Document de travail du LEM / Discussion paper LEM
2022-01

Mothers and Fathers: Education, Co-residence and Child Health

Elodie DJEMAI

Université Paris-Dauphine, Université PSL, IRD, LEDa, DIAL, 75016 Paris, France. | elodie.djemai@dauphine.psl.eu

Yohan RENARD

Université Paris-Dauphine, Université PSL, IRD, LEDa, DIAL, 75016 Paris, France.

Anne-Laure SAMSON

Univ. Lille, CNRS, IESEG School of Management, UMR 9221 - LEM - Lille Économie Management, F-59000 Lille, France.

https://lem.univ-lille.fr/

Les documents de travail du LEM ont pour but d'assurer une diffusion rapide et informelle des résultats des chercheurs du LEM. Leur contenu, y compris les opinions exprimées, n'engagent que les auteurs. En aucune manière le LEM ni les institutions qui le composent ne sont responsables du contenu des documents de travail du LEM. Les lecteurs intéressés sont invités à contacter directement les auteurs avec leurs critiques et leurs suggestions.

Tous les droits sont réservés. Aucune reproduction, publication ou impression sous le format d'une autre publication, impression ou en version électronique, en entier ou en partie, n'est permise sans l'autorisation écrite préalable des auteurs.

Pour toutes questions sur les droits d'auteur et les droits de copie, veuillez contacter directement les auteurs.

The goal of the LEM Discussion Paper series is to promote a quick and informal dissemination of research in progress of LEM members. Their content, including any opinions expressed, remains the sole responsibility of the authors. Neither LEM nor its partner institutions can be held responsible for the content of these LEM Discussion Papers. Interested readers are requested to contact directly the authors with criticisms and suggestions.

All rights reserved. Any reproduction, publication and reprint in the form of a different publication, whether printed or produced electronically, in whole or in part, is permitted only with the explicit written authorization of the authors.

For all questions related to author rights and copyrights, please contact directly the authors.

Mothers and Fathers: Education, Co-residence and Child Health*

Elodie DJEMAI† Université Paris-Dauphine
Yohan RENARD‡ Université Paris-Dauphine
Anne-Laure SAMSON§ Université de Lille

Abstract

We use four waves of Demographic and Health Surveys from Zimbabwe to evaluate the effect of mother's and father's education on child health. We identify causal effects using exposure to the 1980 education reform. A simultaneous-equation model is estimated to take into account possible selection into co-residence between parents and children, endogeneity biases and parental education sorting. Our results suggest father's education affects health outcomes of their under-5 children and matters more than mother's. Results are robust when we exclude parents partially exposed to the reform and when we restrict the sample to parents born in years close to 1965. There is selection in our sample. The inverse Mills ratios capturing the likelihood of living with one's father or mother significantly affect child health. Last, parental educational sorting is shown to be important. Our findings suggest that not considering both parents' education simultaneously may produce misleading conclusions.

JEL Codes: I10, I26, O12, J12, C36, C34.

Keywords: Education, Human Capital, Couples, Child's Health, Sub-Saharan Africa

*Previous versions circulated under the title "The impact of mother's and father's education on child's health: Evidence from a quasi-experiment in Zimbabwe". This project has benefited from the financial support of Health Chair - a joint initiative by PSL, Université Paris-Dauphine, ENSAE, MGEN and ISTYA under the aegis of the Fondation du Risque (FDR). We would like to thank Richard Akresh, Eric Bonsang, Andrew Clark, Brigitte Dormont, Martin Karlsson, Carole Treibich, Jean-Noel Senne and participants at the LEGOS seminar (Paris, November 2017), the "Journées des Economistes de la Santé Francais" (Marseille, December 2017), the DIAL-Gretha Workshop (Paris, June 2018), the 4th IRDES Workshop on applied health economics and policy evaluation (Paris, June 2018) and the DIAL Development Conference (Paris, July 2019) who provided insightful comments. The usual disclaimer applies. Declarations of interest: none.

†Université Paris-Dauphine, Université PSL, IRD, LEDa, DIAL, 75016 Paris, France.
Corresponding author: elodie.djemai@dauphine.psl.eu
Université Paris-Dauphine, Place du Maréchal de Lattre de Tassigny, 75016 Paris (France)).

‡Université Paris-Dauphine, Université PSL, IRD, LEDa, DIAL, 75016 Paris, France.

§Univ. Lille, CNRS, IESEG School of Management, UMR 9221 - LEM - Lille Économie Management, F-59000 Lille, France.

1. Introduction

The factors leading to better health are as important to economists as to other researchers in social sciences and policy-makers. Out of the eight Millennium Development Goals, three concern health and access to health care in developing countries. The lack of resources at both the governmental and individual levels has long been highlighted as the main barrier to improving health in developing countries. Poor people in low-income countries face a variety of health-related risks, with young children accounting for most of the global disease burden.

Of the 55.4 million deaths in 2019, 9.3% were of children under the age of five. In Africa this figure reached 34.8%.[1] Over 46% of all deaths in low-income countries in 2019 were caused by so-called "Group I" conditions, which include communicable diseases, maternal causes, conditions arising during pregnancy and childbirth, and nutritional deficiencies. By way of contrast, only 6.6% of deaths in high-income countries were due to these causes[2] (World Health Organization 2020). These conditions caused 52.9% of all deaths in the WHO African Region in 2019. As such, most deaths could be avoided by adopting preventive actions (Banerjee and Duflo 2011) such as vaccination, water filtering, breastfeeding and the use of bed-nets. Education plays a key role here via its induced demand for prevention.

Since the model of health demand in Grossman (1972), the education-health relationship has appeared in a wide body of theoretical and empirical research. On average, the more-educated have better health and live longer than the less-educated (e.g. Lleras-Muney 2005). Education not only affects the adults' own health, but parental education also impacts the health of their children.

There are many channels through which education might affect health. The first is wealth. The educated are likely to have better labor opportunities and higher wages, so that they can more likely afford the cost of prevention, treatment and private health-insurance, and have better access to health care and health centers. Second, the educated are more likely to understand the prevention messages they receive than their less-educated counterparts. Third, they have greater incentives to invest in preventive behaviors as, given the wage differential, the gap in terms of the future loss from illness is higher for the educated than for the less-educated. Last, education teaches discipline, compliance with rules and exams, exertion of effort and accepting constraints, as noted in Basu (2002). As such, it might help educated people to adopt costly preventive behaviors. Most of these mechanisms also apply when explaining why parental education might help improve child health.

Using the four waves of the Demographic and Health Surveys in Zimbabwe[3] from 1994 to 2010/11,

[1] Authors' calculations from World Health Organization (2020).

[2] The gap was even larger in 2000: 65.4% in low-income countries v.s. 6.6% in high-income countries.

[3] Zimbabwe is a low-income country of 16 million inhabitants (with GDP per capita of 2,085.7 current international \$ in PPP in 2017 (World Bank, World Development Indicators)) located in Southern Africa. The under-5 mortality rate was 70.7 in 2015 (World Health Organization 2017). Life expectancy at birth was 61 in 1985, 44 in 2002 and

we examine the health outcomes of children aged 0-59 months born between 1990 and 2011. We compare the outcomes of children with educated mothers and fathers to those whose parents are less-educated.

The major problem in this comparison is the endogeneity of education, from the correlation between the unobservable characteristics leading to education and those leading to health investments. Two examples of these unobserved characteristics are ability and time preference. Education and health are two indicators of human capital. As such, investing in education and investing in health both imply costly investment today for a future uncertain benefit. In addition, if educated parents are in better health than are less-educated parents, this affects the child's health via the intergenerational transmission of health (Bhalotra and Rawlings 2011).

A number of contributions have exploited exogenous variation in education to identify the causal relationship between education and outcomes such as employment, fertility and health. Recent articles have explored the relationship between education and health in developing countries, as major reforms to the latter's school systems took place between 1970 and 2000. Using information on reforms allows us to estimate the causal effect of education on health outcomes in a quasi-experimental setting, as it provides exogenous variation in enrolment in primary or secondary school, the number of years of completed schooling or the likelihood of dropping out of school in instrumental-variable or regression-discontinuity approaches. Examples of these reforms are compulsory school-enrollment (Agüero and Bharadwaj 2014; Grépin and Bharadwaj 2015; Güneş 2015), the rise of the school-leaving age (Albouy and Lequien 2009; Kemptner et al. 2011), the supply of schools (Bhalotra and Clarke 2014; Breierova and Duflo 2004; Silles 2009), the provision of trained teachers (Shrestha and Shrestha 2020), the implementation of Universal Primary Education policies (Behrman 2015; Delesalle 2021; Osili and Long 2008) and changes in school fees (Chicoine 2021; Masuda and Yamauchi 2020; Oyelere 2010; Silles 2009; Zenebe Gebre 2020). We here exploit the exogenous increase in education produced by the 1980 reform to estimate the causal effect of mother's and father's education on child health in Zimbabwe. The 1980 Education reform is a nationwide reform that mostly consists in compulsory primary school-enrollment, the removal of primary school fees and automatic admission to secondary school. It affects all school aged children, and most importantly children born after 1965 as they were aged 15 or less at the time of the reform and exogenously more likely to pursue in secondary schools compared to those born earlier.

Co-residence between parents and children might also bias the estimates, as it might not be random in the population and covers a non negligible share of children: only 52.7% of our survey children aged 0-59 months live with both parents. It is well-established in the literature that children growing up in single-parent households acquire less human capital, whether the parents divorced or

60.3 in 2015 (World Bank, World Development Indicators). The large fall at the end of the 1990s reflects high HIV prevalence. The HIV prevalence rate in the Demographic and Health Surveys was 21% for women aged 15-49 and 15.5% for men in 2005 (vs. 16.7 and 10.5 respectively in 2015).

one died (see Adda et al. 2011; Fitzsimons and Mesnard 2014). Living with both parents, compared to living with only one or neither, is not random and affects child health. We treat this as a selection issue, as the education of the parent is not observed if he or she does not live in the same household as the observed child. The selection equations, one for each parent, are identified using exogenous variations in community practices (e.g. the share of mothers who give birth before being married). Our analysis of selection into co-residence provides new insights into the current literature on the education-health relationship that has to date neglected this dimension. Emran et al. (2018) document this source of bias, calling it a truncation bias due to co-residency in the estimations of intergenerational mobility.

We also contribute to the literature on the respective role of mothers and fathers on child outcomes. The role of father's education has been overlooked in the current literature, with only relatively few contributions (Alderman and Headey 2017; Apouey and Geoffard 2016; Breierova and Duflo 2004; Case and Paxson 2001; Chou et al. 2010; De Neve and Subramanian 2017; Lindeboom et al. 2009). This could reflect the common wisdom that mothers matter more than fathers in raising children. Another purely-empirical reason is that mothers are more likely than fathers to live with their children in many countries, leading to empirical challenges when trying to evaluate the role of fathers. Case and Paxson (2001) study the role of father's and mother's education and co-residence in child health in the US, but without modeling selection into co-residence or marital sorting.

Our work here also takes into account the marital education sorting of parents as an additional source of bias in the estimates, with the size of the bias being *a priori* even larger in articles that examine the effect of each parent's education in separate models. If the correlation between education levels is high, the estimate of the effect of mother's education on child's health without controlling for father's education may instead pick up the effect of father's education. This source of bias is acknowledged, even though not resolved, in Carneiro et al. (2013) who estimate the effect of maternal education on child outcomes. Using data from Nepal and investigating the role of female education on child welfare, Fafchamps and Shilpi (2014) conclude that "at least part of the predictive power of mother education on child welfare is driven by marriage market effects and higher father's education." The bias may also come from unobservable characteristics (such as ability and time preference) that drive (un)educated people to match together. Marital sorting based on education has been documented in developed and developing countries (e.g. Azam and Djemai 2019; Chiappori et al. 2009; Van Bavel and Klesment 2017).

The father's contribution is modeled in three ways in recent work. First, the effect of the average mother's and father's education is estimated in Breierova and Duflo (2004). However, this does not allow us to consider differences between parents nor to use exposure to the reform as an instrumental variable, as men are usually older than their spouses. Second, two separate models are estimated, one controlling for mother's education and the other for father's education, as in Apouey and Geoffard (2016), Chou et al. (2010) and De Neve and Subramanian (2017). From our viewpoint, this is debatable for two reasons: in the case of educational marital sorting, part of the effect of mother's education may reflect that of the father's, and there is no discussion about co-residence, even though the sample sizes vary from one estimation to the other. If one parent is absent because of divorce or death, the parent who is living with the child might compensate for the absence, and all the more so when (s)he is more educated and as such, has more room to adjust. Some papers have explored the role of the absence of one parent on the formation of human capital and suggest that human capital is greatly affected. One example is Adda et al. (2011), who evaluate the long-term consequences of parental death and find that mothers and fathers have differential effects on child cognitive and non-cognitive skills. The third approach is to estimate the effect of both mother's and father's education in the same equation, as in Lindeboom et al. (2009) and Alderman and Headey (2017). In the latter, maternal and paternal education are referred to even for non-biological parents, whereas the effect might be different, given work on child fostering and step-mothers (e.g. Case and Paxson 2001). In this paper, we focus on the role of biological mothers and fathers, and estimate their respective effects in a single equation.

Grépin and Bharadwaj (2015), De Neve and Subramanian (2017) and Makate and Makate (2018) are closest to our analysis, as they consider the 1980 education reform in Zimbabwe to estimate the causal effect of parental education on child health. Grépin and Bharadwaj (2015) focus on the effect of maternal education on child mortality and Makate and Makate (2018) on dietary practices and nutrition, while we here estimate the effect of both father's and mother's education on child's current health, prenatal care and child birth. De Neve and Subramanian (2017) estimate the effect of father's and mother's education on child malnutrition, as we do, but their estimation strategy differs from ours in several respects: they estimate the respective effects in separate regressions, the outcomes are different, they do not take marital sorting into account nor selection into co-residence. De Neve and Subramanian (2017) find a negative correlation between mother's duration of schooling and nutrition outcomes (namely being stunted, wasted or underweight) and same negative correlation with father's duration of schooling, but no evidence of a causal effect of parental schooling when schooling is instrumented by exposure to the reform.

Our results mostly confirm the existing evidence: there is a high correlation between mother's education and child health, and between father's education and child health. We instrument education by the exposure to the 1980 reform that affects those born in 1965 or later. The instrumented effect of father's education on prenatal care and birth conditions continues to be positive; however, that of mother's education becomes zero. This conclusion continues to hold when we exclude parents born in 1965 and 1966, who are only partially exposed to the reform and when we restrict the sample to parents born in years close to 1965. We also provide some supporting evidence that fertility preferences and wealth act as mediators. Lastly, our finding shows that if father's education is not included in the equation, the instrumented effect of mother's education is significant. This suggests that the effect of the father's education is confounded in the effect of mother's education when not considered, especially due to assortative matching of the parents.

The remainder of our paper is organized as follows. Section 2 presents the data. Section 3 describes the reform and its impact on parents' education. The estimation strategy is presented in Section 4. The empirical results are described in Section 5, and the robustness checks and extensions appear in Section 6. Last, Section 7 concludes.

2. Data

2.1. Sample

We use household-level data collected by the Demographic and Health Surveys in Zimbabwe. This survey is nationally representative of households and was collected in 1994, 1999, 2005/06 and 2010/11.[4] The sampling is in two stages and independent in each survey round. First, enumeration areas are selected based on the most recent available census. Second, a complete listing of the households living in the selected enumeration areas (also called communities here) is established in order to randomly select the sampled households, and in the latter every women aged 15-49, whether permanent residents or visitors (who slept in the household the night before the survey) are eligible for interview. We here use the data files from the household roster, the female questionnaire, and to a lesser extent the male questionnaire.

The household roster includes the complete list of household members and, for each member, his age and highest level of education. For children, the identification codes of the mother and father are listed if they live in the same household. As such, we have different types of households and family composition. We observe children who are not living with their parents (e.g. children fostered in another household) and children living with either one or both parents. By construction, if a sampled mother is not living with one of her children, this child is not a household member and is not present for the collection of anthropometric measures.

The analysis focuses on children aged between 0 and 59 months old. Mothers are asked specific questions about children in this age range as part of the female questionnaire. These questions cover prenatal care, delivery conditions and vaccination. We also have anthropometric measures for children in this age group. The four rounds of survey data cover 19,702 children aged 0-59 months. We then observe young children born over the period 1990 and 2011. However, to observe the level of education of both parents, we need to restrict the sample to children who currently live with both parents (N=10,381 children). We will discuss later the potential selection bias arising from the co-residence. Lastly, mothers from this sample are born between 1916 and 1995, and fathers

[4]Two additional waves are available: 1988 and 2015. First, we cannot use the 1988 survey wave in our strategy design since it is impossible to link the household children to their fathers. Second, we do not use the 2015 survey wave because as parents are younger, the number of parents who have not been exposed to the reform is very small. Only 0.2% of the children in this wave were born to unexposed mothers, and 5.5% to unexposed fathers.

between 1898 and 1993. To circumvent survival bias of the parents, we removed the oldest parents from this sample, i.e. all parents born before 1950.[5] Our analytical sample is composed of 9,365 children.

Note that it might be the case that parents are observed more than once if they have more than one child aged 0-59 months at the time of the survey. One may argue that the duplicates are more likely to concern younger parents and/or less educated parents on average. Such types of parents would as a result stand for a greater weight in the regression. In our analytical sample, 77% of the households have only one observed child and 23% have more than one child 0-59 months old. Our core empirical results hold when we weight the observations by the number of observations for each mother-father couple.[6]

2.2. Data Description

The summary statistics for the entire sample appear in column 1 of Table 1. A full description of the variables are provided in Appendix Tables A1 and A2.

Over the entire sample, 50% of the children are girls, the average age is 2 and 24% live in urban areas. For 98% of the 0-59 months children in sample households the mother is still alive, and for 95% the father is alive. Co-residence with the mother is 31 percentage points more likely than co-residence with the father: 85% of children live in the same household as their mother, and 54% in the same household as their father. The average age of mother at child birth is 26.4, and when the fathers are present their observed average age at birth is 34.3.

The summary statistics for the outcome variables appear in Panel B. These can be grouped into three categories: (1) outcomes related to prenatal care and birth, namely a dummy for having had at least four prenatal visits, being born in a health facility and having been assisted by medical staff at birth; (2) malnutrition with a dummy for being stunted (too short for their age) and a dummy for being wasted (too thin for their height)[7]; and (3) prevention (vaccination and sleeping under a mosquito bed-net).[8]

69% of sampled children were born in a health facility, and 68% of births were assisted by a skilled medical attendant. In 71% of cases, at least four prenatal visits were attended, as recommended

[5] Only 100 children have mothers born before 1950 and 1,004 have fathers born before 1950.

[6] Results available upon request.

[7] Children are stunted if their height-for-age Z-score is more than two standard deviations below the reference value. Children are wasted if their weight-for-height Z-score is more than two standard deviations below the reference value.

[8] The use of bed-nets is not asked in the 1994 and 1999 survey waves. We do not analyze breastfeeding as 98% of children were breastfed. We are unable to estimate the effect of parental education on child mortality because it is impossible to link deceased children to their fathers, as the information to do so is only available for alive children living in the sampled households.

by the World Health Organization. We use the anthropometric measures in the survey to construct common malnutrition indicators: 33% of young children are stunted and 6% are wasted. The average number of injections from the recommended immunization package (BCG, Diphteria-Pertussis-Tetanus, measles, polio) received by a child aged 0-59 months is 5.73 (out of 8), and 9% of sample children slept under a bed-net the night before the survey.

Our unit of observation is the child even for the statistics of the parents. In the analytical sample, we have a total of 7,497 mothers. 5,744 have only one observed child 0-59 months old. For the remaining observations, mothers and fathers appear at least twice.

Mother and father education appear in Panel C of Table 1. In the analytical sample in column 3, the average number of years of schooling is 7.8 for mothers and 8.6 for fathers. 71% (77%) of mothers (fathers) completed primary school, and 54% (63%) attended at least one year of secondary school.

Other control variables appear in Panel D. Mothers' average age is 28.3 and that for fathers 36.2. This age difference corresponds to the usual age-difference figure found in existing work (e.g. d'Albis et al. 2012). The entire sample in column 1 is used to estimate the probability of the children to live with his father or mother. The average proportion of women (men) who are separated, divorced or widowed in the community is 17% (9%), the proportion with first child born before marriage is 20%, and the average proportion of polygamous households is 14%. Lastly as Zimbabwe has been dramatically affected by the HIV/AIDS epidemic that has increased orphanhood, we include a measure of AIDS mortality in the selection equation. The measure is gender-specific assuming that AIDS mortality among women and among men affects the probability that the child lives with her mother or her father. We use a time series of AIDS-related mortality per 1,000 population at the national level coming from UNAIDS statistics[9] to compute the average of the AIDS-related mortality levels over the period between child's year of birth and the survey year. This measure is on average 7.18 and 7.21 per 1,000 population, for females and males respectively.

Note that the descriptive statistics for the entire sample (column 1), for the sub-sample of children living with both parents (column 2) and for the sub-sample of children who live with both parents and who have parents born in 1950 or later (column 3) are very similar. This suggests that our analytical sample is not highly selected.

[9] Available at https://aidsinfo.unaids.org/.

Table 1. Summary statistics on the main variables

	(1) All	(2) Sample of children living with both parents	(3) Sample of children living with both parents born in 1950 or later
Panel A – *Child characteristics*			
Girl	0.50	0.50	0.50
Age	2.00	1.88	1.84
Urban	0.24	0.30	0.32
Rich	0.33	0.39	0.41
Polygamous household	0.14	0.12	0.10
Mother alive	0.98	1.00	1.00
Father alive	0.95	1.00	1.00
Mother present	0.85	1.00	1.00
Father present	0.54	1.00	1.00
Mother's age at birth	26.44	26.79	25.94
Father's age at birth	34.26	34.27	32.06
1994	0.21	0.21	0.17
1999	0.19	0.19	0.18
2005	0.30	0.30	0.32
2010	0.30	0.30	0.33
Panel B – *Outcomes*			
At least 4 prenatal visits	0.71	0.71	0.71
Health Facility Birth	0.69	0.67	0.68
Birth assisted by medical staff	0.68	0.67	0.68
Stunted	0.33	0.32	0.32
Wasted	0.06	0.06	0.06
Number of injections received by child	5.73	5.71	5.71
Slept under net last night	0.09	0.12	0.12
Panel C – *Parental Education*			
Years of education (mother)	7.57	7.48	7.82
Complete primary at least (mother)	0.69	0.67	0.71
Secondary school at least (mother)	0.52	0.51	0.54
Years of education (father)	8.27	8.26	8.64
Complete primary at least (father)	0.73	0.73	0.77
Secondary school at least (father)	0.59	0.59	0.63
Panel D – *Other Characteristics*			
Mother's age	28.30	28.67	27.78
Women separated (% in cluster)	0.17	0.16	0.16
First child born before marriage (% in cluster)	0.20	0.17	0.17
Female AIDS-related mortality (per 1,000)	7.18	7.17	7.29
Father's age	36.16	36.15	33.89
Men separated (% in cluster)	0.09	0.08	0.08
Polygamous (% in cluster)	0.14	0.14	0.14
Male AIDS-related mortality (per 1,000)	7.21	7.20	7.25
N	19,702	10,381	9,365

Source: Authors' calculations from the Demographic and Health Surveys 1994, 1999, 2005 and 2010

Notes: Unweighted statistics. The data covers children aged 0-59 months at survey time. The sub-sample in Column 2 corresponds to 0-59 months children living with both parents.

3. The Reform and its Impact on Education

3.1. The Reform

Before 1980 when the United Kingdom officially recognized the Independence of Zimbabwe, there were enormous inequalities in education between Whites and Blacks. For Whites, who represented 3.5% of the population, education was free and compulsory until the age of 15 and admission to secondary school was automatic after the pupils passed their primary school final exam (Dorsey 1989). However, education was neither free nor compulsory for Blacks, who faced considerable selection at each grade. As a result, in the 1970s, only 4% of Black pupils were in secondary school: the analogous figure was 43% for White pupils (Dorsey 1989). There was also inequality between boys and girls. In 1975, the girl/boy ratio was 85% in primary school and 71% in secondary school (see Appendix Table A3).

The first Black majority government - led by the Zimbabwe African National Union (ZANU) party - came to power with Independence in 1980. Education was one of its top priorities and the new Constitution declared education as a fundamental human right (Education Act 2004). From 1980 on, the Government launched a vast intervention campaign to raise school attendance and the education of every child (Colclough et al. 1990). This expansion was universal as it concerned both girls and boys and covered the whole country. The main policy changes took place in 1980 and can be summarized as follows:

- Primary education became free and compulsory for all pupils. Given the official duration of primary education, all children would leave school with at least 7 years of education.
- Admission to secondary school became automatic for all pupils, whatever their performance in the primary-school final exam. Secondary education remained paying.
- Age-restrictions were removed to allow older children to re-enter school.
- The government changed the school zoning system that gave Whites access to the best schools; it also introduced double-session schooling in almost all urban schools and some rural ones.

The reform took place in 1980, and was accompanied by an increase in the supply of schools and teachers in the forthcoming years. The Government reconstructed all schools that had been destroyed during the war and built new primary and secondary schools, in particular in marginalized areas and disadvantaged urban centers (Kanyongo 2005). New teachers were recruited. The World Development Indicators (World Bank) statistics in Appendix Table A3 show a huge jump in the number of primary-school teachers between 1980 and 1985 and an even larger jump in secondary-school teachers (from 3,782 to 19,507). This implies that the pupils to teachers ratio remained quite stable at around 40 in primary schools and 28 in secondary schools. Lastly, government expenditure on education rose sharply around the time of the reform, from 2.5% of GDP in 1980 to 12.5% in 1990.

3.2. Parental Education and Exposure to the Reform

The reform has affected children of primary-school and secondary-school age. It aimed to increase access to primary education and facilitate entry to secondary school, theoretically at age 13 as primary education lasts seven years.[10] Consequently, we can define as being exposed to the Education reform all individuals who were 15 or younger in 1980, in other words all individuals born in or after 1965. Indeed this definition includes the individuals fully exposed (who were 13 or younger in 1980) and those who were 14 and 15 in 1980 as the policy allowed over-age individuals to re-enter school, they are thus partially exposed.

The exposure to the reform has been used in previous analyses as it provides exogenous variation in educational attainment (Agüero and Bharadwaj 2014; Croke et al. 2016; De Neve and Subramanian 2017; Grépin and Bharadwaj 2015; Makate and Makate 2018). The cohorts born in 1967 or later are defined as treated in all settings. The cohorts born in the period 1963-1966 are considered slightly differently across the articles (excluded, control or partially treated).[11] We will discuss the robustness of our results to their inclusion in the treated group as a robustness check.

In our sample, 89% (70%) of the children have mothers (fathers) who were exposed to the reform (Table 2, panel A). Mothers exposed to the reform have an average of 8.2 years of education, versus 4.8 years for those not exposed (columns 2 and 3). 76% of the exposed mothers (37% of non-exposed mothers) completed primary school, and 59% attended secondary school (18% of the non exposed). On average, fathers exposed to the reform had 9.4 years of education versus 6.9 years for those not exposed. 85% of the fathers exposed to the reform (column 4) completed primary school versus 59% of those not exposed (column 5), and 74% attended secondary school versus 38% of the non-exposed. The impact of the reform is then about three to four additional years of education for both men and women. Men had much more education than women before the reform, and this gender difference remains after the reform.

[10] Up to 1986, children started primary school at age 7 (see Appendix Table A3).

[11] Agüero and Bharadwaj (2014) and Grépin and Bharadwaj (2015) restrict their sample to the individuals who were aged 9-20 in 1980. Agüero and Bharadwaj (2014) define the treatment variable taking on the value of 1 for individuals whose age in 1980 is less than or equal to 15, and 0 otherwise. Grépin and Bharadwaj (2015) consider women who were aged 13 and younger in 1980 to have been fully exposed to the policy, women aged 14 and 15 in 1980 to have been partially treated, and women aged 16 or older in 1980 the control group. Croke et al. (2016) and De Neve and Subramanian (2017) do not exclude the partially treated. The only difference between their two definitions is that those born in 1963 appear in the "partially treated" group in Croke et al. (2016) and in the control group in De Neve and Subramanian (2017).

Table 2. Summary statistics - Parents' exposure to the reform and education levels

	(1)	(2)	(3)	(4)	(5)
	Analytical sample	Mother		Father	
		Exposed	Not Exposed	Exposed	Not Exposed
Panel A – *Exposure to the 1980 Education Reform*					
Mother exposed	0.89	1.00	0.00	1.00	0.63
Father exposed	0.70	0.79	0.01	1.00	0.00
Panel B – *Education*					
Years of education (mother)	7.82	8.19	4.82	8.46	6.28
Complete primary at least (mother)	0.71	0.76	0.37	0.78	0.55
Attended secondary school (mother)	0.54	0.59	0.18	0.62	0.36
Years of education (father)	8.64	8.94	6.20	9.36	6.92
Complete primary at least (father)	0.77	0.81	0.51	0.85	0.59
Attended secondary school (father)	0.63	0.68	0.25	0.74	0.38
N	9,365	8,335	1,030	6,599	2,766

Source: Authors' calculations from the Demographic and Health Surveys 1994, 1999, 2005 and 2010

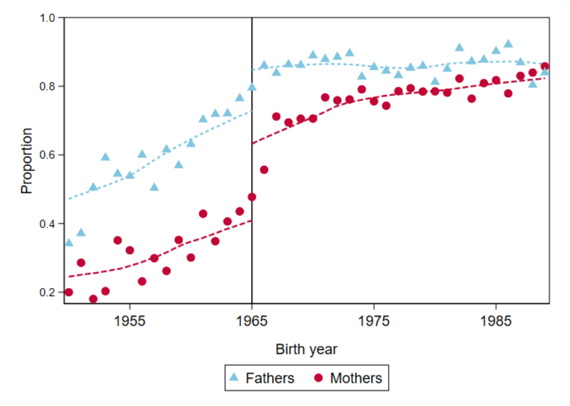
Notes: Unweighted statistics. The data covers children aged 0-59 months at survey time. The analytical sample corresponds to 0-59 months children living with both parents. Exposed mothers and fathers are those born in or after 1965.

Figure 1 depicts the proportion of mothers and fathers in each birth cohort who completed primary school (1(a)), who attended secondary school for at least one year (1(b)), and their respective number of years of education (1(c)). The vertical line corresponds to the 1965 cohort, that is the first cohort exposed to the new education policy. There are two main features.

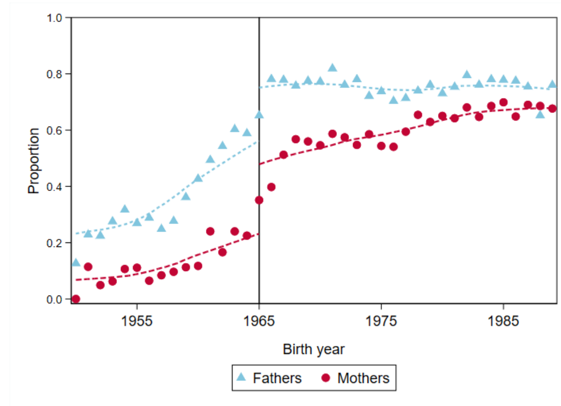
First, as shown in Figures 1(a) and 1(b), the reform resulted in an expansion of pupils completing primary school, but the largest change was found in secondary education. For this reason, we will focus on both secondary-school attendance and the number of years of completed schooling as education variables, the latter combining the reform's effects on the primary and secondary level.

Second, school attainment started to rise even among those not directly affected by the reform, in the sense that schooling was not compulsory for cohorts born before 1966. These cohorts were affected via easier school access after 1980. Figures 1(a) and 1(b) show that this increase reflected both primary- and secondary-school attendance. The increase in education thus applied to both sexes, even though it is more pronounced for men. Men benefited from an easier access to secondary school as they were already more likely to be enrolled in primary school than women.

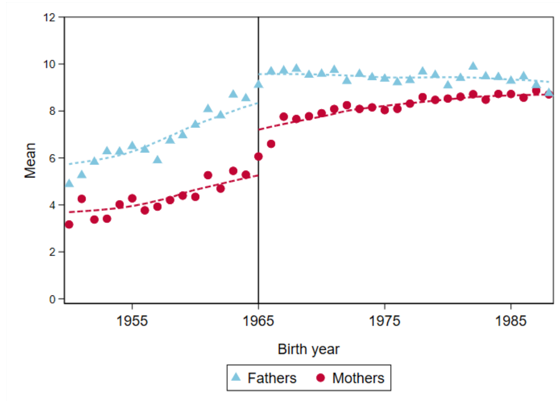
Figure 1. Mother's and Father's education by birth year



(a) Completed primary school



(b) Attended secondary school



(c) Years of education

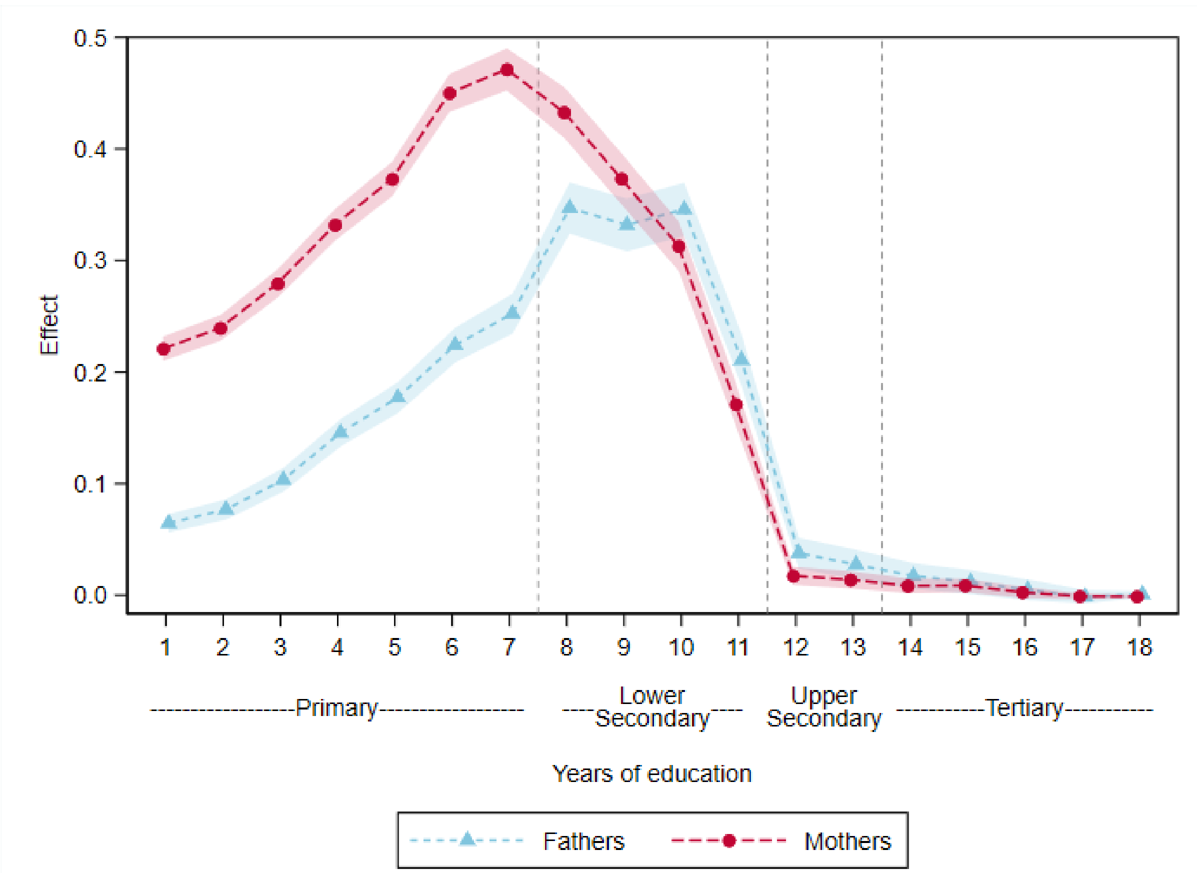
Source: Authors, based on Demographic and Health Surveys 1994, 1999, 2005 and 2010
Notes: The figure represents the average educational attainment of fathers and mothers depending on their year of birth, in terms of (a) probability of having completed primary school, (b) probability of having attended secondary school, and (c) number of years of education completed. The vertical lines represent the first cohort exposed to the 1980 Education Reform.

In Zimbabwe, primary education lasts seven years and secondary education six years,[12] so pupils completing both cycles have 13 years of education. We estimate the effect of being exposed to the reform on each additional year of education, and Figure 2 displays the effect of exposure to the 1980 reform on the probability to reach at least a certain number of years of education, by gender.[13] For both mothers and fathers, the reform had a considerable impact on attendance to primary and lower secondary school while the impact of the reform on the probability to reach upper secondary

[12]Lower secondary education lasts 2 years while upper secondary education lasts 4 years.
[13]Coefficients come from the estimation of several linear probability models, separately by gender.

and tertiary school is very small and fails to be significantly different from 0 for reaching at least 16-18 years of education. Exposure to the reform increased attendance to all primary and lower secondary school levels. For mothers, the highest effects are observed on the probability to reach at least 6 (+45 percentage points), 7 (+47 percentage points) and 8 (+43 percentage points) years of education, i.e. the end of primary school and the beginning of secondary education. For fathers, the reform has the highest estimated effects on the probability to reach at least 8, 9 and 10 years of education (+35, +33, and +35 percentage points), i.e. lower secondary education.

Figure 2. Reform exposure and educational attainment



Source: Authors, based on Demographic and Health Surveys 1994, 1999, 2005 and 2010

Notes: The figure represents the effect of exposure to the 1980 Education Reform on the probability to have reached at least the corresponding number of years of education and 95 percent confidence intervals. Each coefficient is from a separate linear probability model.

4. Econometric Specification

We estimate the joint impact of father's and mother's education on a number of child-health outcomes. Given the way in which the Demographic and Health Surveys are collected (as described above), this is only possible when the child lives with both parents. Our econometric strategy therefore tackles three econometric issues: i) the endogeneity of father's and mother's education; ii) marital education sorting (i.e. homogamy); and iii) selection into co-residence, as the sample of children who live with both parents is not random.

4.1. The Endogeneity of Education

In the child's outcome equation, father's and mother's education are likely endogenous, leading to inconsistent estimates of the impact of education on health outcomes. Unobservable parental characteristics (such as time preference, ability and intrinsic motivation) make them more likely to invest both in their human capital (education) and the health of their children. In addition, education is correlated with parental health status, and healthy parents are more likely to have healthy children. Not controlling for parent's own health status can then lead to a second source of endogeneity bias.

As shown in Figure 1, free and compulsory primary education as well as easier access to secondary education brought about an exogenous rise in educational attainment. Fathers and mothers born in or after 1965 (i.e. who were 15 or younger in 1980, or were not yet born) were young enough to benefit from the 1980 Education Reform, which substantially enlarged their schooling opportunities, as opposed to those born before. The exogenous variation in education levels due to the age-specific nature of the reform can be exploited using two different frameworks to tackle the endogeneity of parents' education.

One could use the regression discontinuity (RD) approach in a fuzzy design, as in Grépin and Bharadwaj (2015), the only difference being that we have two "running variables" that partially determine mother and father education levels: both the mother's and father's birth year. Using the discontinuity in the probability to attend secondary school and in the average number of years of education due to the education reform for those born in 1965, observed in Figure 1, it would amount to implement a double regression discontinuity approach (as in Stancanelli and Van Soest 2012 or Müller and Shaikh 2018 who study retirement decisions within couples). However, this approach is not suitable in our case. Indeed, the RD approach provides a local estimate of the effect of education on outcomes, around the 1965 birth year threshold. The sample used for the estimates is restricted to observations that are close to this cut-off. However, by construction, the size of our sample is already small, and restricting it to parents born for example 10 years around the reform (which seems the largest credible bandwith to implement a RD approach) would lead to a sample containing only 3,645 observations, which is too small to provide precise estimates. More importantly, the double RD approach imposes the use of the same bandwidth for fathers and mothers, meaning that we should run the estimates on mothers and fathers both born between, for example, 1955 and 1975. This would create a huge selection bias. Indeed, contrary to OECD countries where the age gap between spouses is quite low and such identical bandwidth can be used (as in Lindeboom et al. 2009 in the UK for example), the age gap is much larger in Zimbabwe (7 years on average): this would lead to keep only couples of the same age.

We therefore prefer to rely on the 2SLS procedure, with two first-stage regressions: one each for mother's and father's education using the individual exposure to the reform as the instrumental variable. Both first-stage equations are defined as follows:

$$Educ^M_{iht} = \beta^M_0 + \beta^M_1 T^M_{iht} + X'_{iht}\theta^M + \delta_t + \varepsilon^M_{iht} \tag{1}$$

$$Educ^F_{iht} = \beta^F_0 + \beta^F_1 T^F_{iht} + X'_{iht}\theta^F + \delta_t + \varepsilon^F_{iht} \tag{2}$$

where i refers to the child ($i = 1, ..., N$; N denotes the size of the analysis sample),[14] h the household and t the survey year. M denotes child i's mother and F the father.

We consider two alternative dependent variables. $Educ^M$ and $Educ^F$ refer either to the number of years of education reported by child i's mother and father respectively, or to dummies indicating whether the mother and father attended secondary school. The continuous variable is our preferred measure of education: indeed, the reform had an impact on the attendance to both primary and secondary school, hence this variable reflects the full effect of the reform.[15]

T^M_{iht} (T^F_{iht}) equals one if the mother (father) was born in or after 1965, i.e. was 15 or younger in 1980, and zero otherwise. The direct impacts of the reform are given by β^M_1 for the mother and β^F_1 for the father. We will include specific trend before and after the 1965 birth year threshold in equations (1) and (2) in an additional specification as in Agüero and Bharadwaj (2014) to model the different trends in education for the cohorts born before and after 1965, as suggested in Figure 1. In this latter specification, we will add $(B - 1966)1_{B<1966}$ pre-reform and $(B - 1966)1_{B\geq 1966}$ post-reform, where B is the parent's birth year. In other words, the two trends for the mother will be added to the mother's first-stage equation (1) and the two trends for the father will be added to

[14] We find the same results if we estimate these equations on the initial sample, i.e. the sample not restricted to having both mothers and fathers currently living with the observed child. These results are available upon request.

[15] The continuous education variable is strictly positive for almost all parents in the sample: only 3% of fathers and 5% of mothers have no education. This small share of zero values justifies our use of OLS regressions in the first stage.

the father's first-stage equation (2).

X_{iht} is a vector of control variables that will be included in the second stage equations (such as child sex and age) and that have to be included in the first-stages for identification purposes. We also control for survey year fixed effects, δ_t. Given our econometric strategy, other variables also need to be included in these first-stage regressions: these will be described in Section 4.4, where the final model is set out.

For the instrumental variable to be valid, it has to be correlated with the observed level of education and not correlated with the error term of the second-stage equation (Equation 9 below). The first correlation is discussed when presenting the results from the first-stage estimations in Section 5.1. The second requirement is the exclusion restriction: the instrument should have no effect on the outcome other than through the first-stage channel. The exclusion restriction here is that having a year of birth before or after 1965 does not affect child health through any other mechanisms than parental education. Given Zimbabwe's Independence, the Education reform occurred at the same time as other reforms, mainly in the social sector, of which the main one concerned health care (Grepin and Bharadwaj 2015). No other major reform (of transport, the labor market etc.) was carried out immediately following independence. The health-care reforms likely improved individual health from 1980 on, whatever their age. The parents in our sample, who were alive in 1980 or born soon after, were affected by these reforms via an increased supply of health care and a better immunization program (Grepin and Bharadwaj 2015). These reforms may have changed their own decisions about education and health, which will feed through to the decisions they take regarding their children. We may worry that part of the effect of the health-care reform may be captured by the education variable. We however think that this is unlikely. As stated by Grepin and Bharadwaj (2015), the health-care reform was not age-specific: all individuals were covered. There is no reason to believe that those born after 1965 were more affected than those born before 1965. Moreover, our sample children were born many years after the reform. Note that we do not find significant effect of the exposure to the reform on mother's probability of being underweight (not shown).

4.2. Assortative mating

Marital educational sorting may be an issue in our model. In the analysis sample, 86% of children whose mothers have completed primary school have a father who also completed primary school, and 82% of children whose mothers have attended secondary school have a father who also attended secondary school. There is consequently substantial correlation between mother's and father's years of education: 0.64. Women and men with similar education tend to live with or marry each other. As a result, the unobservable characteristics that explain mothers' education (such as intrinsic motivation, time preference) may well be correlated with unobservables that explain fathers' education.

In our final model, mother's and father's education (equations (1) and (2)) are therefore estimated simultaneously, taking into account the correlation between the residuals of both equations (ε^M_{iht} and ε^F_{iht}). We find a positive and very-significant correlation (0.56 for the number of years of education and 0.41 for secondary-school attendance) between these residuals: fathers and mothers with similar intrinsic incentives or aspirations towards human-capital investment tend to live and have children with each other.

4.3. Selection into Co-residence

Of the 19,702 sampled children aged 0-59 months, 52.7% live with both parents, 13.5% with neither, 1.3% with their father only and 32.5% with their mother only. More details on the different possible cases are presented in Appendix B. The percentage of children living with both parents is fairly stable over time: 52.8% in 1994, 52% in 1999, 53% in 2005 and 52.7% in 2010. This low percentage of children living with both parents is not particular to Zimbabwe, although it does have one of the lowest percentages in Sub-Saharan Africa according to Pilon and Vignikin (2006).[16]

Our estimations may suffer from selection bias due to the co-residence restriction, for which we need to correct. The unit of analysis here is all children 0-59 months living in sampled households, and selection bias is addressed via Heckman's two-step procedure.[17] We estimate two probit selection equations, one each for the mother and father. Let $Coresidence^M_{iht}$ ($Coresidence^F_{iht}$) be a dummy for child i living with her mother M (father F) and zero otherwise. We have:

$$Coresidence^M_{iht} = 1 \text{ if } Coresidence^{*M}_{iht} > 0, \text{ 0 otherwise}$$

$$Coresidence^F_{iht} = 1 \text{ if } Coresidence^{*F}_{iht} > 0, \text{ 0 otherwise}$$

where $Coresidence^{*M}_{iht}$ and $Coresidence^{*F}_{iht}$ are latent variables defined as follows:

$$Coresidence^{*M}_{iht} = \alpha^M_0 + Z'^M_{iht}\alpha^M_1 + X'_{iht}\phi^M + \theta_t + \mu^M_{iht} \tag{3}$$

[16] Pilon and Vignikin (2006) document large disparities in Sub-Saharan Africa: in Namibia, only 26% of children below the age of 15 live with both parents, 33% in South Africa, 50% in Zimbabwe and Rwanda, 65% in Benin, 71% in Ethiopia and 78% in Burkina Faso.

[17] We estimate a selection equation to explain why children are or are not currently living with each parent. Only 2% of mothers and 5% of fathers of sampled children are dead. Fathers/mothers who do not live with their child are therefore mainly parents who have somehow decided not to live together: divorcees, temporary migrants who quit the household and those who have entrusted their child to somebody else's care. We hypothesise that all of these potential (unobserved) reasons can be summarized by one single selection equation, a hypothesis that is of course debatable. However we do not impose these reasons to be the same for mothers and fathers. And we see that the proportion of children who live with their mother only is much higher than the proportion of children who live with their father only. We estimate separately the two probabilities.

$$Coresidence^{*F}_{iht} = \alpha^F_0 + Z'^F_{iht}\alpha^F_1 + X'_{iht}\phi^F + \theta_t + \mu^F_{iht} \quad (4)$$

As before, i indexes the child ($i = 1, ..., N_T$, where N_T denotes the size of the initial sample), h the household and t the year of the survey.

The estimation of these selection equations requires exclusion restrictions, i.e. variables that influence co-residence but have no direct effect on the outcome. We use community-level variables. For the mother, we use four variables (Z^M_{iht}): the proportion of sampled women who gave birth to their first child before getting married in each community, the proportion of sampled women who are currently divorced, separated or widowed in each community, the female and male national AIDS-related mortality rates averaged between the child birth year and the survey year. For fathers, the excluded variables (Z^F_{iht}) are the proportion of sampled men currently divorced, separated or widowed in each community, the percentage of sampled men living in a polygamous household in each community as well as the female and male national AIDS-related mortality rates. There are 1,264 different communities in the entire sample, each of which is large enough to be distinct from the individual considered (comprising, on average, 21 households and 39 adults). The models also include the child characteristics (X'_{iht}) from the outcome equation as described below, and survey year fixed effects, θ_t.

4.4. Final Specification

Our final specification aims to identify the causal effect of parental education on a number of child-health outcomes. We address education endogeneity via the policy reform that allowed some parents to enroll and stay longer in school when they were school-aged. We do so via 2SLS estimation. Selection into co-residence is taken into account using a two-step Heckman selection model, and marital homogamy using correlated error terms between fathers' and mothers' education equations. We use the procedure described in Wooldridge (2010) to estimate a full model that takes all these issues into account in the five-equation model described below.

$$
\begin{cases}
Coresidence^{*M}_{iht} = \alpha^M_0 + Z'^M_{iht}\alpha^M_1 + X'_{iht}\phi^M + \theta_t + \mu^M_{iht} & (5)\\[2ex]
Coresidence^{*F}_{iht} = \alpha^F_0 + Z'^F_{iht}\alpha^F_1 + X'_{iht}\phi^F + \theta_t + \mu^F_{iht} & (6)\\[6ex]
Educ^M_{iht} = \beta^M_0 + \beta^M_1 T^M_{iht} + \beta^M_{2,M}\lambda^M_{iht} + \beta^M_{3,F}\lambda^F_{iht} + X'_{iht}\theta^M + \delta_t + \varepsilon^M_{iht} & (7)\\[2ex]
Educ^F_{iht} = \beta^F_0 + \beta^F_1 T^F_{iht} + \beta^F_{2,M}\lambda^M_{iht} + \beta^F_{3,F}\lambda^F_{iht} + X'_{iht}\theta^F + \delta_t + \varepsilon^F_{iht} & (8)\\[6ex]
H_{iht} = \gamma_0 + \gamma^M_1 Educ^M_{iht} + \gamma^F_1 Educ^F_{iht} + \gamma^M_2\lambda^M_{iht} + \gamma^F_2\lambda^F_{iht} + X'_{iht}\Gamma + \upsilon_t + e_{iht} & (9)
\end{cases}
$$

In equation (9), H_{iht} is child health, and $Educ^M_{iht}$ and $Educ^F_{iht}$ are variables for mother's and father's level of education respectively.[18] Child health is measured using the different outcomes, presented in Section 2.2.

The outcome equation includes some exogenous variables that also appear in the selection and first-stage equations: X_{iht} includes child characteristics (age and sex) and we control for survey year fixed effects. In the main models, we do not include variables such as household wealth quintiles, urban location, or dummy variables for province of residence as they are very likely to be endogenous. However we consider them in the section that discusses the mechanisms either as the outcome variables or as additional control variables.

As described in Wooldridge (2010), we correct for any selection bias by adding the Inverse Mills ratios from the Probit estimation of equations (5) and (6) to both the first-stage ((7) and (8)) and outcome (9) equations. The two Inverse Mills ratios are λ^M and λ^F, and a test for selection bias is $\gamma^M_2 = 0$ and $\gamma^F_2 = 0$ in (9).

Equations (5) and (6) are estimated separately via Probits, and Equations (7) to (9) are estimated simultaneously using linear-probability models. This joint estimation allows us to take into account any correlation between the error terms: ε^M_{iht} and ε^F_{iht} may be correlated due to assortative matching; ε^M_{iht} and e_{iht} as well as ε^F_{iht} and e_{iht} may also be correlated if mothers (fathers) have unobserved characteristics that influence both their choice of education and their ability to improve their child's health. We later discuss the sign and significance of all these correlations. We do not consider any correlation between μ^M_{iht} and ε^M_{iht}, μ^F_{iht} and ε^F_{iht} or μ^M_{iht}, μ^F_{iht} and e_{iht}, as these error terms refer to samples of different sizes. Last, note that standard errors are clustered at the enumeration area level in all equations as proportions computed at the enumeration area level are included in the set

[18]We are unable to include an interaction between mother's and father's education, to test for complementarity between the two. There is no child who are born from an unexposed mother and an exposed father.

of right-hand side variables.

Lastly, selection equations (5) and (6) have to be estimated using the whole initial sample. In our baseline analysis, equations (7), (8) and (9) are estimated on the analytical sample.

5. Results

5.1. First-stage Results

The estimation results of both first-stage equations (1) and (2) are presented in Table 3.[19] To improve readability, coefficients of X_{iht} are not reported in this table.[20] Panel A and B differ according to the education variable considered: the number of years of education in Panel A or the dummy for having attended secondary school in Panel B. In both panels, columns 1 and 2 refer to the mothers' first-stage regression and columns 3 and 4 to the fathers'. In column 2 and 4, additional pre- and post-reform linear trends are added.

Exposure to the reform has a tremendous impact on educational attainment. The average number of school years is 2.95 years higher for mothers exposed to the reform compared to the non-exposed (column 1), with a corresponding figure of 2.35 years for fathers (column 3). The reform therefore had a huge impact on parents' education levels and a greater effect on mothers than fathers (and significantly so at the 1% level).[21]

[19] In practice, as the first-stage and outcome equations are estimated simultaneously, we have as many first-stage regressions as outcomes. Given that the sample size varies slightly between outcomes, depending on the number of missing values, the results from the first-stage estimations may also vary. However, this turns out not to be the case: the results are very similar across outcomes and sample sizes. In this section, and in the paper in general, we only report and comment on the first-stage regressions for the analytical sample.

[20] The final specification, described in Section 4.4, also includes the Inverse Mills ratio, that corrects for possible selection bias in the outcome equation. These ratios also have to be included as a right-hand side variable in the first-stage equations; in Table 3, there is no correction for selection, but results including this correction are very similar, as the inverse Mills ratio is not significant (results are available upon request).

[21] The pre-reform level of education differs between mothers and fathers: see the descriptive statistics in Table 2 and Figure 1.

Table 3. First-stage equations

	(1)	(2)	(3)	(4)
	Mother's education		Father's education	
Panel A	*Years of education*			
Exposed	2.955***	2.143***	2.351***	0.923***
	(0.168)	(0.285)	(0.121)	(0.195)
Pre-reform trend		0.147***		0.249***
		(0.046)		(0.021)
Post-reform trend		0.019**		−0.029***
		(0.008)		(0.008)
N	9,337	9,337	9,288	9,288
Mean of dep.	7.82	7.82	8.64	8.64
F-Statistic (excluded instrument)	310.31	56.72	374.84	22.42
p-value (excluded instrument)	0.000	0.000	0.000	0.000
	Mother's education		Father's Education	
Panel B	*Attended secondary school*			
Exposed	0.340***	0.277***	0.334***	0.140***
	(0.019)	(0.032)	(0.016)	(0.025)
Pre-reform trend		0.011**		0.033***
		(0.005)		(0.003)
Post-reform trend		0.002		0.003**
		(0.001)		(0.001)
N	9,343	9,343	9,315	9,315
Mean of dep.	0.54	0.54	0.63	0.63
F-Statistic (excluded instrument)	322.06	76.80	442.45	30.16
p-value (excluded instrument)	0.000	0.000	0.000	0.000

Source: Authors' calculations from the Demographic and Health Surveys.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects, as well as for control variables of the second stage equation (child's sex and age). The F-statistic of excluded instruments is obtained from the estimation of equations (1) and (2). There is no correction for selection into co-residence.

Our first-stage regressions are convincing. The F-statistics on excluded instruments (exposure to the reform) indicate that our instruments are not weak (F=310.31 for mothers; F=374.84 for fathers). Adding pre- and post-reform linear trends decreases the impact of exposure to the reform, with a much smaller coefficient in column 2 (column 4) than in column 1 (column 3), as a large part of the effect of the reform is captured by the trends. The coefficients are however still significant, and significantly larger for mothers (+2.14 years for mothers, +0.92 years for fathers).

The same pattern is observed in Panel B, for attendance to secondary school. Exposure to the reform increases attendance to secondary school by 34 percentage points for mothers (column 1) and 33 percentage points for fathers (column 3). The inclusion of linear trends both decreases the impact of the reform (+28 percentage points for mothers in column 2, +14 percentage points for fathers in column 4), as well as the F-statistic for the excluded instruments.

Given that the F-statistics for the excluded instruments decrease a lot when trends are included in the set of instrumental variables, we prefer to rely on first-stage regressions that use the binary variable for exposure as a unique instrument.

5.2. The Selection-Equation Estimation Results

The selection-equation estimation results appear in columns 1 and 2 of Table 4 respectively. We find that the greater the proportion of sampled women who gave birth to their first child before getting married in each community and the higher the proportion of sampled women who are currently divorced, separated or widowed in each community, the lower the probability that the child lives with his mother. Equally, the higher the proportion of men who are currently divorced, separated or widowed in the community, the lower is the probability that the child lives with his father. However, we do not find the proportion of polygamous households in each community, nor the national AIDS-related mortality rates to be related to the probability of living with their parents.

Table 4. Selection equations for mothers and fathers

	(1) Mother present	(2) Father present
Separated^M (% in cluster)	–0.562***	
	(0.141)	
Separated^F (% in cluster)		–0.889***
		(0.128)
First child born before marriage (% in cluster)	–0.718***	
	(0.080)	
Polygamous (% in cluster)		0.078
		(0.116)
Female AIDS-related mortality (per 1,000 population)	–0.089	–0.103
	(0.107)	(0.068)
Male AIDS-related mortality (per 1,000 population)	0.056	0.078
	(0.111)	(0.071)
Constant	1.367***	0.302***
	(0.133)	(0.082)
N	19,545	19,473
Mean of dep.	0.85	0.54
Correctly specified	55.15	54.49

Source: Authors' calculations from the Demographic and Health Surveys.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects, as well as for control variables of the second stage equation (child's sex and age). The selection equations are estimated for all children present in sampled households.

5.3. Second-stage Results

We use the analytical sample to jointly estimate the effect of father's and mother's education on several outcomes. Results are presented in Tables 5 and 6 where education is measured by the number of years of education and by attendance to secondary school respectively. In both tables, three estimates are shown: the OLS estimates (Panel A), the 2SLS estimates (Panel B) and the 2SLS estimates correcting for selection (Panel C). The results in Panel C come from our preferred specification that deals with all the estimation issues discussed above; this corresponds to the estimation of the final specification presented in section 4.4. However, Panel A and B help us to understand whether and how much our results change when correcting for the endogeneity of education and selection.

In Tables 5 and 6, the OLS estimates (Panel A) report a highly significant correlation between education and all child health outcomes, both for the mother and the father. More education of the mother and the father is associated with more prenatal care and better conditions of birth (i.e. a greater probability of having attended at least four prenatal visits, of being born in a health facility and having a birth assisted by medical staff). Father's education, and to a lower extent, mother's education, are also associated with a better nutritional status (a lower probability to be stunted and wasted). Finally, mother's education is associated with greater prevention behaviours (more injections of the vaccines, and a higher probability to sleep under net for any education measure), while father's education is mostly associated with more child prevention (except that the correlation between immunization and secondary school is not significantly different from zero).

However, when the endogeneity of education is taken into account (Panel B), the effect of mother's education is not significant anymore, whatever the measure of education and the child health outcome considered, the only exception being a small positive impact of mother's education (measured as attendance to secondary school) on the probability to sleep under net, only significant at the 10% level (Table 6). On the contrary, the 2SLS point estimates of the effect of father's education remain positive and statistically significant on the three prenatal care outcomes. For example (Table 5), any additional year of education of the father increases the probability to have at least 4 prenatal visits by 2.3 percentage points, the probability of being born in a health facility by 2.4 percentage points and having a birth assisted by medical staff by 2.2 percentage points. For children whose father has attended secondary school, these probabilities are increased by 15.7, 21.9 and 21 percentage points respectively (see Table 6). However, father's education has no more impact on nutritional status and prevention behaviours.

If we compare the size of the coefficients of father's education in Panel A (OLS coefficients) and Panel B (2SLS coefficients), in most cases, the 2SLS coefficient is more positive than the OLS coefficient, suggesting that the effect is larger if the education levels are randomly distributed in the population. Only when estimating the effect of the number of years of education on birth in health facility and birth assisted by medical staff (columns 2 and 3 of Table 5), the size of the coefficients does not differ across the two models. When measuring the effect of attending secondary school on these two outcomes (columns 2 and 3 of Table 6), the instrumented coefficient is larger and more positive than the naive coefficient but not significantly so. The difference between OLS and 2SLS coefficients is significantly different from zero in terms of education effect on having at least four prenatal visits, and this is true for both measures of education (column 1 of 5 and 6).

The estimated difference between OLS and 2SLS here is consistent with education being larger for individuals who are already more prone to take decisions that lead to healthy behaviors: Were education levels to be randomly allocated, the health gradient by education would be even larger.

Overall, our findings suggest that when endogeneity is controlled for, inequalities in child health are no longer due to differences in mother's education and father' education, except when we consider prenatal care and the conditions of birth. Indeed, father's education significantly improves these outcomes.

Note that, as already mentioned, we find a positive and very significant correlation between the residuals of both first-stage equations: unobserved factors explaining fathers and mothers education are strongly correlated (0.56 for the number of years of education, 0.41 for secondary-school attendance).

These conclusions are maintained when selection into co-residence is taken into account (Panel C, Tables 5 and 6). In these panels, two inverse Mills ratios are included in order to control part of the selection process, i.e. the part of the error term for which the decision of fathers and mothers to live with their child influences their child's health. The 2SLS point estimates are very close to those obtained in Panel B. Therefore, even when the inverse Mills ratios are significant, selection bias due to co-residence appears to be limited.

More precisely, our results show that the mother specific inverse Mills ratio is significant for most outcomes, meaning that unobserved factors that make mothers to live with their child tend to be associated with better conditions of birth, better nutritional status, and greater prevention through more vaccination. Inequalities in children health are therefore more likely to be due to these unobserved characteristics than to mothers' level of education. Father specific inverse Mills ratio, when significant, have the expected sign, meaning that fathers' unobserved characteristics that make them live with their child are associated with better child health.

Our findings suggest that father's education is the main determinant of prenatal care and conditions of birth. Moreover, unobserved characteristics of both the father and the mother, that make them choose to live with their child, once born, are also strong determinants of children health outcomes. However, if it exists, the selection bias that may arise from selection into co-residence is of a limited amount. Indeed, the 2SLS point estimates are very close when we correct (Panel C) or not (Panel B) for selection.

Table 5. The impact of mother's and father's education (years of education)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Prenatal and Birth			*Nutrition*		*Prevention*	
	At least 4 prenatal visits	Health Facility Birth	Birth assisted by medical staff	Stunted	Wasted	Number of injections received by child	Slept under net last night
Panel A – *OLS*							
Years of education^M	0.015***	0.039***	0.039***	–0.009***	–0.001	0.050***	0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.014)	(0.002)
Years of education^F	0.007***	0.021***	0.022***	–0.005**	–0.002*	0.024*	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.014)	(0.002)
N	6,189	7,791	7,831	7,082	7,035	7,804	5,718
Panel B – *2SLS*							
Years of education^M	–0.010	–0.005	–0.001	0.014	0.001	–0.015	0.015
	(0.011)	(0.011)	(0.012)	(0.011)	(0.006)	(0.065)	(0.010)
Years of education^F	0.023**	0.024**	0.022**	0.006	0.008	0.050	–0.005
	(0.010)	(0.010)	(0.010)	(0.010)	(0.005)	(0.064)	(0.009)
$\rho_{(\varepsilon^M),(\varepsilon^F)}$	0.559***	0.559***	0.559***	0.559***	0.559***	0.559***	0.558***
N	6,189	7,791	7,831	7,082	7,035	7,804	5,718
Panel C – *2SLS with correction for selection into coresidence*							
Years of education^M	–0.011	–0.006	–0.002	0.014	0.001	–0.024	0.014
	(0.011)	(0.012)	(0.012)	(0.011)	(0.006)	(0.064)	(0.010)
Years of education^F	0.023**	0.025**	0.023**	0.005	0.008	0.068	–0.006
	(0.010)	(0.010)	(0.010)	(0.010)	(0.005)	(0.064)	(0.009)
Inverse Mills Ratio^M	0.100	0.499***	0.506***	–0.311**	0.028	5.413***	–0.163
	(0.161)	(0.182)	(0.182)	(0.158)	(0.069)	(0.920)	(0.127)
Inverse Mills Ratio^F	0.079	0.250*	0.220*	–0.030	–0.100**	0.585	–0.297***
	(0.112)	(0.129)	(0.129)	(0.107)	(0.049)	(0.651)	(0.107)
$\rho_{(\varepsilon^M),(\varepsilon^F)}$	0.561***	0.561***	0.561***	0.561***	0.561***	0.561***	0.560***
N	6,189	7,791	7,831	7,082	7,035	7,804	5,718
Mean of dep.	0.71	0.68	0.68	0.32	0.06	5.71	0.12

Source: Authors' calculations from the Demographic and Health Surveys.

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects, as well as for child's sex and age. Panel C also controls for the two inverse Mills ratios obtained from the two selection equations. $\rho_{(\varepsilon^M),(\varepsilon^F)}$ denotes the correlation between the residuals of the two first-stage equations.

Table 6. The impact of mother's and father's education (attended secondary school)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Prenatal and Birth			*Nutrition*		*Prevention*	
	At least 4 prenatal visits	Health Facility Birth	Birth assisted by medical staff	Stunted	Wasted	Number of injections received by child	Slept under net last night
Panel A – *OLS*							
Attended secondary school^M	0.096***	0.237***	0.243***	−0.048***	−0.004	0.414***	0.042***
	(0.014)	(0.015)	(0.014)	(0.013)	(0.006)	(0.086)	(0.011)
Attended secondary school^F	0.035**	0.133***	0.137***	−0.030**	−0.013*	0.045	0.024**
	(0.014)	(0.015)	(0.015)	(0.013)	(0.007)	(0.089)	(0.011)
N	6,209	7,814	7,855	7,102	7,055	7,828	5,731
Panel B – *2SLS*							
Attended secondary school^M	−0.068	−0.019	0.013	0.110	0.017	−0.031	0.150*
	(0.094)	(0.095)	(0.097)	(0.091)	(0.048)	(0.547)	(0.087)
Attended secondary school^F	0.157***	0.219***	0.210***	0.007	0.039	0.341	0.000
	(0.057)	(0.059)	(0.061)	(0.055)	(0.029)	(0.371)	(0.050)
$\rho_{(\varepsilon^M),(\varepsilon^F)}$	0.404***	0.404***	0.404***	0.404***	0.404***	0.404***	0.402***
N	6,209	7,814	7,855	7,102	7,055	7,828	5,731
Panel C – *2SLS with correction for selection into co-residence*							
Attended secondary school^M	−0.070	−0.028	0.003	0.115	0.017	−0.117	0.141
	(0.095)	(0.096)	(0.098)	(0.092)	(0.048)	(0.543)	(0.088)
Attended secondary school^F	0.158***	0.227***	0.218***	0.003	0.039	0.433	−0.005
	(0.057)	(0.060)	(0.061)	(0.055)	(0.029)	(0.369)	(0.050)
Inverse Mills Ratio^M	0.115	0.529***	0.539***	−0.312**	0.025	5.426***	−0.175
	(0.161)	(0.180)	(0.179)	(0.157)	(0.069)	(0.924)	(0.127)
Inverse Mills Ratio^F	0.080	0.233*	0.202	−0.042	−0.110**	0.586	−0.314***
	(0.116)	(0.127)	(0.128)	(0.108)	(0.050)	(0.667)	(0.108)
$\rho_{(\varepsilon^M),(\varepsilon^F)}$	0.405***	0.405***	0.405***	0.405***	0.405***	0.405***	0.404***
N	6,209	7,814	7,855	7,102	7,055	7,828	5,731
Mean of dep.	0.71	0.68	0.68	0.32	0.06	5.71	0.12

Source: Authors' calculations from the Demographic and Health Surveys.

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects, as well as for child's sex and age. Panel C also controls for the two inverse Mills ratios obtained from the two selection equations. $\rho_{(\varepsilon^M),(\varepsilon^F)}$ denotes the correlation between the residuals of the two first-stage equations.

6. Discussion

6.1. Robustness Checks

We check the robustness of the causal effects of parental education to alternative specifications of the impact of the reform on educational outcomes. The first-stage estimates appear in Appendix Table C1, and we present the second-stage point estimates in Table 7 (controlling for selection into co-residence). We focus here on the effects of parental education on prenatal care and birth conditions as the causal effects of parental education on the other child health outcomes are not significantly different from zero.

First, we consider the case of the partially treated who are the parents born in 1965 and 1966. Robustness 1 excludes these observations from the sample as in Agüero and Bharadwaj (2014) and Grépin and Bharadwaj (2015) while Robustness 2 considers them as not exposed to the reform. In both cases, the point estimates are unchanged[22]: mother's education does not significantly affect prenatal care and birth outcomes while father's education significantly improves them. The size of the effect of father's education is stable: any additional year of education increases the probability of having four prenatal visits and the probability of birth assisted by two percentage points. Same conclusion is found for attending secondary school.

Second, we restrict the sample to children whose parents are born in years surrounding 1965. In the main specification, we do not restrict the sample based on the parents' years of birth, except that we dropped the children whose parents are born before 1950 to avoid survival bias. This implies that parents are born over a very large period of time.[23] Other reforms may have occurred during this large time period and may have affected their decisions regarding their children health. Restricting the sample to children of parents born around the pivotal year (1965) allows considering parents that are rather homogeneous (as in the RD approach) as they have faced a similar economic, social and political environment. We use different bandwidths in Robustness 3 and Robustness 4. Note that the bandwidths are 7 years larger for fathers than for mothers to take into account the age difference between the parents in our sample.

The more restricted bandwidth appears in Robustness 3 where the sample includes children whose mothers are born 5 years before to 5 years after the pivotal year, and whose fathers are born up to 12 years before and 12 years after 1965. The core results are robust for both bandwidths: the causal effect of father's education is still positive and significant and that of mothers not significant. The size of the effect of father's education is slightly larger for all outcomes and both measures of education. These restrictions reduce the number of observations so that precision is somewhat lower than in the main specification as the effect of the number of years of education is concerned (Table 7).

[22] First-stage estimates (see Appendix Table C1) are very close to those obtained in Table 3. For example, being exposed to the reform increases mothers' education by 2.985 years (versus 2.955 in the initial sample) and increases the probability to go to secondary school by 0.33 percentage points (versus 0.34 percentage points in the initial sample).

[23] Mothers (fathers) are born up to 31 (28) years after 1965.

Table 7. Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of education			Attended secondary school		
	At least 4 prenatal visits	Health Facility Birth	Birth assisted by medical staff	At least 4 prenatal visits	Health Facility Birth	Birth assisted by medical staff
Robustness 1 – *Without parents born in 1965 or 1966*						
Education^M	−0.011	−0.006	−0.003	−0.076	−0.049	−0.019
	(0.011)	(0.012)	(0.012)	(0.101)	(0.103)	(0.105)
Education^F	0.025**	0.024**	0.022**	0.165***	0.220***	0.208***
	(0.010)	(0.010)	(0.011)	(0.062)	(0.063)	(0.065)
N	5,689	7,173	7,206	5,707	7,194	7,228
Mean of dep.	0.71	0.68	0.67	0.71	0.68	0.67
Robustness 2 – *With 1965 and 1966 considered as not exposed*						
Education^M	−0.005	0.000	0.004	−0.025	0.001	0.041
	(0.011)	(0.012)	(0.012)	(0.110)	(0.116)	(0.118)
Education^F	0.024**	0.022**	0.019*	0.157**	0.204***	0.187***
	(0.011)	(0.011)	(0.011)	(0.066)	(0.066)	(0.067)
N	6,189	7,791	7,831	6,209	7,814	7,855
Mean of dep.	0.71	0.68	0.68	0.71	0.68	0.68
Robustness 3 – *Bandwidth mother=+-5 years* and *Bandwidth father=+-12 years*						
Education^M	−0.022	−0.013	−0.005	−0.134	−0.066	−0.008
	(0.019)	(0.020)	(0.019)	(0.133)	(0.136)	(0.134)
Education^F	0.038*	0.042**	0.041*	0.209*	0.330***	0.323***
	(0.021)	(0.021)	(0.021)	(0.116)	(0.119)	(0.119)
N	1,116	1,379	1,396	1,124	1,387	1,405
Mean of dep.	0.77	0.69	0.68	0.77	0.69	0.68
Robustness 4 – *Bandwidth mother=+-8 years* and *Bandwidth father=+-15 years*						
Education^M	−0.018	−0.008	−0.001	−0.135	−0.059	−0.000
	(0.014)	(0.014)	(0.014)	(0.104)	(0.106)	(0.107)
Education^F	0.044***	0.033**	0.030**	0.264***	0.281***	0.262***
	(0.014)	(0.014)	(0.015)	(0.074)	(0.077)	(0.079)
N	1,984	2,463	2,488	1,996	2,475	2,501
Mean of dep.	0.76	0.68	0.68	0.76	0.68	0.68

Source: Authors' calculations from the Demographic and Health Surveys.
Notes: 2SLS estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects, for child's sex and age, as well as for the two inverse Mills ratios obtained from the two selection equations, as in Panel C of Tables 5 and 6.

6.2. The Impact of Mother's Education Only

We complement our analysis by looking at the impact of mother's education on child-health outcomes, as this has been the focus of the literature on parental education and child health. As such, the role of the father in terms of his living with his children and his level of education is not taken into account. We estimate the effect of mother's education in the sample of children who live with both parents as in the core analysis.

We then estimate equations (5), (7) and (9) without including any information on the father. Tables 8 and 9 show the results from the OLS model in Panel A, the 2SLS model in Panel B and the 2SLS model that corrects for selection in co-residence in Panel C.

In the OLS specification in Panel A, the education coefficient has the expected sign, as more education is associated with better health: there are improved antenatal care, conditions of birth, improved child nutrition and increased preventive behaviours if the mother has attended secondary school (Table 9) and if she has a larger number of years of education (Table 8). All of the estimated coefficients are significant, except for that on wasting in Table 8.

Panel B reports the IV point estimates of mother's education on the same outcomes. The IV estimates are less significant. The effect of mother's education remains statistically significant on the probabilities of being born in a health facility, assisted by medical staff and sleeping under bed net. The same results are found in Panel C and these findings are consistent for both measures of education.

Regarding child nutrition outcomes, the findings in columns 4 and 5 in Panel A and B of Tables 8 and 9 can be compared to those in De Neve and Subramanian (2017). Our results are in line with theirs: the OLS estimates of the effect of maternal schooling on the probabilities of being stunted and wasted are negative and significant, while the IV estimates are insignificant.

These results along with those in Tables 5 and 6 suggest that if the child lives with both parents, not controlling for father's education overestimates the effect of mother's education, as the latter captures part of that of father's education (especially if they have similar education). This confirms related evidence from Fafchamps and Shilpi (2014) and Behrman and Rosenzweig (2002). Indeed, our results echo those from Behrman and Rosenzweig (2002) for child education, who no longer find an effect of mother's education on children's schooling when father's education and endogeneity bias are considered. They also find that in contrast, father's education has a significantly positive effect on the educational attainment of the next generation.

Table 8. The impact of mother's education (years of education)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Prenatal and Birth			*Nutrition*		*Prevention*	
	At least 4 prenatal visits	Health Facility Birth	Birth assisted by medical staff	Stunted	Wasted	Number of injections received by child	Slept under net last night
Panel A – *OLS*							
Years of education^{M}	0.020***	0.052***	0.053***	−0.012***	−0.002**	0.065***	0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.012)	(0.002)
N	6,189	7,791	7,831	7,082	7,035	7,804	5,718
Panel B – *2SLS*							
Years of education^{M}	0.007	0.024***	0.026***	0.007	0.002	0.029	0.013**
	(0.008)	(0.008)	(0.008)	(0.007)	(0.004)	(0.043)	(0.006)
N	6,189	7,791	7,831	7,082	7,035	7,804	5,718
Panel C – *2SLS with correction for selection into coresidence*							
Years of education^{M}	0.007	0.024***	0.026***	0.007	0.002	0.029	0.013**
	(0.008)	(0.008)	(0.008)	(0.007)	(0.004)	(0.042)	(0.006)
Inverse Mills Ratio^{M}	0.025	0.435**	0.436**	−0.289**	−0.014	5.329***	−0.251**
	(0.153)	(0.177)	(0.178)	(0.141)	(0.060)	(0.880)	(0.118)
N	6,189	7,791	7,831	7,082	7,035	7,804	5,718
Mean of dep.	0.72	0.68	0.68	0.32	0.06	5.71	0.12

Source: Authors' calculations from the Demographic and Health Surveys.

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects, as well as for child's sex and age. Panel C also controls for the mother-specific inverse Mills ratio obtained from the selection equation for child's co-residence with the mother.

Table 9. The impact of mother's education (attended secondary school)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Prenatal and Birth			*Nutrition*		*Prevention*	
	At least 4 prenatal visits	Health Facility Birth	Birth assisted by medical staff	Stunted	Wasted	Number of injections received by child	Slept under net last night
Panel A – *OLS*							
Attended secondary school^M	0.110***	0.291***	0.299***	−0.061***	−0.009	0.432***	0.051***
	(0.012)	(0.013)	(0.013)	(0.012)	(0.006)	(0.080)	(0.010)
N	6,209	7,814	7,855	7,102	7,055	7,828	5,731
Panel B – *2SLS*							
Attended secondary school^M	0.071	0.232***	0.256***	0.056	0.023	0.278	0.142**
	(0.065)	(0.066)	(0.067)	(0.063)	(0.033)	(0.367)	(0.057)
N	6,209	7,814	7,855	7,102	7,055	7,828	5,731
Panel C – *2SLS with correction for selection into co-residence*							
Attended secondary school^M	0.071	0.232***	0.256***	0.056	0.023	0.268	0.140**
	(0.065)	(0.066)	(0.067)	(0.063)	(0.033)	(0.362)	(0.057)
Inverse Mills Ratio^M	0.029	0.417**	0.421**	−0.288**	−0.020	5.324***	−0.274**
	(0.152)	(0.172)	(0.172)	(0.141)	(0.061)	(0.876)	(0.118)
N	6,209	7,814	7,855	7,102	7,055	7,828	5,731
Mean of dep.	0.72	0.68	0.68	0.32	0.06	5.71	0.12

Source: Authors' calculations from the Demographic and Health Surveys.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects, as well as for child's sex and age. Panel C also controls for the mother-specific inverse Mills ratio obtained from the selection equation for child's co-residence with the mother.

6.3. Mechanisms

Now we consider the potential mechanisms that may drive our results. Parental education may affect many outcomes, which themselves help determine child health: these include labour-market opportunities, wealth, access to health services, urbanisation, parental choice regarding family size.

We evaluate the role of these different mediators in the effect of parental education on child health by estimating the causal effect of father and mother's education on a sequence of outcome variables and replicating the models that appear in Panel B of Tables 5 and 6. Note that controlling for co-residence is less justified here because these outcome variables are not child-specific. They are observed at the mother level, the father level or at the household level.

Columns 1-4 in Table 10 consider the role of fertility preferences and attitudes. Both father and mother education levels have a significant causal effect on the ideal number of children reported by both mothers (column 1) and fathers (column 2).[24] Mother's age at first birth decreases with the

[24] Note that the sample is reduced in column 2 because observing the father's ideal number of children requires the father to be sampled to answer the male questionnaire.

number of years of education of both parents, and the attendance to secondary school. Parental education significantly influences the use of modern contraception at the time of the survey, in column 4. The effects of attending secondary school are of similar size for fathers and mothers: an increase by about 28 percentage points.

Columns 5-7 in Table 10 show that father's attending secondary school increases the probability of living in urban area by 13 percentage points and the probability that the household belongs to the richest quintile by 10 percentage points. This suggests that part of the effects of father's education on prenatal care and birth conditions come from the fact that the children are living in richer households who can afford these health services and in urban households who have a better access to health care services. Father's education reduces both types of barriers to access to health care: geographical and monetary.

Table 10. Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Fertility				*Household characteristics*		
	Mother's ideal number of children	Father's ideal number of children	Mother's age at first birth	Mother's use of modern contraception	Urban	Rich	Wealth Index
Panel A – *Years of education*							
Years of education^M	−0.402***	−0.145**	−0.287***	0.032***	−0.003	0.009	−0.021
	(0.045)	(0.071)	(0.077)	(0.010)	(0.009)	(0.009)	(0.019)
Years of education^F	−0.428***	−0.790***	−0.281***	0.045***	0.007	0.000	−0.021
	(0.045)	(0.080)	(0.066)	(0.011)	(0.009)	(0.010)	(0.019)
N	6,742	4,623	6,816	6,823	7,415	7,415	7,415
Mean of dep.	4.09	4.68	19.29	0.69	0.34	0.43	0.12
Panel B – *Attended secondary school*							
Attended secondary school^M	−3.388***	−1.476**	−2.371***	0.275***	−0.012	0.105	−0.095
	(0.378)	(0.617)	(0.664)	(0.088)	(0.073)	(0.078)	(0.165)
Attended secondary school^F	−2.732***	−4.887***	−0.909**	0.294***	0.130**	0.101*	0.107
	(0.260)	(0.454)	(0.381)	(0.061)	(0.052)	(0.057)	(0.109)
N	6,763	4,629	6,837	6,844	7,439	7,439	7,439
Mean of dep.	4.09	4.68	19.29	0.69	0.34	0.43	0.12

Source: Authors' calculations from the Demographic and Health Surveys.
Notes: 2SLS estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the enumeration area level are in parentheses. Each regression controls for survey year fixed effects.

A complementary approach is to add the mechanisms as additional control variables. We check the robustness of our results when controlling for household's urban status, material wealth and province of residence. As shown in Appendix Tables C2 to C4, conclusion remain unchanged once such covariates are controlled for, either separately (Panels A to C) or simultaneously (Panel D). Overall, these results suggest that father's education not only affects child health indirectly through better living conditions, but also has a direct positive effect on perinatal conditions.

7. Conclusion

Our main results regarding parental education and child health are as follows. Father's education consistently and significantly improves prenatal care and birth conditions. On the contrary, mother's education has no causal impact on these outcomes if father's education is controlled for in the equation. Moreover, once endogeneity is controlled for, child nutrition and prevention through vaccination and use of bed net, are not influenced by parental education. Last, unobserved characteristics of both the father and the mother, that make them live with their child are also strong determinants of children health outcomes. However, if it exists, the selection bias that may arise from selection into co-residence is found to be of a limited amount.

Overall, our results underline the predominance of father's education in determining child health. The model with mother's education only yields over-estimates of its impact on child health: mother's education matters less when father's education is controlled for. As such, the results in the existing literature without father's education may have overestimated the impact of mother's education. This comes about due to the assortative matching in our sample: men and women with similar observed education levels and similar intrinsic motivations or aspirations towards investment in human capital tend to live and have children together.

Last, our results show that education is a tool that can reduce inequalities in child-health outcomes, especially in terms of prenatal care and birth conditions. And as education has risen over recent decades, we may predict that hopefully child and maternal health will drastically increase in line, and that the considerable burden of disease and death borne by children and pregnant women will fall.

References

Adda, J., Bjorklund, A., and Holmlund, H. (2011). The Role of Mothers and Fathers in Providing Skills: Evidence from Parental Deaths. IZA DP No. 5425.

Agüero, J. and Bharadwaj, P. (2014). Do the More Educated Know More about Health? Evidence from Schooling and HIV Knowledge in Zimbabwe. *Economic Development and Cultural Change*, 62(3):489–517.

Albouy, V. and Lequien, L. (2009). Does compulsory education lower mortality? *Journal of Health Economics*, 28:155–168.

Alderman, H. and Headey, D. (2017). How Important is Parental Education for Child Nutrition? *World Development*, 94:448–464.

Apouey, B. and Geoffard, P.-Y. (2016). Parents' education and child body weight in France: The trajectory of the gradient in the early years. *Economics and Human Biology*, 20:70–89.

Azam, J.-P. and Djemai, E. (2019). Matching, Cooperation and HIV in the Couple. *Mimeo.*

Banerjee, A. and Duflo, E. (2011). *Poor Economics, A Radical Rethinking of the Way to Fight Global Poverty.* New York: Public Affairs.

Basu, A. (2002). Why does Education Lead to Lower Fertility? A Critical Review of Some of the Possibilities. *World Development*, 30(10):1779–1790.

Behrman, J. (2015). Does Schooling Affect Women's Desired Fertility? Evidence From Malawi, Uganda and Ethiopia. *Demography*, 52(3):787–809.

Behrman, J. R. and Rosenzweig, M. R. (2002). Does Increasing Women's Schooling Raise the Schooling of the Next Generation? *American Economic Review*, 92(1):323–334.

Bhalotra, S. and Clarke, D. (2014). Educational Attainment and Maternal Mortality. Background paper prepared for the Education for All Global Monitoring Report 2013/4.

Bhalotra, S. and Rawlings, S. (2011). Intergenerational Persistence in Health in Developing Countries: the Penalty of Gender Inequality? *Journal of Public Economics*, 95(3-4):286–299.

Breierova, L. and Duflo, E. (2004). The Impact of Education on Fertility and Child Mortality: do fathers really matter more than mothers? *NBER Working Paper*, 10513.

Carneiro, P., Meghir, C., and Parey, M. (2013). Maternal Education, Home Environments and the Development of Children and Adolescents. *Journal of the European Economic Association*, 11(S1):123–160.

Case, A. and Paxson, C. (2001). Mothers and others: who invests in children's health? *Journal of Health Economics*, 20:301–328.

Chiappori, P.-A., Iyigun, M., and Weiss, Y. (2009). Investment in Schooling and the Marriage Market. *American Economic Review*, 99(5):1689–1713.

Chicoine, L. (2021). Free Primary Education, Fertility, and Women's Access to the Labor Market: Evidence from Ethiopia. *The World Bank Economic Review*, 35(2):480–498.

Chou, S.-Y., Liu, J.-T., M., G., and T., J. (2010). Parental Education and Child Health: Evidence from a Natural Experiment in Taiwan. *American Economic Journal: Applied Economics*, 2(1):33–61.

Colclough, C., Löfstedt, J., Manduvi-Moyo, J., Maravanyika, O., and Ngwata, W. (1990). Education in Zimbabwe. Issues of Quality and Quantity. *Education Division Documents No.50, Swedish International Developement Activity.*

Croke, K., Grossman, G., Larreguy, H. A., and Marshall, J. (2016). Deliberate Disengagement: How Education Can Decrease Political Participation in Electoral Authoritarian Regimes. *American Political Science Review*, 110(3):579–600.

d'Albis, H., Augeraud-Veron, E., Djemai, E., and Ducrot, A. (2012). The Dispersion of Age Differences between Partners and the Asymptotic Dynamics of the HIV Epidemic. *Journal of Biological Dynamics*, 6(2):695–717.

De Neve, J.-W. and Subramanian, S. (2017). Causal Effect of Parental Schooling on Early Childhood Undernutrition - Quasi-Experimental Evidence From Zimbabwe. *American Journal of Epidemiology*, 187(1):82–93.

Delesalle, E. (2021). The effect of the Universal Primary Education program on consumption and on the employment sector: Evidence from Tanzania. *World Development*, 142:105345.

Dorsey, B. (1989). Educational Development and Reform in Zimbabwe. *Comparative Education Review*, 33(1):40–58.

Emran, S., Greene, W., and Shilpi, F. (2018). When Measure Matters: Coresidency, Truncation Bias, and Intergenerational Mobility in Developing Countries. *Journal of Human Resources*, April 19, 2017(0216-7737R1).

Fafchamps, M. and Shilpi, F. (2014). Education and Household Welfare. *Economic Development and Cultural Change*, 63(1):73–115.

Fitzsimons, E. and Mesnard, A. (2014). Can Conditional Cash Transfers compensate for a father's absence? *The World Bank Economic Review*, 28(3):467–491.

Grossman, M. (1972). On the concept of health capital and the demand for health. *Journal of Political Economy*, 80:223–255.

Grépin, K. and Bharadwaj, P. (2015). Maternal education and child mortality in Zimbabwe. *Journal of Health Economics*, 44:97–117.

Güneş, P. M. (2015). The role of maternal education in child health: Evidence from a compulsory schooling law. *Economics of Education Review*, 47:1–16.

Kanyongo, G. (2005). Zimbabwe's Public Education System Reforms: Successes and Challenges. *International Education Journal*, 6(1):65–74.

Kemptner, D., Jurges, H., and Reinhold, S. (2011). Changes in compulsory schooling and the causal effect of education on health: Evidence from Germany. *Journal of Health Economics*, 30:340–354.

Lindeboom, M., Llena-Nozal, A., and van der Klaauw B. (2009). Parental education and child health: Evidence from a schooling reform. *Journal of Health Economics*, 28:109–131.

Lleras-Muney, A. (2005). The Relationship Between Education and Adult Mortality in the U.S. *Review of Economic Studies*, 72(1):189–221.

Makate, M. and Makate, C. (2018). Educated Mothers, Well-Fed and Healthy Children? Assessing the Impact of the 1980 School Reform on Dietary Diversity and Nutrition Outcomes of Zimbabwean Children. *The Journal of Development Studies*, 54(7):1196–1216.

Masuda, K. and Yamauchi, C. (2020). How Does Female Education Reduce Adolescent Pregnancy and Improve Child Health?: Evidence from Uganda's Universal Primary Education for Fully Treated Cohorts. *The Journal of Development Studies*, 56(1):63–86.

Müller, T. and Shaikh, M. (2018). Your retirement and my health behavior: Evidence on retirement externalities from a fuzzy regression discontinuity design. *Journal of Health Economics*, 57:45 – 59.

Osili, U. and Long, B. (2008). Does female schooling reduce fertility? Evidence from Nigeria. *Journal of Development Economics*, 87:57–75.

Oyelere, R. (2010). Africa's education enigma? The Nigerian story. *Journal of Development Economics*, 91:128–139.

Pilon, M. and Vignikin, K. (2006). *Ménages et familles en Afrique subsaharienne*. Editions des archives contemporaines, Paris.

Shrestha, V. and Shrestha, R. (2020). Multigenerational Effects of Education Reform: Mother's Education and Children's Human Capital in Nepal. *The World Bank Economic Review*, 34(3):698–729.

Silles, M. (2009). The causal effect of education on health: Evidence from the United Kingdom. *Economics of Education Review*, 28:122–128.

Stancanelli, E. and Van Soest, A. (2012). Retirement and Home Production: A Regression Discontinuity Approach. *American Economic Review*, 102(3):600–605.

Van Bavel, J. and Klesment, M. (2017). Educational Pairing, Motherhood and Women's Relative Earnings in Europe. *Demography*, 54:2331–2349.

Wooldridge, J. (2010). *Econometric Analysis of Cross Section and Panel Data*, chapter 19, pages 777–851. The MIT Press, 2nd edition.

World Health Organization (2020). *Global Health Estimates 2019: Deaths by Cause, Age, Sex, by Country and by Region, 2000-2019*. Geneva.

Zenebe Gebre, T. (2020). Free Primary Education, Timing of Fertility, and Total Fertility. *The World Bank Economic Review*, 34(3):730–748.