The Intergenerational Transmission of Schooling among the Education-Rationed*

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Abstract

We estimate the intergenerational transmission of schooling in a country where the majority of the population was rationed in its access to education. By eliminating apartheid-style policies against blacks, the 1980 education reform in Zimbabwe swiftly tripled the progression rate to secondary schools. Using a fuzzy regression discontinuity design, we find a robust intergenerational transmission. Several smoothness and placebo tests further validate our design. We show that both marriage and labor markets are key pathways in the schooling transmissions.

Keywords: Schooling, intergenerational effects, Zimbabwe.

JEL codes: I21, J13, J24.

1 Introduction

Worldwide, the schooling attainments of children are positively correlated with the schooling attainments of their parents.[1] This intergenerational correlation is interesting for two different, but related reasons. First, it sheds light on intergenerational mobility.[2] For example, higher intergenerational schooling correlations may suggest lower mobility. Second, it evokes the debate over nature and nurture in child development. Here, it is important to distinguish intergenerational correlations from the *causal effects* of parental schooling on child schooling. Causal effects provide insight into the role of parental nurture in the production function of child human capital (Haveman and Wolfe 1995, Holmlund, Lindahl, and Plug 2011). They are a vital input into the design of policies for child development and intergenerational mobility.

Only a few papers in the literature on intergenerational schooling transmissions has been able to identify these effects.[3] Nearly all of these papers focus on developed countries, and derive identification using one of three different approaches.[4] One approach compares twins;

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[1]See Björklund and Salvanes (2011) for examples from high-income economies and Beegle, Christiaensen, Dabalen, and Gaddis (2016) as well as Ferreira et al (2012) for developing countries.

[2]As Black and Devereux (2011) point out, education allows mobility to be measured at an earlier moment in the life-cycle because most people tend to end their education in their early twenties. Unlike earnings, education is measured with lower error, and can be observed for both the unemployed and those not in the labor force.

[3]See a review by Haveman and Wolfe (1995) and, more recently, by Black and Devereux (2011) and Björklund and Salvanes (2011).

[4]There is also a growing literature, summarized by Grossman (2015), on the effects of parental schooling on child health that includes research on developing countries. While

examples include Behrman and Rosenzweig (2002) and Amin and Behrman (2014) for the US, Pronzato (2012) for Norway and Amin, Lundborg, and Rooth (2015) for Sweden. Another approach uses natural experiments arising from adoption; examples include Sacerdote (2002, 2007) and Plug (2004) who exploit the quasi-randomness in adoption placements in the United States, and Björklund, Lindahl, and Plug (2006) for Sweden. A notable exception to the focus on developed countries is de Walque (2009), who uses family recomposition in the aftermath of the Rwandan genocide to generate an adoptee-adopter sample; however, non-random orphan assignments complicates the interpretation of his finding as causal.[5] A third approach identifies causal effects from changes in schooling laws in the developed world, including the US (Oreopoulos, Page, and Stevens 2006), the UK (Chevalier, Harmon, O'Sullivan, and Walker 2013), Norway (Black, Devereux, and Salvanes 2005) and Germany (Piopiunik 2014). Despite the diversity of empirical methods and causal estimates, our understanding of the mechanisms underlying intergenerational transmissions is incomplete, even for developed countries.

Our paper contributes to this literature by estimating the causal intergenerational transmission of schooling using an education reform in a developing country and by exploring several mechanisms. We focus on Zimbabwe, a fragile, low-income country in southern Africa, where the majority black population was severely rationed in its access to education. We observe a major turning point in 1980 when the post-independence reform introduced automatic progression to secondary school for *all* students, a feature reserved hitherto for whites. Before the reform, black students had to complete primary school (grades 1 through

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important, and related to the effects of parental schooling on child schooling, we do not investigate this intergenerational transmission in our paper. There is another related literature that examines the intergenerational transmission of assets and poverty dynamics, see for example Fafchamps and Quisumbing (2005).

[5]See also Zimmerman (2003) for a related study about fostered children in South Africa.

7), pass a test and hope for a seat in the limited number of secondary schools available to them. Automatic progression eliminated the school-rationing feature of the apartheid era, and brought a swift and *discontinuous* change in the transition rate to secondary school. As Figure 1 shows, the transition rate climbed from 27 percent of the cohort graduating primary school in 1979 to 86 percent of the cohort graduating in 1980.

The timing of the reform provides us a source of exogenous variation in the schooling attainment of blacks, and hence, a fuzzy regression discontinuity design. Our results show positive and significant intergenerational spillovers among black Zimbabweans. One extra year of schooling for the mother is associated with 0.073 additional years for her child and 0.092 for the father-child transmission. However, these estimates are not statistically different from each other. Our findings are robust to alternative specifications applicable in a fuzzy regression discontinuity design, and to controls for potential confounds, such as rainfall shocks at birth. A number of smoothness and placebo tests failed to detect discontinuities and reinforces the validity of our methodology.

As in all papers that use an education reform to identify causal effects, our estimation strategy provides a Local Average Treatment Effect (LATE) of the parameter measuring the intergenerational transmission of schooling. That is, the causal effect is estimated from people whose behavior is influenced by the policy change. However, our paper differs from the literature using compulsory schooling laws in three important ways. First, Zimbabwe's rule of automatic progression to secondary school creates a different and, arguably, larger set of compliers. With compulsory schooling laws, the set of compliers is characterized by those who would drop out in the absence of the laws, but must stay in school under the new regime. The law does not change the behavior of those who already wanted to remain in school. Under Zimbabwe's reform, described in the next section, the set of compliers is formed by those who wanted to stay in school but couldn't due to the apartheid-style regime. Second, the "treatment" with compulsory laws is the addition of an extra year of secondary education (or high school). In Zimbabwe, the "treatment" is gaining entrance to secondary school. Third, Oreopoulos (2006) argues that most compulsory laws like the ones implemented in the United States, "typically affect fewer than 10 percent of the population exposed to the instrument" (p. 153). Zimbabwe's reform affected a much larger share of its population. When given the chance to advance to secondary school, 86 percent of the eligible students changed their behavior, more than tripling the transition rate of the previous year. This implies that our LATE is closer to an average treatment effect (ATE) as the share of non-takers in the reform is quite small. Thus, our LATE is highly relevant to developing countries and to the Sustainable Development Goal of removing barriers to secondary education – the "bottleneck" of many education systems (UNESCO 2011).

Our work is also related to a new set of papers that have attempted to estimate intergenerational *associations* in developing countries emphasizing the external validity of their analyses. For instance, Schady et al. (2015) estimate wealth gradients in five Latin American economies (Chile, Ecuador, Colombia, Nicaragua and Peru) and focus on outcomes related to early childhood cognitive development. Behrman et al. (forthcoming) explore a more geographically diverse set of countries from the Young Lives Project (Ethiopia, India, Peru, and Vietnam) and seek to estimate the association between parental resources (income and schooling) and a wide set of human capital outcomes of the next generation. A detailed exploration of the causal estimates of the intergenerational transmission of schooling sets us apart from these studies, and represents an important contribution of our paper, especially for developing countries.

Furthermore, the use of a population census allows us to explore the father-to-child transmission and not just maternal effects. This lets us avoid a major limitation imposed by the use of women-centric surveys such as the Demographic and Health Surveys (DHS) for developing countries. Thus, while two other papers have used the reform to test for the impact of education on health outcomes using the Zimbabwe DHS (Agüero and Bharadwaj 2014, Grépin and Bharadwaj 2015), our paper is the first to exploit the natural experiment inherent in the reforms to estimate impacts on the schooling of women *and* men. This widens the external validity of our findings. It also allows us to investigate the indirect pathways of the schooling spillovers, such as the marriage and labor markets. We find that more-schooled women delayed childbearing and had fewer children, implying a quantity-quality trade off.

In that regard, we further contribute to the literature on intergenerational spillovers by investigating several mechanisms for the transmission of schooling. We find that more-schooled women delayed childbearing and had fewer children, implying a quantity-quality trade off. While additional schooling did not lead to higher labor force participation rates or a greater likelihood of paid work, we find that it did induce a change in occupation (for fathers and mothers). We also find a high degree of positive assortative mating: more-educated adults married among themselves.[6] Uncovering the marriage market mechanics of the intergenerational spillover is an important contribution in its own right since it holds particular implications for design and evaluation of the long-term impact of policies trying to break the intergenerational transmission of poverty in developing countries.

This paper is organized in six sections including this introduction. Section 2 discusses the education reform. The data and methodology are described in sections 3 and 4, followed by results in section 5. Section 6 concludes and discusses the policy implications of our findings.

2 Post-Independence Schooling Reform

In southern Rhodesia – as pre-Independence Zimbabwe was known – blacks where heavily rationed in their access to schools and education opportunities.[7] For instance, in 1976, for

[6]As discussed later in more detail, this assortative mating is not mechanical. Zimbabweans do not tend to marry within their age cohort; rather men marry women, who are on average, six years younger. Other patterns in partner age gap suggest a deliberate behavior of assortative mating.

[7]For a history of the apartheid-era education system and the policies dictating the quantity and quality of schooling permitted to Africans, see Atkinson (1972) and O'Callaghan

every 1,000 black school-aged children, 250 never went to school, 337 completed primary school, 60 enrolled in Form I, the eighth grade and first year of secondary school, and fewer than three finished high school (Riddell 1980). By contrast, whites obtained universal primary enrollment, and near-universal transition to Form I. Racial disparities continued in secondary school: in 1975, as many as 3,000 white students were enrolled in secondary classes leading to university entrance, compared to only 790 black students (Chidzero 1977). New construction of black secondary schools was heavily restricted. Between 1961 and 1972, only one new public secondary school was built to accommodate nearly eight thousand new black students nationwide (Zvobgo 1981).

Elections in April 1980 brought the Republic of Zimbabwe into existence with Robert Mugabe as Prime Minister. His party had campaigned with the goal of "establishing free and compulsory primary and secondary education for all Zimbabwean children regardless of their race, sex or class." (Nhundu 1992, p.78). The ensuing reforms, documented by Edwards (1995), Edwards and Tisdell (1990) and Dorsey (1989), implemented four initiatives: (1) the introduction of free and compulsory primary education; (2) the removal of age restrictions to allow over-age children to enter school; (3) building community support for education and; (4) automatic promotion from primary to secondary school, i.e. from grade 7 to Form I. It is the last feature of the reform that we consider in our analysis. The automatic promotion rule made it illegal to deny Form I admission to any student graduating from primary school on the grounds of poor test results or classroom seating constraints. As a result, the reform year saw an unprecedented fraction of black primary school graduates entering secondary school. Figure 1 shows yearly transition rates to secondary school. In 1979, the last year before the reform, this rate was 27 percent; in 1980, the first reform year, it jumped to 86 percent. In absolute numbers, Form I enrollment soared from 22,201 to 83,491. Throughout the eighties, transition rates remained high, averaging about 75 percent.

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and Austin (1977).

Figure 2 shows the rise in secondary enrollment from under 100,000 in 1979 to over 650,000 in 1989. Over the same period, secondary schools grew ten-fold on the strength of an expanding education budget. Up to a fifth of the national budget was allocated to the education sector between 1980 and 1985, the bulk of which was used to open new secondary schools, especially in rural areas (Dorsey 1989).

Education in Zimbabwe is structured as a 7 + 4 + 2 system: seven grades of primary, followed by four forms of secondary, capped by two years of high school. Children had to be at least seven years old to enter primary school,[8] which implies that the first cohort of students who could have taken advantage of the automatic transition rule were black Zimbabweans finishing grade 7 in 1980. These students would have been fourteen to fifteen years old, and *disproportionately* more likely to benefit from the reform compared to their slightly older counterparts. Our inferred point of discontinuity in black schooling has been validated in the Zimbabwe Demographic and Health Survey (ZDHS) data by Agüero and Bharadwaj (2014), Fenske (2015) and Grépin and Bharadwaj (2015). In the next section, we show that the discontinuity is also observed in our dataset, the 2002 population census.

3 Data

We use a 10 percent random sample of the 2002 Zimbabwe Population Census. Because we are interested in linking the schooling attainments of the school-age generation at the time of the reforms to the schooling levels of the children of that generation, we begin by selecting the sample based on two rules: adult black Zimbabweans, comprising individuals born between 1959 and 1974, and their children in the ages of 6 to 15 years in 2002. This means that the schooling levels in the child sample are intermediate outcomes.[9] We divide

[8]The entry age was lowered to 6 years in 1989.

[9]In this regard, our study differs also from those estimating the intergenerational associations for adult children. See for example, Beegle, Christiaensen, Dabalen, and Gaddis (2016)

the sample along gender lines, and drop the handful of observations missing schooling data.[10] Additionally, we restrict the female sample to women who gave at least one live birth and report age at first birth. The census allows us to identify the parent of a child if the adult in question is either the household head or the head's spouse, so we derive two analysis samples: 91,480 mother-child matches and 50,026 father-child matches.[11]

Table 1 reports descriptive statistics for our two analysis samples. The average black mother is thirty-five years old and has eight years of schooling. Fathers are slightly older and have slightly more schooling. The average child is about 10 years old, and daughters and sons occur in equal proportion in both samples. Ninety-seven percent of children are currently attending school, and 100 percent of them have *ever* attended school. Thus, the critical outcome for us is not whether the child attends school; rather, it is whether she attends a grade appropriate to her age. For example, in the census, while 98 percent of eleven year-olds are enrolled in school, the average eleven-year-old black student is already a full year behind in terms of grade-for-age. For simplicity and comparison with the rest of the literature, we choose the child's current grade attainment as our outcome of interest and

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and Behrman, Gaviria, and Székely (2001).

[10]We also drop polygamous households. This is a minor restriction because polygamy occurs in only two per cent of the households in our sample, and a recent paper finds no schooling effect on polygamy in Zimbabwe (Fenske 2015). Furthermore, we find that the schooling outcomes of children excluded from our analysis, because they are not the offspring of household heads, are very similar to the schooling outcomes of the children in our analysis. This information is available upon request.

[11]As shown in Appendix Table A3, it is common in Sub-Saharan African countries to be able to match more mothers to children than fathers to children. Indeed, South Africa and Botswana have a similar sample size ratio of mother-child matches to father-child matches, as Zimbabwe.

we employ fixed effects for child age to absorb variations in grade attainment related purely to the child's time-in-school.[12] In the next section, we describe our identification strategy to obtain causal estimates of the intergenerational transmission of schooling among black Zimbabweans.

4 Identification Strategy

The intergenerational schooling relationship may be captured by the equation[13]

$$y_i = \alpha_1 + \beta s_i + X_i'\gamma + \epsilon_i \tag{1}$$

where y_i is the schooling outcome of child i and s_i is the schooling attainment of her parent. X_i' is a vector of child and parental characteristics (e.g. age and gender) and some location indicators.[14] The parameter of interest capturing the intergenerational spillover is β. How-

[12]Our results are robust to differences in recall precision, so long as recall error is zero-mean for educated and less educated parents.

[13]See Black and Devereux (2011) for alternative ways to measure intergenerational mobility and Holmlund, Lindahl, and Plug (2011) for the specific case of schooling.

[14]In our models, we do not control for the spouse's education. As Holmlund, Lindahl, and Plug (2011) explain, including the schooling of the spouse complicates the interpretation of the intergenerational effects. There is also a statistical difficulty in our case because we estimate the intergenerational coefficients as instrumental variable estimates. In the presence of assortative mating, spouse schooling levels and reform exposures will tend to be strongly correlated. With highly-correlated instruments, we are in danger of ending up with very imprecise 2SLS estimates of the intergenerational effects. A partial workaround is suggested by Oreopoulos, Page, and Stevens (2006) who use the sum of maternal and paternal schooling as the endogenous regressor of interest. Their approach overcomes the

ever, omitted unobserved variables in ϵ_i correlated with parental schooling would bias the least-squares estimate of this parameter. To minimize this possibility, we seek a source of exogenous variation in parental schooling.

As detailed in section 2, the reform made the transition from grade 7 to Form I automatic. Although the reform did not go so far as to impose an age cut-off on its potential beneficiaries, the timing induced very different probabilities of secondary school enrollment among black Zimbabweans. Specifically, those who were younger than fifteen years of age in 1980 were *disproportionately* more likely to achieve the primary-to-secondary transition than those who were slightly older. The probabilities diverge at age fifteen because this was the typical transition age to Form I at the time; furthermore, transitioning to this level was the natural next step for those who had not been rationed out of a seat in grade 7 before 1980 (Dorsey 1989, Nhundu 1992).

The reform-induced jump in secondary school enrollment probability creates a fuzzy regression discontinuity design. This provides an instrumental variable for parental schooling s_i in the point of discontinuity, $\overline{A}$. We can now estimate intergenerational schooling relationship via 2SLS with the following equations:

$$y_i = \alpha_1 + \beta\, E[s_i|A_i] + X_i'\gamma + \epsilon_i \tag{2}$$

$$E[s_i|A_i] = \alpha_2 + X_i'\delta + f(A_i) \tag{3}$$

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multicollinearity problem at the cost of having less informative intergenerational estimates. This trade-off is worthwhile if the relative sizes of maternal and paternal effects are not particularly relevant; however, our paper seeks to estimate these effects separately and then test for the presence of assortative mating.

where $f(\cdot)$ takes the form

$$f(A_i) = \sum_{k=1}^{K} \phi_{1k}\, {A_i}^k + \sum_{k=1}^{K} \phi_{2k}\, (\overline{A} - A_i)^k\, 1\{A_i \leq \overline{A}\}$$

Following van der Klaauw (2002), we choose a piece-wise linear representation for $f(\cdot)$ with $\overline{A} = 15$. However, as shown later, our results are unaffected by approximations where $K > 1$ in $f(\cdot)$.

Based on Hahn, Todd, and Van der Klaauw (2001), a fuzzy regression discontinuity design implies that a consistent estimation of β by 2SLS requires two assumptions. First, the reform needs to have discontinuously altered schooling levels of the target population at the threshold to avoid a weak-instrument problem. Second, the reform needs to have affected children's schooling only through the schooling attainments of their parents.

We formally test the first assumption in section 5, but Figure 3 supplies visual support. Following Imbens and Lemieux (2008), and consistent with the increase in schooling attainments over time in African countries,[15] we plot the conditional expectation function (CEF) of parental schooling de-trended in age, i.e. we display the residuals from a regression of parental schooling on a linear polynomial in the running variable (parent age in 1980) and an interaction between the threshold and polynomial term. In Figures 3a and 3b, we observe a clear discontinuity in completed years of schooling around the threshold for black mothers and black fathers, respectively.[16]

The second assumption, that the discontinuity satisfies the exclusion restriction, is not

[15]Negative correlations between schooling and age have been noted before in cross-sectional data. Using household data from South Africa, Nigeria, Côte d'Ivoire, Kenya, Burkina Faso and Ghana, Schultz (2004) describes the progress in female attainment over time as slow but continuous. By contrast, Zimbabwe's reform delivered universal primary education and literacy in a span of just eleven years.

[16]The unconditional functions, without de-trending, show similar patterns and have been

directly testable; however, we provide several ways to validate this assumption. First, we consider several "smoothness of covariates" tests. We start by showing, in Appendix Figures A1a and A1b, that the histograms of parent age are smooth around the point of discontinuity. Furthermore, despite the fact that the census does not provide a large set of variables that could be considered predetermined with respect to the reform, in Figures A2, A3 and A4, respectively, we consider the adult sample's race composition, the sex composition of children in the mother and father samples and parental height, which is largely determined by early life factors (obtained from the 2010 Zimbabwe DHS). These figures are all quite smooth around the threshold age of fifteen. Second, as a placebo test, we explore whether the discontinuity observed in Zimbabwe at age 15 in 1980, is also present in other Sub-Saharan African countries where no major reforms took place in 1980, much less any that affected the same age cohorts as in Zimbabwe. This investigation is shown in Figures A5 and A6 for women and men, respectively. The data were taken from IPUMS (`https://international.ipums.org/international/`); we looked for countries with a population census circa 2002 so the placebo samples could contain individuals who were contemporaries of the parents in our Zimbabwe sample.[17] As expected, the graphs for these countries show a smooth relationship of schooling with the running variable at the point of discontinuity.[18]

Third, we explore if Zimbabwe undertook any other policies, pre- or post-Independence

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used elsewhere (Agüero and Bharadwaj 2014, Grépin and Bharadwaj 2015, Fenske 2015).

[17]The eleven countries in our placebo are Botswana, Ghana, Malawi, Mali, Rwanda, Senegal, Sierra Leone, South Africa, Uganda, Tanzania and Zambia. See Table A2 for details about the census years.

[18]In principle, one could consider the behavior of white Zimbabweans as an alternative placebo test. However, the white samples are very small ($N < 250$), and it is not clear that their response to the reform in Zimbabwe was necessarily unique. Thus, we do not use them for our placebo tests. We thank a referee for this insight.

that may have directly affected the schooling of the children via parent wealth or parent outcomes in the labor market. Because we use a fuzzy regression discontinuity design, our identification strategy is threatened only if there is another policy affecting those who were fifteen in 1980 *discontinuously* compared to those who were sixteen in 1980. Our identification is not threatened by policies that targeted or affected the black population according to age unless those policies had discontinuous impacts on 15 year-olds and 16 year-olds in 1980. We found no such policy related to the labor market. However, a land redistribution program, a major post-Independence policy initiative, could have affected the nature of rural labor markets (see Oryoie, Alwang, and Tideman 2017, Deininger, Hoogeveen, and Kinsey 2004, Kinsey 2004). In the early 1980s, Mugabe's government conceived the land redistribution program as a form of redress for rural impoverishment under apartheid. The program's resettlement activities peaked for a brief time in the mid-1980s but implementation fizzled soon after (Kinsey 2004). Clear eligibility criteria for resettlement were not developed until the program was re-evaluated in the following decade, and there is no indication that the treated or control cohorts used in our paper were targeted for resettlement in any systematic way.[19] We discovered no other reforms at the national or provincial level that could have affected the intergenerational schooling relationship and at the same time those aged 15 in 1980 vis-á-vis those aged 16. This is not surprising given how fragile Zimbabwe was politically and economically as it emerged from apartheid. The state was in no position to finance additional large programs at the same time that it launched the very popular and ambitious reforms of school expansion and land resettlement. In the next section, we present the regression counterparts of these graphical analyses and consider additional robustness tests for the intergenerational spillovers in schooling.

[19]A similar pattern is followed by health campaigns, including family planning ones. Boohene and Dow Jr (1987) show that this policy had no impact until the second half of the 1980s and even then it was very small and not related to our age groups.

5 Results

5.1 First stage: Impact of the reform on parental schooling

Table 2 reports estimates of the first-stage, with standard errors clustered on the discrete assignment variable, age in 1980 (Lee and Card 2008). In Appendix Table A4, we consider six other clustering options and find that inference is similar across clustering protocols.[20]

In Panel A of Table 2, column (1) shows that at the discontinuity, black mothers have 0.819 more years of schooling compared to their slightly older counterparts. In Panel B, column (1) shows that black fathers have 0.683 more years of schooling at the threshold. Both estimates are significantly different from zero, and the F statistics attest to a strong first-stage. Compared to the program estimates in Duflo's (2001) seminal study of Indonesia, where an additional primary school per 1,000 children delivered an additional 0.12 to 0.19 years of schooling, the impact of Zimbabwe's reform is at least three times as large. In column (2), we control for background parental characteristics using rainfall in parent year of birth; we find that this does not change the estimated reform effect in either panel (we return to this issue in the next subsection).

In column (3), we run the same specification as column (1) in a pooled sample of eleven Sub-Saharan African countries. Limiting the sample to natives of these countries in the ages of six through twenty-one years in 1980, we test for a discontinuity in years of schooling at age 15 in 1980 for women and men, separately. Consistent with the visual evidence presented earlier, we find no discontinuity in either sample. The estimates (0.154 for women and 0.109

[20]The variations we consider include no clustering, clustering by parent's age in 1980 (i.e. the parent's year of birth), clustering at parent province of birth, clustering at parent district of birth, two-way clustering on age and province, and wild-cluster bootstrap-t procedures with age and province clusters, respectively. The bootstrap procedure with 16 age clusters also incorporates finite-cluster corrections. See Appendix Table A4 for more details.

for men) are smaller but more precise than the estimates for Zimbabwe (each regression sample has more than 1.5 million observations); positive but not statistically different from zero. This conclusion is reinforced by the country-by-country results shown in appendix Table A2 and the scatterplots discussed earlier. These placebo tests bolster the validity of our identification strategy.

Table 3 reports smoothness tests on covariates. As columns (1) to (3) show, predetermined variables like the child's sex, the parent's height (in centimeters) and the parent's height-for-age z-score are not discontinuous around the age threshold. In the women's sample under column (4), the coefficient on the discontinuity is significant at the one percent level but it is tiny: it implies that at age fifteen, the probability that a woman is black increases by 0.1 percentage points from a base of 99 percent. This coefficient is statistically zero in the sample of men.

In sum, we base our empirical strategy on the evidence that the education reform *discontinuously* affected cohorts that were close in age, viz. those aged 15 or less in 1980 compared to those aged 16 or more. While independence brought much economic change as well as political and social reform to Zimbabwe – as in many other African countries – our identifying assumption is that these changes did not *discontinuously* affect the cohorts around the cutoff age of 15 years in 1980. Indeed, on the strength of previous research and the new evidence presented here, we conclude that there is strong evidence of a fuzzy regression discontinuity in the schooling of black Zimbabweans, and that it provides an exogenous source of variation in the schooling attainments of the parent generation in our sample.

5.2 Estimates of the intergeneration spillover

We now present our estimates for the intergenerational transmission of schooling in the sample of black Zimbabweans. We start by showing the reduced-form effects graphically. Figure 4 plots the conditional expectation function of child grade attainment against parent age in the two samples of mothers and fathers respectively. For mothers, we observe a clear jump at the discontinuity (Figure 4a). For fathers, the discontinuity is less pronounced (Figure 4b) but in both cases, the reduced form estimates show a difference at the threshold that is statistically different from zero and as discussed below, robust to several specifications.[21] Table 4 complements these figures with the regression estimates of the intergenerational transmission of schooling. In column (1), we show OLS estimates. For both mothers (Panel A) and fathers (Panel B), we find a positive and statistically significant intergenerational association. Because these estimates could be biased by the omission of unobservable covariates of parental schooling, such as ability, we present in column (2), 2SLS estimates using the fuzzy discontinuity design. We find that a one-year increase in the schooling of the mother is associated with 0.073 years increase in the schooling of her child. For fathers, the effect is 0.092 additional years of child schooling. For both samples, the estimates are statistically different from zero at the one percent level and we cannot reject the null hypothesis that the mother and father effects are equal to each other. The 2SLS estimates are smaller than the OLS, which suggests that unobserved covariates like ability account for the gap between the two.

To put our 2SLS estimates in perspective, we estimated intergenerational associations in schooling in each of the eleven Sub-Saharan African countries we invoked in the placebo tests discussed before.[22] Limiting the placebo samples to native children aged 6–15 with parents in the ages of six through twenty-one in 1980, we obtained estimates for mothers in the range of 0.068 to 0.288, and for fathers in the range of 0.073 to 0.215 (see Appendix

[21]For mothers the reduced-form estimate is 0.061 (s.e.=0.018) and 0.062 (s.e.=0.011) for fathers.

[22]These countries provide a better comparison to Zimbabwe than Scandinavia (Holmlund, Lindahl, and Plug 2011) or Latin America (Behrman, Gaviria, and Székely 2001) where the level of economic development, and quality of institutions differ markedly from those in African countries.

Table A3 for details.) Therefore, our Zimbabwe estimates are consistent with schooling spillovers for other countries in the region. This is also consistent with the evidence presented by Beegle, Christiaensen, Dabalen, and Gaddis (2016) who show that Africa has greater intergenerational educational mobility than Latin America.

5.3 Robustness checks

We conduct a number of robustness and specification checks as shown in Table 5 and Table 6. Table 5 reproduces our 2SLS estimate of the intergenerational spillover in column (1). In column (2), we check if the original estimate is biased by the omission of parental background characteristics correlated with the timing of the reforms. We know from Almond, Currie, and Herrmann (2012) and Currie and Vogl (2013) that early-life shocks often have long-lasting health effects. In a predominantly agrarian society like Zimbabwe, at-birth exposure to drought or flood could slow or permanently reduce human capital accumulation.[23] Using a forty-year time series, Richardson (2007) finds that the growth rate of Zimbabwe's per capita GDP is strongly correlated with annual rainfall. From Figure 5, which displays standardized rainfall data[24] for the years 1959-1985, it is clear that rainfall is *smooth* in the neighborhood of 1965, the birth-year of fifteen year-olds in 1980. This explains why adding rainfall to the first-stage regression did not change the reform's effect on parental schooling (see column (2) of Table 2). For the same reasons, adding rainfall to the 2SLS estimation does not alter

[23]For example, Maccini and Yang (2009) find that Indonesian women born in years of plentiful rain were taller, completed more grades of schooling and lived in better homes. Alderman, Hoddinott, and Kinsey (2006) report that early-life exposure to drought in Zimbabwe was associated with delay in enrollment, lower schooling attainment and poorer health, whileHoddinott (2006) finds negative impacts on household assets.

[24]The rainfall data come from 38 stations across Zimbabwe. We standardize annual rainfall using the mean and standard deviation of precipitation over the period, 1959-2001.

our main findings (see column (2) of Table 5).

In column (3) of Table 5, we deal with province-level unobservables potentially confounding our 2SLS estimates by adding province of birth fixed-effects to our original specification. We find that the intergenerational estimates remain highly significant, but become marginally smaller. We find similar results when we control for parents born in an urban area (column 4). In column (5), we omit units at the threshold (i.e. parents aged fifteen in 1980) and re-run the original 2SLS specification. Once again, the estimated effects (0.066 in the mothers sample, and 0.079 in the fathers sample) are statistically different from zero and belong within the confidence interval of the baseline 2SLS. In all our robustness checks, we continue to find a very strong first-stage based on the F-statistics, and p-values.

We also investigate the sensitivity of our estimates to four alternate specifications of the running variable and for two age spans (6-21 and 0-30 in 1980). The specifications differ according to the degree of the polynomial in parent age, and whether ages 14 *and* 15 in 1980 are included in or omitted from the estimation sample. In Table 6, column (1) displays the baseline 2SLS estimates for ease of comparison; columns (2) and (4) use quadratic polynomials in parent age with the difference that (4) omits parents at or near the threshold from the estimation. The implied point estimates for mothers and fathers are not statistically different from the baseline estimate. This is also the case in column (3), where we omit 14 and 15 year-olds in 1980 while retaining a linear polynomial in parent's age. When we expand our estimation sample to include parents aged 0 through 30 years in 1980, we find that the estimates are more sensitive but continue to show a statistically significant intergenerational transmission of schooling for fathers and mothers. Overall, these sensitivity checks suggest that our baseline specification is conservative in the sense that alternative specifications yield larger estimates of the intergenerational schooling effect.[25]

[25]This conclusion is reinforced by additional tests in Appendix Table A5. Here, we start by comparing those aged 14–15 against those aged 16–17 in 1980, and then increase the age interval smoothly. We find that the 2SLS estimates grow larger as we narrow the sample

5.4 Heterogeneous effects

We now examine heterogeneity in the intergenerational spillovers. Table 7 shows the effects on daughters and sons separately. In all four parent-child pairings, we find a strong and statistically significant transmission of schooling at the one percent level. While the mother's effect is larger for daughters than for sons, and the opposite holds for fathers, we cannot reject the null hypothesis that the mother (and father) effects are the same for daughters and sons. This suggests that there are no systematic gender preferences in the schooling transmission.

Figure 6 explores whether the transmission varies with the child's age. The top panel shows that mothers clearly raise child grade attainment when children are in the ages of 7–10 and again, for children in the ages of 12–14. Likewise, the bottom panel shows that fathers affect children positively when children are between 9 and 12 years of age. Until children enter teenage years, the father's schooling (point) estimate outweighs the mother's effect. Yet, this does not imply that mothers have a negligible effect on children's progress through school; it is clear that their intergenerational spillovers are highly significant, and that their impact grows relative to fathers at a critical period in children's lives. Thus, these differences suggest a "complementarity" in the timing of the schooling effects: they are consistent with a model where the marriage market plays an important role in shaping the intergenerational spillover. In the next section, we provide evidence in favor of this mechanism as we uncover significant positive assortative mating on schooling attainment by parents.

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around the discontinuity. Because the number of clusters shrinks greatly as we narrow the sample, the estimates from the smallest age interval warrant caution. In light of this, our baseline estimate is a conservative estimate of the intergenerational spillover.

5.5 Additional child outcomes

In Table 8, we explore the intergenerational spillovers on three additional child outcomes. In column (1) we look at the effect of parental schooling on whether the child is currently attending school. We find that this margin is not affected by parental schooling. Together with the fact that nearly all children have ever attended school, we infer no selection based on parental schooling. So, having a more educated parent affects the grade attainment of a child but not his/her school enrollment. In columns (2) and (3), we investigate the intergenerational schooling effect on child labor.[26] In column (2), we define child labor as market work participation; the binary outcome is one if the main activity of a child in the last twelve months was one of paid market work, own-account work, unpaid family work, or unemployed as opposed to student, homemaker or other. In column (3), we widen the definition to encompass domestic chores; thus, homemaker children are also regarded as child labor. We see no effect of parental schooling on either measure of the prevalence of child labor.[27]

5.6 Possible mechanisms

A key advantage of our paper is the ability to explore in the census data some of the pathways through which intergenerational spillovers in schooling operate. Towards this end, we replace child's schooling in Equation (2) with a new set of outcome variables. Figures 7 to 10 provide a visual analysis of these pathways for mothers and fathers.

[26]Like most questionnaires, Zimbabwe's census collects main activity data only on persons ten years or older. Thus, our child labor results are based on a subset of the main analysis samples; specifically, the parent-child pairs where children are in the ages of 10-15.

[27]Edmonds (2007) argues that the relevant margin for child labor might be the number of hours worked. However, this is not possible to measure in most population censuses, including Zimbabwe's.

We start by examining if schooling changes household formation by altering the marital status of parents or their likelihood of living together. Many papers in the literature give considerable weight to this mechanism including, for instance, by Behrman (2010) and Fafchamps and Shilpi (2014). Furthermore, in the context of Norway, Kalil, Mogstad, Rege, and Votruba (forthcoming) show that a father's presence alters the magnitude of the intergenerational transmission. In Table 9, column (1), we find that schooling does not alter the marital status of mothers (Panel A) but it does make fathers more likely to be married (Panel B). Exploring if educated parents are more likely to live with their partners, measured by co-residency on the night before the census, we do not find an effect for mothers or for fathers (column 2). This contrasts with the literature from developed countries but echoes Fenske (2015) who finds no effect of schooling on polygamy in Zimbabwe using the ZDHS. Thus, schooling does not seem to causally affect the probability of entering the marriage market.

We now consider the quality of the match in the marriage market. This aspect has been discussed as a possible mechanism for developed economies (e.g., Charles, Hurst, and Killewald 2013, Edwards and Roff 2016). For lower-income countries, Foster (2002) develops a model of the marriage market in which prospective mates seek a match to maximize own private consumption, as well as the human capital of their future child in the marriage, given their incomes and tastes for schooling. Foster finds that marital selection accounts for a significant portion of the *cross-sectional* correlation between parental and child schooling. We extend his finding with causal estimates for this mechanism in Zimbabwe. In Table 9, columns (3) and (4) examine how a parent's own schooling relates to the schooling of the co-resident partner. The dependent variables are the partner's completed years of schooling (column 3) and an indicator that the partner's attainment is at the secondary schooling level or higher (column 4). We find strong evidence of positive assortative mating. In Panel A, an additional year of maternal schooling is associated with 0.56 more years in her partner's education, and a 7.2 percentage-point (or 13 percent) higher probability that her partner is educated beyond primary school. In Panel B, we find that a well-educated father tends to have a well-educated wife: an additional year in his schooling is associated with 0.49 more years for his partner's, and again, a 13 percent (=0.079/0.6) greater probability that his partner's educational attainment is beyond primary school. All of these estimates are statistically significant at the one percent level.

These findings are not mechanical. If Zimbabweans married within the same age cohort, assortative mating would simply be a mechanical consequence of the reform. In that scenario, finding that educated parents are more likely to have educated spouses would be uninformative. However, Zimbabweans do not marry within the same age cohort. As shown in Figures 7e and 9e, mothers have partners who are, on an average, seven years older. Most importantly, there is no discontinuity in that age gap at the threshold (column (5) in Table 9). For fathers, their spouses are six years younger on an average, and again, there is no discontinuity. We can therefore reject the possibility that parents married within the same age cohort. Educated parents, however, do not deviate from the "social norm" of women (men) marrying people older (younger) than themselves, just as they their less educated counterparts do. But within the pool of possible partners (that belong to a different age cohort), educated parents seem to have chosen more educated partners. This result is arguably behavioral, not mechanical.

Using only the sample of mothers, we examine how, if at all, schooling affected fertility. In policy circles and academic writing, fertility is regarded as a key driver of the impact of maternal schooling on child welfare (Summers 1992, Behrman 1997, Schultz 2007). The last two columns of Table 9 provide evidence in support of this view. In column (6), we find that an additional year of schooling increases women's age at first birth by 0.56 years, a three percent increase from the mean. In column (7), we show that each extra year of education reduces the number of children women bore by four percent. Thus, schooling caused women to postpone childbearing, and to make a quantity-quality trade-off by having fewer but more-educated children.[28]

The intergenerational transmission of education could also be explained via income effects in the labor market. Unfortunately, neither the Population Census used here nor the Demographic and Health Surveys contain information on earnings, which would have allowed us to test for an education premium in the Zimbabwean labor markets.[29] However, the census allows us to estimate the effects of schooling at the extensive margin, such as labor force participation and the status of being a paid worker. Paid employment is an important labor market outcome, especially in the context of Sub-Saharan Africa (Vijverberg 1992). Yet, in our analysis (Table 10), neither mothers nor fathers show any schooling effects on their workforce participation (column 1) or probability of being a paid worker (column 2). This is not very surprising because for the fathers in our sample, participation in the labor force and in paid labor is near-universal (98 percent and 96 percent, respectively), leaving a very small margin to be affected by schooling. The result is not a surprise for mothers either. It is well-documented that women in low-income countries, such as Zimbabwe, are more likely to be employed relative to their counterparts in middle-income countries (Mammen and Paxson 2000). In our sample, 75 percent of mothers are in the labor force and 95 percent of working mothers are paid for their labor.

We do find an effect, however, on the type of occupation where the educated are employed. As columns (3) and (4) of Table 10 show, both educated fathers and mothers are less

[28]The difference in half a year in the delayed age at first birth suggest that we can rule out the possibility that children of educated parents are overrepresented in the younger ages. It is unlikely that our results have a composition bias.

[29]The lack of labor force surveys and longitudinal household data in Zimbabwe is also discussed in Montenegro and Patrinos (2014). Among the 819 surveys from 139 countries used in their paper, there are no surveys from Zimbabwe to estimate Mincerian returns to schooling.

likely to be in primary sector employment or manual labor (e.g., farming, hunting, fishing, logging, mining, quarrying, brick-laying, masonry, painting, cleaning or subsistence work in general) and more likely to be in jobs demanding higher skills (e.g., programmers, academics, accountants, lawyers, doctors, engineers, artists, executives, librarians, bankers among others). It is this margin of the labor market that matters for the intergenerational transmission of education.

5.7 Quantity and quality of education

When describing the 1980 education reform in Zimbabwe, Kanyongo (2005) notes that "[t]he emphasis [of the reform] was not so much on quality and cost effectiveness of the education system, but on accessibility to education". This narrow focus helped Zimbabwe achieve universal primary enrollment as early as 1990, and the highest adult literacy rate in sub-Saharan Africa (UNICEF 2011). Yet, as several descriptive studies report, the quality of education in Zimbabwe declined even as the number of schools increased. For instance, Edwards (1995) calls 1984 the last year with "good" quality outcomes, at least in primary education. Nhundu (1992), reviewing documents from the Ministry of Education on the decline in quality after 1984, observes that school enrollment in the 1980s occurred "faster than classrooms and teacher's houses could be built" (p.87). According to Dorsey (1989) and Nhundu (1992), growth in teacher-staff failed to keep pace with enrollment. The share of untrained teachers in secondary schools rose from near-zero in 1980 to 28 percent in 1988. Because a significant number of secondary schools had been built as extensions of existing primary schools, schools resorted to "hot seating", a practice of reducing the length of a school day to accommodate more students. Nhundu (1992) further reports that between 1981 and 1988, the number of students taking exams to gain admission to higher levels of secondary education (O-level exams) grew by 2,253 percent and the failure rate increased by 7,220 percent.

Though it is clear that the quality of education declined over the 1980s, we lack the data to determine if the decline occurred discontinuously but it is rather likely. However, as a phenomenon, decline in quality is not necessarily unique to Zimbabwe. In several low- and middle-income countries, the rapid expansion of education has been marked by serious concerns about a decline in quality (Grisay and Mahlck 1991, World Bank 2018). Even developed countries enacting minimum compulsory schooling laws could have experience problems with education quality. Unfortunately, the literature has not evaluated such impacts. For Zimbabwe, our 2SLS estimates should be regarded as lower-bounds to the "true" effects that could have been obtained if quality was held constant. Understanding whether and how much these reforms impact on the quality of education is an important item for future research.

6 Conclusion

Though the literature on intergenerational transmissions of schooling has grown in recent years, developing countries remain understudied. Our paper presents causal estimates of the schooling transmission in the context of a low-income country and a population that was systematically deprived of access to education for several generations. In particular, the scale of Zimbabwe's natural experiment, facilitating the transition to secondary school, bestows a large degree of external validity on our estimates, as the set of compliers (those whose behavior is affected by the reform) was large. Exploiting the regression discontinuity design spawned by the 1980 reforms, we estimate an intergenerational spillover for children between 6 and 15 of approximately 0.07 years of schooling from the average mother, and 0.09 years of schooling from the average father.

An important contribution of our paper is the exploration of a number of mechanisms to explain the intergenerational spillover. We find that both labor and marriage markets are important channels in the transmission of parental schooling. We find that more educated couples exhibit assortative mating on schooling, delayed childbearing and quantity-quality trade-offs in family size, which suggests that they reap larger marriage surpluses than less educated couples. However, in a dynamic setting, assortative mating reduces mobility. Thus, an important question for future research concerns the long-term implications of higher schooling transmissions and assortative mating for intergenerational mobility.

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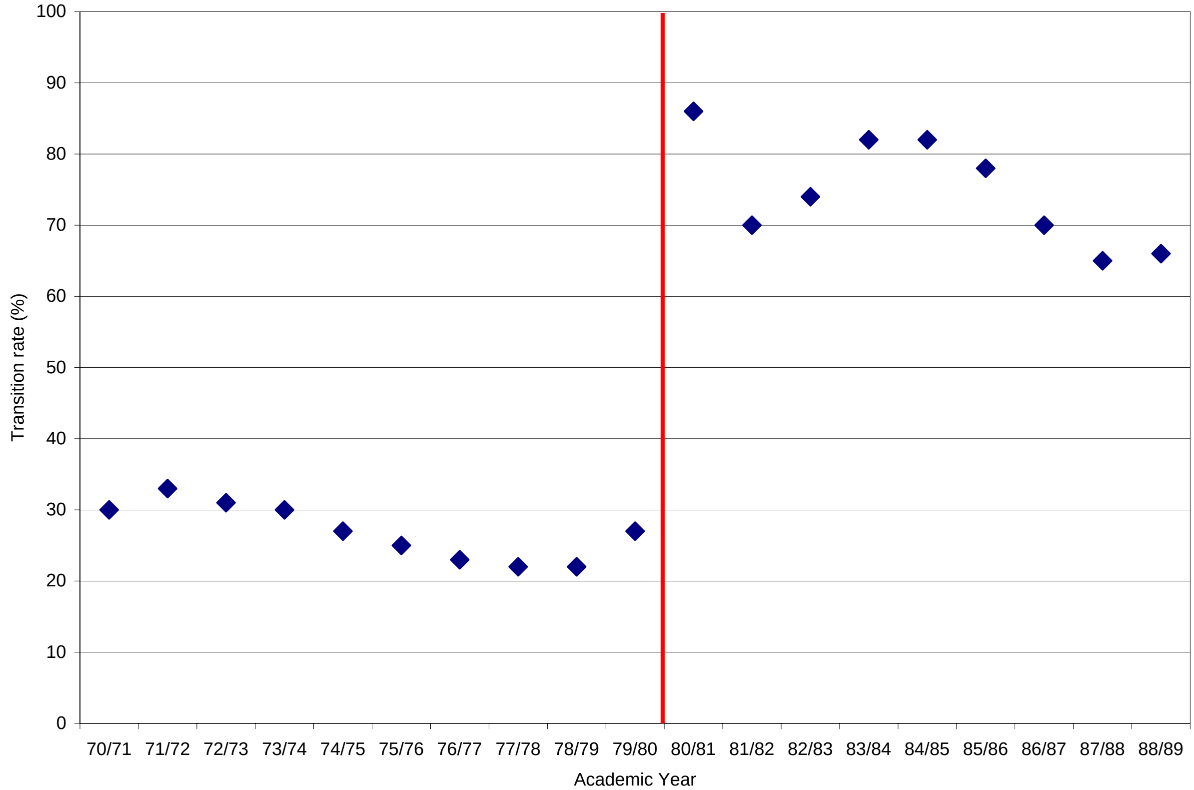


Figure 1: PRIMARY TO SECONDARY TRANSITION RATES: 1970/71-1988/89

Note: the transition rate is the percentage of students, who after graduating from the seventh grade (primary school), enroll in the eighth grade (Form I, secondary school).
Data source: Riddell and Nyagura (1991), Table 1.1.

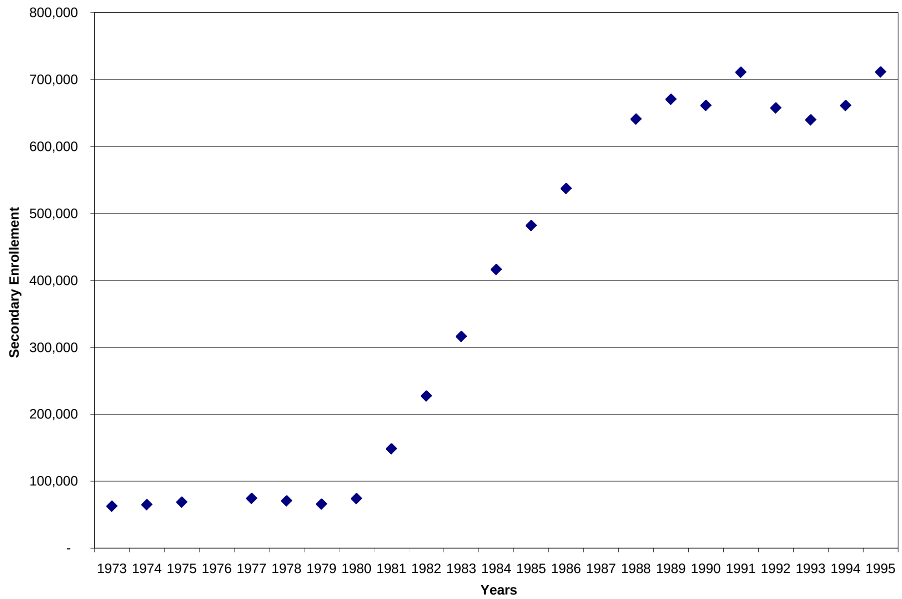


Figure 2: Annual secondary school enrollment in Zimbabwe: 1973-1995

Data source: United Nations, *Statistical Yearbook*, 1975, 1980, 1982, 1984, 1985-1989, 1992, 1994, 1995 and 1997.

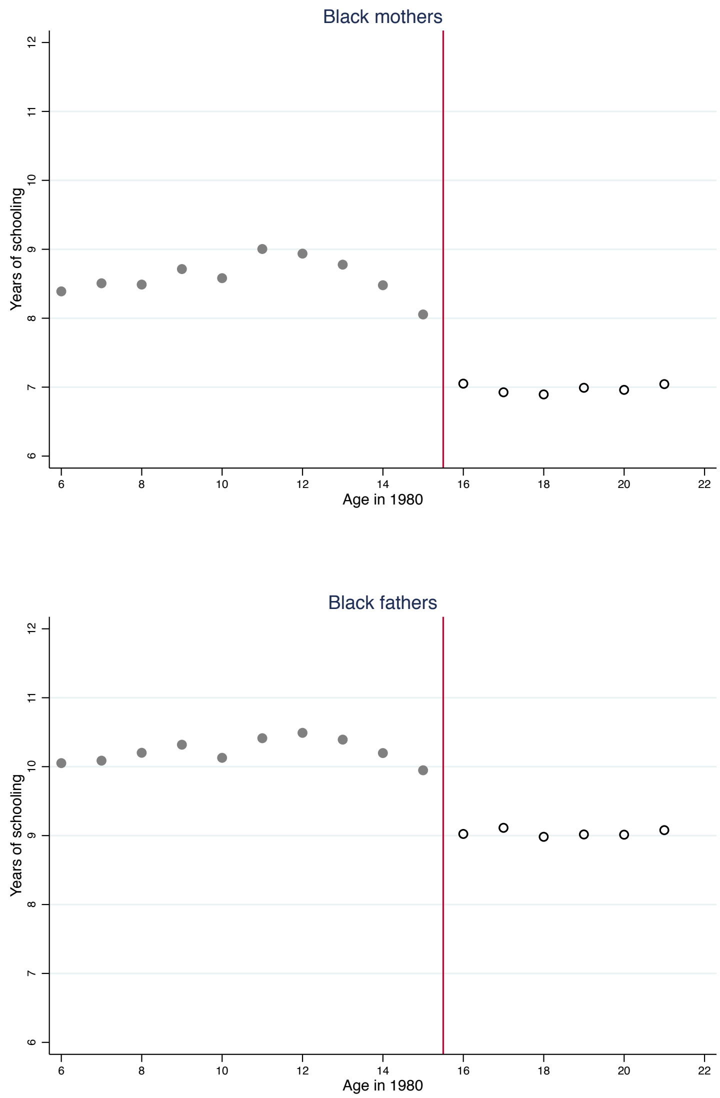
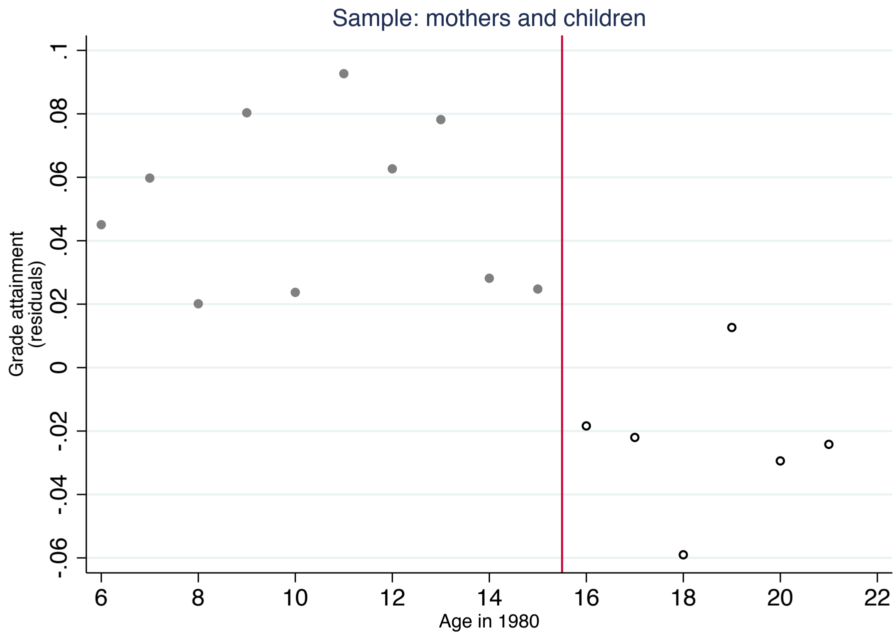
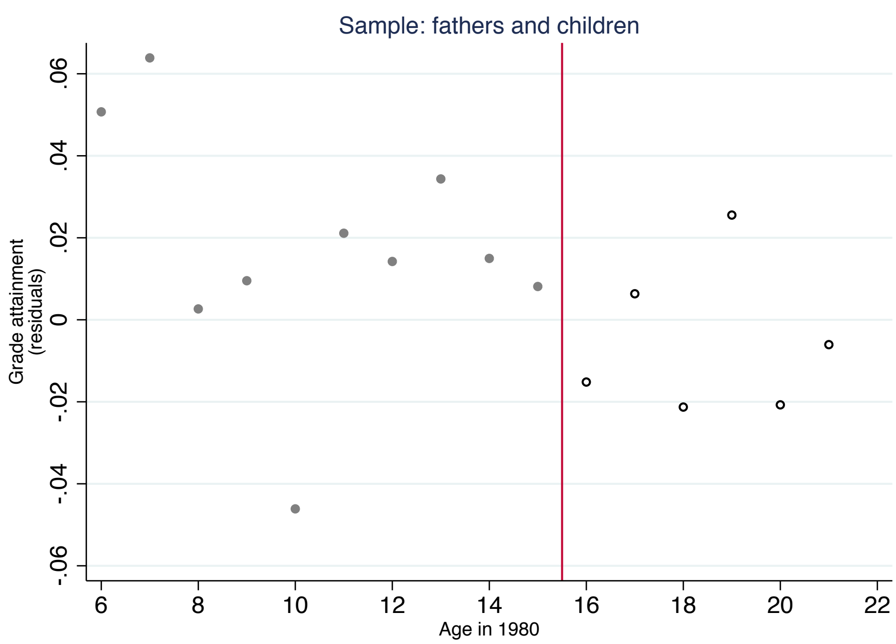


Figure 3: Schooling attainment by age in 1980

Note: figures plot mean years of schooling against age in 1980 for black mothers and black fathers respectively. Black parents are in the ages of six through twenty-one years in 1980. The vertical line (at 15.5 on the age axis) marks the treatment threshold under the reform.



(a)



(b)

Figure 4: Mean child grade attainment by parent age

Note: in each figure, circles mark mean child grade attainment by parent age in 1980. Because children in both samples are still in school, grade attainment data was purged of time-in-school variation by running a piecewise linear regression with fixed effects for child age and extracting the residuals. These graphs capture the mean of these residuals at each value of parent age.

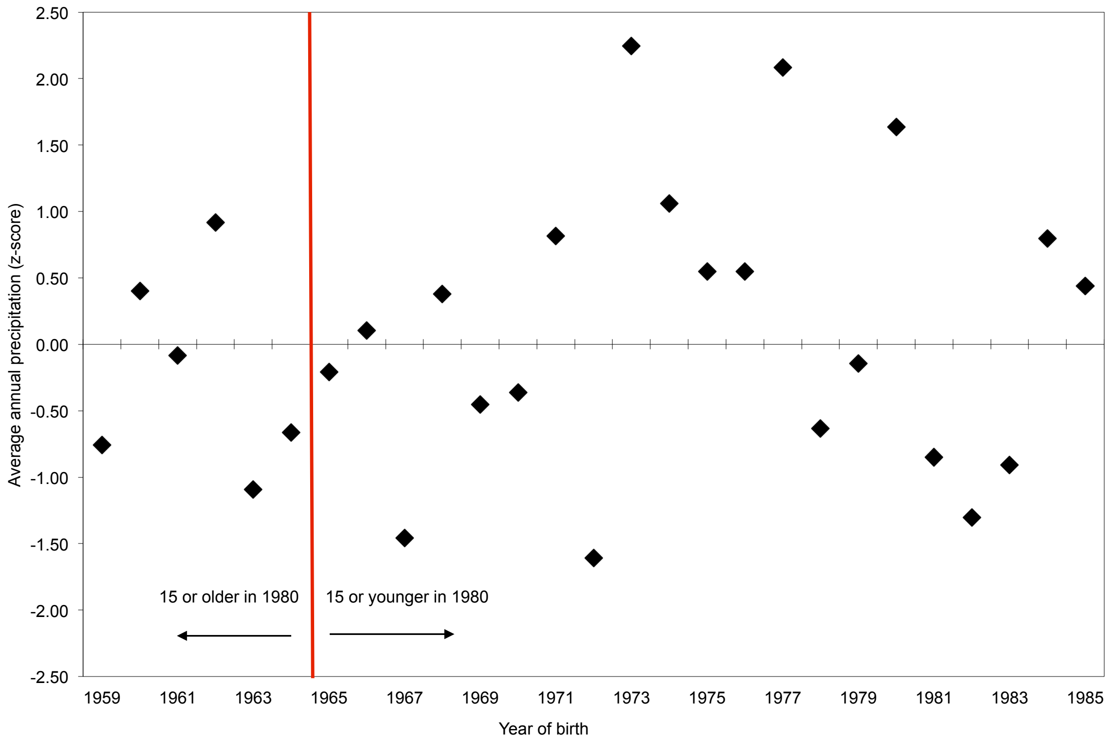


Figure 5: Annual rainfall: 1959-1985

Note: time series data on annual rainfall comes from a sample of 38 stations across Zimbabwe for the years 1959-2001; above, we standardize and plot this data for the period 1959-1985. A given year, such as 1970, refers to the 1970-1971 crop-year. The vertical line represents the year of birth for the cohort aged 15 in 1980.
Data source: Zimbabwe Meteorological Service Department.

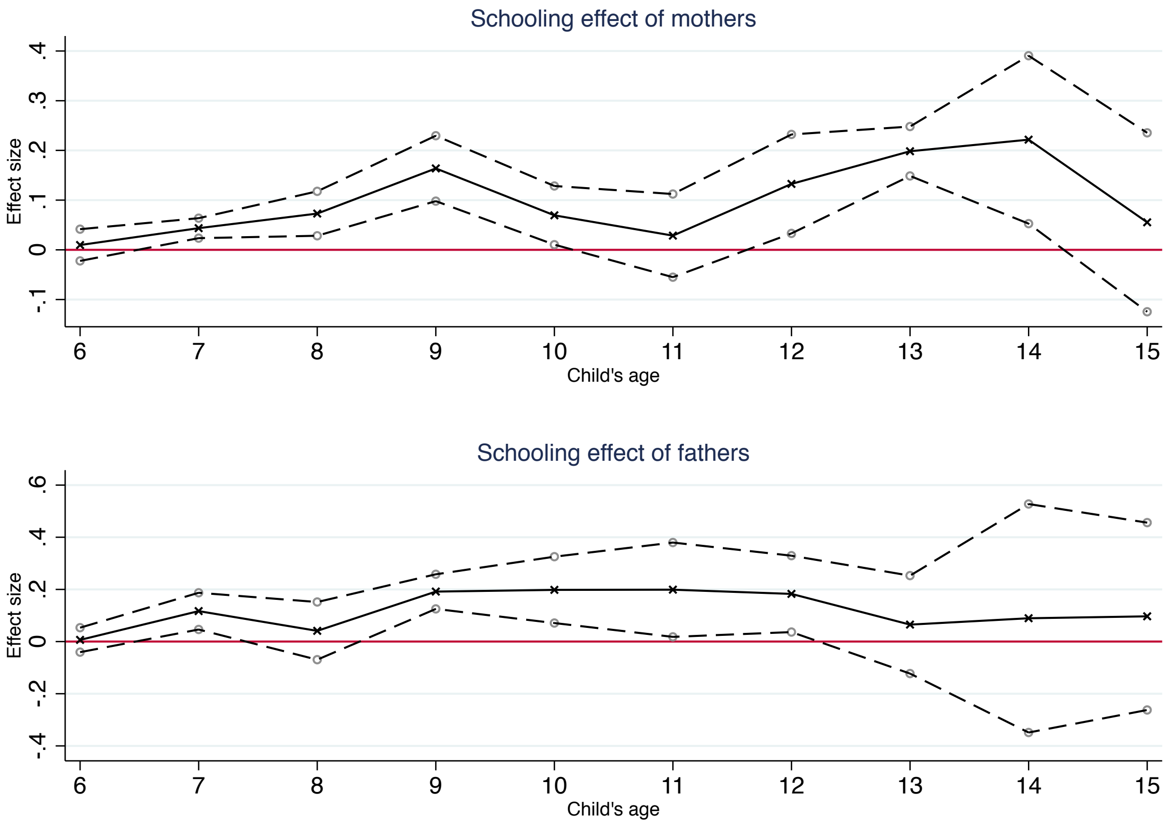
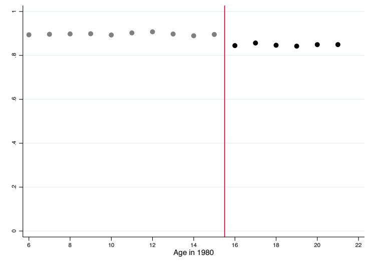
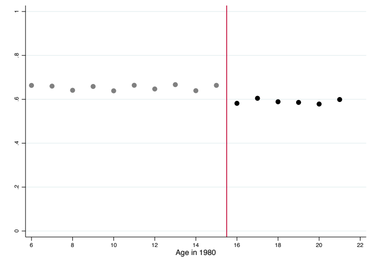


Figure 6: Heterogeneity in the effects of parental schooling

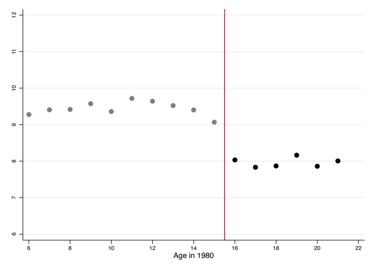
Note: in each figure, the solid line connects the 2SLS estimates of the effect of parent schooling on child schooling at child ages six through fifteen years. The dashed lines represent the 95% robust confidence bounds clustered by parent year of birth. Each 2SLS regression controls for the child's sex, uses linear splines in parent age in 1980 on both sides of the discontinuity and instruments parent schooling with the discontinuity at age 15 in 1980.



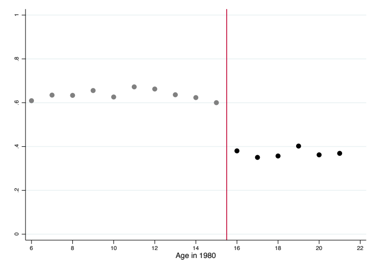
(a) Is married



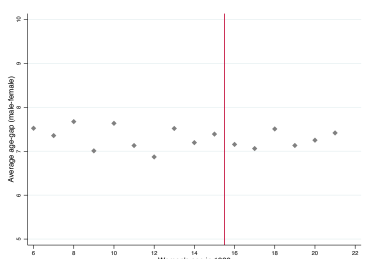
(b) Partner co-resides



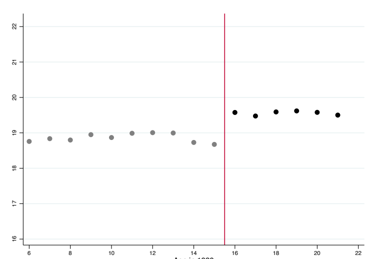
(c) Partner's years of schooling



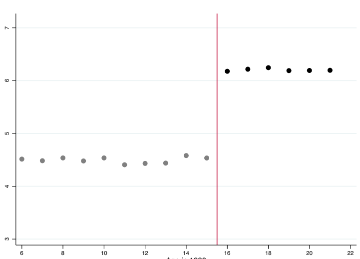
(d) Partner has secondary+ schooling



(e) Age gap with partner



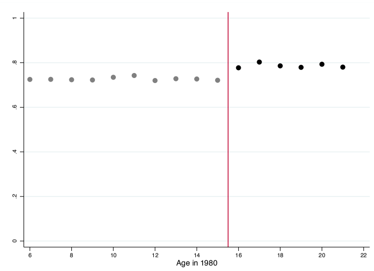
(f) Age at first birth



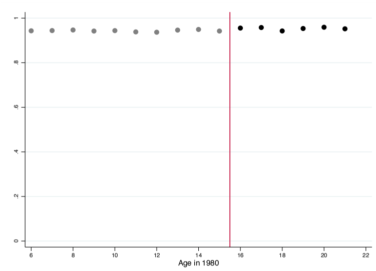
(g) Number of children borne

Figure 7: HOUSEHOLD PATHWAYS FOR THE INTERGENERATIONAL TRANSMISSION OF MOTHER'S SCHOOLING

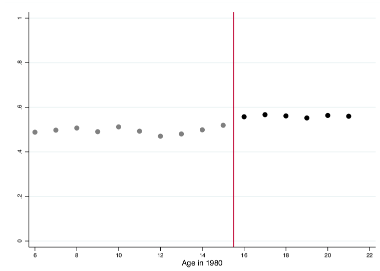
Note: in each figure, circles represent outcome means obtained by regressing the outcome on a linear spline in mother's age on either side of the cut-off (vertical line at age 15); the sample includes sixteen cohorts of black mothers in the ages of 6 through 21 years in 1980. In figure 7e, the age gap is defined as the excess of the male partner's age over the mother's age.



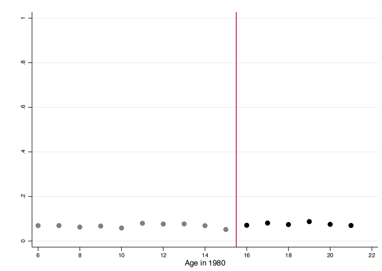
(a) Participates in the labor force



(b) Has paid work



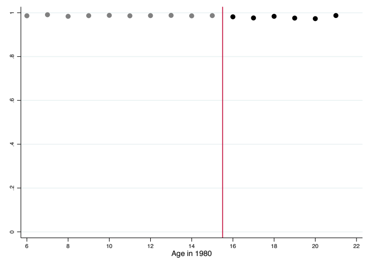
(c) Employed in the primary sector



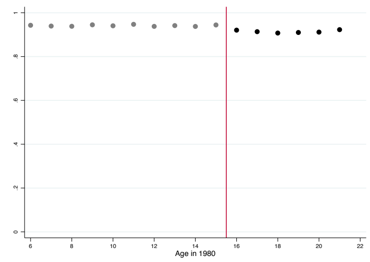
(d) Employed in a high-skill job

Figure 8: LABOR MARKET PATHWAYS FOR THE INTERGENERATIONAL TRANSMISSION OF MOTHER'S SCHOOLING

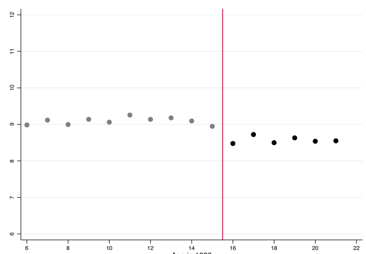
Note: in each figure, circles represent outcome means obtained by regressing the outcome on a linear spline in mother's age on either side of the cut-off (vertical line at age 15); the sample includes sixteen cohorts of black mothers in the ages of 6 through 21 years in 1980.



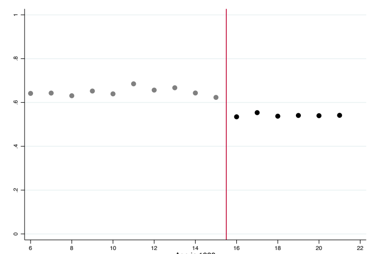
(a) Is married



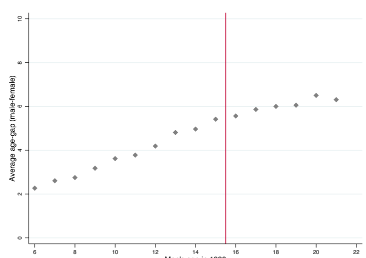
(b) Partner co-resides



(c) Partner's years of schooling



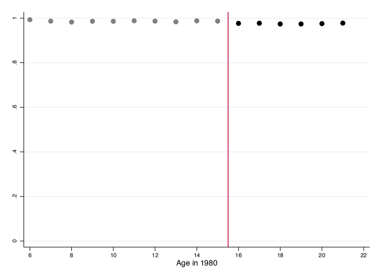
(d) Partner has secondary+ schooling



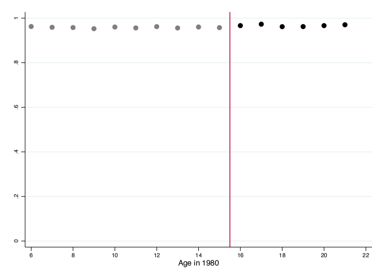
(e) Age gap with partner

Figure 9: Household pathways for the intergenerational transmission of father's schooling

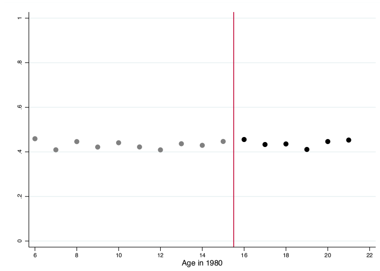
Note: in each figure, circles represent outcome means obtained by regressing the outcome on a linear spline in father's age on either side of the cut-off (vertical line at age 15); the sample includes sixteen cohorts of black mothers in the ages of 6 through 21 years in 1980. In figure 9e, the age gap is defined as the excess of the father's age over the female partner's age.



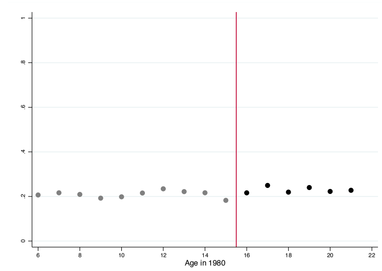
(a) Participates in the labor force



(b) Has paid work



(c) Employed in the primary sector



(d) Employed in a high-skill job

Figure 10: Labor market pathways for the intergenerational transmission of father's schooling

Note: in each figure, circles represent outcome means obtained by regressing the outcome on a linear spline in father's age on either side of the cut-off (vertical line at age 15); the sample includes sixteen cohorts of black fathers in the ages of 6 through 21 years in 1980.

Table 1: Descriptive Statistics

Variable	Mean	SD	Min	Max
Sample: 91480 black mothers				
Mother's age	35.2	4.4	28	43
Mother's years of schooling	8.1	3.2	0	16
Mother's age at first birth	19.5	3.1	12	39
Children born to the mother	4.7	2.1	1	15
Child's age	10.4	2.7	6	15
Daughters	0.5	0.5	0	1
Child is currently attending school	0.97	0.16	0	1
Child's years of schooling	3.51	2.55	0	10
Sample: 50026 black fathers				
Father's age	36.7	4.1	28	43
Father's years of schooling	9.7	3.3	0	16
Child's age	9.7	2.7	6	15
Daughters	0.5	0.5	0	1
Child is currently attending school	0.97	0.16	0	1
Child's years of schooling	2.96	2.48	0	10

Note: the first sample in this table is composed of black children in the ages of 6 through 15 years at the time of the 2002 population census matched with their black mothers; the second sample is composed of the same age-group of children matched with their black fathers. In both samples, the parents were restricted to the ages of six through twenty-one years in 1980, and to persons identified in the census as either the head of household or the unique spouse of the household head.

Table 2: THE EFFECT OF THE REFORMS ON PARENT SCHOOLING: FIRST STAGE

	Dependent variable: Parent schooling		
	Blacks in Zimbabwe		Placebo: other countries in Sub-Saharan Africa
	(1)	(2)	(3)
Panel A. mothers sample			
$1\{A_i \leq 15\}$	0.819***	0.816***	0.154
	[0.216]	[0.210]	[0.133]
Control for rain?	No	Yes	No
F test	14.44	15.10	1.33
p value	0.002	0.001	0.266
Observations	91480	91480	1833418
Panel B. fathers sample			
$1\{A_i \leq 15\}$	0.683***	0.682***	0.109
	[0.133]	[0.123]	[0.090]
Control for rain?	No	Yes	No
F test	26.40	30.81	1.46
p value	0.000	0.000	0.246
Observations	50026	50026	1666480

Note: each cell represents an estimate from a separate regression. Cluster-robust standard errors appear below the estimates in brackets. Clustering is at parent year of birth, i.e. parent age in 1980. All regressions include linear splines in parent age (coefficients not shown). Rainfall data is standardized with reference to the period 1959-2001. Columns 1 and 2 use data from the 2002 Zimbabwe Population Census. Column 3 combines eleven population censuses circa 2002 from Sub-Saharan African countries. See footnote 17 for details
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 3: CONTINUITY IN EXOGENOUS CHARACTERISTICS

Dependent variable	Child is a girl (1)	Height (centimeters) (2)	Height-for-age (Z score) (3)	Adult is black (4)
Panel A. mothers sample				
$1\{A_i \leq 15\}$	-0.005	-0.079	-0.012	0.001***
	[0.004]	[0.335]	[0.052]	[0.000]
F test	1.14	0.055	0.054	9.69
p value	0.303	0.818	0.820	0.007
Observations	91480	2031	2027	91718
Means	0.5	160.2	-0.577	0.997
Panel B. fathers sample				
$1\{A_i \leq 15\}$	0.021	-0.486	-0.082	0.002
	[0.012]	[0.676]	[0.113]	[0.001]
F test	3.04	0.517	0.519	1.67
p value	0.102	0.483	0.483	0.215
Observations	50026	1648	1648	50267
Means	0.5	171.3	1.276	0.995

Note: each cell represents an OLS estimate from regressing a different dependent variable on the discontinuity. The dependent variable is binary in columns (1) and (4), and continuous in columns (2) and (3). The height variables are taken from the 2010-2011 Zimbabwe DHS. Cluster-robust standard errors appear below the estimates in brackets. Clustering is at year of birth, i.e. age in 1980. All regressions include linear splines in parent age (coefficients not shown).
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 4: THE INTERGENERATIONAL EFFECTS OF SCHOOLING

Dependent variable: Child grade attainment		
	OLS	2SLS
	(1)	(2)
Panel A. mothers sample		
Mother's schooling	0.100***	0.073***
	[0.002]	[0.014]
F test		14.04
p value		0.002
Observations	91480	91480
Panel B. fathers sample		
Father's schooling	0.098***	0.092***
	[0.003]	[0.016]
F test		25.13
p value		0.000
Observations	50026	50026

Note: each cell represents an estimate from a separate regression; robust standard errors clustered by age in 1980 appear in brackets. All regressions include linear splines in parent age in 1980 (coefficients not shown), binary indicators for child ages 7 through 15 (omitted age is 6), and a binary variable for girls. The 2SLS regressions instrument parent schooling with the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 5: ROBUSTNESS CHECKS (2SLS)

	Dependent variable: Child grade attainment				
	Baseline 2SLS	Rainfall in birth-year	Province fixed effects	Urban origin	Excluding units at discontinuity
	(1)	(2)	(3)	(4)	(5)
Panel A. mothers sample					
Mother's schooling	0.073***	0.073***	0.064***	0.063***	0.066***
	[0.014]	[0.016]	[0.015]	[0.016]	[0.012]
F test	14.04	14.77	12.57	12.34	30.11
p value	0.002	0.002	0.003	0.003	0.000
Observations	91480	91480	91480	91480	86505
Panel B. fathers sample					
Father's schooling	0.092***	0.092***	0.077**	0.079***	0.079***
	[0.016]	[0.016]	[0.014]	[0.016]	[0.015]
F test	25.13	29.21	26.17	21.15	48.44
p value	0.000	0.000	0.000	0.000	0.000
Observations	50026	50026	50026	50026	47031

Note: each cell represents a separate regression; robust standard errors clustered by age in 1980 appear in brackets. All regressions include linear splines in parent age in 1980, binary indicators for child ages 7 through 15 (omitted age is 6), and a binary variable for girls. The 2SLS regressions instrument parental schooling with the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 6: ADDITIONAL SPECIFICATION TESTS: ALTERNATE PARENT SAMPLES AND POLYNOMIALS IN AGE SPLINES (2SLS)

	Dependent variable: Child grade attainment							
Parent cohorts included	Ages 6–21				Ages 0–30			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. mothers sample								
Mother's schooling	0.073***	0.123	0.069***	0.121***	0.029*	0.128***	0.035***	0.126***
	[0.012]	[0.086]	[0.013]	[0.036]	[0.015]	[0.026]	[0.013]	[0.020]
F test	14.05	4.42	80.10	16.49	23.74	9.76	122.98	51.25
p value	0.002	0.053	0.000	0.001	0.000	0.004	0.000	0.000
Observations	91480	91480	80370	80370	120538	120538	109428	109428
Panel B. fathers sample								
Father's schooling	0.092***	0.194***	0.070***	0.194***	-0.004	0.267***	-0.009	0.224***
	[0.016]	[0.035]	[0.019]	[0.025]	[0.021]	[0.058]	[0.023]	[0.030]
F test	25.13	18.97	151.00	23.14	35.80	10.29	114.16	36.85
p value	0.000	0.000	0.000	0.000	0.000	0.003	0.000	0.000
Observations	50026	50026	43395	43395	78864	78864	72233	72233
Specification:								
Quadratic spline?	N	Y	N	Y	N	Y	N	Y
Dropped ages 14 and 15?	N	N	Y	Y	N	N	Y	Y

Note: each cell represents a separate regression; robust standard errors clustered by age in 1980 appear in brackets. All regressions include linear splines in parent age in 1980, binary indicators for child ages 7 through 15 (omitted age is 6), and a binary variable for girls. The 2SLS regressions instrument parental schooling with the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 7: INTERGENERATIONAL EFFECTS BY CHILD'S SEX (2SLS)

Dependent variable: Child grade attainment		
	Sons	Daughters
	(1)	(2)
Panel A. mothers sample		
Mother's schooling	0.047***	0.097***
	[0.017]	[0.026]
F test	13.47	12.80
p value	0.002	0.003
Observations	45762	45718
Panel B. fathers sample		
Father's schooling	0.112***	0.071***
	[0.028]	[0.021]
F test	33.78	16.63
p value	0.002	0.001
Observations	25218	24808

Note: each cell represents an estimate from a separate regression; robust standard errors clustered by age in 1980 are shown in brackets. All regressions include linear splines in parent age in 1980 and binary indicators for child ages 7 through 15 (omitted age is 6). The 2SLS regressions instrument parental schooling with the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument.

* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 8: INTERGENERATIONAL EFFECTS ON SCHOOL ATTENDANCE AND CHILD LABOR

Dependent variable:	Child is currently attending school (1)	— Child participates in — market work (2)	market and domestic work (3)
Panel A. mothers sample			
Mother's schooling	-0.001	0.004	0.003
	[0.002]	[0.003]	[0.003]
F test	14.04	11.17	11.17
p value	0.002	0.005	0.005
Observations	91480	53790	53790
Panel B. fathers sample			
Father's schooling	0.003*	0.004	0.002
	[0.001]	[0.004]	[0.003]
F test	25.13	23.77	23.77
p value	0.000	0.000	0.000
Observations	50026	24392	24392

Note: each cell represents an estimate from a separate regression; robust standard errors clustered by age in 1980 appear in brackets. All regressions include linear splines in parent age in 1980, fixed effects for child age, and a binary variable for girl-children. The sample size in columns (2) and (3) is smaller than in column (1) because labor supply data is not collected from persons younger than ten years at the time of the census; accordingly, the fixed effects for child age are applied to ages 11 through 15 with ten as the omitted age in the child labor regressions. The two formulations of child labor differ by how they treat domestic work performed by children. See the text for more details. The instrument for parental schooling is the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 9: PATHWAYS OF THE PARENTAL SCHOOLING EFFECT (2SLS)

Dependent variable:	Parent is married (1)	Partner co-resides (2)	— Partner schooling — Years (3)	Secondary/higher (4)	Age gap with partner (5)	Mother's age at first birth (6)	# born to mother (7)
Panel A. mothers sample							
Mother's schooling	-0.010	-0.015	0.563***	0.072***	0.053	0.562***	-0.191***
	[0.008]	[0.013]	[0.059]	[0.009]	[0.184]	[0.070]	[0.036]
F test	14.04	14.04	17.58	17.58	15.22	14.04	14.04
p value	0.002	0.002	0.001	0.001	0.002	0.002	0.002
Observations	91480	91480	55661	55661	57728	91480	91480
Panel B. fathers sample							
Father's schooling	0.013**	0.013	0.487***	0.079***	-0.191		
	[0.006]	[0.009]	[0.115]	[0.009]	[0.160]		
F test	25.13	25.13	22.16	22.16	24.15		
p value	0.000	0.000	0.000	0.000	0.000		
Observations	50026	50026	44767	44767	46440		

Note: each cell represents a separate regression; robust standard errors clustered by age in 1980 appear in brackets. All regressions include linear splines in parent age in 1980, binary indicators for child ages 7 through 15 (omitted age is 6), and a binary variable for girl-children (coefficients not shown). The 2SLS regressions instrument parental schooling with the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument. See text for the definition of the dependent variables.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 10: LABOR MARKET RESPONSES TO PARENTAL SCHOOLING (2SLS)

Dependent variable:	Participates in the labor force	Is engaged in paid work	— Is employed in — the primary sector	a high-skill job
	(1)	(2)	(3)	(4)
Panel A. mothers sample				
Mother's schooling	-0.010	0.003	-0.042***	0.019**
	[0.012]	[0.006]	[0.006]	[0.008]
F test	14.04	10.52	14.20	14.20
p value	0.002	0.006	0.002	0.002
Observations	91480	68451	91268	91268
Panel B. fathers sample				
Father's schooling	-0.001	-0.003	-0.082***	0.072***
	[0.002]	[0.005]	[0.019]	[0.016]
F test	25.13	23.31	24.87	24.87
p value	0.000	0.000	0.000	0.000
Observations	50026	49090	49825	49825

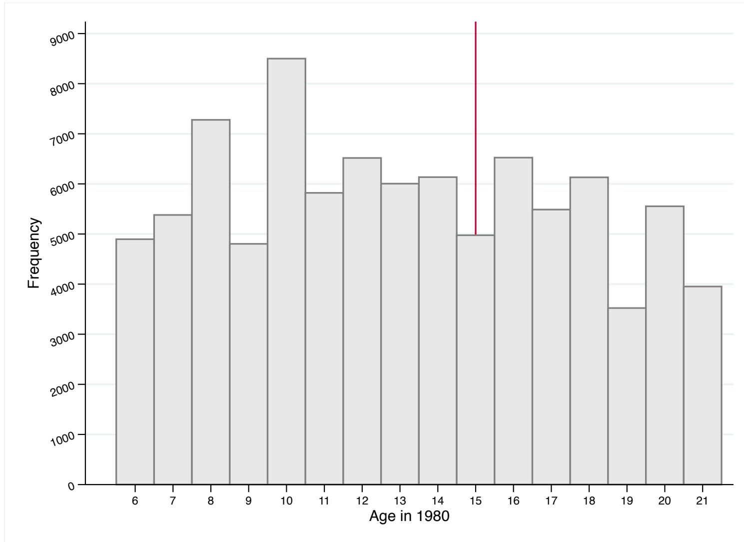
Note: each cell represents a separate regression; robust standard errors clustered by age in 1980 appear in brackets. The sample sizes in columns (3) and (4) are slightly reduced because they discarded those cases where occupation was coded as unknown. A black parent was considered to be employed in the primary sector if their main activity was recorded as agriculture or mining. A black parent was taken to be employed in a high-skilled job if their main occupation code was less than 500 in the census classification. Not all occupation codes above 500 fall in the primary sector. All regressions include linear splines in parent age in 1980, binary indicators for child ages 7 through 15 (omitted age is 6), and a binary variable for girl-children. The 2SLS regressions instrument parental schooling with the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument. See text for more detail on the dependent variables.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Online Appendix: not for publication

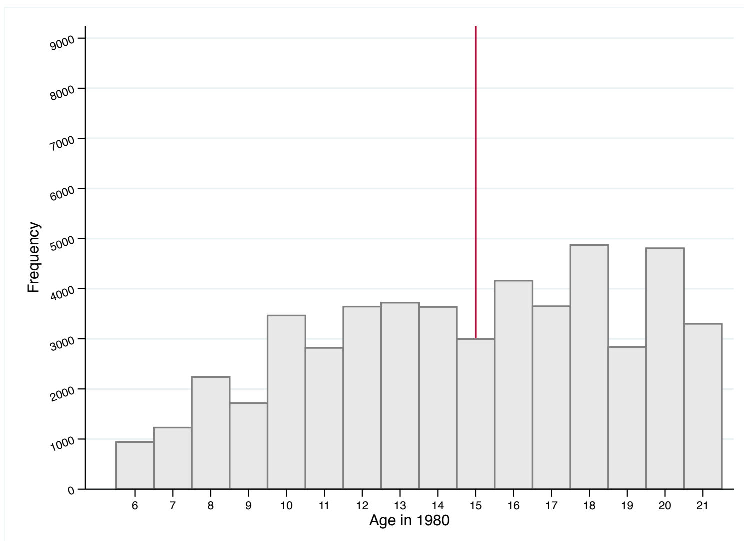
Table A1: Additional summary statistics for parents

Variable	Mean	SD	Min	Max	Observations
Sample: black mothers					
Is married	0.88	0.32	0	1	91480
Partner co-resides	0.63	0.48	0	1	91480
Partner years of schooling	8.97	3.51	0	16	55661
Partner has secondary or higher level of schooling	0.55	0.50	0	1	55661
Participates in labor force	0.75	0.43	0	1	91480
Engages in paid work	0.95	0.22	0	1	68451
Works in the primary sector	0.52	0.50	0	1	91268
Holds a high-skill job	0.07	0.26	0	1	91268
Age at first birth	19.49	3.10	12	39	91480
Number of children born to the mother	4.72	2.08	1	15	91480
Height (cm)	160.2	6.32	115.2	197.4	2031
Height (z-score)	-0.58	1.00	-3.99	5.65	2027
Sample: black fathers					
Is married	0.98	0.13	0	1	50026
Partner co-resides	0.93	0.26	0	1	50026
Partner years of schooling	8.85	2.98	0	16	44767
Partner has secondary or higher level of schooling	0.60	0.49	0	1	44767
Participates in labor force	0.98	0.14	0	1	50026
Engages in paid work	0.96	0.19	0	1	49090
Works in the primary sector	0.44	0.50	0	1	49825
Holds a high-skill job	0.22	0.41	0	1	49825
Height (cm)	171.3	6.76	146.4	191.5	1648
Height (z-score)	1.28	1.11	-2.90	4.66	1648

Notes: Sample is restricted household heads and head-spouses aged six through twenty-one years of age in 1980. All the variables, except for height, are obtain from the 2002 Population Census. The height information comes from the 2010-2011 Zimbabwe Demographic and Health Survey.



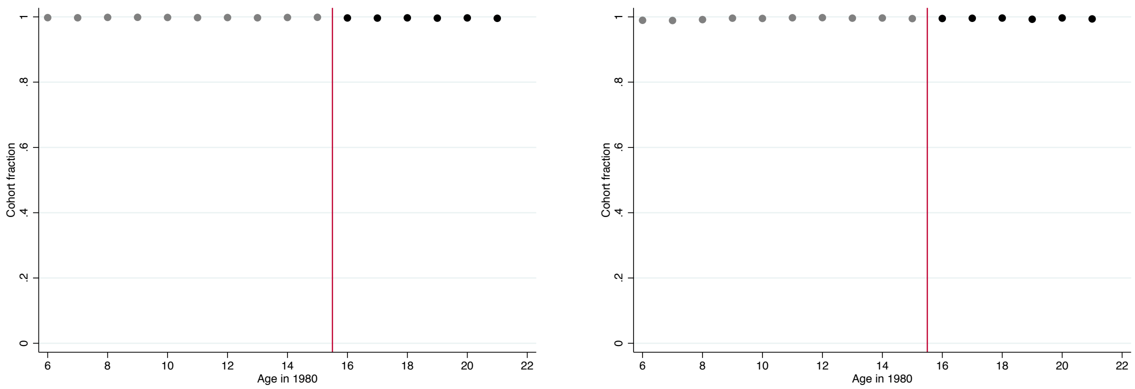
(a) Black mothers



(b) Black fathers

Figure A1: Parent age in 1980

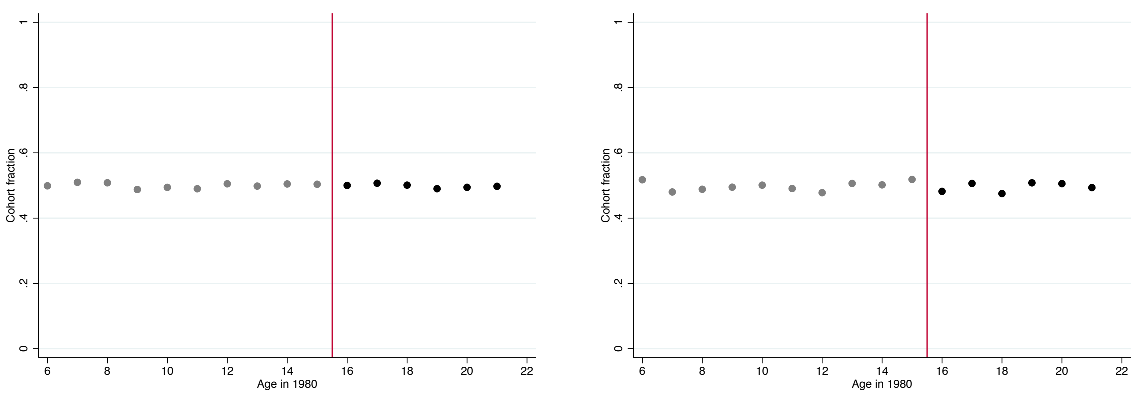
Notes: figure A1a and figure A1b show histograms of age in the respective samples of black mothers and black fathers. The vertical line at age fifteen represents the treatment threshold under the reforms.



(a) Women (b) Men

Figure A2: Race composition of adult cohorts

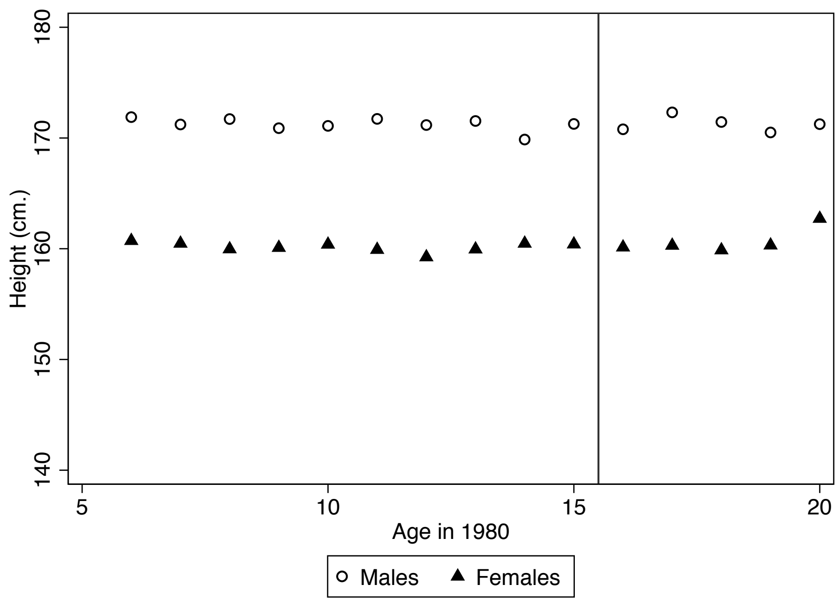
Notes: figure A2a shows the fraction of Zimbabwean women who are black by age in 1980; similarly, figure A2b shows the fraction of Zimbabwean men who are black by age in 1980. The samples in these figures are respectively, 91,718 women and 50,267 men in the ages of 6 through 21 years in 1980. The vertical line at age fifteen represents the treatment threshold under the reforms.



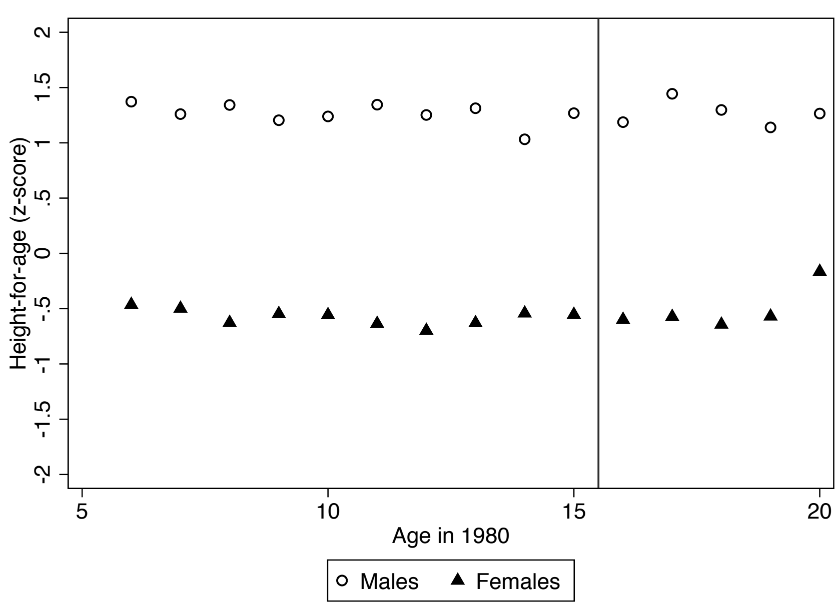
(a) Black mothers (b) Black fathers

Figure A3: Sex composition of children by parent age in 1980

Notes: figure A3a shows the fraction of daughters among the children in the sample of black mothers by the mother's age in 1980; similarly, figure A3b shows the fraction of daughters among the children in the sample of black fathers by the father's age in 1980. All children included in the two samples are in the ages of six through fifteen years in 2002. The vertical line at age fifteen represents the treatment threshold under the reforms.



(a) Height (cm.)



(b) Height-for-age (Z score)

Figure A4: Mean height and mean height-for-age of parent cohorts

Notes: figure A4a shows mean height by age in 1980, while figure A4b shows mean height-for-age by age in 1980. Height data for men and women were taken from the 1999 Zimbabwe DHS.

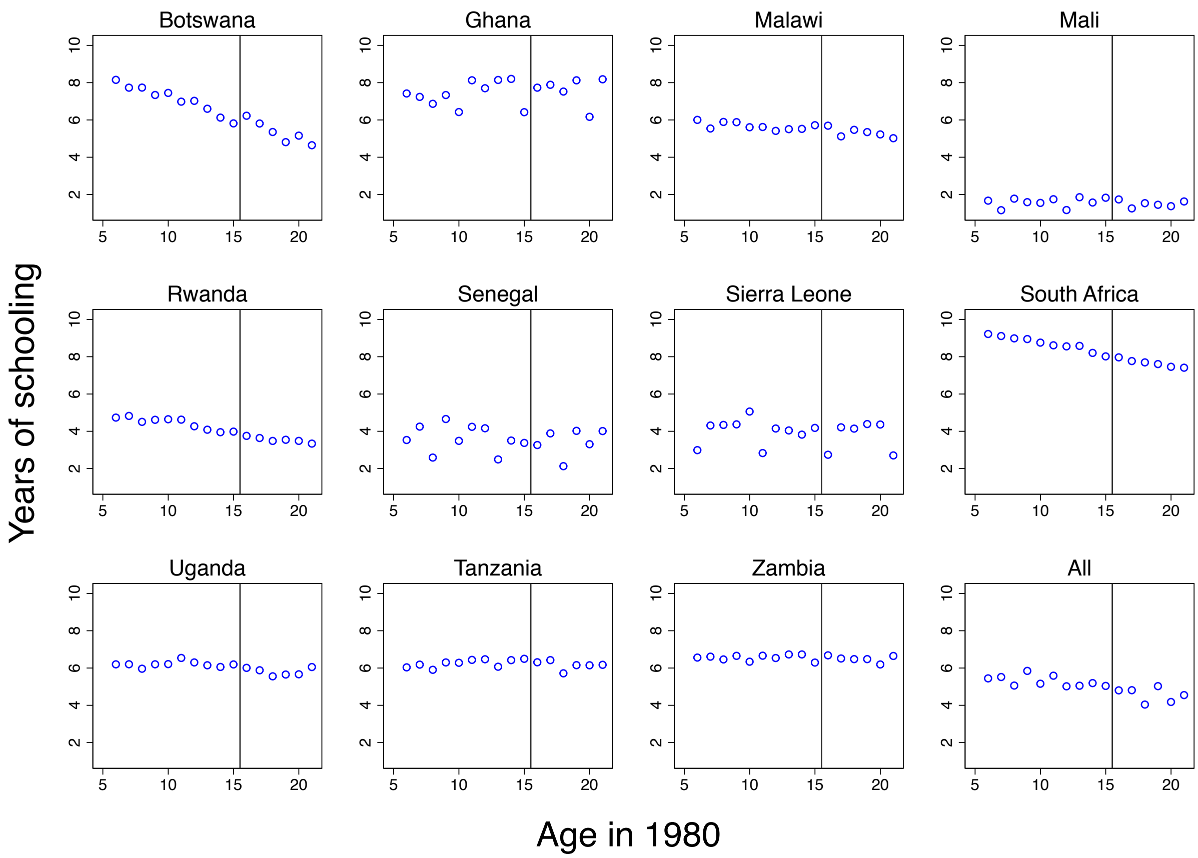


Figure A5: Women's years of schooling by age in 1980: Sub-Saharan Africa

Notes: figures show women's mean years of schooling by age in 1980 for each age from six through 21 years in 1980. In this placebo test, the vertical line represents the threshold at the relevant age for those exposed to the Zimbabwean education reform.

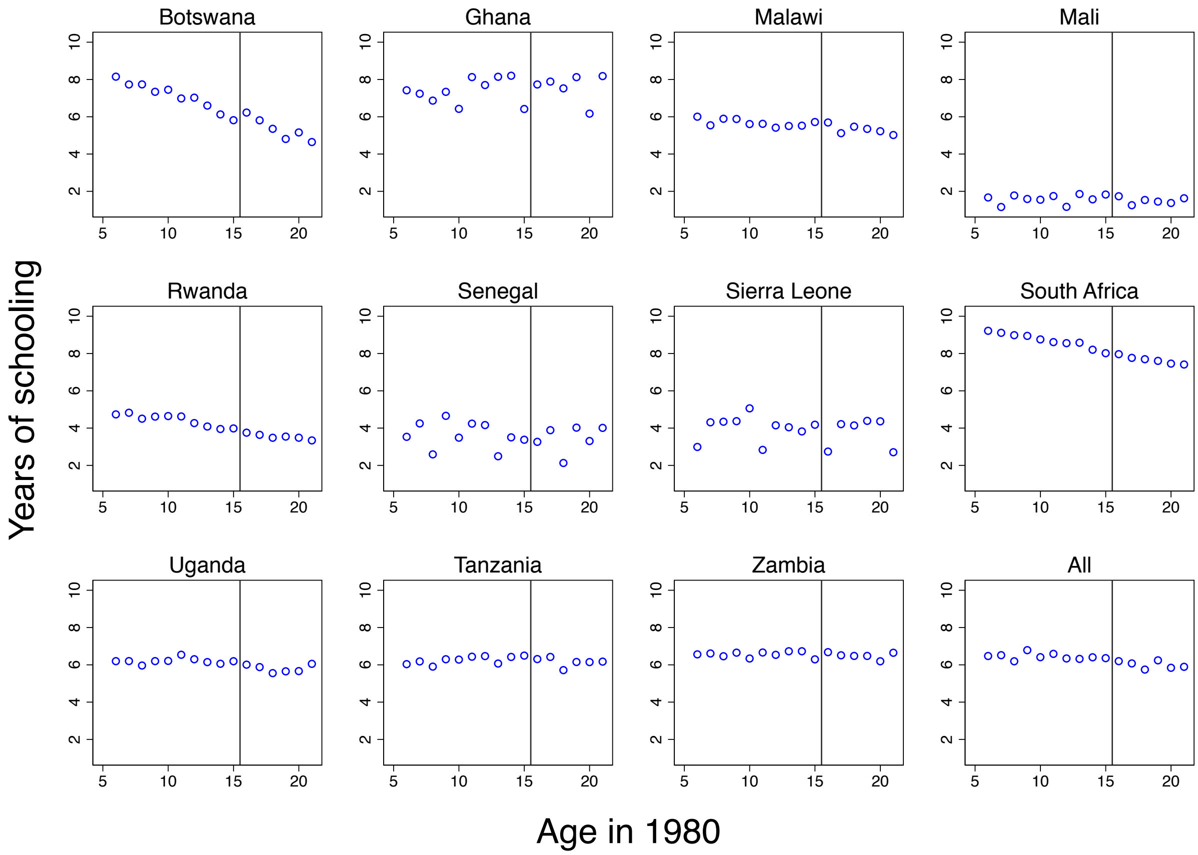


Figure A6: Men's years of schooling by age in 1980: Sub-Saharan Africa

Notes: figures show men's mean years of schooling for each age from six through 21 years in 1980. In this placebo test, the vertical line represents the threshold at the relevant age for those exposed to the Zimbabwean education reform.

Table A2: Placebo: schooling attainment in Sub-Saharan African countries by age in 1980

	Dependent variable: Years of schooling											
	Botswana	Ghana	Malawi	Mali	Rwanda	Senegal	Sierra Leone	South Africa	Uganda	Tanzania	Zambia	All
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Panel A. mothers sample												
$1\{A_i \leq 15\}$	0.018	-1.390*	-0.266	0.066	0.161	0.353	0.480	0.093	0.467	0.743	-0.196	0.154
	[0.143]	[0.759]	[0.290]	[0.266]	[0.104]	[0.698]	[0.470]	[0.069]	[0.303]	[0.456]	[0.131]	[0.133]
Observations	16793	185989	102527	91152	64439	89574	48432	445089	193751	343002	88876	1833418
Mean schooling	7.087	4.878	3.318	0.678	3.012	1.861	1.578	8.112	3.884	5.047	5.125	4.870
Panel B. fathers sample												
$1\{A_i \leq 15\}$	-0.352**	-0.967	-0.095	0.124	0.212**	0.572	0.693	0.111	0.362*	0.224	-0.175	0.109
	[0.157]	[0.638]	[0.251]	[0.285]	[0.075]	[0.770]	[0.696]	[0.076]	[0.195]	[0.228]	[0.148]	[0.090]
Observations	14930	158696	100292	72630	52279	79552	41087	383281	204518	315705	93011	1666480
Mean schooling	6.657	7.201	5.557	1.488	4.116	3.302	3.464	8.371	6.063	6.166	6.513	6.184
Census year	2001	2000	1998	1998	2002	2002	2004	2001	2002	2002	2000	

Note: each cell represents the OLS estimate from regressing years of schooling on the placebo threshold (indicated by age fifteen in 1980) in a sample of adults in the ages of six through twenty-one in 1980 from a different Sub-Saharan African country. Cluster-robust standard errors appear below the estimates in brackets. Clustering is at age in 1980. All regressions include linear splines in age (coefficients not shown).
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table A3: Intergenerational transmission of schooling in Sub-Saharan African countries (OLS)

	Dependent variable: Child years of schooling										
	Botswana	Ghana	Malawi	Mali	Rwanda	Senegal	Sierra Leone	South Africa	Uganda	Tanzania	Zambia
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Panel A. mothers sample											
Mother's schooling	0.092***	0.077***	0.183***	0.288***	0.116***	0.238***	0.149***	0.068***	0.133***	0.115***	0.143***
	[0.002]	[0.001]	[0.001]	[0.002]	[0.001]	[0.002]	[0.002]	[0.000]	[0.001]	[0.001]	[0.001]
Observations	25236	315363	190120	206406	153552	212507	85429	620775	541167	714032	204347
Mean of dependent variable	3.051	2.628	2.207	0.856	1.790	1.860	2.020	3.729	2.364	2.023	2.157
Panel B. fathers sample											
Father's schooling	0.080***	0.080***	0.157***	0.215***	0.109***	0.206***	0.116***	0.073***	0.117***	0.120***	0.107***
	[0.003]	[0.001]	[0.001]	[0.002]	[0.001]	[0.001]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	12163	249929	149117	175547	99124	183489	65783	344173	456783	512498	174748
Mean of dependent variable	3.144	2.543	2.222	0.794	1.762	1.786	1.991	3.862	2.325	2.014	2.173
Census year	2001	2000	1998	1998	2002	2002	2004	2001	2002	2002	2000

Note: each cell represents the OLS estimate from regressing child years of schooling on her/his parent's years of schooling. All regressions control for the child's sex, and include age fixed effects for parents as well as children (coefficients not shown). Samples are restricted to native children aged 6-15 at the time of the census. Cluster-robust standard errors appear below the estimates in brackets. Clustering is at parent age in 1980.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table A4: FIRST-STAGE ESTIMATES: IMPACT OF THE REFORMS ON PARENTAL SCHOOL ATTAINMENT

Sample	Black mothers (N=91,480)							Black fathers (N=50,026)						
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]
$1\{A_i \leq 15\}$	0.819***	0.819***	0.819***	0.819***	0.819***	0.819***	0.819***	0.683***	0.683***	0.683***	0.683***	0.683***	0.683***	0.683***
Standard error	[0.047]	[0.216]	[0.076]	[0.017]			[0.073]	[0.060]	[0.133]	[0.058]	[0.003]			[0.081]
F test	300.12	14.44	117.53				125.69	130.04	26.40	140.27				71.09
p value for F test	0.000	0.002	0.000				0.000	0.000	0.000	0.000				0.000
χ^2				2259.36							47260.45			
p value for χ^2				0.000							0.000			
Wald (t) statistic					3.799	10.841						5.138	11.844	
Standard normal percentile					0.000	0.000						0.000	0.000	
Wild cluster bootstrap-t %le					0.054	0.001						0.010	0.000	
T %le for finite clusters					0.002	0.000						0.000	0.000	

Each column represents a separate regression. All regressions fit a piece-wise linear regression to parental schooling around the point of discontinuity in treatment-probability. Robust standard error in brackets. The sample is restricted to black Zimbabweans. The standard errors are robust but not clustered in column [1] and [8]; they are clustered by parent year of birth (i.e. parent age) in [2] and [9], by province in [3] and [10], by both age and province in [4] and [11], and by parent district of birth in [7] and [14]. The standard errors are derived from a wild-cluster bootstrap-t procedure in [5], [6], [12] and [13]; clustering is on age in [5] and [12] and on province in [6] and [13]. The number of bootstrap reps is 999. The number of clusters is 16 in columns [5] and [12] and 10 in columns [6] and [13]. For asymptotically-consistent inference in the presence of finite clusters, the degrees of freedom invoked in the T distribution = number of clusters -1. The F statistics correspond to the null that the impact of $1\{A_i \leq 15\}$ is zero. The p values refer to the probability of obtaining the calculated F statistic under the null.

* indicates statistical significance at 10, ** at 5 and *** at 1.

Table A5: THE INTERGENERATIONAL EFFECT IN AN EXPANDING INTERVAL AROUND THE DISCONTINUITY (2SLS)

Dependent variable: Child grade attainment				
	IV (1)	Observations (2)	F test (3)	p value (4)
Panel A. mothers sample				
14–15 v. 16–17	0.409*** [0.005]	23122	1681.52	0.000
13–15 v. 16–18	-0.023 [0.073]	35258	15.36	0.011
12–15 v. 16–19	0.146** [0.060]	45297	9.30	0.019
11–15 v. 16–20	0.096*** [0.037]	56671	11.28	0.008
10–15 v. 16–21	0.096*** [0.021]	69123	8.96	0.012
Panel B. fathers sample				
14–15 v. 16–17	0.170*** [0.004]	14441	36570.30	0.000
13–15 v. 16–18	0.146*** [0.016]	23032	38.64	0.002
12–15 v. 16–19	0.188** [0.028]	29511	45.76	0.000
11–15 v. 16–20	0.129*** [0.020]	37138	29.72	0.000
10–15 v. 16–21	0.128*** [0.011]	43902	19.12	0.001

Note: each row reports the 2SLS estimate with cluster-robust standard error, sample size, F test and associated p value from a different regression; each row corresponds to a specific interval around the discontinuity; robust standard errors are clustered by age in 1980 and shown in brackets. All regressions include linear splines in parent age in 1980 and control for binary indicators for child ages 7 through 15 (omitted age is 6). The 2SLS regressions instrument parental schooling with the discontinuity at age 15 in 1980. The reported F-statistics refer to this excluded instrument.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.