

# Intergenerational Transmission of Human Capital: Effects of Maternal Education on Child Education

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## **Abstract**

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 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 and having a more educated father are important channels through which maternal education matters for children's schooling.

*Keywords:* Human capital formation, child development, development, Africa

JEL codes: H30, I2, J13, O12, O15

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# 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.<sup>1</sup> 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.<sup>2</sup> 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 et al. \(2013\)](#) 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 et al., 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 beliefs. Although different studies have also examined the effects of paternal education ([Agüero and Ramachandran, 2018](#); [Black et al., 2005](#); [Chevalier et al., 2013](#); [Holmlund et al., 2011](#); [Lundborg et al., 2014](#)), only few studies including [Agüero and Ramachandran \(2018\)](#) and [Chevalier et al. \(2013\)](#) find positive significant effects.<sup>3</sup>

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<sup>1</sup>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%).

<sup>2</sup>Future outcomes include adult health, wages, criminal behavior ([Clark and Royer, 2013](#); [Heckman et al., 2018](#); [Lochner and Moretti, 2004](#)).

<sup>3</sup>Generally, the estimates from twin and adoption studies approaches find that paternal education has significant effects on child education ([Behrman and Rosenzweig, 2002](#); [Bingley et al., 2009](#); [Björklund et al., 2006](#); [Plug, 2004](#)).

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.<sup>4</sup> 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 income 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.<sup>5</sup>

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, I compare the outcomes of school-age children whose mothers were born shortly before and after 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 its total effect, 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.<sup>6</sup> I validate this assumption by testing for bunch-

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<sup>4</sup>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).

<sup>5</sup>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.

<sup>6</sup>The fuzzy RD design allows overage enrollment.

ing 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 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 et al. \(2018\)](#) for Indonesia (0.04 SD).<sup>7</sup> 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

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<sup>7</sup>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 \(2018\)](#)).

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.<sup>8</sup> This finding is consistent with [Akresh et al. \(2018\)](#); [Carneiro et al. \(2013\)](#); [Cui et al. \(2019\)](#); [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 for future research. I also find that more educated mothers are involved in decisions relating to their children’s education. 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.<sup>9</sup> 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 and 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.<sup>10</sup> Third, most of the studies on the causal relationship between maternal and child

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<sup>8</sup>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.

<sup>9</sup>There are studies that examine the effects of maternal education on child health- [Chou et al. \(2010\)](#); [Currie and Moretti \(2003\)](#); [Keats \(2018\)](#). However, that is not the focus of this paper.

<sup>10</sup>[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](#)).

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 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. \(2019\)](#)).<sup>11</sup> 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.<sup>12</sup> 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 from causal studies hold different

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<sup>11</sup>[Akresh et al. \(2018\)](#); [Mazumder et al. \(2019\)](#); [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.

<sup>12</sup>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.

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 to 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 15.74 NGN (\$19.97) 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.

## 2 The Nigerian Education System

### 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.<sup>13</sup> 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.<sup>14</sup>

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<sup>13</sup>Lower middle-income economies have GNI per capita between \$1,026 and \$3,995. <https://data.worldbank.org/country/nigeria>.

<sup>14</sup>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 and Owojaiye,

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.

### **2.1.1 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 ([Csapo, 1983](#); [Abernethy, 1969](#)). The western region was the forerunner in education and educational imbalances across the different regions became substantial after independence ([Osili and 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 and 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 intellectual and social outlook of any society has to be preceded by an educational revolution." During the oil boom, a majority

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[1981](#)).



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 and 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.<sup>15</sup> 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). The reform resulted in a large increase in primary school enrollment in states with low prior educational attainment. 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).<sup>16</sup>

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

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<sup>15</sup>The dollar equivalent is in 1976 dollars. See Table A.1 for expenditure in 1976.

<sup>16</sup><https://data.worldbank.org/indicator/SE.PRM.ENRR.FE?end=2014&locations=NG-ZG-XM-XL&start=1970&view=chart>

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 and Marshall, 2017; Osili and 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 Data and Empirical Strategy

#### 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. 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 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.<sup>17</sup>

*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:* As shown in Appendix Table A.1, 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 and Marshall, 2017).<sup>18</sup> 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.<sup>19</sup> 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 1 and 2 for geographical variation in intensities. Darker areas on the map reflect higher UPE intensities and within states, UPE intensities are different.

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

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

<sup>18</sup>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.

<sup>19</sup>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.

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 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 1. 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.<sup>20</sup>

### 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

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<sup>20</sup>This is representative of the country where more than half of the population live in rural areas.

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 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 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.<sup>21</sup> I restrict the sample to households in the highest intensity areas and use a fuzzy regression discontinuity design (Imbens and Lemieux, 2008; Lee and Lemieux, 2010) to estimate the effect of maternal education on child education outcomes by instrumenting for maternal education with the reform eligibility.<sup>22</sup> 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 gold standard for analysis 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

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<sup>21</sup> 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.

<sup>22</sup> 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.

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 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. 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:

$$Y_{im} = \beta_0 + \beta_1 M_m + \beta_2 X_{im} + \epsilon_{im} \quad (1)$$

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.  $\epsilon$  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:

$$M_{im} = \gamma_0 + \gamma_1 T_i + \gamma_2 f(R_{im}) + \gamma_3 T_i \cdot f(R_{im}) + \gamma_4 X_{im} + \mu_{im} \quad (2)$$

$$Y_{im} = \delta_0 + \delta_1 T_i + \delta_2 f(R_{im}) + \delta_3 T_i \cdot f(R_{im}) + \delta_4 X_{im} + \epsilon_{im} \quad (3)$$

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  $\gamma_1$  being the effect of the reform on maternal education.  $\delta_1$  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.<sup>23</sup>

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 kernel weights.<sup>24</sup> 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.<sup>25</sup> Plotting the mean absolute

<sup>23</sup>I also run an alternative specification where I exclude the age of the child and the results are unchanged.

<sup>24</sup>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).

<sup>25</sup>However, this test is more suited for continuous running variables.

error against the different bandwidths, Appendix Figure A.2 shows that except for bandwidth 2, 7 years gives the smallest MAE. Also in Appendix Figure A.3, the first stage estimates become relatively stable after a bandwidth of 7.<sup>26</sup> 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.

### 3.3.1 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 3 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 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 3 is a general pattern in the survey, Appendix Figure A.1 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.

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<sup>26</sup>The estimates are larger and imprecise at smaller bandwidths because the number of clusters shrinks.



In Table 2, 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 2 and shown graphically in Appendix Table 4. The estimates are zero and not statistically significant. Rather than using all covariates in a single model, in Appendix Table A.4, I focus on the covariates individually, and the results are consistent with Table 2.

### 3.4 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 and Marshall, 2017) to exploit the temporal and spatial variation of the UPE reform using year of birth and area of residence.<sup>27</sup> 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 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

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<sup>27</sup>This method has also been used by Chou et al. (2010); Duflo (2001); Osili and Long (2008).

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:

$$Y = \delta_1(PostUPE \cdot Intensity) + \delta_2Intensity + \delta_3PostUPE + \delta_4X + \delta_s + \delta_t + \delta_{st} + \delta_r + \epsilon \quad (4)$$

Where  $Y$  represents the different schooling outcomes,  $\delta_1$  is the reduced form effects of the reform on children's schooling. I include time-fixed effects ( $\delta_t$ ) to capture trends in education that are not correlated with the reform. The inclusion of state fixed effects ( $\delta_s$ ) absorbs time-invariant characteristics across states. The specification also includes state-specific linear time-trends ( $\delta_{st}$ ) 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  $\delta_r$  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.<sup>28</sup>

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

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<sup>28</sup>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 9).

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.<sup>29</sup>

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 and the results might reflect a quantity-quality trade-off.<sup>30</sup> 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 10 and Appendix Table A.2).<sup>31</sup> 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).<sup>32</sup> 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.<sup>33</sup>

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<sup>29</sup>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.7. 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.

<sup>30</sup>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.

<sup>31</sup>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 et al., 2006](#)).

<sup>32</sup>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.

<sup>33</sup>Generally, in Nigeria, most children leave their parents' homes when they leave for college, employment or marriage.

## 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.

### 4.1 RD Design: The Effects of UPE on Maternal Education

Figure 5 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 are shown in Table 3. 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.<sup>34</sup> Column 2 of Table 3 presents the effects without controls and the results are similar to the base specification in column 1. Appendix Figure A.3 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

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<sup>34</sup>As a validity check to show that the reform only affected areas in need of primary schools, in Appendix Table A.5 column 1, I show that the reform did not affect women living in the lowest intensity areas (the coefficient is negative). In column 2, the effect on women living in median intensity areas is positive but not significant at conventional levels (0.574).

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 percentage points.<sup>35</sup>

## 4.2 The Effects of Maternal Education on Child Education

Panel A of Table 4 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 6, 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-

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<sup>35</sup>Odunowo (2019) shows that the reform also improved literacy for women.

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.<sup>36</sup> Following [Anderson \(2008\)](#), I present the False Discovery Rate (FDR) Adjusted Q-values for the different measures of schooling in the bottom Panel of Table [Table 4](#). 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.4](#), [A.5](#), and [A.6](#) 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 terms of magnitude, when I compare the reduced form estimates in Panel B with the 2SLS estimates in Panel C, I find that most of the effect of the reform on children education is passed through maternal education. It accounts for about 75% of the effect on grade-for-age, 69% of the effect of primary school completion and 68% of the effect of attending secondary school. 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.

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<sup>36</sup>These effects are larger than the OLS estimates and are in line with similar studies on intergenerational mobility ([Oreopoulos et al., 2006](#); [Carneiro et al., 2013](#)). 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.

### 4.3 Heterogeneous Effects

I test for differences across gender and regions (urban/rural). The results are presented in Table 5. Panel A shows the effects are larger for girls; the results are statistically significant.<sup>37</sup> There are no statistically significant differences between the outcomes of children living in rural and urban areas.

### 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 7 is a dynamic difference-in-difference graph showing the reform did not affect mothers born before 1970. Table 6 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.<sup>38</sup> 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

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<sup>37</sup>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 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.

<sup>38</sup>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.

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.<sup>39</sup> While columns 4-6 show that the reform induced some individuals to have more than primary education.

#### 4.5 Second Generation Impacts of UPE (total effects of the reform)

Table 7 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.7, A.8, A.9 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.

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<sup>39</sup>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.



## 5 Robustness Checks, Mediating Factors and Discussion

### 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 8, 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 9. 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.6.<sup>40</sup> 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 heaped with the treatment variable. This approach removes the bias by allowing the heaped and non-heaped data to have different intercepts and slopes.<sup>41</sup> 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.

## 5.2 Other Robustness Checks

Appendix Table A.7 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 et al. (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

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<sup>40</sup>Alternative clustering include state and cluster level, two-way clustering (year of birth and state), wild cluster bootstrap (year of birth and state, respectively).

<sup>41</sup>However, if the treatment effects for the heaped and non-heaped data are different, this method will not recover the unbiased average treatment effect.

group, in areas with low and high UPE intensities. Creating a placebo cohort 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). Finally a perform simple bounding exercise to show that migration cannot explain the results.

### 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.<sup>42</sup> Though there could be different potential mechanisms, I only focus on a few due to data availability.<sup>43</sup>

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<sup>42</sup>A mediator will have a significant causal relationship with maternal education and reduce the effect of maternal education on child outcomes when included in the same regression.

<sup>43</sup>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 10 and 11 show the effect of maternal education on a set of potential mediators- wealth status, involvement in child’s educational decisions, labor market, and spousal characteristics. Table 10 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.<sup>44</sup> 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.<sup>45</sup> 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.”

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

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<sup>44</sup>? finds no effect on birth spacing.

<sup>45</sup>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.

cognitive outcomes. This could arise because mothers learn in school that schooling requires efforts and they assist their children in their studies. In Table 11, 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 on their children's education. I find a marginally significant effect of education increasing women's participation in decisions on children's education (6.7 percentage points). In a companion paper ?, 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 12 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.<sup>46</sup> 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 totally drives 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.<sup>47</sup> Testing

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<sup>46</sup>In Panel 1 of Table 12 the coefficient on maternal education is 0.052 and reduces to 0.048 when wealth index is included, so  $((0.052-0.048)/0.052) \times 100 = 7.7\%$

<sup>47</sup>Using the sample of men born between 1963 and 1977 in the highest intensity areas, 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 ( $t=1.73$ ).

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.

## 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. \(2018\)](#) finds 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 lower in sub-Saharan Africa, 68.75% and 34.4% respectively at 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.<sup>48</sup> The reform increased the probability of attending secondary school by 0.19 SD for children and 0.34 SD for the mothers.<sup>49</sup> 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

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<sup>48</sup>For the first-generation outcome, the effect is similar to other studies on Nigeria [Osili and Long \(2008\)](#) and [Larreguy and Marshall \(2017\)](#)

<sup>49</sup>I do not present results for years of education for the second generation since many of them have not yet completed their schooling.

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 and 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 of about 75.56 NGN (or \$95.89).<sup>50</sup>

Since I find that for the full sample, the UPE increased maternal educational attainment by 0.48 SD, the implied cost is 15.74 NGN (\$19.97) per 0.1 SD increase.<sup>51</sup> 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.

## 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

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<sup>50</sup>Enrollment in 1975 was 5,950,297 and 15,214,481 in 1981 (difference = 9,264,184). Cost per capita: 75.96 NGN = 700,000,000/9,264,184. Osili and Long (2008) show that 0.788 NGN = \$1 in 1976.

<sup>51</sup>See (Glewwe and 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 reforms (\$1-14/0.1 SD increase), select CT reforms (\$77-\$100/0.1 SD increase) computer introduction (\$2-33/0.1 SD increase) and teacher incentive reforms (\$1/0.1 SD increase).

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, 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 and 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 15.74 NGN (\$19.97) per 0.1 SD increase.

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## A Figures and Tables

Table 1: 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
<b>Percent of children with:</b>					
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
<b>Mothers</b>					
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
<b>Fathers</b>					
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 1: Proportion of females born between 1960 and 1969 not completing primary school, by state

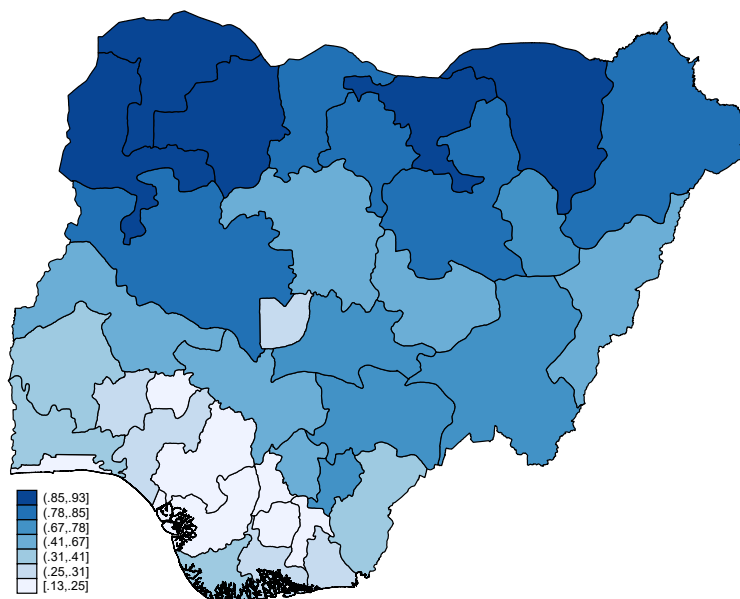
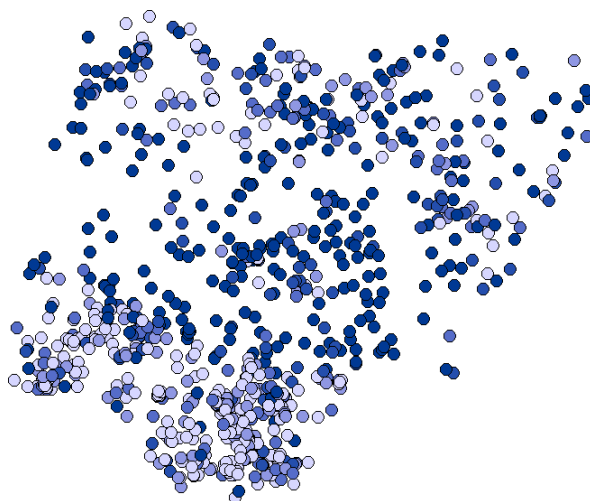


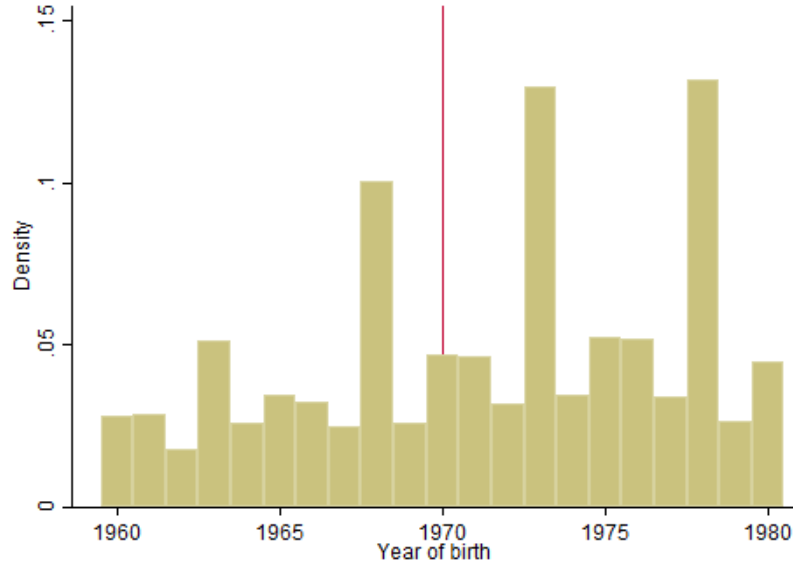
Figure 2: Proportion of females born between 1960 and 1969 not completing primary school, by clusters



*Notes:* The maps show the UPE intensities for the 36 states and the Federal Capital Territory in Figure 1, and for the survey clusters in Figure 2, 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 3: Distribution of maternal year of birth



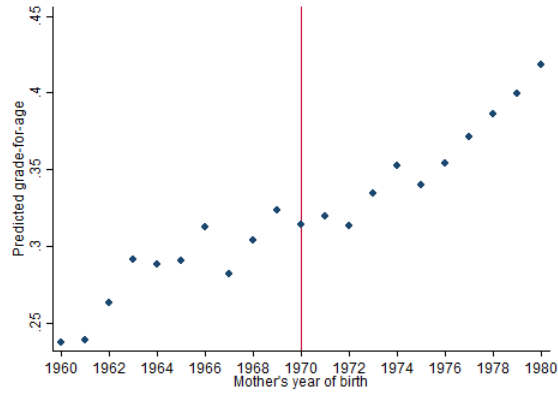
Notes: The graph represents the distribution of maternal year of birth between 1960 and 1980. Data is from the Demographic and Health Surveys between 2003 and 2013. There is a rounding age pattern in the survey. The most obvious is at multiples of 5 years, which represents the spikes at 1963, 1968, 173,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

Table 2: Smoothness of baseline covariates: Effect of UPE reform on predicted schooling

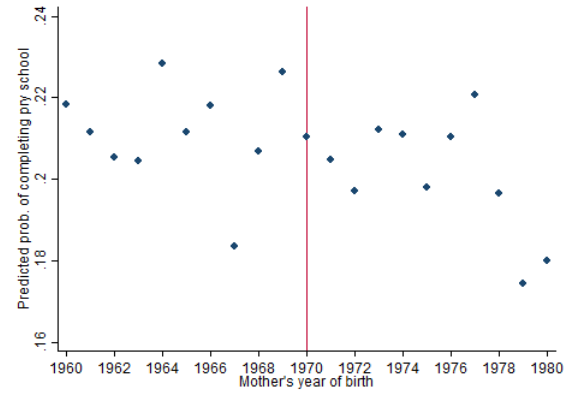
	<b>Grade-for-age</b>	<b>Complete primary school</b>	<b>Attend secondary school</b>
	1	2	3
Post-UPE	-0.002 (0.006)	-0.002 (0.007)	-0.003 (0.004)
N	9,579	3,418	3,393

*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.

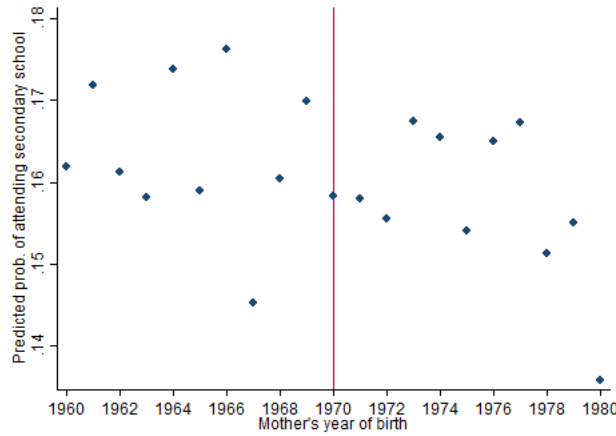
Figure 4: Smoothness of baseline covariates: Effect of UPE reform on predicted schooling



(a) Grade-for-age



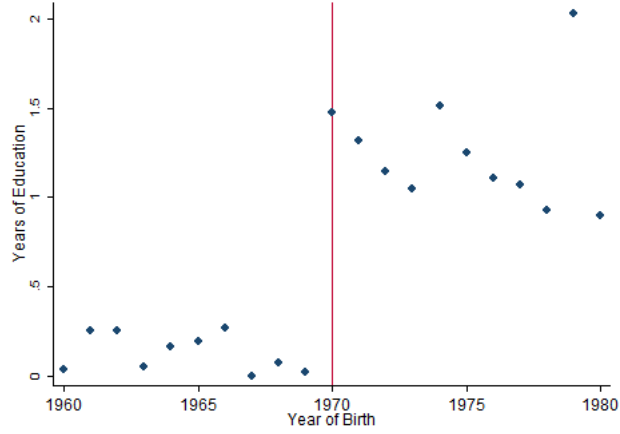
(b) Primary school completion



(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 5: First stage: Effect of UPE reform on maternal education



(a) Educational attainment of mothers by birth cohort

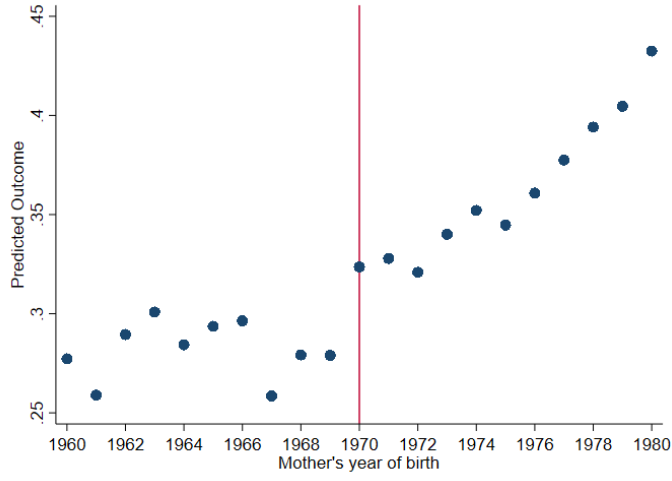
*Notes:* This graph shows the average years of education for each cohort living in the highest intensity areas. Sample includes women born between 1960-1980. With the reform starting in 1976 and the official school starting age being six, women born in 1970 and later are eligible for the reform. The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

Table 3: Effects of UPE reform on maternal education

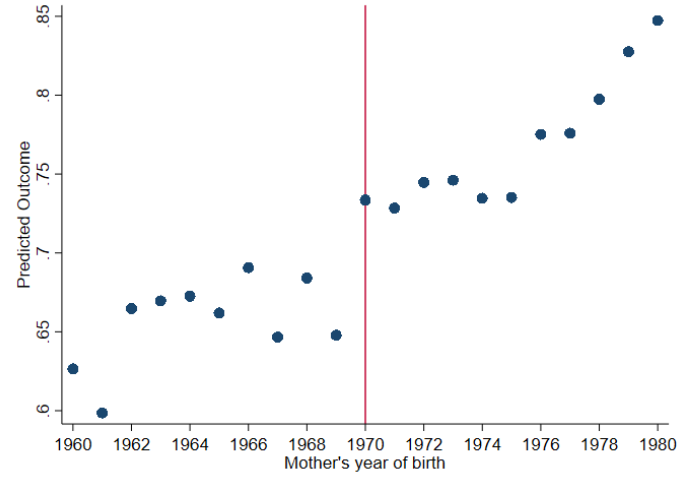
	Years of education				Complete primary school	Incomplete secondary school	Complete secondary school
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-UPE	1.331*** (0.090)	1.328*** (0.089)			0.156*** (0.008)	0.058*** (0.007)	0.033*** (0.007)
Placebo 1965 reform			0.133 (0.099)				
Placebo 1975 reform				0.168 (0.231)			
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

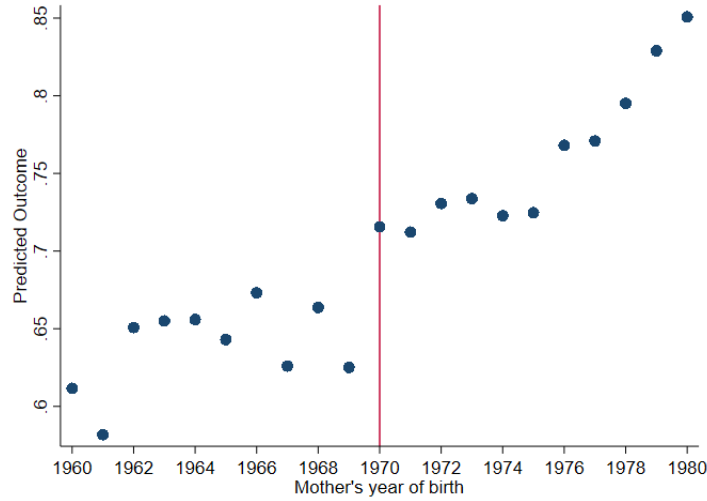
Figure 6: Reduced form estimates: UPE reform and child schooling



(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 4: Effects of maternal education on child schooling

	Grade-for-age	Complete primary school	Attend secondary school
	1	2	3
<b>OLS</b>			
Maternal education	0.027*** (0.003)	0.037*** (0.004)	0.037*** (0.004)
<b>Reduced Form</b>			
Post UPE	0.057** (0.019)	0.068*** (0.019)	0.069*** (0.015)
<b>2SLS</b>			
Maternal education	0.043*** (0.015)	0.047*** (0.013)	0.047*** (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

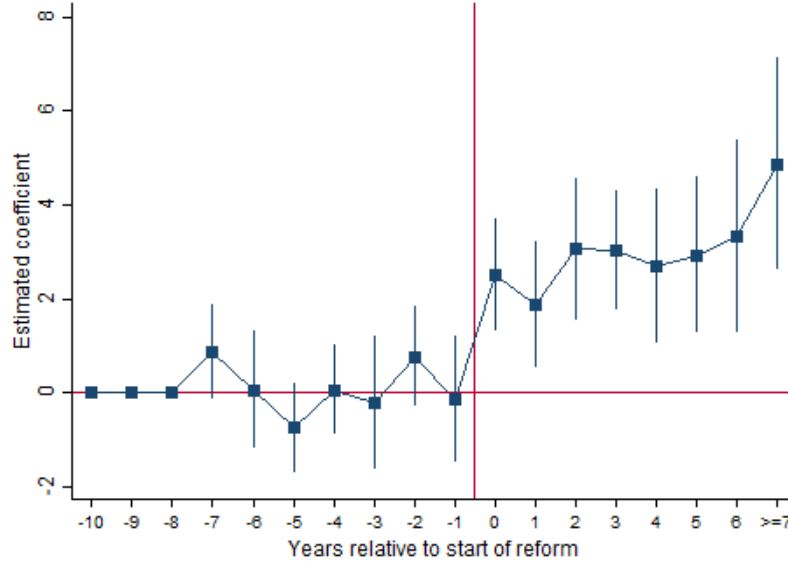
*Notes:* *Post-UPE*: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. With the reform starting in 1976 and the official school starting age being six, women born in 1970 and later are eligible for the reform. *Maternal education*: total number of years 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 5: Effects of maternal education on child schooling, by gender and region

	Grade-for-age 1	Complete primary education 2	Attend secondary school 3
<b>Gender</b>			
Mother's education x Male	-0.017** (0.008)	-0.020 (0.017)	-0.029** (0.015)
<b>Region</b>			
Mother's education x Urban	-0.006 (0.026)	-0.037 (0.040)	-0.015 (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 7: 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 6: 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*** (0.315)	2.571*** (0.332)		0.240*** (0.029)	0.125*** (0.030)	0.064** (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 7: Effects of UPE reform on child schooling (full sample)

	Grade-for-age	Complete primary education	Attend secondary school
	1	2	3
<b>Reduced Form</b>			
Post UPE x Intensity	0.042* (0.023)	0.067** (0.025)	0.073*** (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 8: Robustness checks

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

	Grade-for-age	Complete primary school	Attend secondary school
	1	2	3
<b>A: Uniform weights</b>			
Maternal schooling	0.031* (0.016)	0.046*** (0.013)	0.051*** (0.011)
N	[9,579]	[3,418]	[3,393]
<b>B: 5 years bandwidth</b>			
Maternal schooling	0.058*** (0.015) [7,445]	0.057*** (0.016) [2,693]	0.050*** (0.011) [2,676]
<b>C: 6 years bandwidth</b>			
Maternal schooling	0.054*** (0.015)	0.051*** (0.015)	0.047*** (0.011)
N	[8,455]	[3,014]	[2,994]
<b>D: 8 years bandwidth</b>			
Maternal schooling	0.041*** (0.014)	0.043*** (0.012)	0.046*** (0.010)
N	[12,301]	[4,134]	[4,015]
<b>E: 9 years bandwidth</b>			
Maternal schooling	0.039*** (0.014)	0.041*** (0.011)	0.044*** (0.010)
N	[12,877]	[4,331]	[4,303]
<b>F: Robust standard errors</b>			
Maternal schooling	0.043*** (0.011)	0.047** (0.019)	0.047*** (0.017)
N	[9,579]	[3,418]	[3,393]
<b>G: Controlling for heaps (allowing different intercept)</b>			
Maternal schooling	0.038*** (0.013)	0.056*** (0.012)	0.050*** (0.010)
N	[9,579]	[3,418]	[3,393]
<b>H: Controlling for heaps (allowing different intercept and slope)</b>			
Maternal schooling	0.025** (0.010)	0.050*** (0.013)	0.045*** (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 10: 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 birth 4	Number of children 5	Wealth index 6
Maternal education	0.001 (0.004)	0.923*** (0.208)	-0.355 (0.469)	0.052 (0.129)	-0.038 (0.106)	0.074** (0.033)
N	9,116	9,342	9,342	9,188	9,621	9,621
Controls	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 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 11: Effect of maternal education on potential mediators (continued)

	Worked in the last 12 months 1	Paid work 2	Involved in children education decision 3
Maternal education	0.021 (0.013)	-0.028 (0.029)	0.067* (0.039)
N	9,170	6,255	1,152
Controls	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 the child's education decisions 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 12: Mediation analysis

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

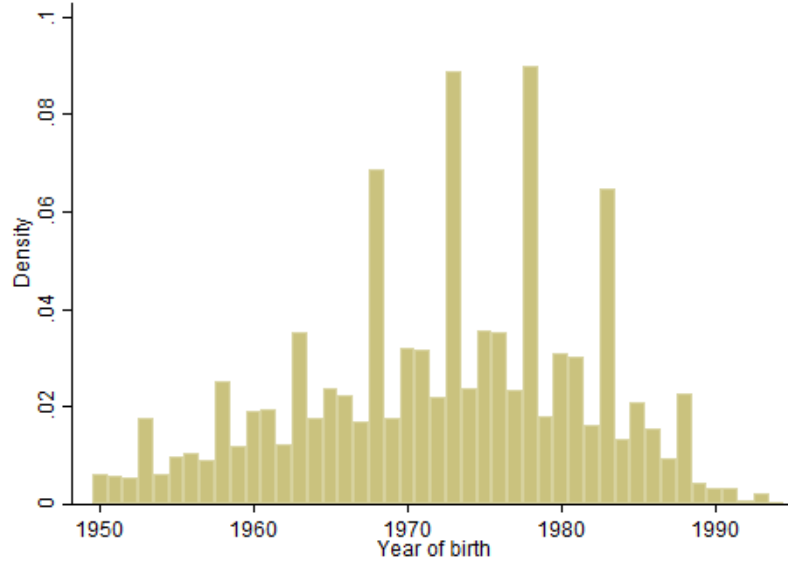
## B Appendix

Table A.1: Federal capital funds allocated to states for the UPE reform (1976)

State	Region	Amount (Naira)
Anambra	Eastern	8,342,532
Bauchi	Northern	2,973,215
Bendel	Midwestern	10,062,666
Benue	Northern	3,175,804
Borno	Northern	2,601,302
Cross River	Eastern	10,256,206
Gongola	Northern	5,005,509
Imo	Eastern	8,271,194
Kaduna	Northern	11,116,441
Kano	Northern	12,131,038
Kwara	Northern	9,538,412
Lagos	Western	13,890,626
Niger	Northern	2,025,000
Ogun	Western	321,524
Ondo	Western	717,838
Oyo	Western	1,744,305
Plateau	Northern	6,287,450
Rivers	Eastern	5,821,876
Sokoto	Northern	8,369,744

Source: Fourth National Development Plan 1981-1985, Federal Government of Nigeria. The third column lists the funds allocated to each state for the 1976 UPE reform.

Figure A.1: 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.2: Testing for selection: Effects of UPE on fertility

	Total number of children born	Prob. of having a child
Post UPE x Intensity	0.112 (0.145)	0.020 (0.018)
N	25,452	25,452
First stage <i>F</i> 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.3: 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 (139.066)	18.895 (162.742)	-1009.417 (7946.025)	-1183.563 (9070.144)	-1394.737 (10368.288)
N	8,347	8,347	8,347	8,347	8,347
Controls	No	No	No	No	No

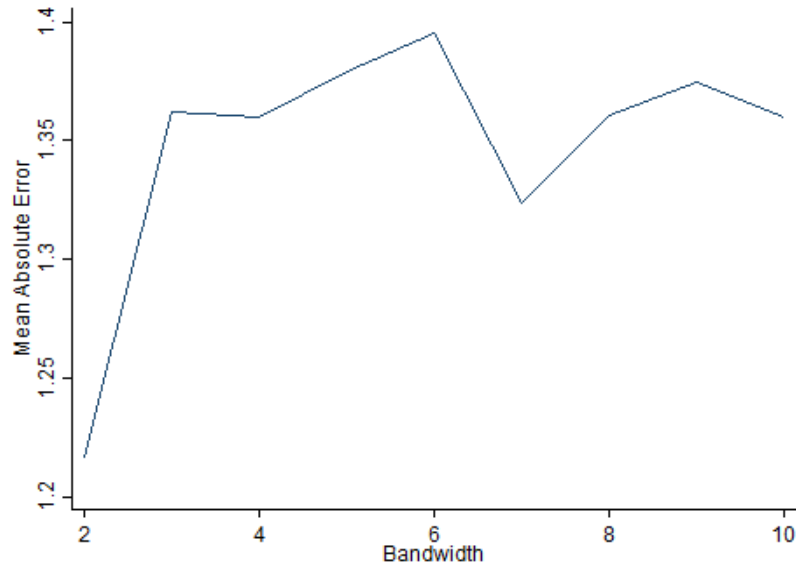
*Notes:* *Global human footprint:* average of an index between 0 (extremely rural) and 100 (extremely urban) for the location within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster. *Gross cell production:* average purchasing power parity in 2005 US dollars for the 2 km (urban) 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.4: Smoothness of individual characteristics, by bandwidths

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

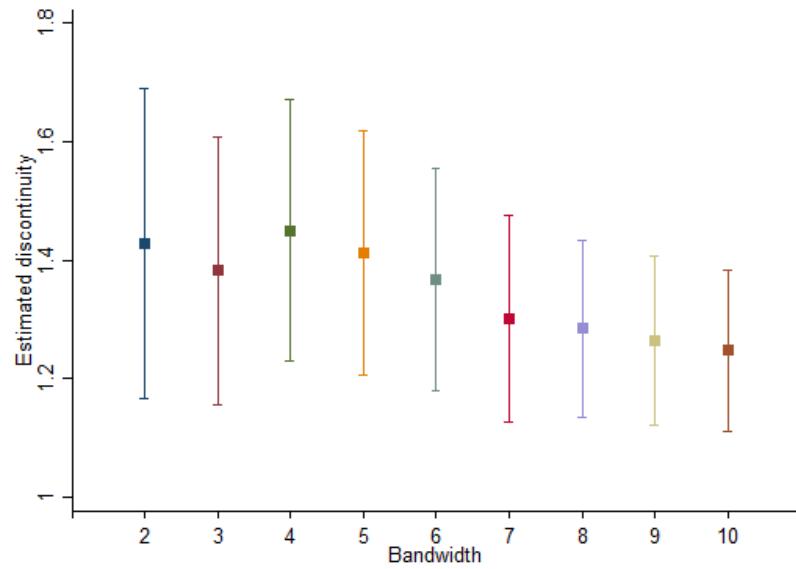
*Notes:* This table shows the smoothness of individual covariates across the threshold for varying bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth and reported in parentheses. \* Significant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

Figure A.2: 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.3: 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.

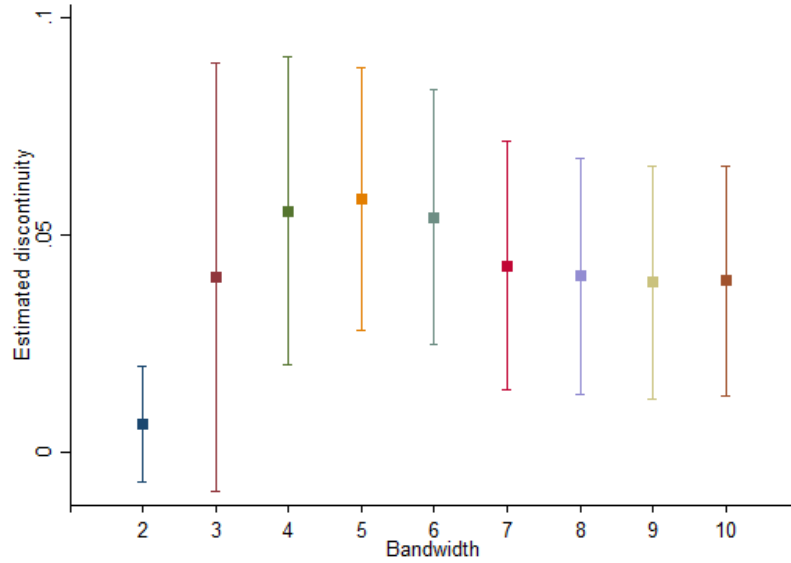


Table A.5: Falsification test: Effect of the reform on maternal education in other intensity areas

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

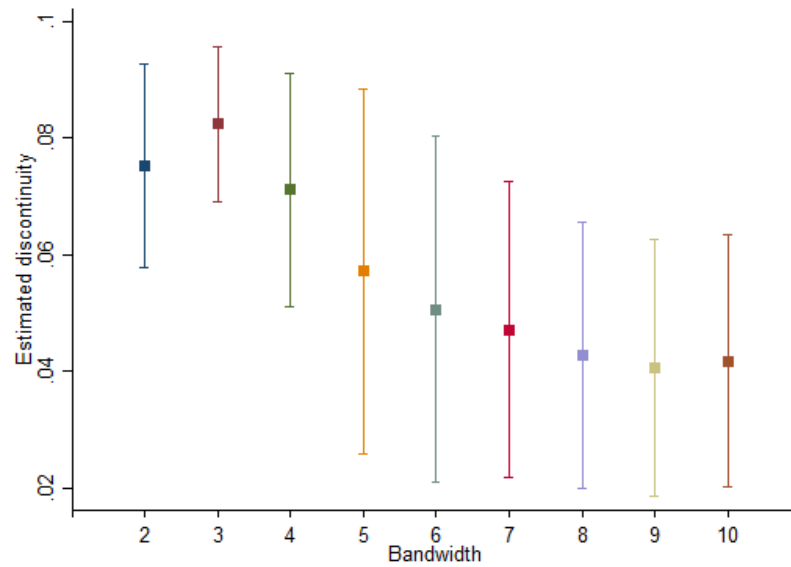
*Notes:* *Post-UPE*: a dummy variable that takes on one if maternal year of birth is 1970 or later, and zero otherwise. *Lowest intensity areas*: a region where all women born between 1960-1969 had completed primary school. *Median intensity areas*: a region where about 70% of women born between 1960-1969 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.4: Effects of maternal education on grade-for-age (all bandwidths)



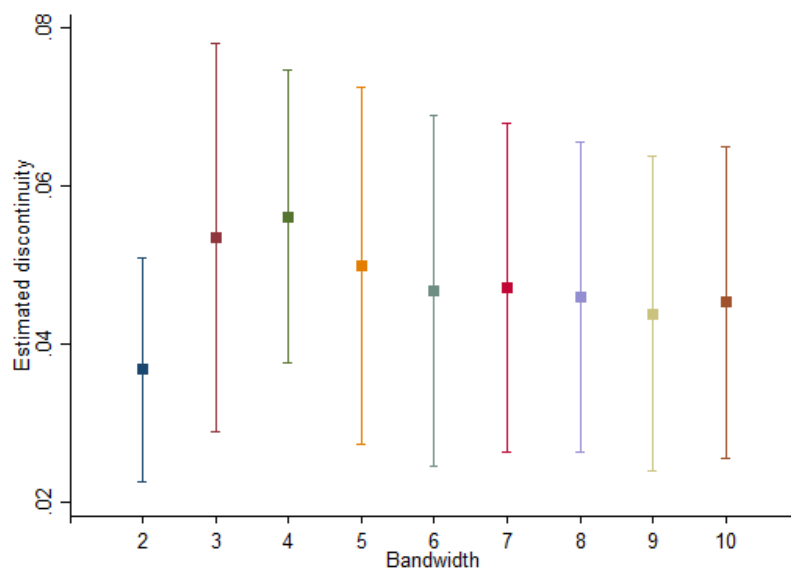
*Notes:* The y-axis shows the estimated discontinuity from the regression of grade-for-age on treatment, across different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth. The x-axis shows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

Figure A.5: Effects of maternal education on primary school completion (all bandwidths)



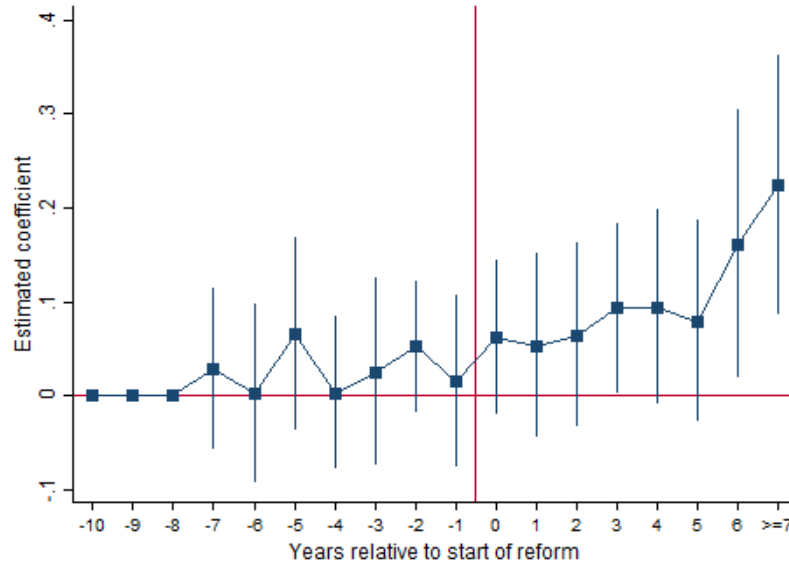
*Notes:* The y-axis shows the estimated discontinuity from the regression of primary school completion on treatment, across different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth. The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

Figure A.6: Effects of maternal education on attending secondary school (all bandwidths)



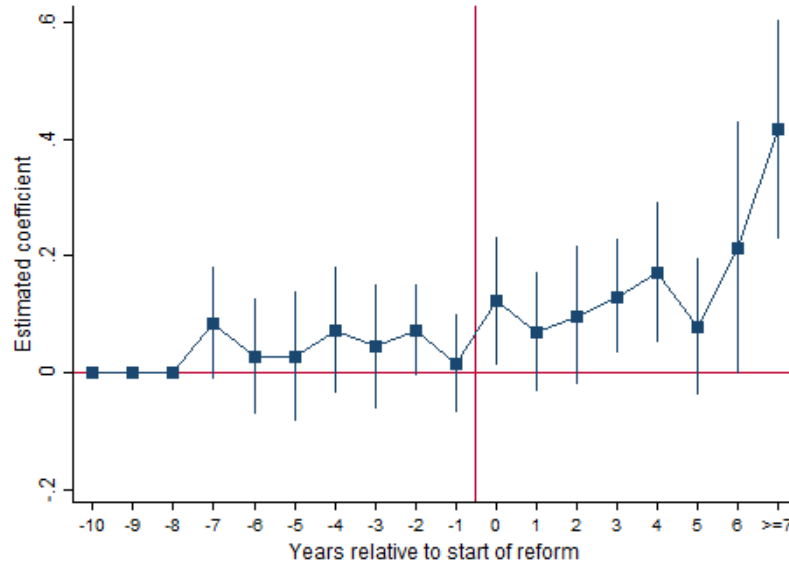
*Notes:* The y-axis shows the estimated discontinuity from the regression of attending secondary school on treatment, across different bandwidths. Bandwidth refers to the number of bins (maternal year of birth) on either side of the threshold. Standard errors are clustered at maternal year of birth.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

Figure A.7: Reduced form: Effects of UPE reform on grade-for-age



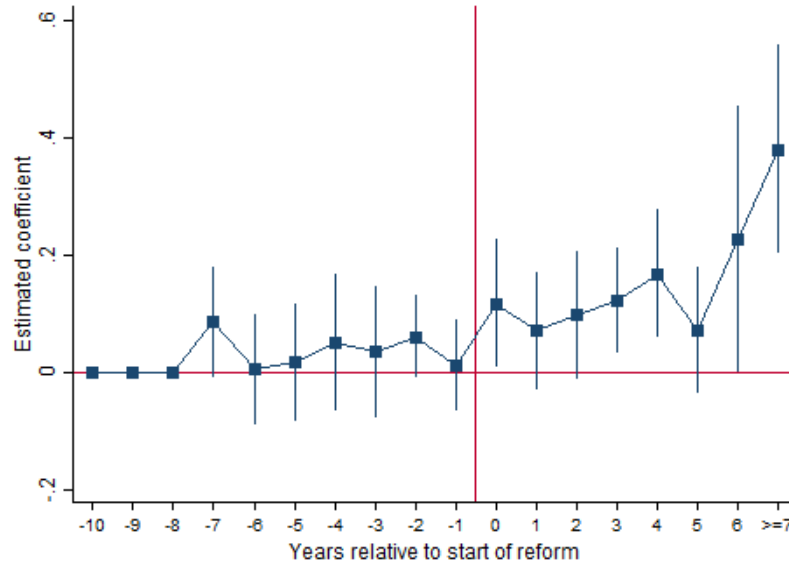
*Notes:* Dynamic difference-in-differences estimates from equation 4. The regression includes maternal year of birth fixed effects, state fixed effects and state-specific linear trends. Data is from the full sample of all mothers born between 1960-1980. The y-axis shows the coefficients from the regression 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.8: 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.9: 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.6: Alternative clustering specifications

	Grade-for-age	Complete primary school	Attend secondary school
	1	2	3
<b>Clustering at survey cluster level</b>			
Maternal schooling	0.043*** (0.014)	0.047** (0.020)	0.047*** (0.017)
<b>State level clustering</b>			
Maternal education	0.043*** (0.014)	0.047*** (0.014)	0.047*** (0.011)
<b>Two way clustering (year of birth and state)</b>			
Maternal education	0.043** (0.019)	0.047* (0.027)	0.047** (0.022)
<b>Wild cluster bootstrap (year of birth)- P-value</b>			
Maternal education	0.086*	0.007***	0.036**
<b>Wild cluster bootstrap (state)- P-value</b>			
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.7: Robustness checks

	Grade-for-age	Complete primary education	At least some secondary education
	1	2	3
<b>A: Main specification</b>			
Post UPE x Intensity	0.042* (0.023) [44220]	0.067** (0.025) [18098]	0.073*** (0.025) [17981]
<b>B: Controlling for other government programs</b>			
Post UPE x Intensity	0.041* (0.023) [44220]	0.064** (0.026) [18098]	0.070*** (0.025) [17981]
<b>C: Clustering standard errors at 1976 states</b>			
	0.042* (0.023) [44,220]	0.067** (0.026) [18,098]	0.073** (0.027) [17,981]
<b>D: Clustering standard errors at survey cluster level</b>			
	0.042** (0.020) [44,220]	0.067*** (0.025) [18,098]	0.073*** (0.025) [17,981]
<b>E: Placebo 1968</b>			
	0.022 (0.038) [12,465]	0.018 (0.044) [6,171]	0.001 (0.041) [6,141]
<b>F: No pre-trends</b>			
	0.084*** (0.015) [44,220]	0.101*** (0.019) [18,098]	0.108*** (0.018) [17,981]
<b>G: State -cohort fixed effects</b>			
	0.067** (0.029) [44,220]	0.100*** (0.037) [18,098]	0.102*** (0.034) [17,981]
<b>H: Excluding partially treated cohort</b>			
	0.043 (0.026) [35,457]	0.069** (0.032) [14,254]	0.074** (0.031) [14,160]
<b>I: Full sample</b>			
	0.045** (0.021) [57,640]	0.052** (0.024) [21,959]	0.066** (0.025) [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.8: Other robustness checks

	Grade-for-age	Complete primary school	Attend secondary school
	1	2	3
<b>A: Dropping highest intensity regions (1970-1980)</b>			
Maternal education	0.043*** (0.015) [9,548]	0.047*** (0.013) [3,405]	0.047*** (0.011) [3,380]
<b>B: Children 6-17</b>			
Maternal education	0.046*** (0.016) [8,799]		
<b>C: Without controlling for child's age</b>			
Maternal education	0.044*** (0.015) [9,579]	0.050*** (0.012) [3,418]	0.050*** (0.011) [3,393]
<b>D: Probit estimation</b>			
Maternal education	0.042*** (0.014) [9,579]	0.041*** (0.012) [3,418]	0.039*** (0.008) [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. 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