The Intergenerational Transmission of Schooling among the Education-Rationed*

Jorge M. Agüero† Maithili Ramachandran‡

August 17, 2017

*We would like to thank the personnel at the Central Statistical Office of Zimbabwe for making a 10% sample of the 2002 Population Census available to us, and Craig Richardson for generously sharing the rainfall data. We are grateful to Chris Barrett, Prashant Bharadwaj, Marianne Bitler, Resul Cesur, Pascaline Dupas, Steven Helfand, Mindy Marks, Todd Sorensen and Aman Ullah for suggestions on an earlier draft of this paper, as well as seminar participants at Cornell-IBEPP, Cal-State Fullerton, PACDEV, Oxford-CSAE, the NBER Children's Program Meeting, Claremont McKenna and the PAA Economic Demography Workshop for helpful comments and discussions. The views expressed here as well as any outstanding errors are our own.

†University of Connecticut, Department of Economics and El Instituto, 365 Fairfield Way, Unit 1063, Storrs, CT 06269-1063, USA. Email: jorge.aguero@uconn.edu.

‡University of California, Riverside, Department of Economics, 900 University Avenue, Riverside, CA 92521, USA. Email: mrama001@ucr.edu.

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 large and robust intergenerational transmissions. Placebo tests for white Zimbabweans further validate our design. Evidence of assortative mating suggests that the marriage, rather than the labor, market is a key mechanism for these transmissions. We discuss how our results impact the long-term success and design of antipoverty policies.

Keywords: Schooling, intergenerational effects, Zimbabwe.

JEL codes: I21, J13, J24.

1 Introduction

A growing number of developing countries have sought policies to break the "vicious cycle of poverty" where, for example, children of low-schooled parents tend to have low schooling themselves. A widely favored intervention is the conditional cash transfer (CCT) program that incentivizes poor families to send their children to school by increasing the income of the current generation as a way to remove the barriers limiting their children's human capital investments (e.g. Levy 2007, Fiszbein et al 2009). The long-term success of these kinds of programs depends on whether the observed intergenerational transmission of schooling is causal, so the policies that increase education among children today will also increase the education of children tomorrow through intergenerational spillovers.[1]

A few papers in the literature on intergenerational schooling transmissions have been able to identify causal effects[2]. Within this subset, nearly all of these papers focus on developed countries, and derive their identification using one of three different approaches.[3]

[1]For a discussion of the medium-term impact of Mexico's *Progresa* see, for example, Behrman, Parker, and Todd (2011). Filmer and Schady (2014) and Barrera-Osorio, Linden, and Saavedra (2015) evaluate of similar programs.

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

[3]For examples of intergenerational *correlations* in schooling see Beegle, Christiaensen, Dabalen, and Gaddis (2016) and Ferreira et al (2012). There is also growing literature, summarized by Grossman (2015), on the effects of parental schooling on child health that includes research on developing countries. While important, and related to the effects 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).

One approach compares twins and 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: Sacerdote (2002, 2007) and Plug (2004), for instance, exploit quasi-randomness in adoption placements in the United States; Björklund, Lindahl, and Plug (2006) do the same 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.[4] 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 evidence, recent reviews of the literature agree that 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 that affected a large share of its population and by exploring several mechanisms.[5] In particular, we focus on Zim-

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

[5]Although the program originally called *Progresa* is nearly 20 years old, this and many other cash-transferring programs do not easily lend themselves to the identification of intergenerational schooling transmissions. While the experimental nature of these programs helps, children who acquired more schooling due to the program usually have parents who received cash transfers, which compromises the validity of the exclusion restriction. Note that unconditional cash transfers also suffer from this limitation. The addition of health visits required by many CCTs further invalidates the use of these policies as credible sources

babwe, 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 extended the automatic progression to secondary school for *all* students, a feature reserved hitherto for whites. Before the reform, black students had to complete primary school (ending at grade 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 swiftly created a *discontinuous* change in the transition rate to secondary school: from 27 percent for those graduating in 1979 to 86 percent for their slightly older counterparts graduating in 1980 (see Figure 1).

The timing of the reform is our source of exogenous variation in the schooling attainment of blacks, a fact we exploit to set up a fuzzy regression discontinuity design. Our results show positive and significant intergenerational spillovers among black Zimbabweans. One extra year of schooling for mothers is associated with 0.20 additional years for her child; the corresponding effect from fathers is 0.29 years. Both estimates are larger than the impact of 0.10 years on child schooling reported, for example, by Holmlund, Lindahl, and Plug (2011) in Nordic countries. Also, the gender difference is consistent with the studies by Orazem and King (2007) and Schultz (2002), who note similar patterns in cross-sectional studies from developing countries. 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 placebo test for white Zimbabweans shows, as expected, no effect of the reform on their schooling attainments, reinforcing the validity of our methodology.

As in all the 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

of exogenous variation in schooling.

literature using compulsory schooling laws in an important way. The automatic progression to secondary school in Zimbabwe creates a different and, arguably, larger set of compliers. This allows us to explore a LATE that is more relevant to developing countries. For instance, with compulsory 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. As Black and Devereux (2011) point out, the set of compliers for compulsory schooling laws, such as those implemented in Nordic countries, tends to be a small fraction of the population. Oreopoulos (2006) argues that similar policies in the US "typically affect fewer than 10 percent of the population exposed to the instrument" (p. 153). In the Zimbabwean reform, described below in section 2, the set of compliers is formed by those who wanted to stay in school but couldn't due to the apartheid-style regime. When given a 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. Those who wanted to drop out were allowed to do so even after the reform because the progression was not compulsory. Thus, by estimating the intergenerational transmission of schooling using a reform that removes barriers limiting children's schooling, our LATE affects a lager share of the population in developing countries and it is closer to the goals set by policies trying to break the vicious cycle of poverty, such as CCTs.

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 considered the post-independence 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 one to apply this policy change to estimate the impact of education for women *and* men. The possibility to causally estimate these effects separately further widens the external validity of our findings and permit us to explore pathways arising, not only from direct links (parents to children), but also through the marriage market.

In that regard, we investigate 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. Yet we do not find effects on their labor supply. Specifically, there is no association between schooling, as induced by the reform, and labor force participation or working for pay. This was also the case for fathers. However, we find a high degree of positive assortative mating, as more-educated individuals marry among themselves, suggesting that an important mechanism for the transmission takes place in the marriage market rather than in the labor market. Uncovering the marriage market mechanics of the intergenerational spillover is an important contribution in its own right since it contains particular implications for the long-term impact of policies trying to break the intergenerational transmission of poverty in developing countries. As the next generation gains more schooling due to safety net policies, these more-educated individuals create families with other similarly educated partners, leading to a larger *marriage surplus*. Evaluations of CCTs and similar policies must therefore incorporate estimates of these long-term benefits.

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 Zimbabwe was known before 1980 – blacks where heavily rationed in their access to schools and education opportunities under the apartheid-style regime.[6] For instance, in 1976, for every 1,000 black school-aged children, 250 never went to school. Only 337 finished primary school (grade 7) and just 60 children reached Form I, the first year of secondary school. Fewer than three finished high school (Riddell 1980). This compares poorly to white students who enjoyed universal primary enrollment and transition rates close to 100%. In a country where the overall ratio of blacks to whites was 21:1 in 1975, the ratio of enrolled students was 14:1 in grade 7, and a mere 2:1 in Form I. For the same year, Chidzero (1977) reports that there were approximately 3,000 white students in secondary classes leading to university entrance compared to only 790 black students. New construction of African secondary schools was also 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

[6]For a history of the apartheid-era education system and policies dictating the quantity and quality of schooling permitted to Africans, see Atkinson (1972) and O'Callaghan and Austin (1977).

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 students graduating from primary school admission to Form I on the grounds of poor test results or classroom seating constraints. Thus, 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 had stood at 27 percent; in 1980, the first reform year, it soared to 86 percent. In absolute numbers, Form I enrollment rose from 22,201 to 83,491. Throughout the eighties, transition rates remained high, averaging about 75 percent.

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,[7] 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).[8] In the next section,

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

[8]As shown in the histograms of parent age, Figures A1a and A1b, there is no evidence of

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 our interest lies in the schooling levels of reform-affected parents-to-be and their children, we restrict our study sample to school-age children, 6 to 15, in 2002. The census allows us to identify the parent of a child if the adult in question is either the head of the household or his/her spouse[9]. We then restrict the sample to parents born between 1959 and 1974 so that our largest possible estimation sample comprises parents aged 6 through 21 in 1980.[10] After removing observations with missing schooling data, our final sample for black Zimbabweans has 91,480 mother-child matches and 50,026 father-child matches. Using similar rules, we construct a sample of white Zimbabweans to run placebo tests, and obtain 238 white women and 241 white men.[11]

heaping at the age cutoff, ruling out possible biases in the sample.

[9]This restriction does not bias our sample. For instance, the schooling outcomes of these children and the excluded sample are remarkably close. This information is available upon request.

[10]We also dropped polygamous households from the analysis. 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).

[11]For whites, the sample includes adults who are head of household (or their spouses) aged 6-21 in 1980, with or without children in the 2002 census. This was done in oder to increase their sample size. The disparity in black and white sample sizes reflects the historically small fraction whites made up in the total population. When southern Rhodesia was reconstituted as Zimbabwe in 1980, whites formed 3.4 percent of the total population; by the 2002 census,

Table 1 reports descriptive statistics for our main samples, sliced by parent sex. The average black mother is thirty-five years old and has eight years of schooling. Fathers are slightly older and have more schooling. The average child is about 10 years old and daughters and sons occur in equal proportion in all samples. The data show near universal enrollment among children in our study sample.[12] 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 his grade progress. Thus, our outcome of interest is the number of completed years of schooling, relative to the average child in a given age group, i.e. a schooling z-score, where the mean and standard deviation of schooling are obtained from the 10 percent sample of the census, including blacks and whites. Children in our sample are slightly more educated (0.1 of a standard deviation) mainly because their parents are young relative to the population at large. 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

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

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) as well as some

they had declined to less than one per cent.

[12]The enrollment rate is 95 percent for the full sample in the 6-15 age range.

location indicators.[13] The parameter of interest capturing the intergenerational spillover is β. However, omitting an unobserved variable 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, and furthermore, transitioning was a natural next step for those who had not been rationed out of a seat in grade 7 before 1980 (Dorsey 1989, Nhundu 1992).

---

[13]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 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.

The jump in secondary school enrollment probability generated by the reform induces a fuzzy regression discontinuity design. This provides an instrumental variable for parental schooling s_i in the point of discontinuity, $\overline{A}$, that can be estimated via 2SLS with the following equations:

$$
\begin{aligned}
y_i &= \alpha_1 + \beta\, E[s_i|A_i] + X_i'\gamma + \epsilon_i && (2)\\
E[s_i|A_i] &= \alpha_2 + X_i'\delta + f(A_i) && (3)
\end{aligned}
$$

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 centered at $\overline{A} = 15$. However, as shown below, 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 two-stage least-squares (2SLS) needs to rely on 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,[14] we plot the conditional expectation function (CEF)

[14]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

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 the left-hand-side panel of Figure 3, we observe a clear discontinuity in completed years of schooling around the threshold for black mothers and black fathers.[15]

The second assumption, that the discontinuity satisfies the exclusion restriction, is not directly testable; however, Figure 3 again provides support. In the context of an RD design, predetermined and unrelated variables must be smooth around the threshold. Since the goal of the reforms was to undo racial disparities, it should not have induced discontinuities in the schooling of white Zimbabweans (or any other non-black racial group). Such discontinuities would invalidate our identification strategy. In the right-hand-side panel of Figure 3, the schooling graphs for white women and men show no discontinuities, strongly reinforcing the validity of our identification strategy. In the next section, we present the regression counterparts of these graphical analyses and consider additional robustness tests for the intergenerational spillovers in schooling.

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 A2, we consider

but continuous. By contrast, Zimbabwe's reform delivered universal primary education and literacy in a span of just eleven years.

[15]The unconditional functions, without de-trending, show similar patterns and have been used elsewhere (Agüero and Bharadwaj 2014, Grépin and Bharadwaj 2015, Fenske 2015).

six other clustering options and find that inference is similar across clustering protocols.[16]

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), black fathers have 0.683 more years of schooling at the threshold. In both cases, the estimates are significantly different from zero and suggest very strong first-stages, judging by the F-statistics. To put our results in perspective, Duflo's (2001) seminal study of Indonesia finds an increase of 0.12 to 0.19 years of schooling for each primary school constructed per 1,000 children. Her highest estimate is less than a third of the schooling impact of the Zimbabwean reforms. Table 2 also shows that our first-stage results are robust. In column (2), controlling for background parental characteristics, such as rainfall at birth, leaves the estimates under Panels A and B unchanged (we return to this issue in the next subsection).

As placebo tests, we run the original specification but for the sample of whites (without controls for rainfall) in column (3). Consistent with the visual evidence presented earlier, there is no effect of the reform on the population not targeted by the policy change. For white females, we even find a negative association that is statistically insignificant. This underscores the validity of our identification strategy and complements the findings of Agüero and Bharadwaj (2014), who combining data from the last three waves of the Demographic and Health Surveys for Zimbabwe, find no discontinuity in female height, and therefore infer that the reform affected the schooling attainments of parents but not their health. The authors also show that in neighboring countries, such as South Africa and Zambia, schooling

[16]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 A2 for more details.

does not display any discontinuity at age 15 in 1980, allowing them them to rule out regional changes in education around 1980.

In sum, we base our empirical strategy on the fact 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 (Agüero and Bharadwaj 2014). Indeed, on the strength of previous research and the new evidence presented here about white Zimbabweans, 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 the (standardized) years of schooling for children against the running variable, parent age, in the mother and father samples separately. Both CEFs reveal a clear jump at the threshold: children whose parents were 15 years old in 1980 display a *discontinuously* greater level of schooling compared to their counterparts whose parents were slightly older than 15 in 1980. Table 3 complements these figures and displays 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, 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 0.076 standard deviations in the schooling of her child. This effect translates into 0.20 additional years of child schooling (=0.076*2.60). For fathers, the effect is 0.115 standard deviations or equivalently, 0.29 (=0.115*2.48) extra years of child schooling. For both parents, the estimates are statistically different from zero at the one percent level. The implication that fathers have a larger effect than mothers is not uncommon in the literature, as reviews by Orazem and King (2007) and Schultz (2002) show. Holmlund, Lindahl, and Plug (2011) also find this pattern in most of the 17 studies they review.

Furthermore, our 2SLS estimates rank near the higher end of the scale reported in the literature and are clearly higher than the 0.10 years of increase in child schooling reported, for example, by Holmlund, Lindahl, and Plug (2011) for the Nordic countries.[17] As Behrman and Rosenzweig (2002) point out, an important mechanism explaining the magnitude of the effects lies in the labor and marriage market. We explore these in section 5.5.

5.3 Robustness checks

We conduct a number of robustness and specification checks, which are shown in Table 4. First, we address the concern that there could be unobserved background characteristics correlated with the timing of the reforms and influencing child schooling. A vast literature shows that early-life shocks often have long-lasting effects (Almond, Currie, and Herrmann

[17]For example, Black, Devereux, and Salvanes (2005) compute 2SLS estimates using exposure to a reform (Norway's raising of mandatory minimum schooling in 1959) as an instrument for the completed schooling of parents. In a sample from the lower end of the schooling distribution, where they expect to find the largest effects, the authors detect a highest effect of 0.12 years. However, Behrman and Rosenzweig (2002) find larger father schooling effects in their twins study: a spillover of 0.36 years compared to our 0.29 years. Their analysis even shows a negative effect for mothers, that is statistically significant only at the ten percent level.

2012, Currie and Vogl 2013). In the case of a mainly agrarian economy such as Zimbabwe, at-birth exposure to droughts or floods could slow or permanently reduce human capital accumulation. 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.

For Zimbabwe, Alderman, Hoddinott, and Kinsey (2006) find that some of the long-term consequences of early-life exposure to drought include lower schooling, greater delay in enrollment and poorer health. Hoddinott (2006) finds that droughts have an adverse impact on household assets. 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[18] 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 estimated impact on parental schooling as shown before in column (2) of Table 2. For the same reasons, controlling for rainfall in our 2SLS estimates does not alter our main findings either. This is shown in column (2) of Table 4.

We next consider other province-level unobservables that could be correlated with parental schooling. In column (3) of Table 4, we add province of birth fixed-effects to our original specification, and find that the intergenerational estimates remain highly significant and only marginally smaller. In column (4), we omit units at the threshold (i.e. parents aged fifteen in 1980) and re-run the original 2SLS specification. The estimated effects (0.070 SD for mothers and 0.108 SD for fathers) are largely unchanged in magnitude and significance with respect to our baseline estimates. In all our robustness checks, we continue to find a very

[18] We standardize annual rainfall values, collected from 38 stations across the country, by dividing deviations from the mean by the standard deviation, where both summary statistics are calculated for the period 1959-2001.

strong first-stage as shown by 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 5, 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. In column (3), omitting 14 and 15 year-olds in 1980 while retaining a linear polynomial in parent age results in slightly smaller estimates (from 0.076 to 0.073 SD for mothers and from 0.115 to 0.101 SD for fathers). 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 even larger intergenerational estimates.

5.4 Heterogeneous effects

We now examine heterogeneity in the intergenerational spillovers. In Table 6 we show the effects on daughters and sons separately. In all four parent-child pairings, we find a strong and statistically significant intergenerational transmission of schooling at the one percent level (and at the five percent for the father-son effect). While the point estimates suggest that the mother effect is larger for sons relative to daughters, and the opposite for fathers, we cannot reject the null hypothesis that the mother (and father) effects are the same for daughters and sons. This evidence suggests no systematic gender preferences in the schooling transmission.

Figure 6 explores whether the transmission varies with the child's age. For mothers, the top panel shows that the effect is concentrated in two age groups: 7-10 and 12-14. For fathers, we find clear impacts when children are between 9 and 12 years of age (bottom panel). We continue to observe that the effect is larger for fathers than for mothers. However, in Zimbabwe, school enrollment peaks at the age of 11, so while the intergenerational spillover is smaller for mothers, it could be argued that its impact occurs at a more critical period compared to fathers. These differences also suggest a "complementarity" in the timing of the schooling effects and 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.

5.5 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 and 8 provide a visual analysis of these pathways for mothers and fathers, respectively.

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 7, 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). Yet, when exploring whether 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 7, 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 0.48 more years for his partner's, and again, a 13 percent (=0.078/0.6) greater probability that his partner's educational attainment is beyond primary school. All of these estimates are statistically significant at the one percent level.

The literature also suggests that education strengthens the bargaining power of women by increasing their options outside the household through the labor market (Thomas 1990, Duflo 2012). 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.[19] However, the census allows us to

[19]The lack of labor force surveys and longitudinal household data in Zimbabwe is also

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, neither mothers nor fathers show any schooling effects on their workforce participation (column 5) or probability of being a paid worker (column 6). 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 thus, a very small margin to be affected by schooling. The result is not much of 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. Thus, we do not have enough evidence to reject or confirm the mechanism of an improvement in the labor market or outside options. Furthermore, Grépin and Bharadwaj (2015) do not find an effect on women's empowerment using the reform.[20] Taken together, these results suggest that schooling brought little increase in intra-household bargaining power as measured by certain labor market outcomes.

Finally, 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

---

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.

[20]The authors construct an index of empowerment by combining questions about whether women alone, jointly with their partners, or their partners alone had some say over important decisions within the household. For each variable, the authors labeled women as lacking empowerment when their spouse alone made the decision. See Grépin and Bharadwaj (2015) for details.

impact of maternal schooling on child welfare (Summers 1992, Behrman 1997, Schultz 2007). The last two columns of Table 7 provide evidence in support of this view. In column (7), 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 (8), we show that each extra year of education reduces the number of children women bore by 4 percent. Thus, schooling allows women to postpone childbearing, and make a quantity-quality trade-off by having fewer but more-educated children. In sum, the behavioral responses in the marriage market and in fertility emerge as the most prominent pathways in the intergenerational transmission of parental schooling in Zimbabwe.

5.6 Quantity and quality of education

Evaluating 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), post-Independence, growth in teacher-staff failed to keep pace with growth in enrollment. The proportion of qualified teachers also declined. The share of untrained teachers in secondary schools went from almost zero in 1980 to 17 percent in 1984 and to 28 percent in 1988. A significant number of secondary schools had also been built as extensions of existing primary schools. Schools adopted "hot seating" (multiple shifts), where the length of the school day was reduced to accommodate a larger number of students. The curriculum was never fully redesigned to reflect the needs and economic opportunities of a post-colonial society (Nhundu 1992)

The reforms allowed unobstructed progress through eleven grades until Form IV of secondary school. At the end of Form IV, students took their O-level exams, and had to pass five different subjects to gain admission to lower form VI. The O-level pass rates provide an insight into the decline in education quality and learning outcomes. In 1981, 71 percent of the exam-takers passed the O-levels, and only two percent of the failing group did not pass a single subject. In 1984, the first cohort to complete secondary school under the reforms took the O-levels; just 22 percent passed all five subjects while 38 percent of the failing group failed in all five subjects. By 1988, only ten percent of test-takers passed the O-levels, and 43 percent of the failing group failed all five subjects. Nhundu (1992) reports that between 1981 and 1988, the number of O-level takers grew by an incredible 2,253 percent and the failure rate increased by 7,220 percent. Although black Zimbabweans taking the O-levels pre- and post-reform faced very different political and school environments, the cohorts who attended secondary school entirely post-reform show successively lower pass-rates at the O-levels.

Judged by teacher quality or by O-level pass-rates, the evidence suggests a constant deterioration of education quality post-reform. One could make the case that the quality of education did not decline *discontinuously* the same instant the reforms expanded secondary schooling opportunities to blacks. If the decline was *smooth*, the fifteen year-old just transitioning to secondary school in 1980 would have been exposed to a similar quality of education as the fifteen year-old in 1979. Recall from our first-stage that the reforms increased completed schooling by little less than one year at the threshold. Thus, in terms of quality, students just above and below the threshold probably experienced a very comparable quality of education. If this is somehow not the case, our estimates of the intergenerational spillover would be lower-bounds to the effects that could have been obtained if quality held constant.

6 Conclusion

The recent literature on intergenerational transmissions of schooling has collected a range of causal estimates, largely from the developed world. Yet, it is in developing countries that this knowledge is more urgent because the size of the parent-to-child schooling transmission is crucial to the design of policies meant to narrow intra- and intergenerational inequalities. Our paper presents causal estimates of the schooling transmission in the context of a poor country and a population that was systematically deprived of access to education for several generations. The latter characteristic allows our estimates to carry a larger external validity as the set of compliers (those whose behavior is affected by the reform) approaches the population at large.

Exploiting the regression discontinuity design spawned by the 1980 reforms, we estimate an intergenerational spillover of 0.20 years from mothers and 0.29 years from fathers. Both estimates are larger than the 0.10 years of increase in child schooling reported, for example, by Holmlund, Lindahl, and Plug (2011) for the Nordic countries. An important contribution of our paper to the broader literature is the exploration of a number of mechanisms to explain the intergenerational spillover. We uncover that, unlike the labor market, the marriage market is an important channel in the transmission of the parental schooling effect. A high degree of assortative mating on schooling attainment, combined with the outcomes of delayed childbearing and smaller families suggests that more educated couples are able to reap larger marriage surpluses. Therefore, evaluations of CCTs must explicitly consider the long-term contribution of assortative mating and its multiplier effects. The design of future policies that seek to advance economic mobility and reduce inequality must explore how to take advantage of the intergenerational benefits of assortative mating. Such a design remains a pending question for future research.

References

Agüero, J. M., and P. Bharadwaj (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.

Alderman, H., J. Hoddinott, and B. Kinsey (2006): "Long term consequences of early childhood malnutrition," *Oxford Economic Papers*, 58(3), 450–474.

Almond, D., J. Currie, and M. Herrmann (2012): "From infant to mother: Early disease environment and future maternal health," *Labour Economics*, 19(4), 475–483.

Amin, V., and J. Behrman (2014): "Do more-schooled women have fewer children and delay childbearing? Evidence from a sample of US twins," *Journal of Population Economics*, 27(1), 1–31.

Amin, V., P. Lundborg, and D.-O. Rooth (2015): "The intergenerational transmission of schooling: Are mothers really less important than fathers?," *Economics of Education Review*, 47, 100 – 117.

Atkinson, N. (1972): *Teaching Rhodesians: A History of Educational Policy in Rhodesia.* Longman, London.

Barrera-Osorio, F., L. L. Linden, and J. E. Saavedra (2015): "Long Term Educational Consequences of Alternative Conditional Cash Transfer Designs: Experimental Evidence from Colombia," *Unpublished manuscript.*

Beegle, K., L. Christiaensen, A. Dabalen, and I. Gaddis (2016): *Poverty in a rising Africa.* World Bank Publications.

Behrman, J. R. (1997): "Mother's Schooling and Child Education: A Survey," Discussion Paper PIER Working Paper 97-025, Penn Institute for Economic Research.

——— (2010): *Investment in Education Inputs and Incentives*, vol. 5 of *Handbook of Development Economics*. Elsevier.

Behrman, J. R., S. W. Parker, and P. E. Todd (2011): "Do conditional cash transfers for schooling generate lasting benefits? A five-year followup of PROGRESA/Oportunidades," *Journal of Human Resources*, 46(1), 93–122.

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

Behrman, J. R., W. Schott, S. Mani, B. T. Crookston, K. Dearden, L. T. Duc, L. C. H. Fernald, and A. D. Stein (forthcoming): "Intergenerational Transmission of Poverty and Inequality: Parental Resources and Schooling Attainment and Childrens Human Capital in Ethiopia, India, Peru, and Vietnam," *Economic Development and Cultural Change.*

Björklund, A., M. Lindahl, and E. Plug (2006): "The origins of intergenerational associations: Lessons from Swedish adoption data," *The Quarterly Journal of Economics*, pp. 999–1028.

Björklund, A., and K. G. Salvanes (2011): *Education and Family Background: Mechanisms and Policies*, vol. 3 of *Handbook of the Economics of Education*. Elsevier.

Black, S. E., and P. J. Devereux (2011): *Recent Developments in Intergenerational Mobility*, vol. 4 of *Handbook of Labor Economics*. Elsevier.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2005): "Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital," *American Economic Review*, 95(1), 437–449.

Charles, K. K., E. Hurst, and A. Killewald (2013): "Marital sorting and parental wealth," *Demography*, 50(1), 51–70.

Chevalier, A., C. Harmon, V. O'Sullivan, and I. Walker (2013): "The impact of parental income and education on the schooling of their children," *IZA Journal of Labor Economics*, 2(1), 1–22.

Chidzero, B. T. (1977): *Education and the Challenge of Independence*. International University Exchange Fund, Geneva.

Currie, J., and T. Vogl (2013): "Early-Life Health and Adult Circumstance in Developing Countries," *Annual Review of Economics*, 5(1), 1–36.

de Walque, D. (2009): "Parental Education and Children's Schooling Outcomes: Evidence from Recomposed Families in Rwanda," *Economic Development and Cultural Change*, 57(4), 723–746.

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

Duflo, E. (2001): "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment," *American Economic Review*, 91(4), 795–813.

——— (2012): "Women Empowerment and Economic Development," *Journal of Economic Literature*, 50(4), 1051–79.

Edwards, G. (1995): "Rapid educational expansion and primary school efficiency in Zimbabwe, 1983-9," *Development Southern Africa*, 12(1), 87–95.

Edwards, G., and C. Tisdell (1990): "Post-independence Trends in Education in Zimbabwe," *South African Journal of Economics*, 58(4), 298–307.

Edwards, R. D., and J. Roff (2016): "What mom and dads match means for junior: Marital sorting and child outcomes," *Labour Economics*, 40, 43–56.

Fafchamps, M., and A. Quisumbing (2005): "Assets at marriage in rural Ethiopia," *Journal of Development Economics*, 77(1), 1–25.

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

Fenske, J. (2015): "African polygamy: Past and present," *Journal of Development Economics*, 117, 58–73.

Ferreira, F. H., J. Messina, J. Rigolini, L.-F. López-Calva, M. A. Lugo, R. Vakis, L. F. Ló, et al. (2012): *Economic mobility and the rise of the Latin American middle class*. World Bank Publications.

Filmer, D., and N. Schady (2014): "The medium-term effects of scholarships in a low-income country," *Journal of Human Resources*, 49(3), 663–694.

Fiszbein, A., N. Schady, F. H. Ferreira, M. Grosh, N. Keleher, P. Olinto, and E. Skoufias (2009): *Conditional Cash Transfers : Reducing Present and Future Poverty*, no. 2597 in World Bank Publications. The World Bank.

Foster, A. D. (2002): "Marriage-market selection and human capital allocations in rural Bangladesh," Discussion paper, Manuscript. Providence, RI: Brown Univ., Dept. Econ.

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

Grossman, M. (2015): "The relationship between health and schooling: What?s new?," Discussion paper, National Bureau of Economic Research.

Hahn, J., P. Todd, and W. Van der Klaauw (2001): "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design," *Econometrica*, 69(1), 201–09.

Haveman, R., and B. Wolfe (1995): "The Determinants of Children's Attainments: A Review of Methods and Findings," *Journal of Economic Literature*, 33(4), 1829–1878.

Hoddinott, J. (2006): "Shocks and their consequences across and within households in Rural Zimbabwe," *The Journal of Development Studies*, 42(2), 301–321.

Holmlund, H., M. Lindahl, and E. Plug (2011): "The Causal Effect of Parents' Schooling on Children's Schooling: A Comparison of Estimation Methods," *Journal of Economic Literature*, 49(3), 615–51.

Imbens, G. W., and T. Lemieux (2008): "Regression discontinuity designs: A guide to practice," *Journal of Econometrics*, 142(2), 615–635.

Kalil, A., M. Mogstad, M. Rege, and M. E. Votruba (forthcoming): "Father presence and the intergenerational transmission of educational attainment," *Journal of Human Resources*.

Kanyongo, G. Y. (2005): "Zimbabwe's public education system reforms: Successes and challenges," *International Education Journal*, 6(1), 65–74.

Lee, D. S., and D. Card (2008): "Regression discontinuity inference with specification error," *Journal of Econometrics*, 142(2), 655–674.

Levy, S. (2007): *Progress against poverty: sustaining Mexico's Progresa-Oportunidades program*. Brookings Institution Press.

Maccini, S., and D. Yang (2009): "Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall," *American Economic Review*, 99(3), 1006–26.

Mammen, K., and C. Paxson (2000): "Women's Work and Economic Development," *Journal of Economic Perspectives*, 14(4), 141–164.

Montenegro, C. E., and H. A. Patrinos (2014): "Comparable estimates of returns to schooling around the world," Policy Research Working Paper Series 7020, The World Bank.

Nhundu, T. J. (1992): "A Decade of Educational Expansion in Zimbabwe: Causes, Consequences, and Policy Contradictions.," *Journal of Negro Education*, 61(1), 78–98.

O'Callaghan, M., and R. Austin (1977): *Southern Rhodesia: The Effects of a Conquest Society on Education, Culture and Information.* Syndenhams Printers, Dorset.

Orazem, P. F., and E. M. King (2007): "Schooling in developing countries: The roles of supply, demand and government policy," *Handbook of development economics*, 4, 3475–3559.

Oreopoulos, P. (2006): "Estimating average and local average treatment effects of education when compulsory schooling laws really matter," *The American Economic Review*, 96(1), 152–175.

Oreopoulos, P., M. E. Page, and A. H. Stevens (2006): "The Intergenerational Effects of Compulsory Schooling," *Journal of Labor Economics*, 24(4), pp. 729–760.

Piopiunik, M. (2014): "Intergenerational Transmission of Education and Mediating Channels: Evidence from a Compulsory Schooling Reform in Germany," *The Scandinavian Journal of Economics*, 116(3), 878–907.

Plug, E. (2004): "Estimating the Effect of Mother's Schooling on Children's Schooling Using a Sample of Adoptees," *American Economic Review*, 94(1), 358–368.

Pronzato, C. (2012): "An examination of paternal and maternal intergenerational transmission of schooling," *Journal of Population Economics*, 25(2), 591–608.

Richardson, C. J. (2007): "How much did droughts matter? Linking rainfall and GDP growth in Zimbabwe," *African Affairs (London)*, 106(424), 463–478.

Riddell, A. R., and L. M. Nyagura (1991): "What causes differences in achievement in Zimbabwe's secondary schools?," Policy Research Working Paper Series 705, The World Bank.

Riddell, R. (1980): *Education for employment*. Mambo Press, Gweru, Zimbabwe.

Sacerdote, B. (2002): "The Nature and Nurture of Economic Outcomes," *American Economic Review*, 92(2), 344–348.

——— (2007): "How large are the effects from changes in family environment? A study of Korean American adoptees," *The Quarterly Journal of Economics*, pp. 119–157.

Schady, N., J. Behrman, M. C. Araujo, R. Azuero, R. Bernal, D. Bravo, F. Lopez-Boo, K. Macours, D. Marshall, C. Paxson, et al. (2015): "Wealth gradients in early childhood cognitive development in five Latin American countries," *Journal of Human Resources*, 50(2), 446–463.

Schultz, T. (2007): "Fertility in Developing Countries," Working papers, Economic Growth Center, Yale University.

Schultz, T. P. (2002): "Why governments should invest more to educate girls," *World Development*, 30(2), 207–225.

Schultz, T. P. (2004): "Evidence of Returns to Schooling in Africa from Household Surveys: Monitoring and Restructuring the Market for Education," *Journal of African Economies*, 13(02), ii95–ii14.

Summers, L. (1992): "Investing in all the people," Working Papers 905, The World Bank.

Thomas, D. (1990): "Intra-Household Resource Allocation: An Inferential Approach," *Journal of Human Resources*, 25(4), 635–664.

UNICEF (2011): "Education in Emergencies and Post-crisis Transition," 2010 report evaluation, United Nations International Children's Education Fund.

van der Klaauw, W. (2002): "Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach," *International Economic Review*, 43(4), 1249–1287.

Vijverberg, W. (1992): "Measuring Income from Family Enterprises with Household Surveys," Working papers, World Bank - Living Standards Measurement.

Zimmerman, F. J. (2003): "Cinderella Goes to School The Effects of Child Fostering on School Enrollment in South Africa," *Journal of Human Resources*, 38(3), 557–590.

Zvobgo, C. J. M. (1981): "African Education in Zimbabwe: The Colonial Inheritance of the New State, 1899-1979," *Issue: A Journal of Opinion*, 11(3/4), 13–16.

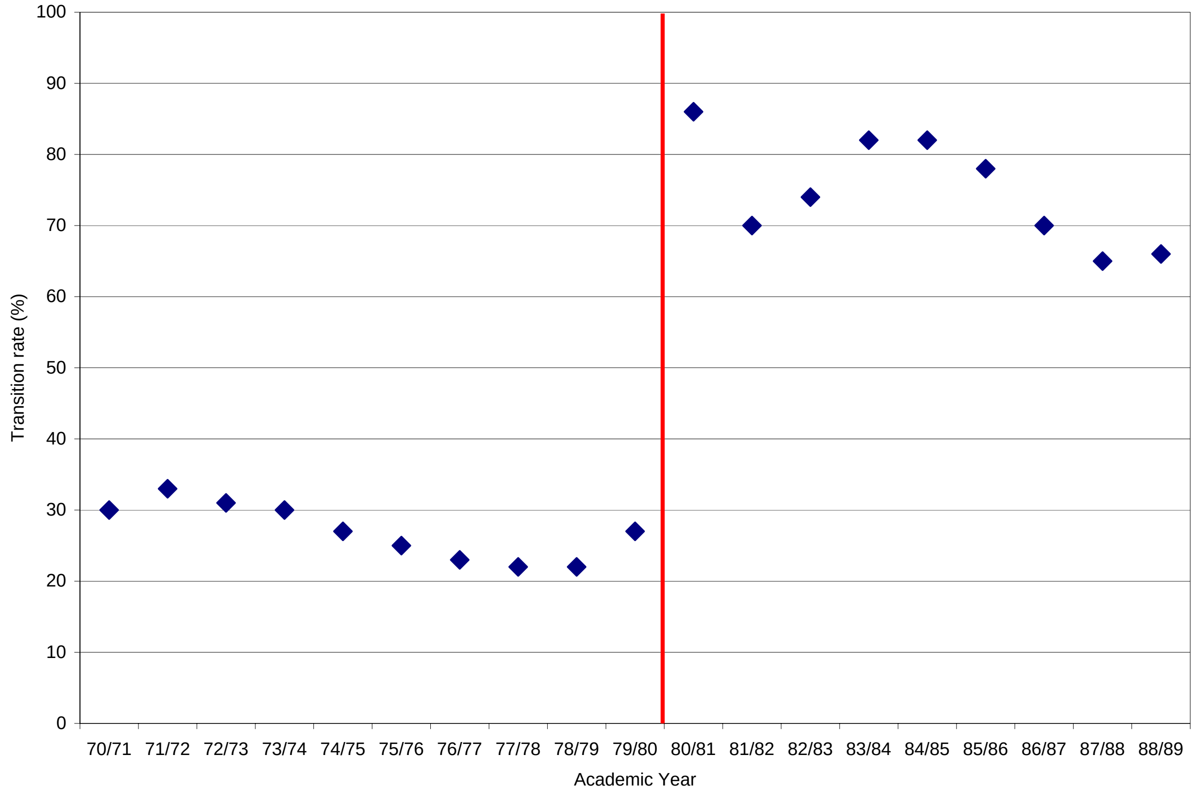


Figure 1: Primary to secondary transition rates: 1970/71-1988/89

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

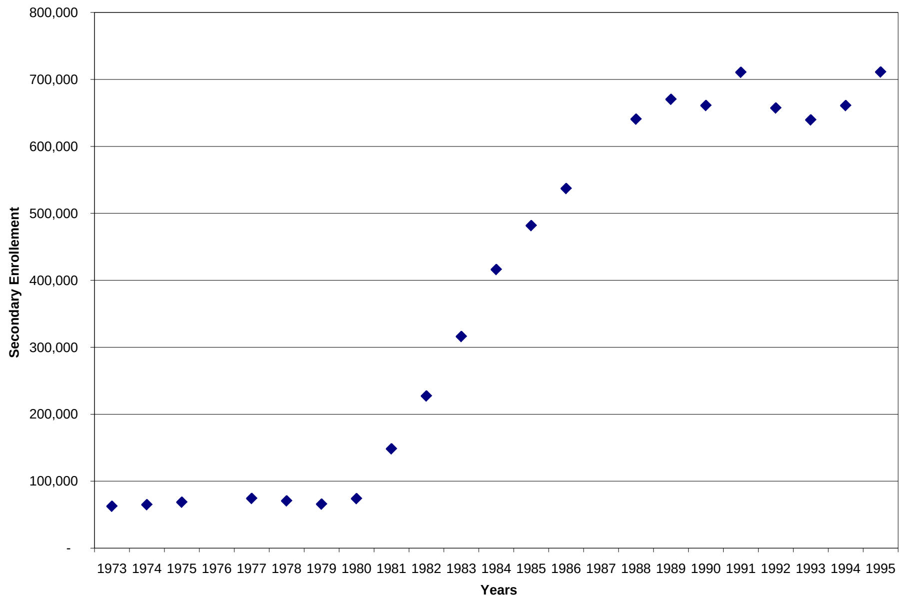


Figure 2: Annual secondary school enrollment in Zimbabwe: 1973-1995

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

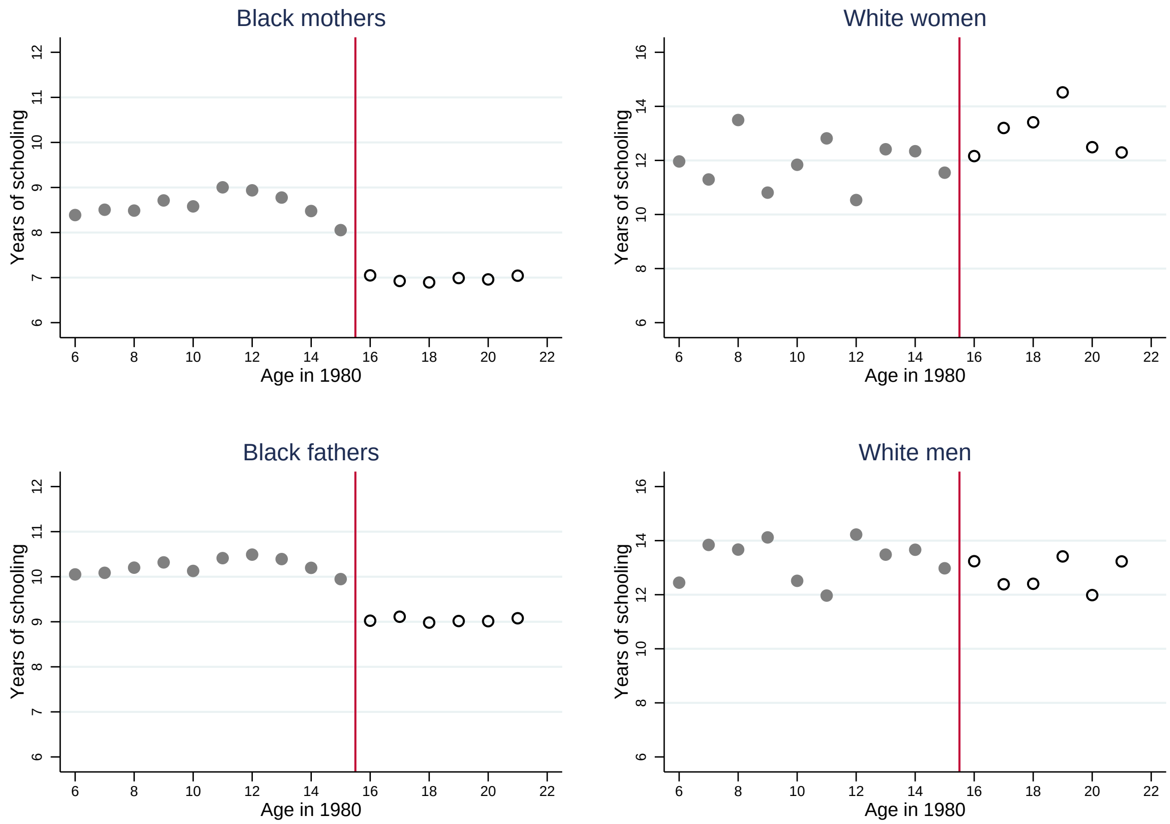
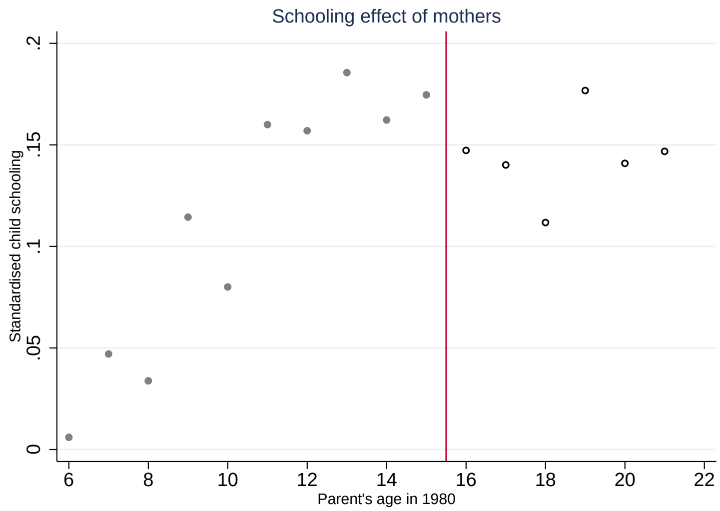
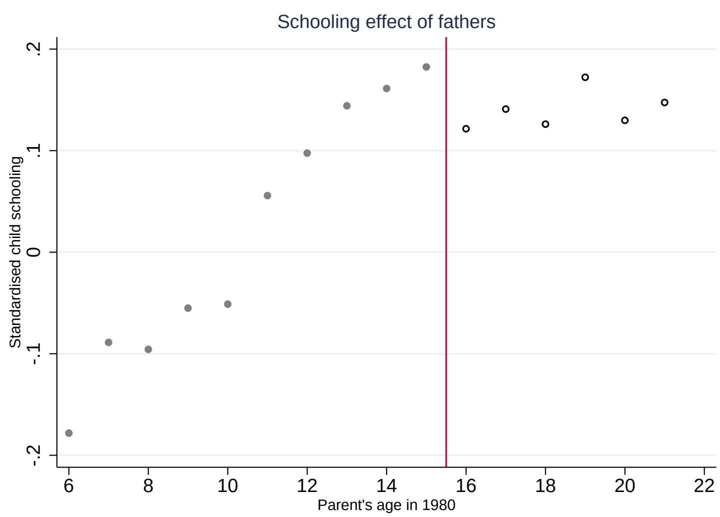


Figure 3: Schooling attainment by age in 1980

Note: in each figure, circles show average years of de-trended schooling in samples of Zimbabweans aged six through twenty-one in 1980. The trend in parental schooling was removed by regressing it on linear splines in parent age on either side of the threshold (age 15 in 1980; vertical line).



(a) sample of child-mother pairs



(b) sample of child-father pairs

Figure 4: Mean child schooling score by parent cohort

Note: in each figure, circles show average z-scores of child schooling. The z-scores were calculated using age-specific means and standard deviations of child schooling years. The sample in figure 4a pairs black mothers aged 6–21 in 1980 with their children aged 6–15 in 2002; figure 4b does likewise with black fathers and children.

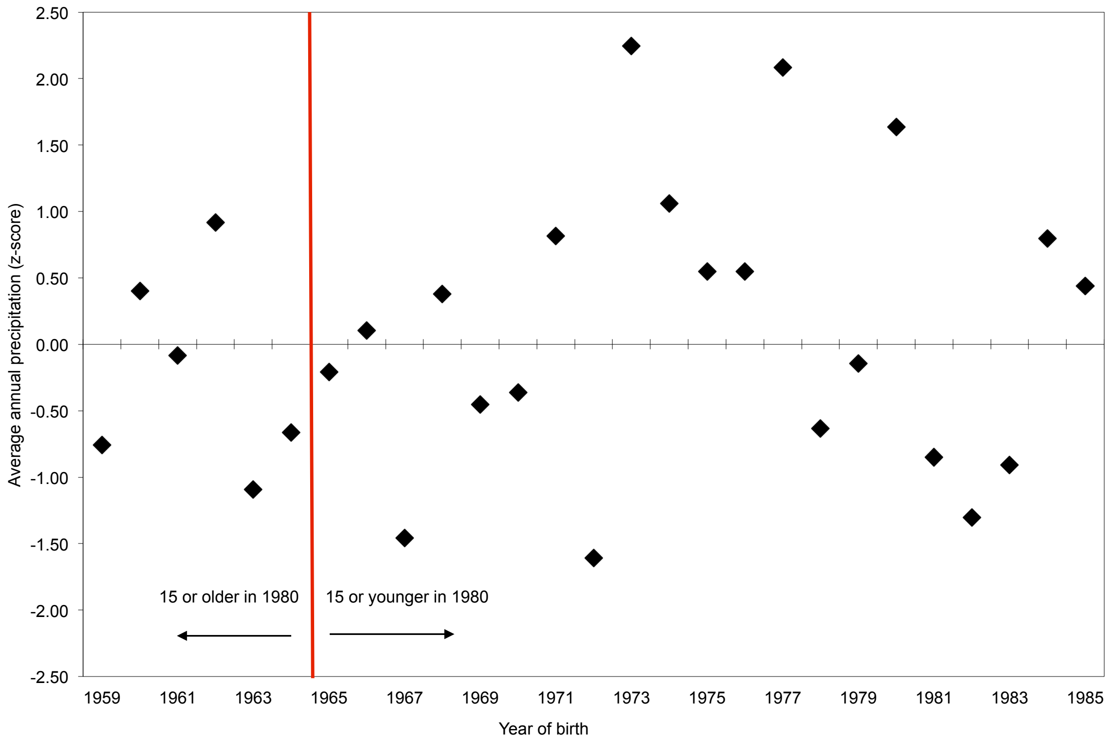


Figure 5: Annual rainfall: 1959-1985

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.

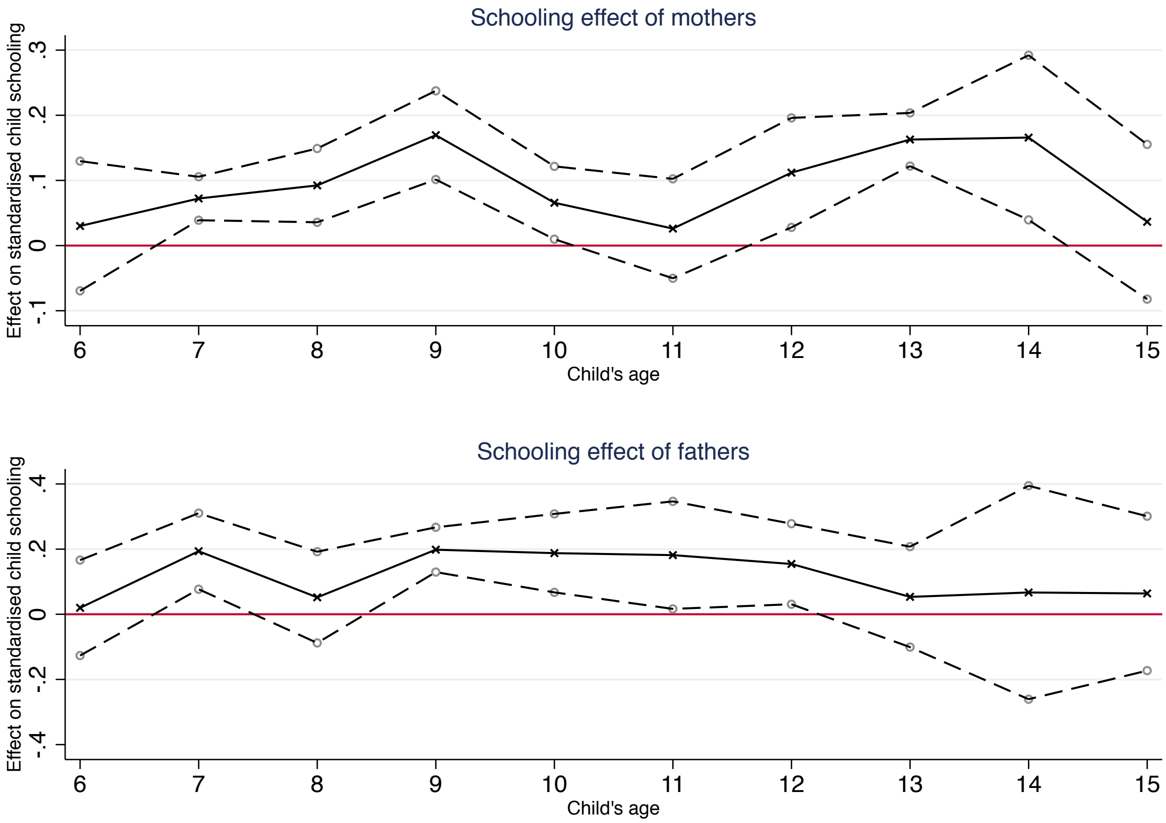
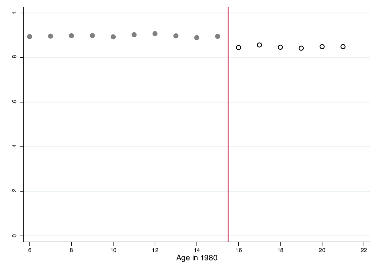
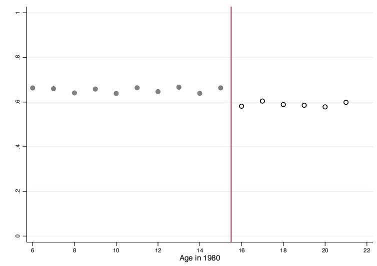


Figure 6: Heterogeneity in the effects of parental schooling

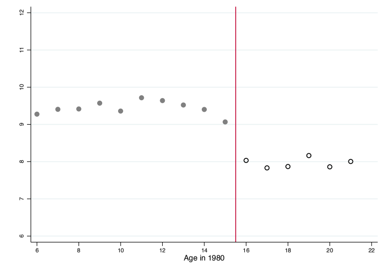
Note: each figure shows the 2SLS estimates of the parental schooling on their children schooling (z-score), separately for each age cohort of children (solid line). The dashed lines indicates the 95% robust confidence intervals clustered by parent year of birth. All regressions include linear splines in parent age in 1980 on both sides of the discontinuity and the child's sex. Parental schooling is instrumented with the discontinuity at age 15 in 1980. The samples contain children aged 6 to 15 in 2002 and born to black parents aged 6 to 21 in 1980.



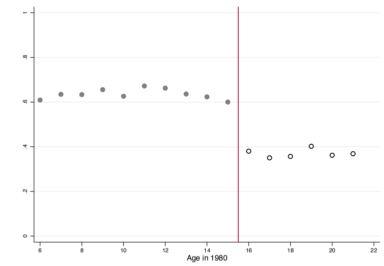
(a) Is married



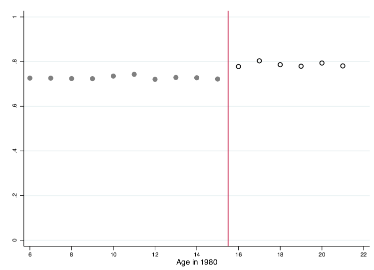
(b) Partner co-resides



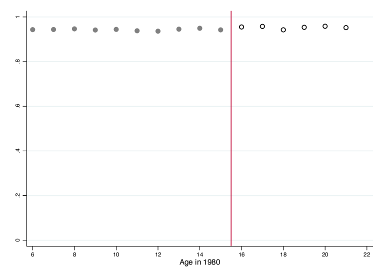
(c) Partner's years of schooling



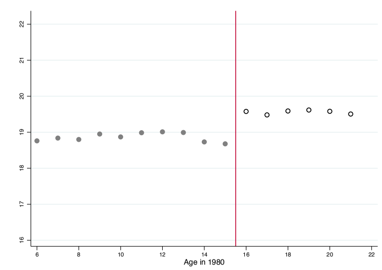
(d) Partner has secondary+ schooling



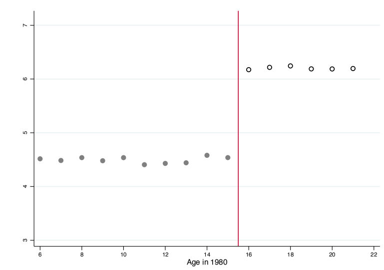
(e) Participates in the labour force



(f) Has paid work



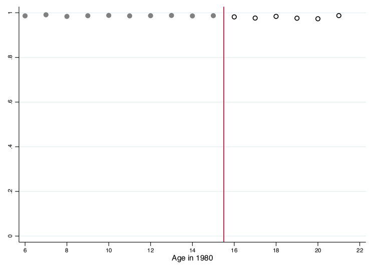
(g) Age at first birth



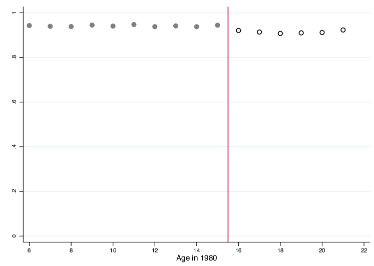
(h) Number of children borne

Figure 7: Pathways for the intergenerational transmission of schooling: mothers

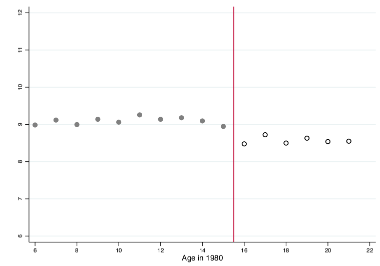
Note: in each figure, circles represent outcome means obtained by regressing the outcome on a linear spline in mother's age on either side of the cut-off (vertical line at age 15); the sample includes sixteen cohorts of black mothers in the ages of 6 through 21 years in 1980.



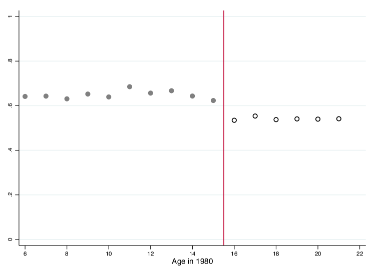
(a) Is married



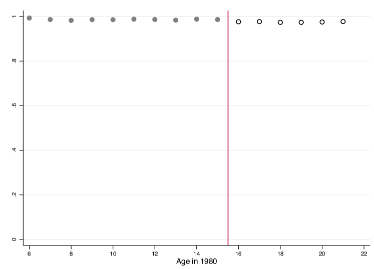
(b) Partner co-resides



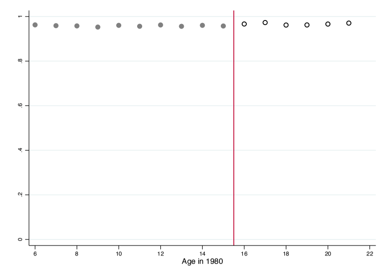
(c) Partner's years of schooling



(d) Partner has secondary+ schooling



(e) Participates in the labour force



(f) Has paid work

Figure 8: Pathways for the intergenerational transmission of schooling: fathers

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 in school	1.0	0.2	0	1
Child's years of schooling	3.5	2.6	0	10
Child's standardized schooling	0.1	1.0	-4.9	2.8
Sample: 50026 black fathers				
Father's age	36.73	4.13	28	43
Father's years of schooling	9.67	3.27	0	16
Child's age	9.73	2.67	6	15
Daughters	0.5	0.5	0	1
Child is currently in school	0.98	0.15	0	1
Child's years of schooling	2.96	2.48	0	10
Child's standardized schooling	0.09	0.99	-4.89	2.76

Note: each sample includes black parents aged 6 to 21 in 1980, who are either household heads or the spouse of a household head, with children in the ages of 6 to 15 in the 2002 population census.

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

	Dependent variable: Parent's schooling		
	Blacks parents	*Blacks parents*	*Whites*
	[1]	[2]	[3]
Panel A. Females			
$1\{A_i \leq 15\}$	0.819***	0.816***	-0.761
	[0.216]	[0.210]	[0.762]
Control for rain?	No	Yes	No
F test	14.44	15.10	1.00
p value	0.002	0.001	0.334
Observations	91480		238
Panel B. Males			
$1\{A_i \leq 15\}$	0.683***	0.682***	0.519
	[0.133]	[0.123]	[0.491]
Control for rain?	No	Yes	No
F test	26.40	30.81	1.12
p value	0.000	0.000	0.307
Observations	50026		241

Note: each cell represents an estimate of the reform effect 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 is measured as precipitation z-scores using the mean over 1959–2001.
* indicates statistical significance at 10%, ** at 5% and *** at 1%.

Table 3: THE INTERGENERATIONAL EFFECTS OF SCHOOLING

	Dependent variable: Child's schooling (z-score)	
	OLS	2SLS
	[1]	[2]
Panel A. child-mother sample		
Mother's schooling	0.094***	0.076***
	[0.002]	[0.012]
F test		14.05
p value		0.002
Observations	91480	
Panel B. child-father sample		
Father's schooling	0.094***	0.115***
	[0.003]	[0.011]
F test		24.79
p value		0.000
Observations	50026	

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 and children's age and sex (coefficients not shown). The sample is restricted to black Zimbabweans. 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 4: ROBUSTNESS CHECKS (2SLS)

	Dependent variable: Child's schooling (z-score)			
	Baseline estimate [1]	Rainfall in birth-year [2]	Province fixed effects [3]	Omitting 15-year-olds [4]
Panel A. child-mother sample				
Mother's schooling	0.076***	0.076***	0.068***	0.070***
	[0.012]	[0.013]	[0.013]	[0.010]
F test	14.05	14.75	13.25	30.21
p value	0.002	0.002	0.002	0.000
Observations		91480		86505
Panel B. child-father sample				
Father's schooling	0.115***	0.115***	0.096**	0.108***
	[0.011]	[0.011]	[0.011]	[0.011]
F test	24.79	28.59	22.52	48.71
p value	0.000	0.000	0.000	0.000
Observations		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 and children's age and sex (coefficients not shown). The sample is restricted to black Zimbabweans. 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 5: ADDITIONAL SPECIFICATION TESTS: AGE SPAN, SAMPLE SIZE AND POLYNOMIALS IN AGE SPLINES (2SLS)

	Dependent variable: Child's schooling (z-score)							
Parent age-span	6–21				0–30			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel A. child-mother sample								
Mother's schooling	0.076***	0.101	0.073***	0.101***	0.051***	0.103***	0.059***	0.090***
	[0.012]	[0.075]	[0.010]	[0.032]	[0.013]	[0.025]	[0.010]	[0.018]
F test	14.05	4.44	80.65	16.51	24.20	9.52	132.57	50.09
p value	0.002	0.052	0.000	0.001	0.000	0.005	0.000	0.000
Observations	91,480	91,480	80,370	80,370	120,538	120,538	109,428	109,428
Panel B. child-father sample								
Father's schooling	0.115***	0.202***	0.101***	0.197***	0.023	0.277***	0.022	0.246***
	[0.011]	[0.037]	[0.015]	[0.025]	[0.021]	[0.057]	[0.022]	[0.033]
F test	24.79	17.60	148.04	22.13	36.03	9.91	112.49	35.47
p value	0.000	0.000	0.000	0.000	0.000	0.004	0.000	0.000
Observations	50,026	50,026	43,395	43,395	78,864	78,864	72,233	72,233
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 and children's age and sex (coefficients not shown). The sample is restricted to black Zimbabweans. 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: INTERGENERATIONAL EFFECTS BY CHILD'S SEX (2SLS)

	Dependent variable: Child's schooling (z-score)	
	Daughters	Sons
	[1]	[2]
Panel A. child-mother sample		
Mother's schooling	0.053***	0.097***
	[0.013]	[0.025]
F test	112.44	314.95
p value	0.000	0.000
Observations	45718	45762
Panel B. child-father sample		
Father's schooling	0.136***	0.093**
	[0.026]	[0.023]
F test	32.86	16.54
p value	0.000	0.001
Observations	24808	25218

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 children's age (not shown). The sample is restricted to black Zimbabweans. 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%.

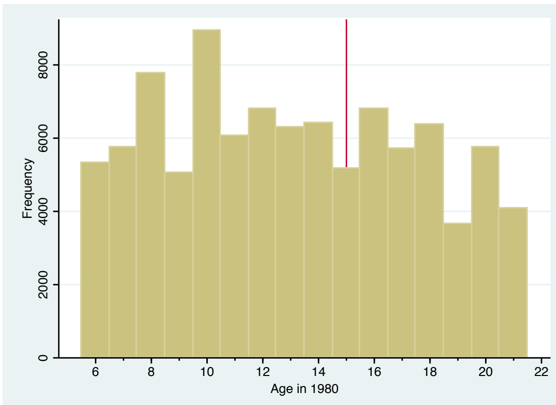
Table 7: PATHWAYS OF THE INTERGENERATIONAL TRANSMISSION OF SCHOOLING (2SLS)

Dependent variable:	Parent is married [1]	Partner co-resides [2]	Partner years of schooling [3]	Partner has secondary or higher schooling [4]	Parent participates in the labour force [5]	Parent has paid work [6]	Mother's age at first birth [7]	Number of children born to mother [8]
Panel A. child-mother sample								
Mother's schooling	-0.011	-0.015	0.561***	0.072***	-0.009	0.003	0.561***	-0.190***
	[0.008]	[0.013]	[0.059]	[0.009]	[0.012]	[0.006]	[0.070]	[0.036]
F test	14.05	14.05	17.53	17.53	10.05	10.50	14.05	14.05
p value	0.002	0.002	0.000	0.000	0.002	0.006	0.002	0.002
Observations	91,480	91,480	55,661	55,661	91,480	68,451	91,480	91,480
Panel B. child-father sample								
Father's schooling	0.013**	0.013	0.481***	0.078***	-0.001	-0.003		
	[0.006]	[0.009]	[0.118]	[0.009]	[0.002]	[0.006]		
F test	24.79	24.79	21.72	21.72	24.79	22.96		
p value	0.000	0.000	0.000	0.000	0.000	0.000		
Observations	50,026	50,026	44,767	44,767	50,026	49,090		

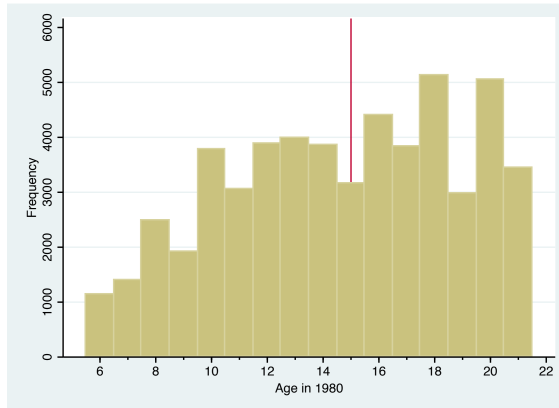
Note: each cell represents a separate regression; robust standard errors clustered by age in 1980 appear in brackets. All regressions include linear splines in parent age in 1980 and children's age and sex (coefficients not shown). The sample is restricted to black Zimbabweans. The 2SLS regressions instrument 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%.

Online Appendix: not for publication



(a) black mothers



(b) black fathers

Figure A1: HISTOGRAMS OF PARENT AGE IN 1980

Notes: the samples in figure A1a and in figure A1b are is 91,480 black mothers and 50,026 black fathers respectively. The horizontal axis shows parent age in 1980, where age fifteen represents the discontinuity and treatment threshold created by the reforms.

Table A1: Further summary statistics on black and white sub-populations

Variable	Mean	SD	Min	Max	Observations
Sample: black mothers					
Mother 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
Mother participates in labour force	0.75	0.43	0	1	91480
Mother has paid work	0.95	0.22	0	1	68451
Mother's 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
Child has a birth certificate	0.81	0.39	0	1	91480
Sample: black fathers					
Father 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
Father participates in labour force	0.98	0.14	0	1	50026
Father has paid work	0.96	0.19	0	1	49090
Child has a birth certificate	0.78	0.41	0	1	50026
Sample: white women					
Age	36.15	4.61	28	43	238
Years of schooling	12.42	2.96	3	16	238
Woman's age at first birth	23.95	4.98	14	39	238
Children born to the woman	2.51	1.32	1	10	238
Sample: white men					
Age	36.31	4.66	28	43	241
Years of schooling	13.08	2.67	2	16	241

Notes: the table shows descriptive statistics for children aged between 6-15 born to parents aged 28-43 in 2002 (i.e. six through twenty-one in 1980). The adults in all four samples are either household heads or the spouse of a household head.

Table A2: 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.