events exposure)		(0.053)		
Number of years of exposure		-0.080***		
		(0.018)		
Exposure before 3		-0.402***		
		(0.056)		
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
N	101,017	101,017	12,992	101,017

Notes: Height-for-age z-score (HAZ) is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Stunting is a dummy that takes on one if the HAZ is less than -2 standard deviations. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. *Violence intensity:* is the share of total Boko Haram events a child is exposed to in the state of residence. *Number of years of exposure:* the number of years a child is exposed to the violence. *Exposure before 3:* is a dummy variable that take on one if a child is exposed to violence before the age of three. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). Column 1 includes only state and year fixed effects, gender and urban residence. Column 3 includes birth weight. Birth-weight is only available for children 2 years and under. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.6: GPS Data: Effects of violence on height-for-age

	Within 5 kms	Within 5 kms	Within 10 kms	Within 20 kms	Within 50 kms	Within 100 kms
Exposure to violence	-0.187**	-0.166**	-0.078	0.002	0.055	-0.099
	(0.081)	(0.081)	(0.089)	(0.084)	(0.088)	(0.156)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time varying cluster characteristics	No	Yes	Yes	Yes	Yes	Yes
N	8,880	8,851	8,851	8,851	8,851	8,851

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Data is from the 2008 and 2013 Demographic and health Surveys which have GPS information. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. The sample includes children born between 2003 and 2013 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.
a: Includes controls for birth-weight

Table A.7: Effects of violence on cognitive ability

	Cognitive ability 1
Exposure to Violence	-0.183**
	(0.085)
Violence intensity	-0.077**
(Share of total events exposure)	(0.037)
Number of years of exposure	-0.034***
	(0.010)
Exposure *in utero*	-0.086**
	(0.033)
Exposure before 3	-0.047
	(0.044)
State fixed effects	Yes
Year fixed effects	Yes
Controls	Yes
N	18,504

Notes: The cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. *Violence intensity:* is the share of total Boko Haram events a child is exposed to in the state of residence. *Number of years of exposure:* the number of years a child is exposed to the violence. *Exposure before 3:* takes on one if a child is exposed to violence before the age of three. The motor skills and cognitive ability outcomes are only available in the 2011 and 2016/17 surveys and for children between 35-59 months. Controls include wealth index,type of residence, religion, ethnicity, and year of birth fixed effects. The sample includes children born between 2006 and 2014 aged 35-59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.8: Timing of *in-utero* exposure: Effect of violence on child outcomes

			Placebo violence exposure	
	Height-for-age 1	Child Dev 2	Height-for-age 3	Child Dev 4
Exposed to Violence- 1st trimester	-0.024	-0.092***	-0.078	-0.130
	(0.049)	(0.029)	(0.085)	(0.147)
Exposed to Violence- 2nd trimester	-0.044	-0.133	-0.199	-0.247
	(0.108)	(0.088)	(0.133)	(0.207)
Exposed to Violence- 3rd trimester	-0.285***	-0.026	-0.091	0.564***
	(0.102)	(0.165)	(0.132)	(0.081)
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	101,017	18,504	95,685	17,266

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Columns 3 and 4 are restricted to the placebo sample-children not exposed to violence while *in-utero*. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.9: Heterogeneous effects: Effects of violence on child outcomes

	Height-for-age	Cognitive ability
Exposure to violence x Male	0.020	0.020
	(0.031)	(0.027)
Exposure to violence x Urban	0.069	-0.106**
	(0.058)	(0.054)
Exposure to violence x Poor	-0.112**	0.040
	(0.049)	(0.042)
Exposed to violence (baseline)	-0.348***	-0.183**
	(0.062)	(0.085)
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	Yes	Yes
N	101,017	18,504

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.10: Robustness checks: Effects of violence on child outcomes

	Height-for-age	Cognitive ability
A: Placebo		
Exposure to violence	-0.073	
	(0.174)	
	[30,262]	
B: State-level clustering		
Exposure to violence	-0.348***	-0.183***
	(0.083)	(0.039)
C: All states affected		
Exposure to violence	-0.252***	-0.041
	(0.037)	(0.038)
D: Non Niger-Delta states		
Exposure to violence	-0.319***	-0.180**
	(0.063)	(0.085)
	[95,527]	[17,428]
E: Pre-determined covariates x cohort		
Exposure to violence	-0.368***	-0.174**
	(0.061)	(0.088)
F: State-cohort fixed effects		
Exposure to violence	-0.590***	-0.587***
	(0.077)	(0.123)
G: State-specific time trend		
Exposure to violence	-0.450***	
	(0.072)	
H: Weighted sample		
Exposure to violence	-0.321***	-0.086**
	(0.103)	(0.041)
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	Yes	Yes

Notes: *Notes:* Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2002 and 2017 aged 0- 59 months. Number of observations in bracket. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.11: Testing for selective fertility: Effects of violence on maternal characteristics and fertility decisions

	Age	Education	Ever had a child	Married	Number of children born
Have a child during violence	0.008	-0.044	0.001	-0.002	0.145
	(0.346)	(0.067)	(0.001)	(0.014)	(0.134)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
N	68,152	68,152	68,152	68,119	68,152

Notes: *Have a child during violence* is a dummy that takes on one if a woman had a child during a violent period interacted with living in a violent state. Data is from the Demographics and Health Surveys (DHS) 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys (MICS). Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes mothers of children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.12: Testing for selective mortality: Effects of violence on child mortality

	Neonatal mortality	Infant mortality	Child mortality	Sex-ratio
Exposure to violence	0.010	0.007	0.005	-0.001
	(0.007)	(0.009)	(0.010)	(0.007)
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No
N	45,517	46,219	46,817	1,115

Notes: Neonatal mortality is an indicator variable that takes on the value of one if the child died within the first 28 days of life. *Infant mortality* is an indicator variable that takes on the value of one if the child died before age one. *Child mortality* is an indicator variable that takes on the value of one if the child died before age five. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Sex-ratio is the ratio of boys to girls for each state-year combination. I only observe mortality outcomes for those in the DHS data and MICS 2016/17. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.13: Bounding estimates: Effects of violence on child outcomes

	Estimates	Lower bound	Upper bound
Height	-0.348***	-0.303***	-0.390***
	(0.062)	(0.063)	(0.062)
Cognitive ability	-0.183**	-0.183**	-0.938***
	(0.085)	(0.085)	(0.033)
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. To construct the lower (upper) bound, I reassign a treatment status to the top (bottom) 0.01% of children in the distribution of the outcomes from non-violent states to violent states, then I re-estimate equation 1. Each cell represents a different regression. Height is the height-for-age z-score using the WHO reference. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.14: Effects of violence on food consumption

	Milk	Grains and cereals	Tubers	Peas
Exposure to violence	-0.015	-0.070***	-0.026*	-0.016
	(0.013)	(0.015)	(0.014)	(0.015))
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	57,804	38,736	38,718	38,708

Notes: The sample includes children born between 2002 and 2016 and less than two years old in the DHS surveys and 2016 survey. Only the 2003 and 2016/17 surveys have detailed level data on children's food consumption. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the survey cluster level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.15: Effects of violence on parental investment and children health

	Time Investment (All)	Time Investment (Mother)	Time Investment (Father)	Material Investment	Morbidity
Exposure to violence	-0.265***	-0.252***	-0.209***	-0.234***	0.051***
	(0.058)	(0.061)	(0.050)	(0.027)	(0.013)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	28,942	28,942	28,942	49,614	101,883

Notes: Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007-2016/17 UNICEF Multiple Indicator Cluster Surveys (MICS). Data on violence is from ACLED, (Raleigh et. al. 2010). I use polychoric principal component analysis to construct a summary measure of the time investment in children. The items used to construct the index include indicator variable for time the parents or any individual over 14 years in the household spent with the child reading, singing, telling stories, playing, took the child outside, named, counted or drew objects with the child in the last 3 days before being surveyed. Material investment index variable is a standardized measure made up from a list of items in the house that a child plays with (toys and books), using the polychoric principal component analysis. Data on parental time and material investment only available in the MICS dataset. Time investment is only available for children between 35-59 months. Morbidity is a dummy variable representing if the child was sick with fever or diarrhea in the past two weeks before the time of the survey. Controls include age, gender, wealth index,type of residence, religion, ethnicity, religion, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.16: Effect of violence on child outcomes and sequential inclusion of potential mediators

		Step 0 ß	Step1 ß', d	Step 2 ß', d	Step 3 ß', d	Step 4 ß', d
A. Height (under age 3)						
Step 0	Violence	-0.154**	-0.145**	-0.146**		
		(0.070)	(0.070)	(0.070)		
Step 1	Food		0.082***	0.082***		
			(0.013)	(0.013)		
Step 2	Morbidity			-0.114***		
				(0.021)		
B. Child development (ages 3-4)						
Step 0	Violence	-0.187**	-0.168*	-0.145*	-0.147*	-0.141*
		(0.086)	(0.087)	(0.085)	(0.085)	(0.086)
Step 1	Mother's time		0.082***	0.070***	0.070***	0.069***
			(0.009)	(0.008)	(0.008)	(0.008)
	Father's time		0.065***	0.055***	0.055***	0.054***
			(0.007)	(0.007)	(0.007)	(0.007)
Step 2	Play objects			0.133***	0.133***	0.130***
				(0.008)	(0.008)	(0.008)
Step 3	Morbidity				0.027**	0.037***
					(0.013)	(0.013)
Step 4	Height-for-age					0.066***
						(0.004)

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Mother (father) time investment is a standardized measure made up of activities that the mother (father) engaged in with the child in the last 3 days before being surveyed, using the polychoric principal component analysis. Material investment index variable is a standardized measure made up from a list of items in the house that a child plays with (toys and books), using the polychoric principal component analysis. Morbidity is a dummy variable representing if the child was sick with fever or diarrhea in the past two weeks before the time of the survey. Each column represents a separate regression. For example in Panel A, Step 0 corresponds to the base regression without the inclusion of additional controls. Step 1 corresponds to the base regression, controlling for food. Step 2 includes the base specification and controls for food and morbidity. The difference in the magnitude of the coefficient on violence across the different steps accounts for the contribution of each mediator. Controls include wealth index,rural/urban, ethnicity, religion, state, cohort and year of birth fixed effects. Confidence intervals are the 95% bias-corrected confidence intervals with 200 replications. The first panel is for children under the age of 3. Information on specific foods is available for children in this age group across the surveys. The second panel is for children between ages 3 and 4. Information on investments is only available for children of this age group. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010).

To show that the estimates are not driven by migration, I construct lower and upper bounds. This exercise hinges on reassigning children with the best and worst development outcomes to treatment status. To construct the bounds, I follow Padilla-Romo (2016) and define $Exposed_t$ and $Not\text{-}exposed_t$ as the number of children exposed and not exposed to the Boko Haram attacks in year t, respectively, depending on their state of residence. Let $\%\Delta p$ represent the effect of Boko Haram attacks on the population ratio (number of children in each state for a given year divided by the total number of children observed in that year). To calculate the percentage of children who will be reassigned to treatment status, I solve for $\%\Delta' p$:

Let $Exposed_t = Not\text{-}exposed_t$

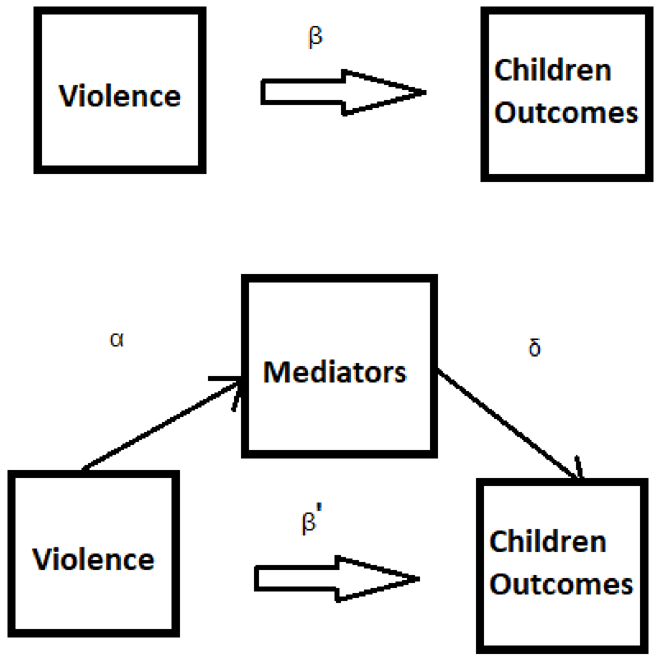
$$Exposed_{t+1} = (1 + 0.5 * \%\Delta p) * Exposed_t \tag{B. 1}$$

$$Exposed_t = (1 - \%\Delta' p) * Exposed_{t+1} \tag{B. 2}$$

Then solving B.1 and B.2,

$$\%\Delta' p = 1 - \frac{1}{(1 + 0.5 * \%\Delta p)} \tag{B. 3}$$

Figure A.4: Mediation chart



$$Y = \gamma_s + \gamma_t + \beta Violence + \gamma X + \epsilon \tag{B. 4}$$

$$Z_1 = \kappa_s + \kappa_t + \alpha_1 Violence + \kappa X + \eta \tag{B. 5}$$

$$Z_2 = \pi_s + \pi_t + \alpha_2 Violence + \pi X + \omega \tag{B. 6}$$

$$Y = \tau_s + \tau_t + \beta' Violence + \delta_1 Z_1 + \delta_2 Z_2 + \tau X + \mu \tag{B. 7}$$

$$Mediated \quad effect = \alpha_1 * \delta_1 + \alpha_2 * \delta_2 = \beta - \beta' \quad (in \quad OLS \quad regressions) \tag{B. 8}$$

Table A.17: Height comparison in centimeters

Age (months)	Nigeria		International	
	Boys	Girls	Boys	Girls
12	73	71.82	75.75	74.02
24	81.47	80.67	87.82	86.42
36	89.91	88.52	96.08	95.05
48	97.32	96.44	103.32	102.73
59	101.22	101.59	109.42	108.89

Notes: Height is measured in centimeters. Source: WHO growth chart (2006) and MCIS and DHS data. 1inch=2.54cms

Table A.18: Pairwise correlations

	Height-for-age	Child development	Mother's time	Father's time	Play objects	Wealth index
Child development	0.379					
Mother's time	0.196	0.396				
Father's time	0.091	0.244	0.378			
Play objects	0.065	0.348	0.249	0.115		
Wealth index	0.263	0.527	0.351	0.178	0.251	
Mother's education	0.249	0.494	0.385	0.207	0.219	0.587

Notes: All correlations are significant at the 1% level.*Notes*: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. Mother (father) time investment is a standardized measure made up of activities that the mother (father) engaged in with the child in the last 3 days before being surveyed, using the polychoric principal component analysis. Material investment index variable is a standardized measure made up from a list of items in the house that a child plays with (toys and books), using the polychoric principal component analysis. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010).

Table A.19: Effects of violence on other children outcomes

	Weight-for-height 1	Weight-for-age 2
Exposed to Violence	-0.010 (0.055) [100,710]	-0.219*** (0.060) [100,913]
Exposed to Violence (Within 40kms)	-0.095 (0.090) [8,851]	-0.166** (0.066) [8,851]
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	Yes	Yes

Notes: *Weight-for-height* measures "thinness or wasting" or "overweight". *Weight-for-age* reflects body mass relative to the age of the child. The variables are standardized with respect to the age, gender and weight of the 2006 WHO reference population. They are measures of a child's nutritional status. Higher numbers mean favorable health status. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. The sample includes children born between 2002 and 2013 aged 0- 59 months. Data is from the Demographics and Health Surveys (2003 - 2013). Data on violence is from ACLED, (Raleigh et. al. 2010). Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.20: Effects of violence on height

	Height
Exposure to violence	-0.454***
	(0.155)
One year before violence	-0.208
	(0.174)
Two years before violence	-0.021
	(0.222)
Three years before violence	-0.181
	(0.192)
Four years before violence	-0.002
	(0.197)
Five years before violence	0.311
	(0.278)
State fixed effects	Yes
Year fixed effects	Yes
Controls	Yes

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.21: Effects of violence on cognitive ability (alternative specifications)

	Child dev 1	Child dev 2
Exposed to Violence (controlling for age dummies)	-0.060** (0.029)	
Exposed to Violence (controlling for polynomial age)		-0.183** (0.085)
Exposed to Violence (3 months age group)		-0.187** (0.086)
Outcome standardized	No	Yes
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	Yes	Yes
N	18,504	18,504

Notes: Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Column 1 includes state and year fixed effects, gender and urban residence. Controls include wealth index,type of residence, religion, ethnicity, state-specific time trend state and year of birth fixed effects. Data is from the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). Sample includes children between 35-59 months. Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.22: Heterogeneous effects: Effects of violence on child outcomes, by wealth

	Height	Child dev
Exposure x Poor	-0.300***	-0.092
	(0.078)	(0.091)
	[47,550]	[9,861]
Exposure x Middle	-0.378***	-0.574**
	(0.116)	(0.287)
	[20,481]	[3,737]
Exposure x Rich	-0.186*	-0.117
	(0.095)	(0.308)
	[32,986]	[5,086]
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	Yes	Yes

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Wealth category is split into poor, middle and rich households. Each column represents a different regression. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.23: Effects persistence: Effects of violence on child outcomes

	Height	Cognitive Ability
Exposure before 3 x](Four years old)	0.041	-0.138***
	(0.056)	(0.029)
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	Yes	Yes
N	39,468	18,469

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2006 and 2014 aged 36- 59 months. Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.24: Other results: Effects of violence on children outcomes

	Height-for-age	Cognitive ability
A: Two-way clustering (region and year of birth)		
Exposure to violence	-0.348***	-0.183**
	(0.046)	(0.084)
B: Excluding state with highest violence occurrence		
Exposure to violence	-0.407***	-0.177**
	(0.067)	(0.086)

Notes: Height-for-age z-score is calculated using the child's height (measured in centimeters), age in months, gender, and standardized with respect to the height of the 2006 WHO reference population. Cognitive ability index is a standardized measure of a child's cognitive ability and motor shills development. It measures the child's ability to recognize the first 10 alphabets and numbers, read at least four words and whether a child can pick up a small object from the ground with at least 2 fingers. *Exposure to violence* is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). The sample includes children born between 2002 and 2017 aged 0- 59 months. Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

Table A.25: Falsification tests: Effects of violence on pre-determined characteristics

	Gender	Mother's age	Mother's education	Head of Household's age
Exposure to violence	0.009	0.111	0.000	-0.423
	(0.009)	(0.147)	(0.024)	(0.272)
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	101,017	101,017	101,017	101,017

Notes: Exposure to violence is a dummy variable that takes on one if a child living in the Northeast is observed in the data the year violence started or after violence started in the state. Controls include age, gender, wealth index,type of residence, religion, ethnicity, mother and head of household's age, educational level, survey rounds and year of birth and state fixed effects. The sample includes children born between 2002 and 2017 aged 0- 59 months. Data is from the Demographics and Health Surveys 2003 - 2013 surveys, and the 2007 - 2016/17 UNICEF Multiple Indicator Cluster Surveys. Data on violence is from ACLED, (Raleigh et. al. 2010). Standard errors are clustered at the enumeration level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

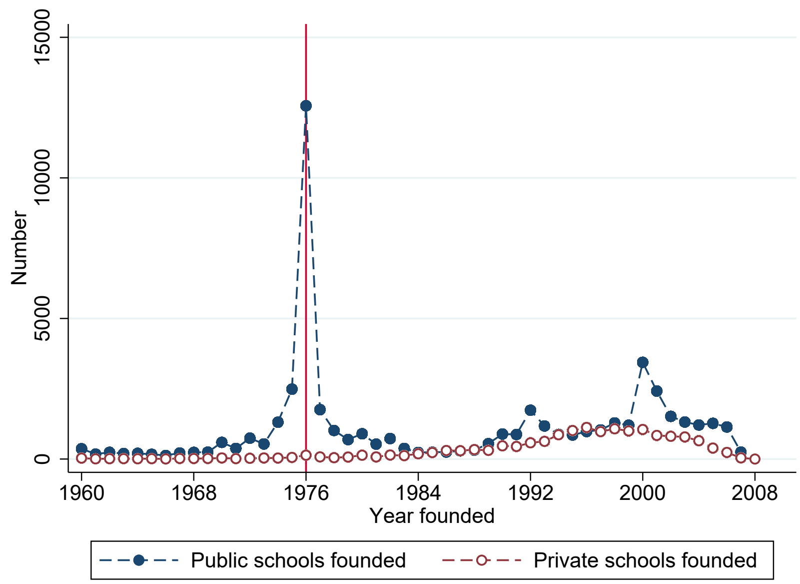
Table A.26: Mediated effects of violence on child outcomes (change in the effect of violence after inclusion of potential mediators)

	Step 1: Food		Step 2: Morbidity					
	Mediated effect	95% CI	Mediated effect	95% CI				
Change in violence								
1. Height (under age 3)	-0.009	(-0.013 -0.0045)***	-0.001	(-0.002 0.003)				
	Step 1: Parent's time investment		Step 2: Material investment		Step 3: Morbidity		Step 4: Height-for-age	
	Mediated effect	95% CI	Mediated effect	95% CI	Mediated effect	95% CI	Mediated effect	95% CI
2. Cognitive ability (ages 3-4)	-0.016	(-0.032 0 .001)**	-0.026	(-0.041 -0.010)***	0.003	(0 .0001 0.007)	-0.007	(-0.022 0.011)

Notes: This table test the significance of the indirect effect of each mediator. The mediated or indirect effect of each mediator is (α x δ) Controls include rural/urban, ethnicity, religion, wealth index, state, cohort and year of birth fixed effects. The first panel is for children under the age of 3. Information on specific foods is only available for children under 3 in the 2016 MICS survey. The second panel are for children between ages 3 and 4. Information on cognition and time investments is only available for children of this age group and in the MICS survey. Confidence intervals are the 95% bias-corrected confidence intervals (CI) with 200 replications. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level.

A.2 Parental Education and Child Schooling

Figure A.5: Primary School Founding Dates



Source:Larreguy and Marshall (2017)

Notes: The graph shows the number of primary schools (public and private) founded between 1960 and 2008.

Table A.27: Number of primary schools built by state (1975-1981)

State	Region	1975	1981	Number of new schools
Sokoto	Northern	732	3,939	3,207
Kano	Northern	678	3,063	2,385
Kaduna	Northern	859	2,875	2,016
Benue	Northern	1,200	2,703	1,503
Plateau	Northern	685	1,661	976
Kwara	Northern	539	1,487	948
Niger	Northern	245	1,067	822
Bauchi	Northern	1,086	1,805	719
Oyo	Western	1,995	2,701	706
Borno	Northern	1,526	2,088	562
Ondo	Western	1,159	1,595	436
Rivers	Eastern	595	1,001	406
Anambra	Eastern	1,708	2,054	346
Lagos	Western	544	863	319
Gongola	Northern	1,564	1,864	300
Bendel	Midwestern	1,562	1,754	192
Cross River	Eastern	1,505	1,690	185
Ogun	Western	1,161	1,262	101
Imo	Eastern	1,880	1,955	75
		21,223	**37,427**	**16,204**

Source: Social Statistics of Nigeria (1979) and Nigerian Annual Abstract of Statistics (1985).

Table A.28: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	44,220	10.35	3.46	5	17
Gender	44,220	0.52	0.50	0	1
Years of education	44,220	2.78	3.19	0	14
Percent of children with:					
Complete primary school	18,098	0.54	0.50	0	1
Attend some secondary school	17,981	0.50	0.50	0	1
Grade-for-age	44,220	0.53	0.50	0	1
Mothers					
Age	44,220	39.54	5.83	23	53
Years of education	44,220	4.31	5.10	0	22
UPE Intensity	44,220	0.64	0.34	0	1
Fathers					
Age	34,786	48.01	6.83	24	63
Years of education	34,786	5.96	5.84	0	21
UPE Intensity	34,735	0.40	0.33	0	1
Wealth index	44,220	2.85	1.41	1	5
Non-migrants	30,570	0.53	0.50	0	1
Urban region	44,220	0.33	0.47	0	1

Notes: Data is from the Nigerian Demographic and Health Surveys 2003-2013. *Grade-for-age*: measures if a child is making normal progress through school. It is a dummy variable that takes on the value of one if the difference between the age of the child and class grade is at most six. *Primary school completion:* probability of completing primary school. It applies to children who are at least 12 years old but also includes younger children who have completed primary school. *Attend some secondary school*: probability of ever attending secondary school. It applies to children who are at least 12 years old but also includes younger children who have enrolled in secondary school. *Intensity*: proportion of females (males) born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest). Wealth index is a composite measure of standard of living that ranges from 1 (poorest) to 5 (richest). Sample size is smaller for the non-migrants variable because only the 2003 and 2008 waves have information on migration. Non-migrants are children whose mothers are still living in the areas where they were born or attended primary school.

Figure A.6: Proportion of females born between 1960 and 1969 not completing primary school, by state

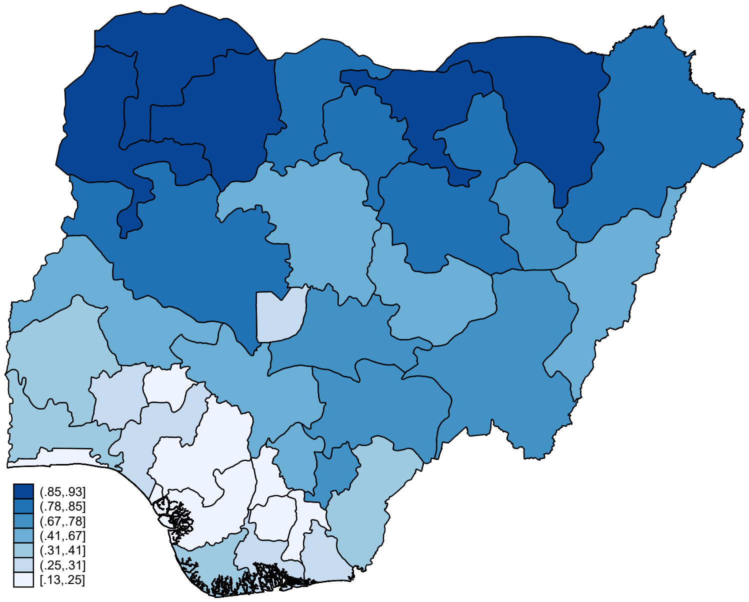
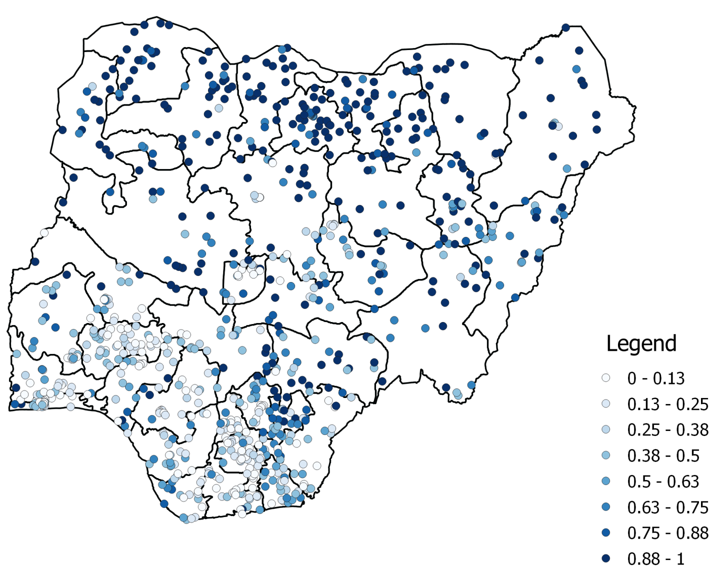
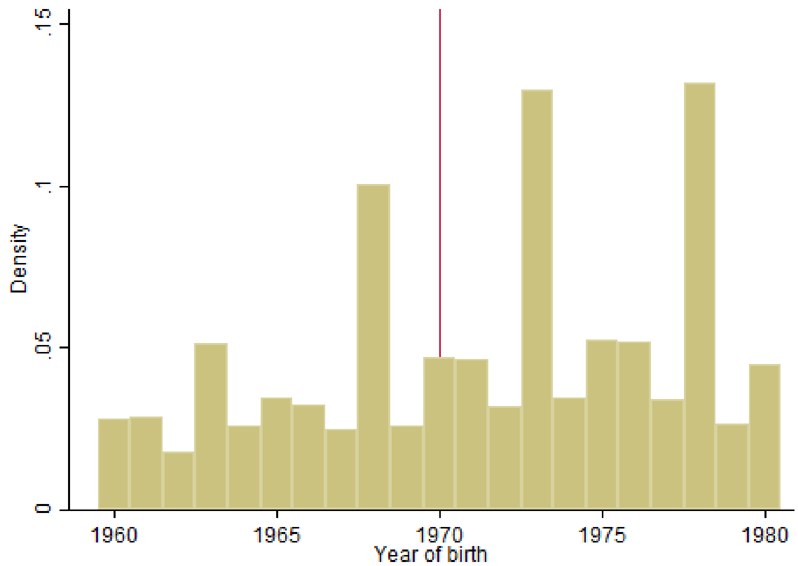


Figure A.7: Proportion of females born between 1960 and 1969 not completing primary school, by clusters



Notes: The maps show the UPE intensities for the the 36 states and the Federal Capital Territory in Figure A.23, and for the survey clusters in Figure A.24, using the 2013 DHS data. Intensity is the proportion of females born between 1960 and 1969, living in an area not completing primary school. Intensity ranges from zero (lowest) to one (highest). Darker colors represent higher UPE intensity areas and lighter colors represent lower UPE intensity areas.

Figure A.8: Distribution of maternal year of birth



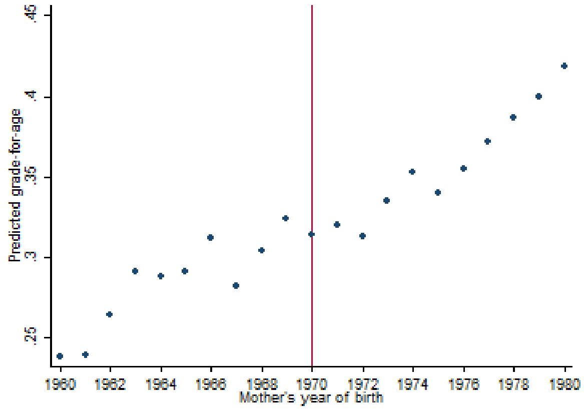
Notes: The graph represents the distribution of maternal year of birth between 1960 and 1980. Data is from the Demographic and Health Surveys between 2003 and 2013. There is a rounding age pattern in the survey. The most obvious is at multiples of 5 years, which represents the spikes at 1963, 1968, 173, 1978, 1983 and 1988. The pattern is consistent across the distribution and is not an evidence of manipulation at the 1970. threshold.

Table A.29: Smoothness of baseline covariates: Effect of UPE reform on predicted schooling

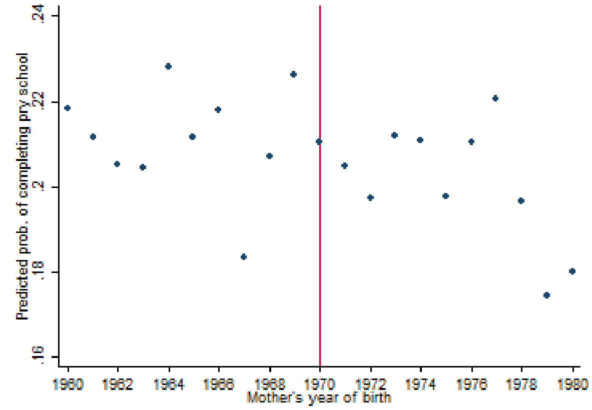
	Grade-for-age	**Complete primary school**	**Attend secondary school**
	1	2	3
Post-UPE	-0.002	-0.002	-0.003
	(0.006)	(0.007)	(0.004)
N	9,579	3,418	3,393

Notes:Grade-for-age: measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. *Post-UPE*: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. Predicted outcomes are based on characteristics that should not vary across the threshold. They include gender, age of child, type of residence, and survey rounds. Sample includes children whose mothers are born between 1960-1980. Standard errors are clustered at maternal year of birth level and reported in parentheses. None of the effects are significant at conventional levels.

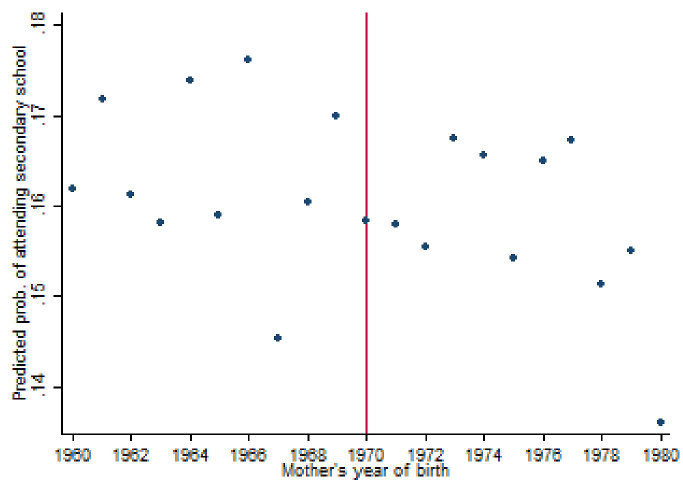
Figure A.9: Smoothness of baseline covariates: Effect of UPE reform on predicted schooling



(a) Grade-for-age



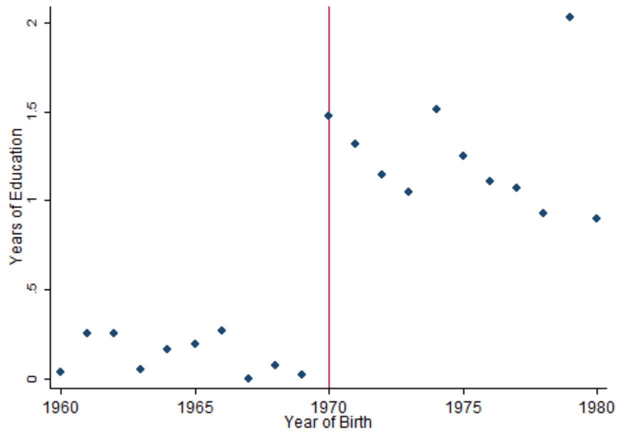
(b) Primary school completion



(c) Secondary school attendance

Notes: Predicted outcomes are based on gender, age of child, type of residence, and survey rounds. They exclude the treatment variable-whether a mother was born before or after the reform. The dots represent averages of predicted schooling for each cohort. Sample includes children whose mothers were born between 1960-1980. *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school.

Figure A.10: First stage: Effect of UPE reform on maternal education



(a) Educational attainment of mothers by birth cohort

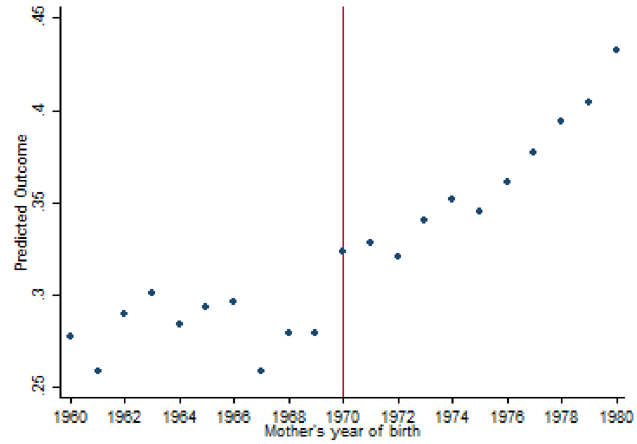
Notes: This graph shows the average years of education for each cohort living in the highest intensity areas. Sample includes women born between 1960-1980. With the reform starting in 1976 and the official school starting age being six, women born in 1970 and later are eligible for the reform. The graph represents averages from the raw data.

Table A.30: Effects of UPE reform on maternal education

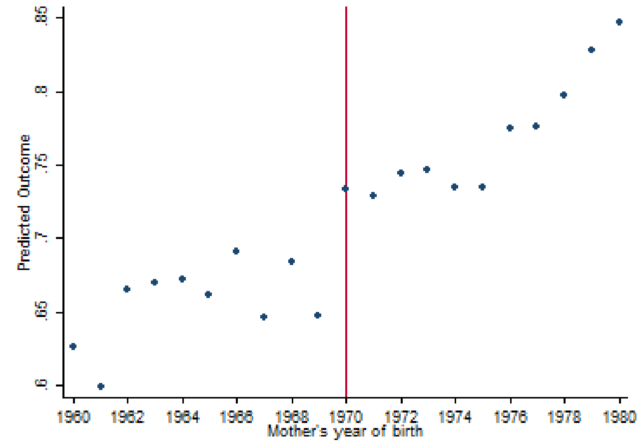
	Years of education				**Complete primary school**	**Incomplete secondary school**	**Complete secondary school**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Post-UPE	1.331***	1.328***			0.156***	0.058***	0.033***
	(0.090)	(0.089)			(0.008)	(0.007)	(0.007)
Placebo 1965 reform			0.133				
			(0.099)				
Placebo 1975 reform				0.168			
				(0.231)			
N	9,579	9,579	4,261	9,725	9,579	9,579	9,579
Outcome Mean	0.79	0.79	0.11	1.20	0.086	0.029	0.017
Outcome SD	2.39	2.39	0.63	2.77	0.28	0.17	0.13
F-Statistics	218.7	221.25	1.81	0.53			
Bandwidth	7	7	5	5	7	7	7
Controls	Yes	No	Yes	Yes	Yes	Yes	Yes

Notes: *Post-UPE*: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. To test for jumps at non-discontinuity points, I split the sample into two: below the threshold and above the threshold. For each sub-sample, I use the median value as a placebo reform year and test for a jump at that point. The two placebo reforms are at 1965 and 1975. *Complete primary school:* probability of completing primary school. *Incomplete secondary school*: probability of having at least some secondary education. *Complete secondary school*: probability of completing secondary school. Control variables include age of child, gender, type of residence, and survey rounds. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

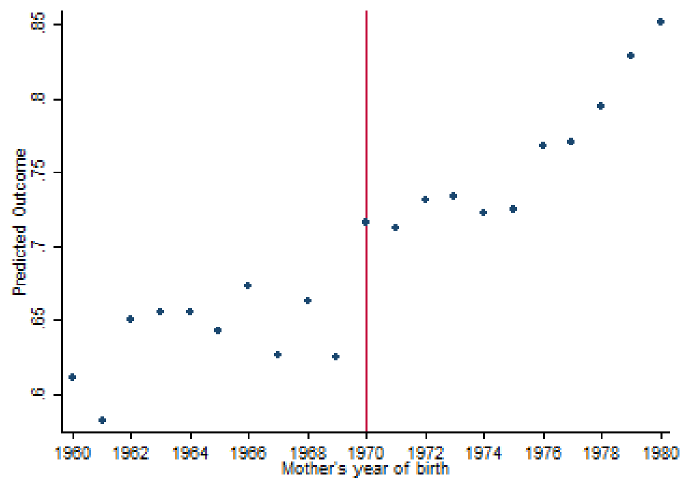
Figure A.11: Reduced form estimates: UPE reform and child schooling



(a) Grade-for-age



(b) Primary school completion



(c) Secondary school attendance

Notes: The graphs represents predicted outcomes of child schooling, without control variables. Each dot represents the cohort average. Sample includes children whose mothers were born between 1960-1980. With the reform starting in 1976 and the official school starting age being six, women born in 1970 and later are eligible for the reform. *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school.

Table A.31: Effects of maternal education on child schooling

	Grade-for-age	**Complete primary school**	**Attend secondary school**
	1	2	3
OLS			
Maternal education	0.027***	0.037***	0.037***
	(0.003)	(0.004)	(0.004)
Reduced Form			
Post UPE	0.057**	0.068***	0.069***
	(0.019)	(0.019)	(0.015)
2SLS			
Maternal education	0.043***	0.047***	0.047***
	(0.015)	(0.013)	(0.011)
N	9,579	3,418	3,393
Outcome Mean	0.32	0.21	0.16
Outcome SD	0.47	0.41	0.37
First stage F statistic	218.70	218.70	218.70
Unadjusted p-value	0.013**	0.02***	0.0002***
Adjusted q-value	0.02**	0.02***	0.0006***
Bandwidth	7	7	7
Controls	Yes	Yes	Yes

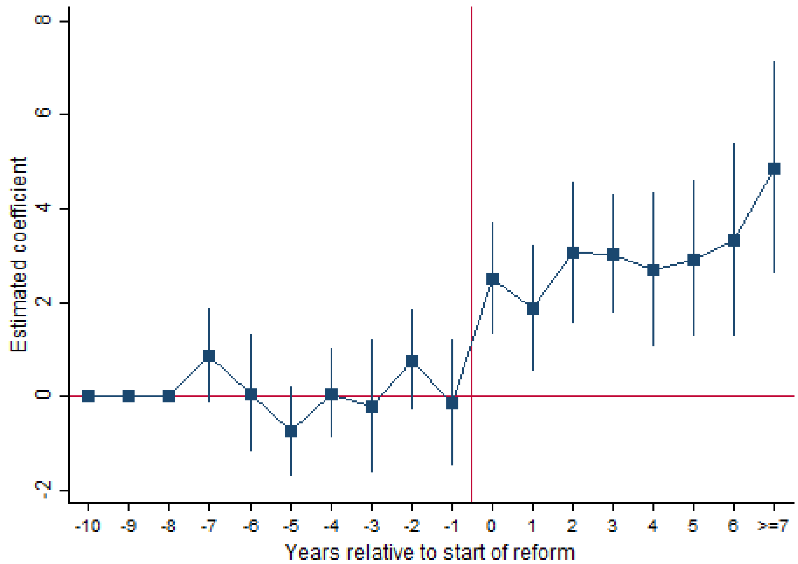
Notes: Post-UPE: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. With the reform starting in 1976 and the official school starting age being six, women born in 1970 and later are eligible for the reform. *Maternal education*: total number of years of maternal schooling. *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. Control variables include age of child, gender, type of residence, and survey rounds. The F-statistics are from test of the reform impact on maternal schooling. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.32: Effects of maternal education on child schooling, by gender and region

	Grade-for-age	**Complete primary education**	**Attend secondary school**
	1	2	3
Gender			
Mother's education x Male	-0.017**	-0.020	-0.029**
	(0.008)	(0.017)	(0.015)
Region			
Mother's education x Urban	-0.006	-0.037	-0.015
	(0.026)	(0.040)	(0.041)
N	9,579	3,418	3,393
Bandwidth	7	7	7
Controls	Yes	Yes	Yes

Notes: Grade-for-age: measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. *Maternal education*: total number of years of maternal schooling. Control variables include age of child, gender, type of residence, and survey rounds. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.12: Effects of UPE reform on maternal education (full sample)



Notes: Dynamic difference-in-differences estimates from equation 4. The regression includes maternal year of birth fixed effects, state fixed effects and state-specific linear trends. Data is from the full sample of mothers from all intensity areas born between 1960-1980. The x-axis measures the distance between when a mother started primary school and when the reform started in 1976. The reform year 1976 is normalized to zero. Since the official school starting age is six, women born in 1979 started school in 1976. Control variables include age of child, gender, type of residence, and survey rounds. Standard errors are clustered at the state level. Confidence intervals are at the 95% significance level.

Table A.33: First stage: Effects of UPE reform on maternal education (full sample)

	Years of education			Complete primary school	Incomplete secondary school	Complete secondary school
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Post-UPE x Intensity	2.450***	2.571***		0.240***	0.125***	0.064**
	(0.315)	(0.332)		(0.029)	(0.030)	(0.028)
Placebo 1968 x Intensity			-0.534			
			(0.449)			
N	44,220	44,220	12,465	44,220	44,220	44,220
Outcome mean	4.31	4.31	4.2	0.44	0.25	0.17
Outcome SD	5.10	5.10	5.2	0.50	0.43	0.37
Instrument SD	0.41	0.41	0.38	0.41	0.41	0.41
First stage F Statistic	60.59	59.8	1.42			
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Other government programs Controls	No	Yes	No	No	No	No

Notes: Post-UPE: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. *Intensity*: proportion of females (males) born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest). *Placebo 1968:* fake reform year in 1968. Placebo sample includes women born between in 1960-1967 and the placebo treated cohort are born between 1964 and 1967. Sample consists of children from all intensity areas. Control variables include age of child, gender, type of residence, and survey rounds. All regressions include mother's year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.34: Effects of UPE reform on child schooling (full sample)

	Grade-for-age	**Complete primary education**	**Attend secondary school**
	1	2	3
Reduced Form			
Post UPE x Intensity	0.042*	0.067**	0.073***
	(0.023)	(0.025)	(0.025)
N	44,220	18,098	17,981
Outcome Mean	0.53	0.54	0.50
Outcome SD	0.50	0.50	0.50
Controls	Yes	Yes	Yes

Notes: Grade-for-age: measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. *Post-UPE*: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. *Intensity*: proportion of females (males) born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest). Sample consists of children from all intensity areas. Control variables include age of child, gender, type of residence, and survey rounds. All regressions include mother's year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.35: Robustness checks

	Grade-for-age	Complete primary school	Attend secondary school
	1	2	3
A: Main estimates			
Maternal education	0.043***	0.047***	0.047***
	(0.015)	(0.013)	(0.011)
N	[9,579]	[3,418]	[3,393]
B: Without controls			
Maternal education	0.040**	0.044***	0.044***
	(0.016)	(0.012)	(0.010)
N	[9,579]	[3,418]	[3,393]
C: With state fixed effects			
Maternal education	0.038**	0.048***	0.044***
	(0.018)	(0.010)	(0.011)
N	[9,579]	[3,418]	[3,393]
D: Controlling for other government programs			
Maternal education	0.042***	0.043***	0.043***
	(0.014)	(0.011)	(0.009)
N	[9,579]	[3,418]	[3,393]
E: Placebo 1965 reform			
Maternal education	0.613	0.509	0.355
	(0.378)	(0.574)	(0.535)
N	[4,234]	[1,706]	[1,694]
F: Placebo 1975 reform			
Maternal education	-0.059	-0.927	-0.485
	(0.137)	(4.816)	(1.855)
N	[9,694]	[2,906]	[2,887]
G: Quadratic functional form of year of birth			
Maternal education	0.052***	0.065***	0.061***
	(0.015)	(0.012)	(0.009)
N	[9,579]	[3,418]	[3,393]
H: Quadratic functional form (intercept and slope)			
Maternal education	0.071***	0.072***	0.051***
	(0.026)	(0.016)	(0.013)
N	[9,579]	[3,418]	[3,393]

Notes: *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. Control variables include age of child, gender, type of residence, and survey rounds. Other government programs include 1976 health and information expenditure implemented across states. Number of observations are in bracket. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.36: Robustness checks (continued)

	Grade-for-age	Complete primary school	Attend secondary school
	1	2	3
A: Uniform weights			
Maternal schooling	0.031*	0.046***	0.051***
	(0.016)	(0.013)	(0.011)
N	[9,579]	[3,418]	[3,393]
B: 5 years bandwidth			
Maternal schooling	0.058***	0.057***	0.050***
	(0.015)	(0.016)	(0.011)
	[7,445]	[2,693]	[2,676]
C: 6 years bandwidth			
Maternal schooling	0.054***	0.051***	0.047***
	(0.015)	(0.015)	(0.011)
N	[8,455]	[3,014]	[2,994]
D: 8 years bandwidth			
Maternal schooling	0.041***	0.043***	0.046***
	(0.014)	(0.012)	(0.010)
N	[12,301]	[4,134]	[4,015]
E: 9 years bandwidth			
Maternal schooling	0.039***	0.041***	0.044***
	(0.014)	(0.011)	(0.010)
N	[12,877]	[4,331]	[4,303]
F: Robust standard errors			
Maternal schooling	0.043***	0.047**	0.047***
	(0.011)	(0.019)	(0.017)
N	[9,579]	[3,418]	[3,393]
G: Controlling for heaps (allowing different intercept)			
Maternal schooling	0.038***	0.056***	0.050***
	(0.013)	(0.012)	(0.010)
N	[9,579]	[3,418]	[3,393]
H: Controlling for heaps (allowing different intercept and slope)			
Maternal schooling	0.025**	0.050***	0.045***
	(0.010)	(0.013)	(0.011)
N	[9,579]	[3,418]	[3,393]

Notes: *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. Heaps in the data are at multiples of 5's in reporting maternal year of birth: 1963, 1968, 1973 and 1978. Control variables include age of child, gender, type of residence, and survey rounds. Number of observations are in bracket. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.37: Effects of maternal education on potential mediators -marriage market (2SLS estimates)

	Living with partner 1	**Paternal education** 2	**Spousal age difference** 3	**Age at first cohabitation** 4	**Age at first birth** 5	**Number of children** 6	**Wealth index** 7
Maternal education	0.001	0.923***	-0.355	0.052	0.088	-0.038	0.074**
	(0.004)	(0.208)	(0.469)	(0.129)	(0.105)	(0.106)	(0.033)
N	9,116	9,342	9,342	9,188	9,187	9,621	9,621
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the effect of maternal education on the potential mediators, instrumenting for maternal education with the reform eligibility. This is the fuzzy RD design used in the children's analysis. *Living with partner*: a dummy variable indicating if a woman is living with her partner. *Paternal education*: years of education of the father. *Spousal age difference*: difference between a woman's age and her spouse's age. *Age at first cohabitation*: age at which a mother started living with a man. *Age at first birth*: age at which a mother had her first child. *Number of children*: number of children a woman has ever had. *Wealth index*: measures the living condition and economic status of a household. Control variables include age of child, gender, type of residence, and survey rounds. Sample size varies by data availability. Standard errors are clustered at the maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.38: Effect of maternal education on potential mediators (continued)

	Worked in the last 12 months 1	**Paid work** 2	**Involved in decisions about children's education** 3	**health** 4
Maternal schooling	0.021	-0.028	0.067*	0.094**
	(0.013)	(0.029)	(0.039)	(0.041)
N	9170	6255	1152	1196
Controls	Yes	Yes	Yes	yes

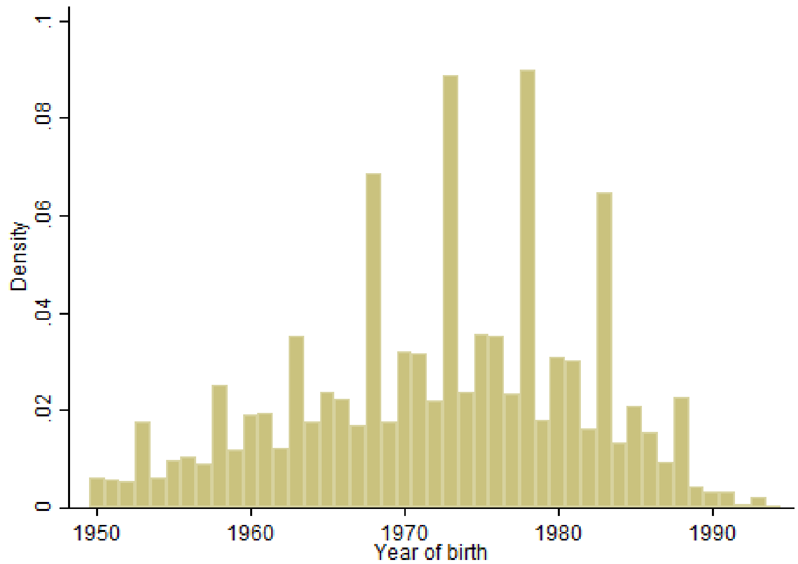
Notes: *Worked in the last 12 months*: a dummy variable that takes on one if the mother was in the labor force in the last 12 months and zero otherwise. *Paid work*: a dummy variable that takes on one if the mother works for pay and zero otherwise. *Involved in child education*: a dummy variable that takes on one if the mother is involved in decisions about the child's education and zero otherwise (available only in the 2003 survey wave). *Involved in child health*: a dummy variable that takes on one if the mother is involved in decisions about the child's health and zero otherwise (available only in the 2003 survey wave) Control variables include age of child, gender, type of residence, and survey rounds. Sample size varies by data availability. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.39: Mediation analysis

	Step 0	Step 1 Wealth Index	Step 2 Paternal education
1. Grade-for-age			
Step 0: Maternal education	0.052***	0.048***	0.047***
	(0.013)	(0.013)	(0.017)
Step 1: Wealth index		0.062***	0.061***
		(0.011)	(0.008)
Step 2: Paternal education			0.001
			(0.004)
Father= Mother (p-value)	0.024		
N	7,667		
2. Complete primary school			
Step 0: Maternal education	0.067***	0.061***	0.057*
	(0.021)	(0.021)	(0.030)
Step 1: Wealth index		0.070***	0.065***
		(0.020)	(0.014)
Step 2: Paternal education			0.004
			(0.008)
Father= Mother (p-value)	0.057		
N	2,700		
3. Attend secondary school			
Step 0: Maternal education	0.061***	0.061***	0.058**
	(0.018)	(0.018)	(0.026)
Step 1: Wealth index		0.070***	0.066***
		(0.017)	(0.013)
Step 2: Paternal education			0.003
			(0.007)
Father= Mother (p-value)	0.032		
N	2,682		

Notes: Each column represents a different regression after the sequential inclusion of a potential mediator. *Wealth index*: measures the living condition and economic status of a household. Step 0 corresponds to the base regression without the inclusion of additional controls. Step 1 corresponds to the base regression, controlling for wealth index. Step 2 includes the base specification and controls for wealth index and father's education. The difference in the magnitude of the coefficient on maternal education across the different steps accounts for the contribution of each mediator. Father = Mother indicates the p-value testing the equality of coefficients of paternal and maternal education. Control variables include age of child, gender, type of residence, and survey rounds. Sample is restricted to children for which information on the fathers is available. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.13: Distribution of maternal year of birth



Notes: There is a rounding age pattern in the survey. The most obvious is at multiples of 5 years, which represents the spikes at 1963, 1968, 173, 1978, 1983 and 1988. The other pattern is consistent across the distribution of year of birth and is not an evidence of manipulation at the cutoff.

Table A.40: Testing for selection: Effects of UPE on fertility

	Total number of children born	Prob. of having a child
Post UPE x Intensity	0.112	0.020
	(0.145)	(0.018)
N	25,452	25,452
First stage F Statistic	51.86	51.86

Notes: *Post-UPE*: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. *Intensity*: proportion of females (males) born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest). The sample used for this test is from the women's file in the DHS survey which includes mothers and non-mothers. All regressions include year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.41: Smoothness of region characteristics

	Global human footprint	**Gross cell production**	**Population (2005)**	**Population (2010)**	**Population (2015)**
	1	2	3	4	5
Post-UPE	36.291	18.895	-1009.417	-1183.563	-1394.737
	(139.066)	(162.742)	(7946.025)	(9070.144)	(10368.288)
N	8,347	8,347	8,347	8,347	8,347
Controls	No	No	No	No	No

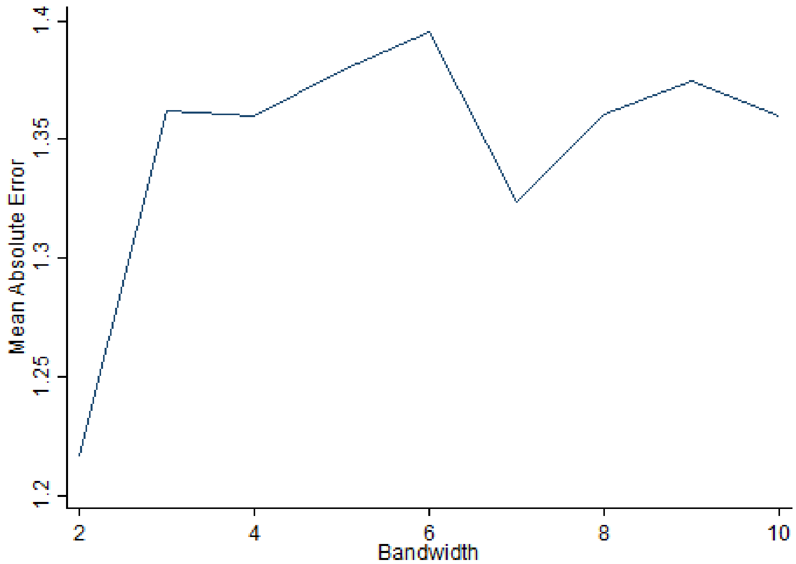
Notes: *Global human footprint*: average of an index between 0 (extremely rural) and 100 (extremely urban) for the location within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster. Gross cell production: average purchasing power parity in 2005 US dollars for the 2 km (urban) or 10 km (rural) buffers surrounding the DHS survey cluster. *Population*: count of individuals living within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster at the time of measurement (2005,2010,2015). Data is from the 2008 and 2013 DHS GPS datasets. Control variables include type of residence, and survey rounds. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.42: Smoothness of individual characteristics, by bandwidths

	Gender	**Region**	**Age**	**Migrants**	**Maternal age**
Bandwidth					
4	0.001	-0.051	0.094	0.052	-0.060
	(0.013)	(0.046)	(0.113)	(0.085)	(0.161)
5	0.000	-0.048	0.122	0.043	-0.171
	(0.008)	(0.033)	(0.092)	(0.069)	(0.346)
6	0.007	-0.042	0.152*	0.061	0.139
	(0.007)	(0.029)	(0.079)	(0.064)	(0.339)
7	0.009	-0.034	0.131*	0.055	-0.029
	(0.007)	(0.024)	(0.067)	(0.051)	(0.310)
8	0.014	-0.030	0.168**	0.065	0.013
	(0.008)	(0.023)	(0.069)	(0.048)	(0.280)
9	0.016	-0.027	0.210***	0.067	0.045
	(0.009)	(0.021)	(0.073)	(0.045)	(0.254)

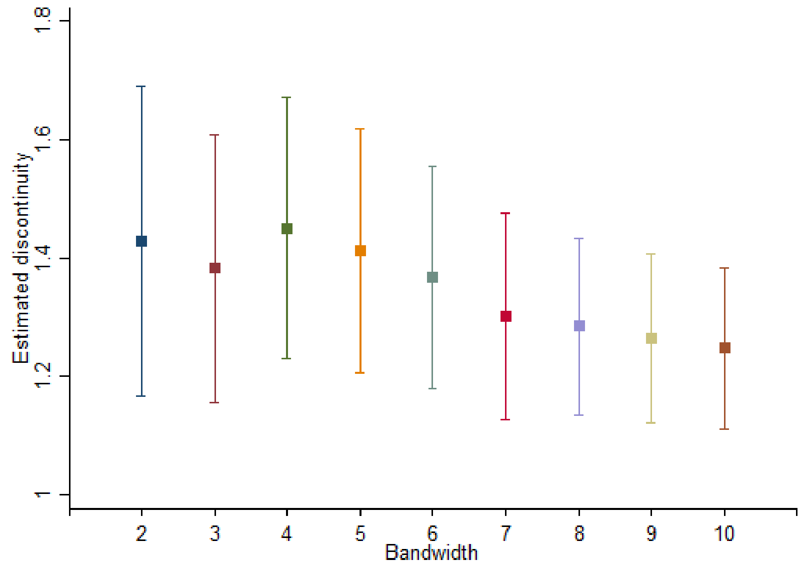
Notes: This table shows the smoothness of individual covariates across the threshold for varying bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.14: Cross-validation: Mean Absolute Error



Notes: The y-axis shows the mean absolute error using the leave-one-out cross validation method. The x-axis shows the different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold.

Figure A.15: Effects of UPE reform on maternal education (all bandwidths)



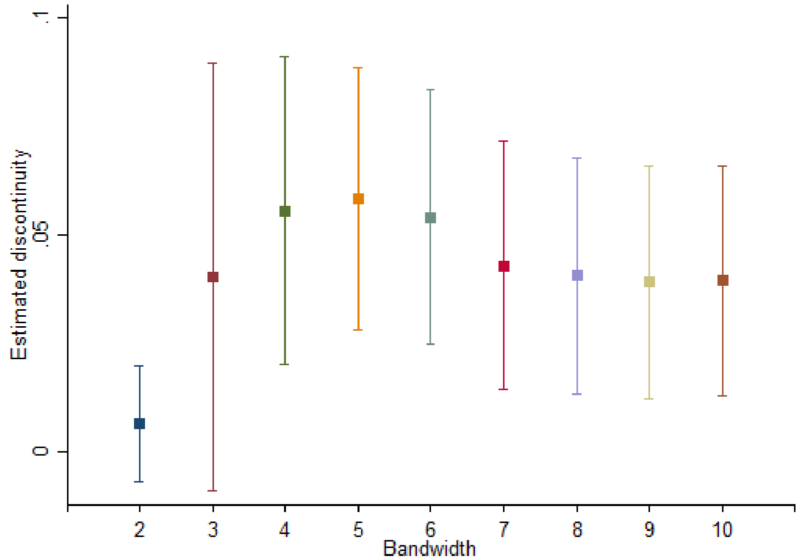
Notes: The y-axis shows the estimated discontinuity from the regression of maternal education on treatment across different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Table A.43: Falsification test: Effect of the reform on maternal education in other intensity areas

	Maternal education	
	Lowest intensity areas	**Median intensity areas**
	1	2
Post-UPE	-1.25*	0.574
	(0.697)	(0.639)
N	2,651	3,517

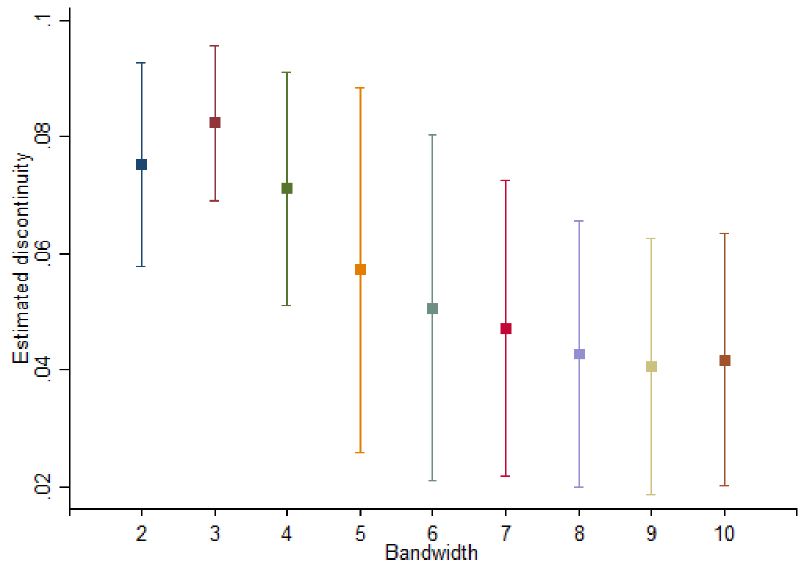
Notes: Post-UPE: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. *Lowest intensity areas*: a region where all women born between 1960-1969 had completed primary school. *Median intensity areas*: a region where about 70% of women born between 1960-1969 had not completed primary school. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.16: Effects of maternal education on grade-for-age (all bandwidths)



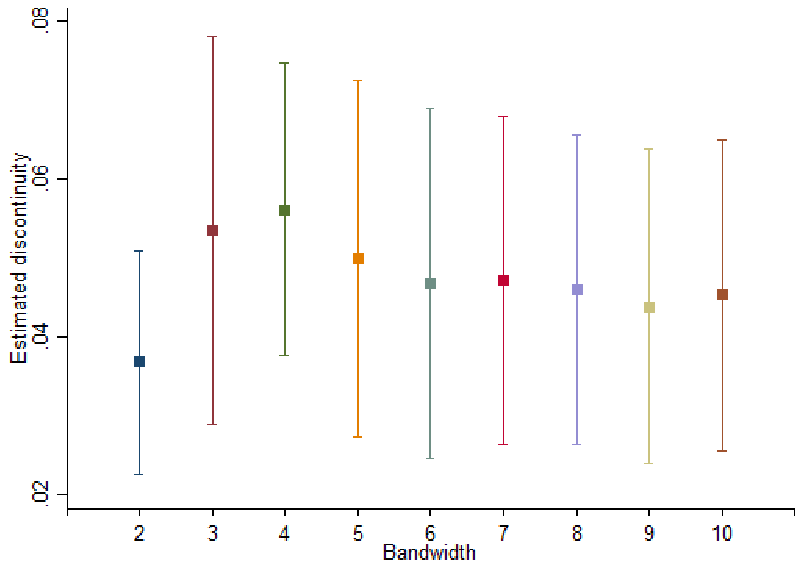
Notes: The y-axis shows the estimated discontinuity from the regression of grade-for-age on treatment, across different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Figure A.17: Effects of maternal education on primary school completion (all bandwidths)



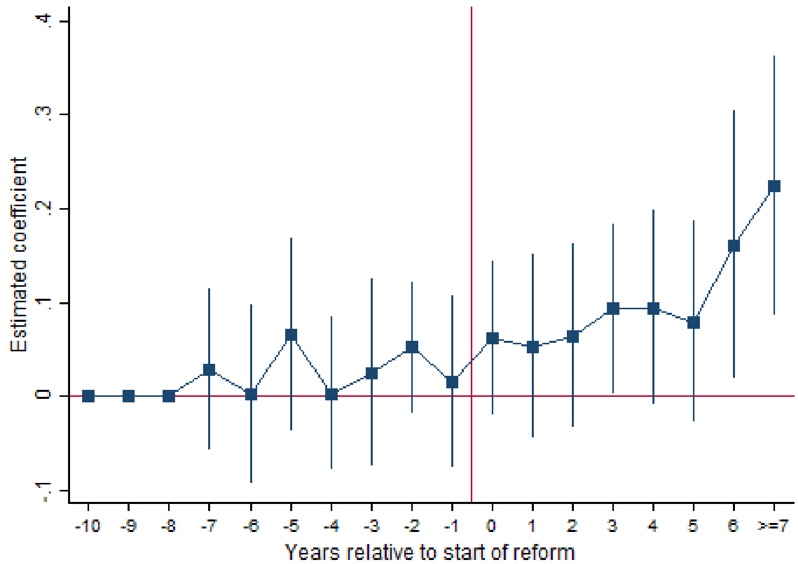
Notes: The y-axis shows the estimated discontinuity from the regression of primary school completion on treatment, across different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Figure A.18: Effects of maternal education on attending secondary school (all bandwidths)



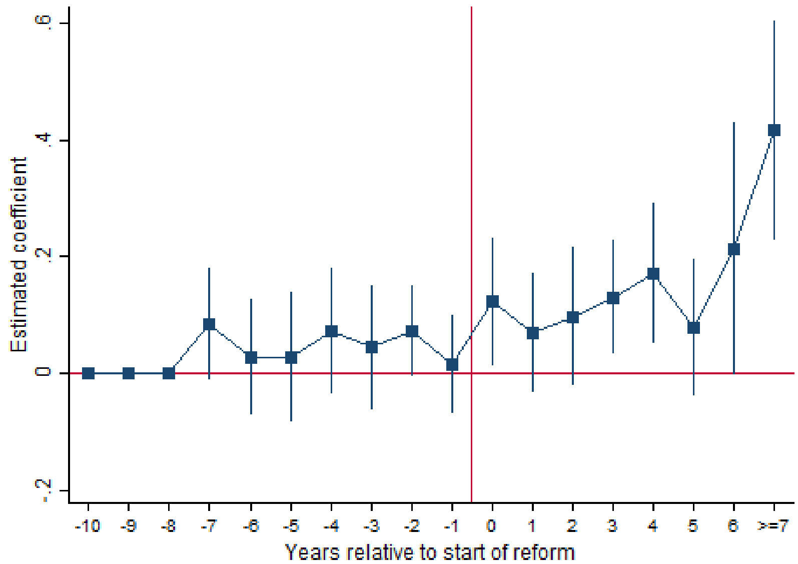
Notes: The y-axis shows the estimated discontinuity from the regression of attending secondary school on treatment, across different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Figure A.19: Reduced form: Effects of UPE reform on grade-for-age



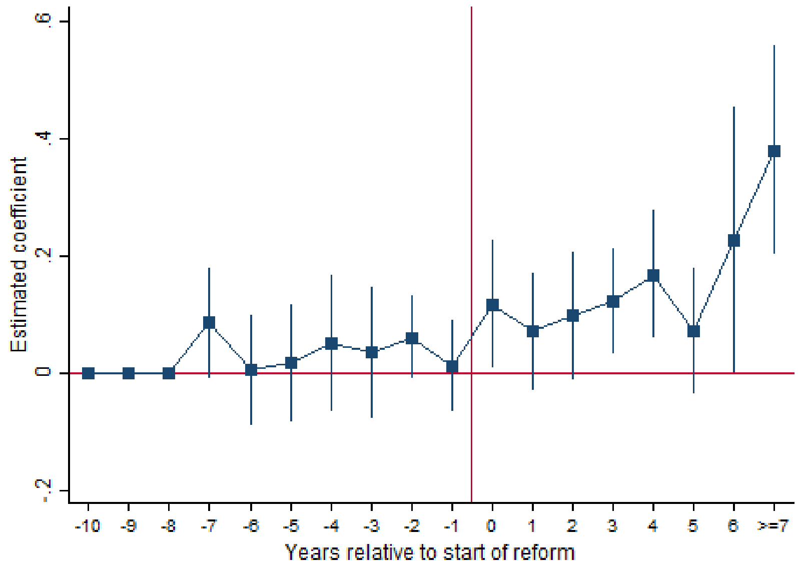
Notes: Dynamic difference-in-differences estimates from equation 4. The regression includes maternal year of birth fixed effects, state fixed effects and state-specific linear trends. Data is from the full sample of all mothers born between 1960-1980. The y-axis shows the coefficients from the regression of primary school completion on the reform. The x-axis is the distance between when a mother started primary school and when the reform started in 1976. The reform year, 1976 is normalized to zero. Control variables include age of child, gender, type of residence, and survey rounds. All regressions include mother's year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level. Confidence intervals are at the 95% significance level.

Figure A.20: Reduced form: Effects of UPE reform on completing primary school



Notes: Dynamic difference-in-differences estimates from equation 4. The regression includes maternal year of birth fixed effects, state fixed effects and state-specific linear trends. Data is from the full sample of all mothers born between 1960-1980. The y-axis shows the coefficients from the regression of primary school completion on the reform. The x-axis is the distance between when a mother started primary school and when the reform started in 1976. The reform year, 1976 is normalized to zero. Control variables include age of child, gender, type of residence, and survey rounds. All regressions include mother's year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level. Confidence intervals are at the 95% significance level.

Figure A.21: Reduced form: Effects of UPE reform on attending secondary school



Notes: Dynamic difference-in-differences estimates from equation 4. The regression includes maternal year of birth fixed effects, state fixed effects and state-specific linear trends. Data is from the full sample of all mothers born between 1960-1980. The y-axis shows the coefficients from the regression of attending secondary school on the reform. The x-axis is the distance between when a mother started primary school and when the reform started in 1976. The reform year, 1976 is normalized to zero. Control variables include age of child, gender, type of residence, and survey rounds. All regressions include mother's year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level. Confidence intervals are at the 95% significance level.

Table A.44: Alternative clustering specifications

	Grade-for-age	**Complete primary school**	**Attend secondary school**
	1	2	3
Clustering at survey cluster level			
Maternal schooling	0.043***	0.047**	0.047***
	(0.014)	(0.020)	(0.017)
State level clustering			
Maternal education	0.043***	0.047***	0.047***
	(0.014)	(0.014)	(0.011)
Two way clustering (year of birth and state)			
Maternal education	0.043**	0.047*	0.047**
	(0.019)	(0.027)	(0.022)
Wild cluster bootstrap (year of birth)- P-value			
Maternal education	0.086*	0.007***	0.036**
Wild cluster bootstrap (state)- P-value			
Maternal education	0.005***	0.003***	0.000***
N	9,579	3,418	3,393

Notes: *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. Control variables include age of child, gender, type of residence, and survey rounds. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.45: Robustness checks

	Grade-for-age	Complete primary education	At least some secondary education
	1	2	3
A: Main specification			
Post UPE x Intensity	0.042*	0.067**	0.073***
	(0.023)	(0.025)	(0.025)
	[44220]	[18098]	[17981]
B: Controlling for other government programs			
Post UPE x Intensity	0.041*	0.064**	0.070***
	(0.023)	(0.026)	(0.025)
	[44220]	[18098]	[17981]
C: Clustering standard errors at 1976 states			
	0.042*	0.067**	0.073**
	(0.023)	(0.026)	(0.027)
	[44,220]	[18,098]	[17,981]
D: Clustering standard errors at survey cluster level			
	0.042**	0.067***	0.073***
	(0.020)	(0.025)	(0.025)
	[44,220]	[18,098]	[17,981]
E: Placebo 1968			
	0.022	0.018	0.001
	(0.038)	(0.044)	(0.041)
	[12,465]	[6,171]	[6,141]
F: No pre-trends			
	0.084***	0.101***	0.108***
	(0.015)	(0.019)	(0.018)
	[44,220]	[18,098]	[17,981]
G: State -cohort fixed effects			
	0.067**	0.100***	0.102***
	(0.029)	(0.037)	(0.034)
	[44,220]	[18,098]	[17,981]
H: Excluding partially treated cohort			
	0.043	0.069**	0.074**
	(0.026)	(0.032)	(0.031)
	[35,457]	[14,254]	[14,160]
I: Full sample			
	0.045**	0.052**	0.066**
	(0.021)	(0.024)	(0.025)
	[57,640]	[21,959]	[21,819]

Notes: *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. Control variables include age of child, gender, type of residence, and survey rounds. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

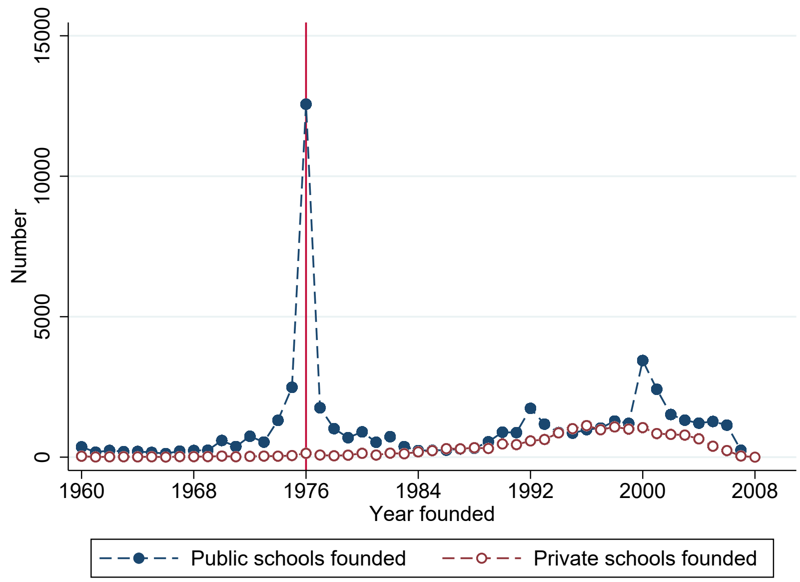
Table A.46: Other robustness checks

	Grade-for-age	**Complete primary school**	**Attend secondary school**
	1	2	3
A: Children 6-17			
Maternal education	0.046***		
	(0.016)		
	[8,799]		
B: Without controlling for child's age			
Maternal education	0.044***	0.050***	0.050***
	(0.015)	(0.012)	(0.011)
	[9,579]	[3,418]	[3,393]
C: Probit estimation			
Maternal education	0.042***	0.041***	0.039***
	(0.014)	(0.012)	(0.008)
	[9,579]	[3,418]	[3,393]
D: Controlling for ethnicity			
Maternal education	0.043***	0.049***	0.049***
	(0.015)	(0.013)	(0.011)
	[9,459]	[3,389]	[3,364]

Notes: *Grade-for-age:* measures a child's normal progress through school. *Primary school completion*: probability of completing primary school. *Attend secondary school*: probability of ever attending secondary school. Control variables include age of child, gender, type of residence, and survey rounds. Standard errors are clustered at maternal year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

A.3 Education and Fertility

Figure A.22: Primary School Founding Dates



Source:Larreguy and Marshall (2017)

Notes: The graph shows the number of primary schools (public and private) founded between 1960 and 2008.

Table A.47: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Years of Education	25,084	4.75	5.34	0	22	6,146	0.72	2.29	0	16
Age	25,084	38.96	6.17	23	49	6,146	37.99	6.47	23	49
Intensity (women)	25,031	0.60	0.35	0	1	6,146	1	0	1	1
Ethnicity										
Yoruba	25,067	0.15	0.35	0	1	6,143	0.02	0.13	0	1
Ibo	25,067	0.15	0.35	0	1	6,143	0.04	0.19	0	1
Hausa	25,067	0.31	0.46	0	1	6,143	0.70	0.46	0	1
Other ethnicity	25,067	0.40	0.49	0	1	6,146	0.25	0.43	0	1
Rural	25,084	0.66	0.47	0	1	6,146	0.87	0.34	0	1
Married	25,084	0.97	0.18	0	1	6,146	0.99	0.08	0	1
Polygyny	25,084	0.66	0.47	0	1	6,146	0.55	0.50	0	1
Fertility										
Total number of children born	25,084	5.62	2.99	0	18	6,146	6.47	3.06	0	18
Number of kids before 25	24,839	2.22	1.76	0	10	6,067	2.75	1.80	0	9
Number of kids before 18	25,084	0.46	0.77	0	5	6,146	0.65	0.86	0	5
Age at first birth	23,755	19.79	4.87	9	45	5,931	18.44	4.34	10	40
Age at first cohabitation	24,278	18.10	5.24	7	46	6,108	15.64	3.95	7	43
Partner characteristics										
Years of Education	23,859	5.79	5.88	0	21	5,996	1.71	3.87	0	18
Age	21,896	49.73	9.83	20	96	5,807	49.99	10.19	20	96
Intensity(men)	24,911	0.36	0.34	0	1	6,111	0.73	0.26	0	1

Notes: Data is from the Nigerian Demographic and Health Surveys 2003-2013. *Total number of children born:* captures the total number of children a born has ever given birth to. *Children born before 25*: captures the total number of children a woman has ever given birth to before the age of 25. *Children born before 18*:captures the total number of children a woman has ever given birth to before the age of 18. *Intensity*: proportion of females (males) born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest).

Figure A.23: Proportion of females born between 1960 and 1969 not completing primary school, by state

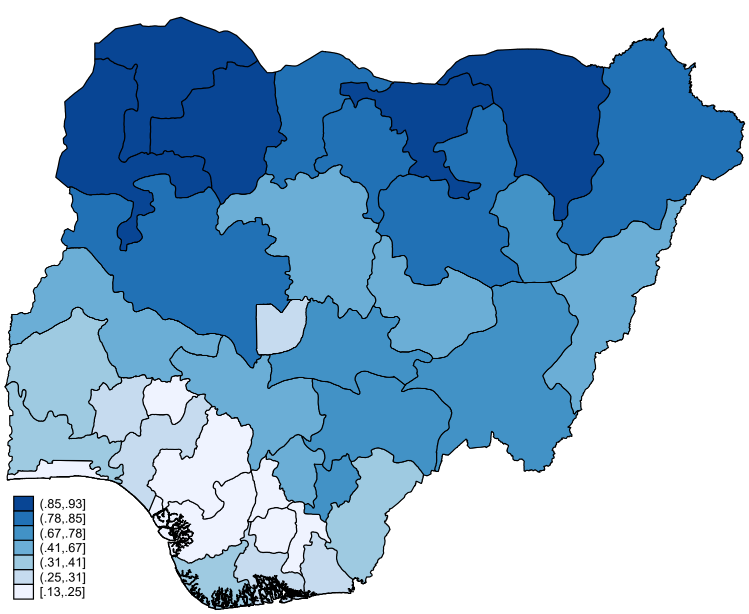
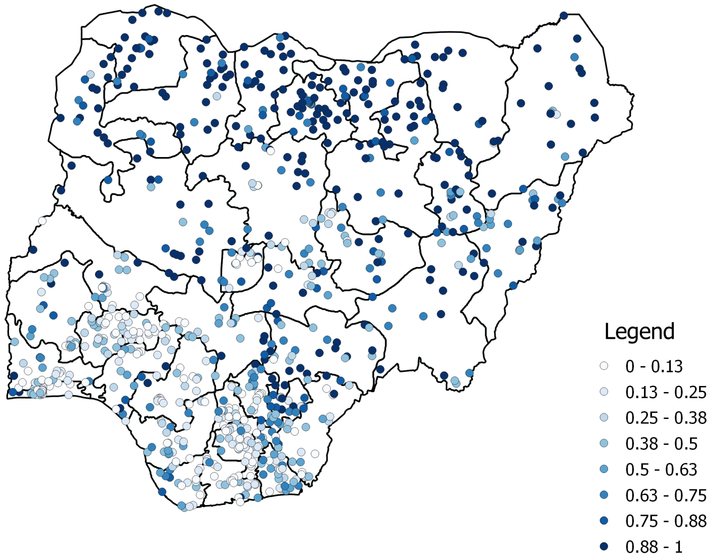
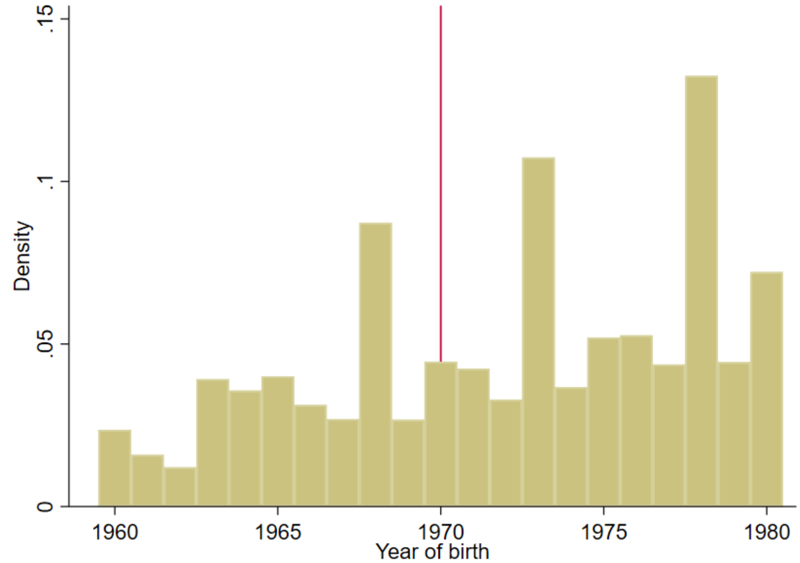


Figure A.24: Proportion of females born between 1960 and 1969 not completing primary school, by clusters



Notes: The maps show the UPE intensities for the the 36 states and the Federal Capital Territory in Figure A.23, and for the survey clusters in Figure A.24, using the 2013 DHS data. Intensity is the proportion of females born between 1960 and 1969, living in an area not completing primary school. Intensity ranges from zero (lowest) to one (highest). Darker colors represent higher UPE intensity areas and lighter colors represent lower UPE intensity areas.

Figure A.25: Distribution of year of birth



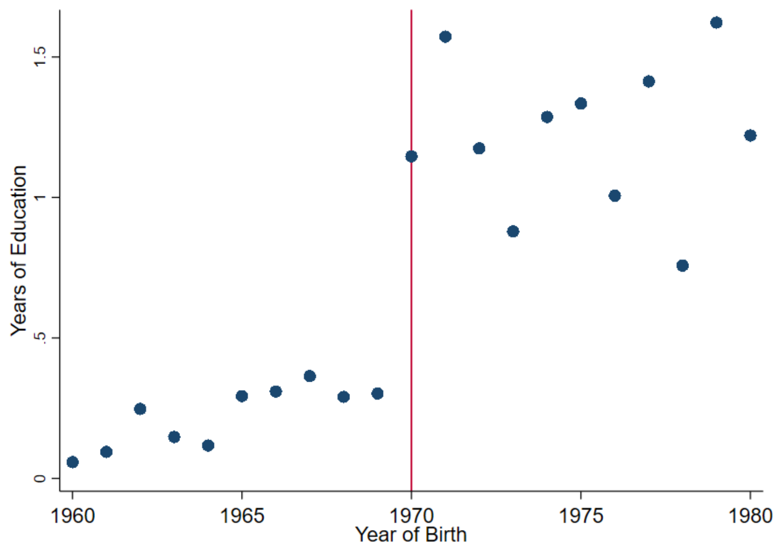
Notes: The graph represents the distribution of year of birth between 1960 and 1980. Data is from the Demographic and Health Surveys between 2003 and 2013. There is a rounding age pattern in the survey. The most obvious is at multiples of 5 years, which represents the spikes at 1963, 1968, 173, 1978, 1983 and 1988. The pattern is consistent across the distribution and is not an evidence of manipulation at the 1970. threshold.

Table A.48: Effects of UPE reform on exogenous characteristics

	Region	Age	Ethnicity	Survey
UPE Cohort	0.019	0.212	-0.141*	0.212
	(0.017)	(0.342)	(0.070)	(0.342)
N	6146	6146	6143	6146
Bandwidth	10	10	10	10

Notes: *UPE Cohort*: a dummy variable that takes on one if year of birth is 1970 or later, and zero otherwise. Standard errors are clustered at year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.26: First stage: Effect of UPE reform on education



(a) Educational attainment by birth cohort

Notes: This graph shows the average years of education for each cohort living in the highest intensity areas. Sample includes women born between 1960-1980. With the reform starting in 1976 and the official school starting age being six, women born in 1970 and later are eligible for the reform. The graph represents averages from the raw data.

Table A.49: Effects of UPE reform on education

	Years of Schooling				Complete primary school	Incomplete secondary school	Complete secondary school
Born >= 1970	0.824*** (0.165)	0.757*** (0.147)			0.043*** (0.007)	0.023*** (0.004)	0.016** (0.006)
Placebo 1: Born after 1965			-0.017 (0.048)				
Placebo 2: Born after 1974				-0.215 (0.214)			
N	6146	6143	2426	3528	6143	6143	6143
First stage F Statistic	26.42	25.08	0.13	1.01			
Controls	Yes	No	Yes	Yes	Yes	Yes	Yes

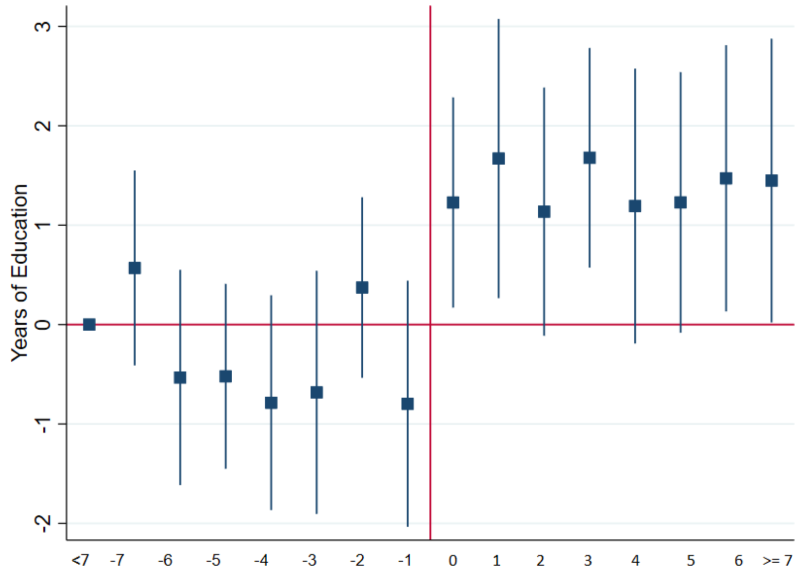
Notes: *UPE Cohort*: a dummy variable that takes on one if year of birth is 1970 or later, and zero otherwise. To test for jumps at non-discontinuity points, I split the sample into two: below the threshold and above the threshold. For each sub-sample, I use the median value as a placebo reform year and test for a jump at that point. The two placebo reforms are at 1965 and 1975. *Complete primary school:* probability of completing primary school. *Incomplete secondary school*: probability of having at least some secondary education. *Complete secondary school*: probability of completing secondary school. Control variables include age, type of residence, ethnicity, and survey rounds. Standard errors are clustered at year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.50: Effects of education on fertility outcomes

	Total number of children born	Number of children born before age 25	18
OLS: Education	-0.067***	-0.056***	-0.025***
	(0.013)	(0.009)	(0.006)
Reduced Form:			
Born $\geqslant$ 1970	0.200	-0.142	-0.142***
	(0.137)	(0.119)	(0.038)
2SLS			
Education	0.264	-0.188	-0.187**
	(0.177)	(0.166)	(0.074)
N	6143	6143	6143
First stage F Statistic	26.42	26.42	26.42
Outcome Mean	6.48	0.65	2.75
Unadjusted p-value	0.136	0.26	0.011
Adjusted q-value	0.204	0.26	0.033
Bandwidth	10	10	10
Controls	Yes	Yes	Yes

Notes: UPE Cohort: a dummy variable that takes on one if year of birth is 1970 or later, and zero otherwise. With the reform starting in 1976 and the official school starting age being six, women born in 1970 and later are eligible for the reform. *education*: total number of years of maternal schooling. *Total number of children born:* captures the total number of children a born has ever given birth to. *Children born before 25*: captures the total number of children a woman has ever given birth to before the age of 25. *Children born before 18*:captures the total number of children a woman has ever given birth to before the age of 18. Control variables include age, type of residence, ethnicity, and survey rounds. The F-statistics are from test of the reform impact on maternal schooling. Standard errors are clustered at year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.27: Effects of UPE reform on education (full sample)



Notes: Dynamic difference-in-differences estimates from equation 4. The regression includes year of birth fixed effects, state fixed effects and state-specific linear trends. Data is from the full sample of mothers from all intensity areas born between 1960-1980. The x-axis measures the distance between when a mother started primary school and when the reform started in 1976. The reform year 1976 is normalized to zero. Since the official school starting age is six, women born in 1979 started school in 1976. Control variables include age, type of residence, ethnicity, and survey rounds. Standard errors are clustered at the state level. Confidence intervals are at the 95% significance level.

Table A.51: First stage: Effects of UPE reform on education (full sample)

	Years of Schooling			Complete primary school	Incomplete secondary school	Complete secondary school
Intensity x UPE Cohort	1.781***			0.149***	0.037**	-0.014
	(0.335)			(0.026)	(0.017)	(0.024)
Intensity x placebo 1968		-0.717*				
		(0.360)				
Intensity x Directly affected			1.787***			
			(0.330)			
N	25014	11295	21803	25014	25014	25014
Instrument SD	0.40	0.40	0.40			
First stage F Statistic	28.23	3.97	29.31			
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: UPE Cohort: a dummy variable that takes on one if year of birth is 1970 or later, and zero otherwise. *Intensity*: proportion of females born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest). *Placebo 1968:* fake reform year in 1968. Placebo sample includes women born between in 1960-1967 and the placebo treated cohort are born between 1964 and 1967. Sample consists of women from all intensity areas. Control variables include age, type of residence, ethnicity, and survey rounds. All regressions include year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.52: Effects of education reform on fertility outcomes (full sample)

	Total number of children born	Number of children born before age 25	18
OLS			
Education	-0.123***	-0.080***	-0.024***
	(0.010)	(0.007)	(0.002)
Reduced Form			
Intensity x UPE Cohort	0.026	0.066	-0.009
	(0.183)	(0.109)	(0.045)
2SLS			
Education	0.015	0.037	-0.005
	(0.103)	(0.064)	(0.024)
N	25014	25014	25014
First stage F Statistic	28.23	28.23	28.23
Outcome Mean	5.23	2.21	0.46
Unadjusted p-value	0.886	0.56	0.83
Adjusted q-value	0.886	0.886	0.886
Controls	Yes	Yes	Yes

Notes: Total number of children born: captures the total number of children a born has ever given birth to. *Children born before 25*: captures the total number of children a woman has ever given birth to before the age of 25. *Children born before 18*:captures the total number of children a woman has ever given birth to before the age of 18. *UPE Cohort*: a dummy variable that takes on one if year of birth is 1970 or later, and zero otherwise. *Intensity*: proportion of females (males) born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest). Sample consists of women from all intensity areas. Control variables include age, type of residence, ethnicity, and survey rounds. All regressions include year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.53: Robustness checks

	Total number of children born	Number of children born before age 25	18
A: Main estimates			
Education	0.264	-0.207	-0.187**
	(0.177)	(0.166)	(0.074)
B: No controls			
Education	0.211	-0.222	-0.186**
	(0.185)	(0.164)	(0.076)
C: With state fixed effects			
Education	0.264	-0.231	-0.187**
	(0.205)	(0.214)	(0.082)
D: Controlling for other government programs			
Education	0.275	-0.209	-0.190**
	(0.173)	(0.169)	(0.075)
E: Placebo: Born $\geqslant$ 1965			
Education	-7.846	5.626	-2.490
	(20.639)	(21.223)	(5.410)
	[2,426]	[2,426]	[2,426]
F: Placebo: Born $\geqslant$ 1975			
Education	-0.148	-1.636	-0.132
	(0.927)	(2.383)	(0.414)
	[3,528]	[3,449]	[3,528]
G: Linear function of year of birth (no quadratics)			
Education	0.220	-0.192	-0.193**
	(0.150)	(0.161)	(0.078)
	[6,143]	[6,143]	[6,143]
H: Quadratic functional form (slope)			
Education	0.107	-0.117	-0.184**
	(0.169)	(0.148)	(0.093)
	[6,143]	[6,143]	[6,143]

Notes: *Total number of children born:* captures the total number of children a born has ever given birth to. *Children born before 25*: captures the total number of children a woman has ever given birth to before the age of 25. *Children born before 18*:captures the total number of children a woman has ever given birth to before the age of 18. Control variables include age, type of residence, ethnicity, and survey rounds. Other government programs include 1976 health and information expenditure implemented across states. Number of observations are in bracket. Standard errors are clustered at year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.54: Robustness checks (continued)

	Total number of children born	Number of children born before age 25	18
A: 5 years bandwidth			
Education	-0.201	-0.139	-0.099
	(0.165)	(0.200)	(0.095)
	[3,991]	[3,991]	[3,991]
B: 6 years bandwidth			
Education	-0.181	-0.223	-0.144
	(0.161)	(0.174)	(0.092)
	[4,392]	[4,392]	[4,392]
C: 8 years bandwidth			
Education	0.166	-0.169	-0.162**
	(0.188)	(0.157)	(0.068)
	[5,516]	[5,516]	[5,516]
D: 9 years bandwidth			
Education	0.232	-0.180	-0.167**
	(0.182)	(0.159)	(0.068)
	[5,741]	[5,701]	[5,741]
E: Uniform weights			
Education	0.360**	-0.301	-0.250**
	(0.181)	(0.211)	(0.100)
	[6,143]	[6,064]	[6,143]
F: Robust standard errors			
Education	0.264	-0.207	-0.187***
	(0.210)	(0.128)	(0.064)
	[6,143]	[6,064]	[6,143]
G: Controlling for heaps (different intercept and slope)			
Education	0.198	-0.272***	-0.183***
	(0.14)	(0.095)	(0.063)
	[6,143]	[6,064]	[6,143]

Notes: *Total number of children born:* captures the total number of children a born has ever given birth to. *Children born before 25*: captures the total number of children a woman has ever given birth to before the age of 25. *Children born before 18*:captures the total number of children a woman has ever given birth to before the age of 18. Heaps in the data are at multiples of 5's in reporting year of birth: 1963, 1968, 1973 and 1978. Control variables include age, type of residence, ethnicity, and survey rounds. Number of observations are in bracket. Standard errors are clustered at year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.55: Effects of education on potential mediators (2SLS estimates)

	Age at first birth 1	Age at first cohabitation 2	Contraceptives use 3	Partner's education 4	Spousal age difference 5
UPE Cohort	0.591* (0.337)	0.068 (0.397)	0.039*** (0.014)	0.873*** (0.244)	-1.782* (0.916)
N	5928	6105	6143	5993	5993
Controls	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the effect of education on the potential mediators, instrumenting for education with the reform eligibility (fuzzy RD design). *Age at first birth*: age at which a woman had her first child. *Age at first cohabitation*: age at which a woman started living with a man. *Contraceptives use*: a dummy variable indicating if a woman reports using modern contraceptives. *Partner's education*: years of education of the father. *Spousal age difference*: difference between a woman's age and her spouse's age. Control variables include age, type of residence, ethnicity, and survey rounds. Sample size varies by data availability. Standard errors are clustered at the year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

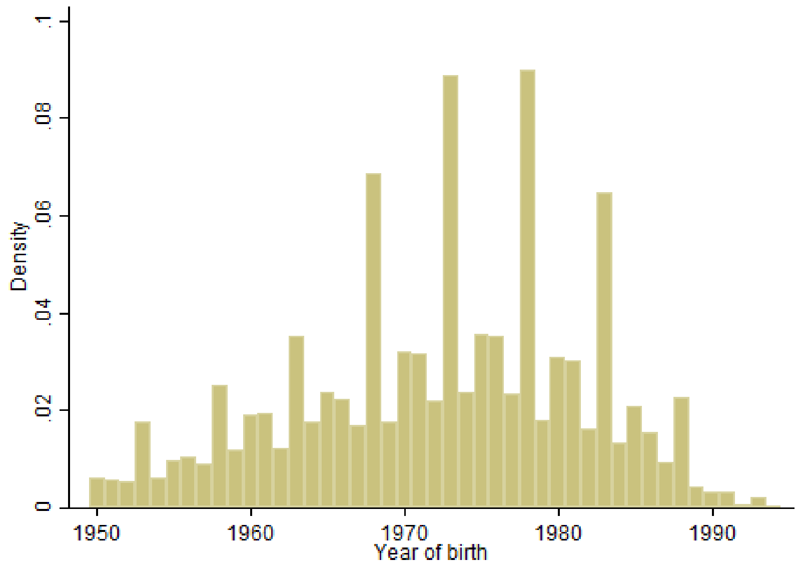
Table A.56: Variables Dictionary

Fertility Outcomes
Total number of children ever born
Number of living children
Age of respondent at first birth
Age at first cohabitation

Questions: Decision-making within the Household
Person who usually decides how to spend respondent's earnings
Person who usually decides on respondent's health care
Person who usually decides on large household purchases
Person who usually decide on household purchases for daily needs
Person who usually decides on visits to family or relatives

Response
1 Respondent alone
2 Respondent and husband/partner
3 Respondent and other person
4 Husband/partner alone
5 Someone else
6 Other

Figure A.28: Distribution of year of birth



Notes: There is a rounding age pattern in the survey. The most obvious is at multiples of 5 years, which represents the spikes at 1963, 1968, 173, 1978, 1983 and 1988. The other pattern is consistent across the distribution of year of birth and is not an evidence of manipulation at the cutoff.

Table A.57: Testing for selection: Effects of UPE on Marriage and Migration

	Married 1	Mover 2
Intensity x UPE Cohort	0.010 (0.011)	0.038 (0.039)
N	33287	19911
Controls	Yes	Yes

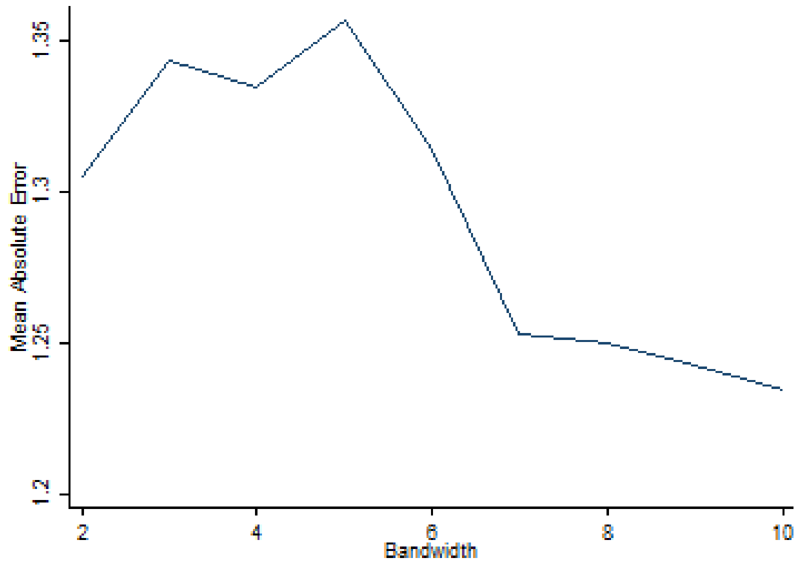
Notes: *UPE-Cohort*: a dummy variable that takes on one if year of birth is 1970 or later, and zero otherwise. *Intensity*: proportion of females (males) born between 1960 and 1969, living in a local government area not completing primary school. The intensity variable ranges from zero (lowest) to one (highest). The sample used for this test is from the women's file in the DHS survey which includes all women. All regressions include year of birth fixed effects, state fixed effects and state-specific linear trends. Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Table A.58: Smoothness of region characteristics

	Global human footprint 1	**Gross cell production** 2	**Population (2005)** 3	**Population (2010)** 4	**Population (2015)** 5
UPE Cohort	20.782 (28.574)	-11.806 (35.533)	4613.156 (5204.674)	5247.403 (5947.460)	5969.072 (6804.699)
N	5081	5081	5081	5081	5081
Controls	No	No	No	No	No

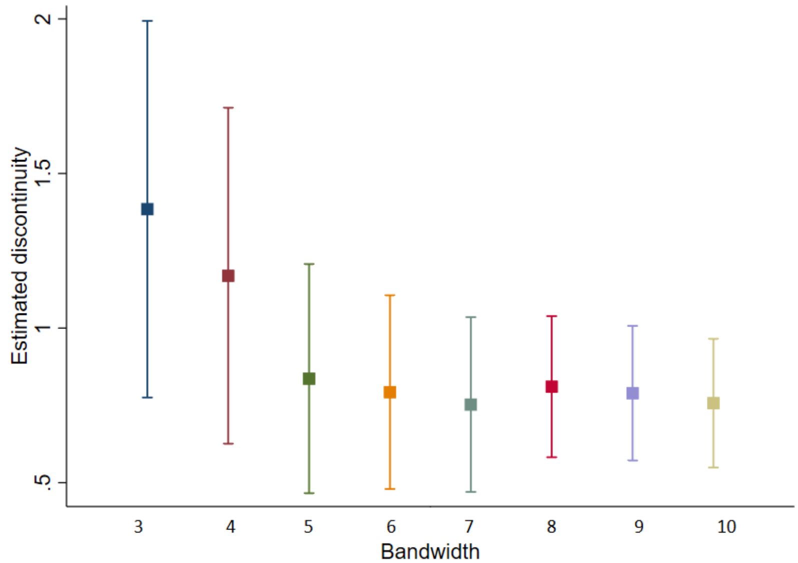
Notes: *Global human footprint*: average of an index between 0 (extremely rural) and 100 (extremely urban) for the location within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster. Gross cell production: average purchasing power parity in 2005 US dollars for the 2 km (urban) or 10 km (rural) buffers surrounding the DHS survey cluster. *Population*: count of individuals living within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster at the time of measurement (2005,2010,2015). Data is from the 2008 and 2013 DHS GPS datasets. Regression includes survey rounds fixed effects. Standard errors are clustered at year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.29: Cross-validation: Mean Absolute Error



Notes: The y-axis shows the mean absolute error using the leave-one-out cross validation method. The x-axis shows the different bandwidths. Bandwidth refers to the number of bins (year of birth) on either side of the threshold.

Figure A.30: Effects of UPE reform on education (all bandwidths)



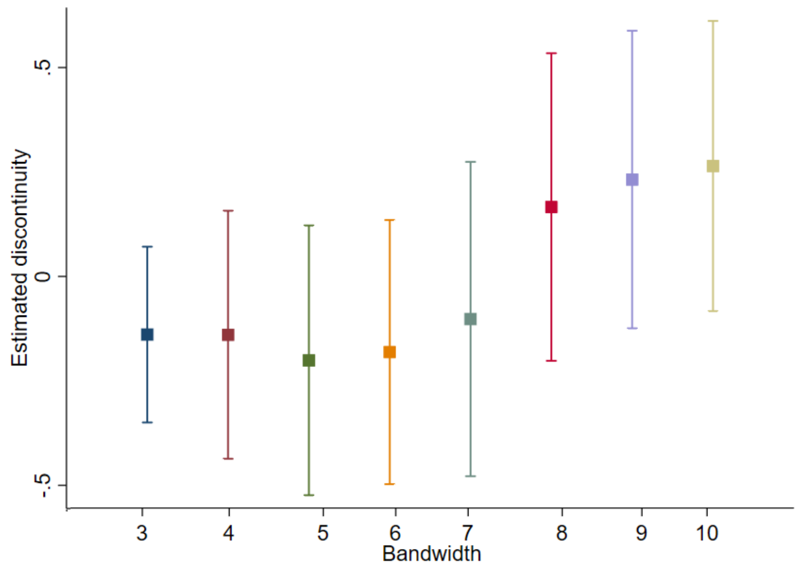
Notes: The y-axis shows the estimated discontinuity from the regression of education on treatment across different bandwidths. Bandwidth refers to the number of bins (year of birth) on either side of the threshold. Standard errors are clustered at year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Table A.59: Falsification test: Effect of the reform on education in other intensity areas

	Lowest intensity areas	**Median intensity areas**
	1	2
Post-UPE	-0.711	0.897
	(0. 537)	(0.590)
N	1962	1288
F-Stats	1.76	2.31

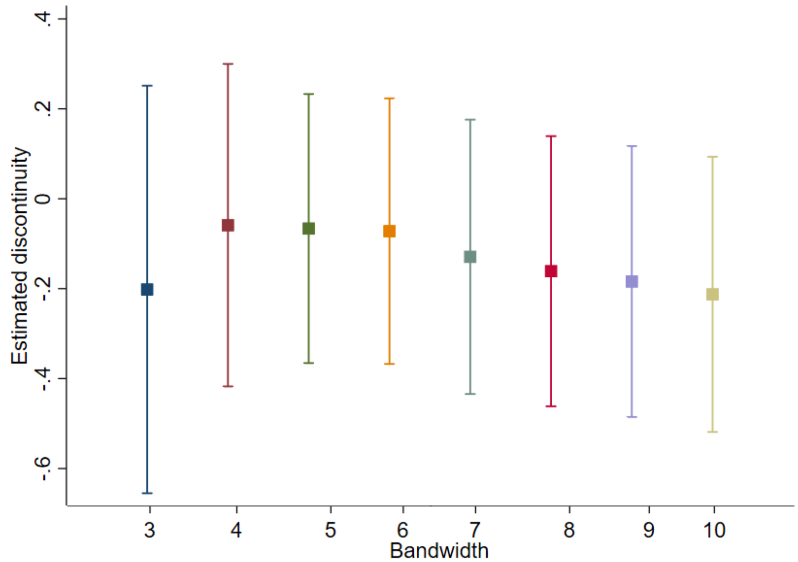
Notes: UPE Cohort: a dummy variable that takes on one if year of birth is 1970 or later, and zero otherwise. *Lowest intensity areas*: a region where all women born between 1960-1969 had completed primary school. *Median intensity areas*: a region where about 70% of women born between 1960-1969 had not completed primary school. Standard errors are clustered at year of birth and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level

Figure A.31: Effects of education on total number of children (all bandwidths)



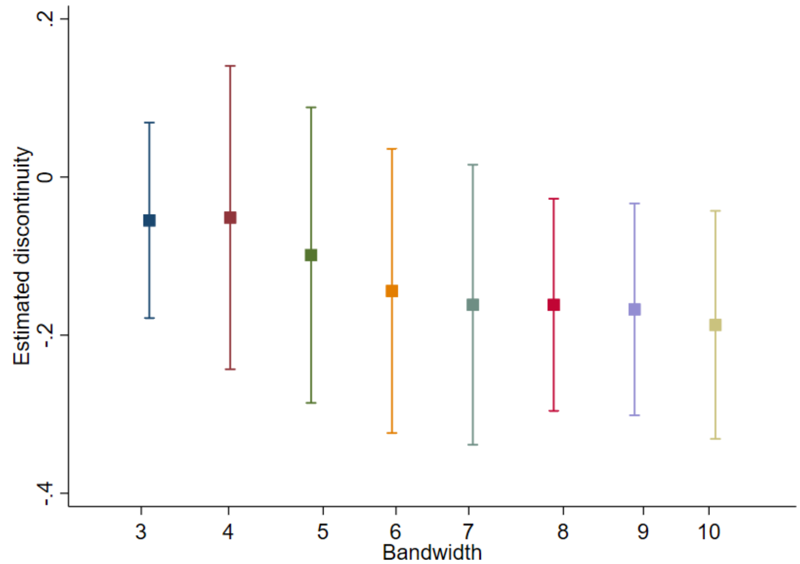
Notes: The y-axis shows the estimated discontinuity from the regression of the total number of children born on treatment, across different bandwidths. Bandwidth refers to the number of bins (year of birth) on either side of the threshold. Standard errors are clustered at year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Figure A.32: Effects of education on number of children born before age 25 (all bandwidths)



Notes: The y-axis shows the estimated discontinuity from the regression of the number of children born before 25, across different bandwidths. Bandwidth refers to the number of bins (year of birth) on either side of the threshold. Standard errors are clustered at year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Figure A.33: Effects of education on number of children born before 18 (all bandwidths)



Notes: The y-axis shows the estimated discontinuity from the regression of the number of children born before 18 on treatment, across different bandwidths. Bandwidth refers to the number of bins (year of birth) on either side of the threshold. Standard errors are clustered at year of birth. The x-axis shows the different bandwidths on either side of the threshold. Confidence intervals are at the 95% significance level.

Table A.60: Robustness checks

	Total number of children born	Number of children born before age 25	18
A: No Sample weights			
Education	0.052	0.095	-0.001
	(0.084)	(0.068)	(0.025)
	[25,014]	[24,773]	[25,014]
B: Testing for potential confounders			
Education	0.013	0.028	-0.005
	(0.105)	(0.064)	(0.025)
	[25,014]	[24,773]	[25,014]
C: State-cohort fixed effects			
Education	0.015	0.085	0.021
	(0.087)	(0.064)	(0.025)
	[25,014]	[24,773]	[25,014]
D: Cluster at 1976 states			
Education	0.015	0.028	-0.005
	(0.102)	(0.059)	(0.026)
	[25,014]	[24,773]	[25,014]
E: Born before 1976 only			
Education	-0.026	0.013	-0.003
	(0.108)	(0.064)	(0.027)
	[20,072]	[20,072]	[20,072]
F: Full sample			
Education	0.063	0.128	0.023
	(0.113)	(0.078)	(0.028)
	[33287]	[32869]	[33287]
G: Exclude Lagos			
Education	0.072	0.054	0.006
	(0.110)	(0.069)	(0.026)
	[24,029]	[23,803]	[24,029]
H: No State trend			
Education	0.276	1.384	0.213
	(0.459)	(0.959)	(0.181)
	[25,014]	[24,773]	[25,014]
I: Alternative Intensity (Born 1960-1964)			
Education	0.080	0.020	0.001
	(0.107)	(0.060)	(0.024)
	[24,862]	[23,234]	[24,862]
J: Placebo Reform (Born 1965-1969)			
Education	0.008	-0.251	-0.036
	(0.330)	(0.174)	(0.067)
	[11,241]	[11,241]	[11,241]

Notes: Standard errors are clustered at the state level and reported in parentheses. * Significant at 0.1 level. ** Significant at 0.05 level. ***Significant at 0.01 level