

# **You're Now Free to Move About the Country: The Effect of Schooling on Migration**

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Preliminary. Comments welcome.

## Abstract

Economic development requires the transformation of the spatial organization of a country. This paper shows that education helps achieve such transformation by inducing geographical mobility. I exploit a fuzzy regression discontinuity design induced by a school reform in Zimbabwe that discontinuously affected 14 year olds vis-à-vis 15 years olds in 1980. I show that one additional year of schooling, as brought by the reform, is associated with a higher migration probability (7.6%) and with an increase in the migration towards the largest cities (8.2%). The effects are even bigger for the rural-to-urban migration: increasing their propensity by 14.2%. Several robustness checks validate these findings, including placebo tests for populations not affected by the reforms: white Zimbabwean and natives in seven other African countries. These results suggest that education facilitates economic development by opening up new labor market opportunities through migration, especially away from rural areas and to larger cities.

Keywords: Mobility, Education, Labor Markets, Zimbabwe.

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## 1. Introduction

Economic development is often described as a process that involves the reallocation of the factors of production from a traditional sector characterized by low productivity, decreasing returns and mostly agrarian to a modern sector with high productivity, increasing returns and mostly industrial. A fundamental part of this reallocation of implies a *spatial* transformation: the migration of a large number of individuals from rural to urban areas and to bigger cities (Bardhan and Udry, 1999). In this paper, I explore the role of education on facilitating geographical mobility.

From a theoretical point of view there are several reasons why schooling could increase internal migration. Migration can be modeled as the outcome of an optimal search process where individuals know their wage in their current location but in order to learn about their *specific* wages in another area, they need to move, at some cost (Sjastaad, 1962; Kennan and Walker, 2011). In such a model, more education facilitates information gathering about the wage distribution in the targeted areas, reducing the uncertainty about expected gains from moving (Rosenzweig, 1995). Education could ease the payment of the migration cost by reducing liquidity constraints, through its income or wealth effect and therefore affecting the type of migration (Kleemans, 2014). It could also help escape geographical poverty traps (Jalan and Ravallion, 2002; Beegle, DeWeerdt, and Dercon, 2011) and it could make workers more attractive for “national” labor market rather than just the local market (Machin et al, 2013).

Furthermore, unlike most pro-migration policies, schooling also increases the human capital of potential migrants and raises their expected earnings (e.g., via a higher mean or better placement in the income distribution of the targeted area). Indeed, the

work by Lagakos and Waugh (2013) suggests that schooling-induced migration could favor sending *and* receiving areas. In their model, the low agricultural productivity in developing countries is explained by the misallocation of workers that, despite having a low productivity working in agriculture, remain in that sector to satisfy a subsistence food requirement. Thus, an exogenous increase in the productivity of workers “trapped” in agriculture –via schooling, for example– would allow them to choose non-agricultural work and move to urban areas where their (new) skills are more valuable and at the same time, increase agriculture productivity in the sending areas by correcting the misallocation of talent. Therefore, schooling could open up new opportunities in the labor market by facilitating migration –mainly to urban or higher-income areas– while promoting overall economic development.

However, schooling and migration are both investment decisions so it is difficult to establish a causal effect due to reverse causality (migrating to acquire more education) or by omitted variables (migration and schooling depend on wealth, preferences, etc.). A few papers have attempted to find causal effects but mainly for advanced economies. For example, Machin, Salvanes, and Pelkonen (2012) use spatial and time variation in the changes of the compulsory school laws in Norway to estimate the impact of education on mobility. They find that education helps mobility for the less educated workers. McHenry (2013) uses a similar design but applied to the U.S. He finds the opposite. More education among the less educated affects negatively the probability of migration.

The goal of my paper is to estimate the effects of schooling on several internal migration outcomes using a rigorous methodology that takes advantage of a natural experiment created by an education reform in Zimbabwe. Until 1979, black

Zimbabweans willing to enroll in secondary school needed to graduate from primary school (finish Grade 7), pass a high school entrance exam and hope for an available seat. As shown in Figure 1, the transition rate to the first year of high school (Form 1) was only around 27% in the 1970s. In 1980, the rules changed. The reform made progression to Form 1 automatic. The only requirement that persisted was graduating from Grade 7. This change created a *discontinuous* jump in the probability of advancing to secondary school<sup>2</sup>. Thus, students finishing Grade 7 in 1980 were disproportionately *more likely* to advance to Form 1 compared to those finishing Grade 7 just a year earlier.

Closer to my paper is the work of Miguel and Hamory (2009). These authors focus also on a developing country and study migration out of one district in Kenya after a decade. The authors take advantage of the randomization created by a deworming intervention to identify causal effects. However, there are several important differences between our papers. First, my work looks at migration outcomes across an entire country and not just from one district so there is a greater external validity by focusing on the Zimbabwe education reform. Second, my study explores migration over a longer period (17 years). Third, the deworming intervention does not allow them to separate the effect of schooling from the health improvements. In my case, the Zimbabwe reform affects schooling only and I show that there are no effects on health outcomes from the reform. Finally, the panel nature of their study implies that attrition is an important concern as discussed by the authors. The use of the regression discontinuity (RD) design allows me to estimate the impact of education in a setting free of attrition.

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<sup>2</sup> Dorsey (1989) shows that the majority of the new secondary schools were built in rural areas, ruling out the possibility of migrating in order to go to school.

Using the fuzzy RD design created by the reform I find that an extra year of schooling is associated with an increase in the overall probability of migration of 7.6% and with an 8.2% increase in the probability of moving to the largest cities: Harare, the capital, and Bulawayo. The effects are even larger for rural-born individuals: an additional year of schooling is associated with a 14% increase in the probability of moving to an urban district. These findings are robust to several checks including placebo tests for white Zimbabweans, a group not affected by the reform. For instance, while the reform increases the years of schooling of black Zimbabweans around the threshold, I find that for whites (and Asians) there is no discontinuity in the schooling outcomes and if any, there is rather a *decline* in such outcomes, however, not statistically different from zero. Furthermore, I expand this analysis by showing that the timing and structure of the reform does not coincide with other region-wide changes in education. Using census data from seven other Sub-Saharan African countries I found no discontinuous jumps in schooling outcomes for those aged 14 in 1980.

The rest of the paper is divided into five more sections. Section two briefly describes the education reform and how it provides a clear identification strategy. The data used in this paper is described in section three followed by the methodology. Section five presents the results and robustness checks. Section six summarizes our findings and concludes.

## **2. Education reform in Zimbabwe**

In April 1980, the newly elected government of Zimbabwe reformed the education system to break with the apartheid-like regime that prevailed in Rhodesia<sup>3</sup>. Prior to 1980, at least 25 percent of black school-aged children failed to enter primary school due to a lack of places (Riddell, 1980). In 1976, for every 1,000 black school-aged children, 250 never started school. Of those who went to school, 377 graduated from primary school, but only 60 of them transitioned into secondary education. Thereafter, 37 reached Form IV and less than 3 reached lower Form VI (Nhundu, 1992, p. 79)<sup>4</sup>

The 1980 education reform has been widely documented in the literature (e.g., Dorsey 1989; Edwards and Tisdell 1990; Edwards 1995). As described by Nhundu (1992), there were four key initiatives undertaken by the new government. First, the government introduced free and compulsory primary education. Second, there was a removal of age restrictions to allow overage children to enter school. This is very important to the validity of identification strategy of this paper. Limits to overage

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<sup>3</sup> For a history of Rhodesia's education system and the policies dictating the quantity and quality of schooling Africans received, see Atkinson (1972) and O'Callaghan and Austin (1977).

<sup>4</sup> Zimbabwe's education system consists of primary education, secondary education and tertiary education. The primary level is a seven-year cycle and the official entry-age is six years. It runs from Grade 1 through Grade 7. Primary education leads to a Grade 7 certificate. Secondary education is divided into three two-year levels: junior, middle and high/advanced. Entering high/advanced secondary school requires the student to pass the O-level examinations.

enrollment before 1980 means that most students were 14 years old by the time they started Form 1, the first year of their secondary education. Third, the reform provided strong community support for education. Fourth, an automatic grade progression was implemented, in particular from primary to secondary school. Prior to the reform, a black Zimbabwean student in the last year of primary school (Grade 7), had to successfully graduate, take an entrance test for secondary school and hope for a space. After the reform, it was enough to finish Grade 7 to enter Form 1. This fourth aspect of the reform, allows me to use a RD design to evaluate the impact of education on geographical mobility.

An immediate impact of these steps was an enormous increase in school enrollment. Between 1979 and 1985, total enrollment (primary and secondary) rose from 885,801 to 2,698,878: an unprecedented 205 percent increase Nhundu (1992). As shown in Figure 2, the greatest expansion took place in secondary education where enrollment grew by 628 percent during the same period (66,215 in 1979 to 482,000 in 1985).

To accommodate the increased demand, the government built new schools and undertook extensive reconstruction and expansion of existing facilities. This increase is shown in Figure 3. Between 1979 and 1983, the overall number of schools grew by 90 percent. Again, the largest increase is found in secondary schools: they grew by 575 percent since 1979 (and 65 percent for primary schools). These figures are consistent with an increase in the budget allocated to education. In the fiscal year of 1979-80, the share of education was 11.6 percent in the national budget. It almost doubled in 1980-81 (22.1 percent), and remained at about 17 percent until 1986-87 (Dorsey, 1989). The early years of the reform focused on the opening new secondary schools especially in rural

areas. The target was to provide a secondary school within walking distance of all rural pupils, especially where geographic and demographic factors were conducive. This emphasis on creating rural schools reduces the possibility that pupils will migrate to go to school. As discussed in the next section, the evidence presented in Dorsey (1989) goes against the possibility that children had to travel to urban areas to gain more education. Therefore, it is less likely that our results are driven but a mechanical effect induced by education (i.e., migrating to go to school).

Mirroring the massive response in enrollments are the transition rates from primary to secondary in Zimbabwe's schools. As Figure 1 showed above, the transition rate from Grade 7 (last grade of primary education) to Form I (first grade of secondary education) remained below 30 percent throughout the 1970s. As discussed before, Zimbabwean children start primary school at the age of six, thus on-time completion of all primary grades would enable them to start secondary school at the age of 14. As shown in Figure 1, there is a clear discontinuity in the probability that a child (boys and girls included) would go to secondary school in 1980. A child graduating from primary school in 1979 had a 27 percent chance of enrolling in secondary school. The same child, but who graduated one year later (1980), was more than three times as likely to enroll in secondary education (86 percent). Therefore, the educational reform of 1980 provides a natural experiment, where for reasons exogenous to their choice, 14 year olds black Zimbabweans could acquire more schooling relative to their slightly older counterparts.

### 3. Data sources

The main data source for this study is the 1997 Zimbabwe Inter-Censal Demographic Survey. The ICDS is a large national representative household survey with the specific mandate to collect information about migration, fertility and mortality (CSO, 1998). I construct the migration outcomes taking advantage of survey responses regarding place of birth, current place of residence, location in the 1992 census, and others.

Thus, I am able to identify three migration-related variables. First, a binary variable takes the value of one if a person lives in a different district from her district of birth. The second variable is applicable only to people born in rural areas and measures whether, in 1997, they reside in an urban area (zero otherwise). The third variable focuses on migrating to the largest cities (Harare, the capital city, or Bulawayo) for the sample that was not born in those cities.

### 4. Econometric model

The econometric model to evaluate the impact of education on migration takes advantage of the (fuzzy) discontinuity in schooling outcomes created by the education reform in 1980. In this sense I follow the identification strategy used in Agüero and Bharadwaj (2014) and Agüero and Ramachandran (2015). Formally, I use the following equations to estimate two stage least squares (2SLS) parameters:

$$S_i = \pi_0 + \pi_1 1(\text{AGE}1980_i \leq 14) + f(\text{AGE}1980_i - 14) + \theta X_i + v_i \quad (1)$$

$$M_i = \beta_0 + \beta_1 S_i + f(\text{AGE}1980_i - 14) + \lambda X_i + v_i \quad (2)$$

In the first stage (Equation 1),  $S_i$  represents the variables capturing the schooling levels of the  $i$ -th person. I consider two possible indicators for  $S_i$ : completed years of schooling

and the probability of attending Form 1 (or more). The term  $I(AGE1980_i \leq 14)$  is an indicator function for whether person  $i$ 's age in 1980,  $AGE1980_i$ , is equal or smaller than the cutoff age of 14. The term  $f(AGE1980_i - 14)$  accounts for the influence of the running variable --age in 1980-- on schooling in a flexible function  $f(\cdot)$ . For instance, in the linear case  $f(AGE1980_i - 14) = \theta_0(AGE1980_i - 14) + \theta_1(AGE1980_i - 14) I(AGE1980_i \leq \alpha)$ . For a higher order polynomial specifications,  $f(\cdot)$  estimates a different polynomial for each side. Vector  $X$  includes a dummy variable for gender and  $u_i$  and  $v_i$  are mean zero errors.

The second stage of the 2SLS (Equation 2), uses the predicted values of  $S_i$  from the first stage to estimate the effect of schooling on migration. Thus,  $\beta_l$  is the parameter of interest as it captures the effect on internal migration that comes from the exogenous changes in schooling created by the reform. The intuition is simple. If we assume that a person's age in 1980 (the running or assignment variable) has a random factor with a continuous density, then the probability of being  $\varepsilon$  years older or  $\varepsilon$  years younger than the cutoff of 14 is the same (for a sufficiently small  $\varepsilon$ : one year, for instance). Even if the expected age in 1980 depends on individual characteristics such as family background (e.g., fertility preferences), eligibility for treatment in the small neighborhood around the cutoff will be as good as randomly assigned (Lee, 2008). In other words, people just below the cutoff can be used as a counterfactual for those just above the cutoff because the identifying assumption implies that the only difference between these two groups is that students below the cutoff receive the treatment (i.e., had more years of schooling due to the reform).

Ideally, one would like to compare the average outcome for individuals in a small neighborhood around the threshold, but usually there are not enough data in this small

vicinity, and thus the estimation could suffer from small sample bias. Therefore, I use a larger bandwidth than just a few years around the threshold. Also, in this paper, the running variable is discrete due to the way age is captured in the ICDS. Therefore, my approach is closer to Lee and Card (2008) and it does not represent any loss relative to having a continuous running variable (Lee and Lemieux, 2010).

Several assumptions are needed to validate the proposed identification strategy. First, the reform needs to alter the schooling levels of the targeted population, black Zimbabweans, in order to avoid a weak instruments problem. This is formally tested in the next section. However, in Figures 4A and 4B, I provide a visual support for this assumption. First, I show that there is a discontinuous jump in the number of completed years of schooling around the threshold. While the values have increased for every new generation, those aged 14 in 1980 have 1.5 additional years of schooling compared to their slightly older counterparts aged 15 in 1980. Similarly, the probability of having Form 1 or more (i.e., having completed at least the first year of secondary education) discontinuously jumps from around 0.48 to 0.61 when comparing 14 and 15 year olds in 1980, respectively.

Second, Figures 5A and 5B provide some initial (graphical) support for the exclusion restriction: all other variables should be smooth around the threshold. For example, since the reform was created to address racial disparities in Rhodesia, we should not find a discontinuous jump around the cutoff for whites Zimbabweans (or for any other non-black racial group in general). Evidence of a discontinuity for these groups would invalidate our identification strategy. As shown in Figure 5A, while there is a clear discontinuity for blacks (blue hollow circles) in terms of completed years of schooling,

there is no such evidence for whites or Asians (red filled circles). Figure 5B shows the same lack of a discontinuity for non-blacks when focusing on the probability of having Form 1 or more.

Note that the reform's elimination of the age restrictions permitted many overage children to remain in or return to school. For instance, while there were 112,890 children enrolled in Grade 6 in 1980 the number of children enrolled in Grade 7 the following year was 15 percent larger (over 129,000). Thus the benefits of the reform extended to children aged 15 in 1980 in addition to those aged 14 or less in 1980. This implies, not only that the discontinuity is rather fuzzy than sharp, but also and more importantly, that some of the 15 year olds could be part of the treatment group. In that case, our estimates are biased *downwards*. Therefore, as part of the set of robustness checks I estimate the effects of education on mobility for samples that drop 14 year olds, 15 year olds and both from the sample.

In the next section I provide further support for the identification strategy and present the estimated impacts of schooling on internal migration patterns.

## **5. Results**

### **5.1. First stage**

The estimates of the first stage equation are shown in Table 1. In this table I restrict the sample to individuals aged between 0 and 40 in 1980. Following Lee and Card (2008), the clustering of the standard errors is done at each value of the discrete assignment variable. In order to avoid the biases induced by having a small number of clusters the 0-

40 age range in 1980 represents the preferred estimates. However, in the appendix (Table A.1) I consider other ranges<sup>5</sup>.

Table 1 shows a clear discontinuity around the threshold. In Panel A, column 1, I restrict the sample to blacks only and consider a linear spline for  $f(AGE1980_i-14)$ . I find that black Zimbabweans aged 14 in 1980 completed 2.1 additional years of schooling compared those aged 15 in 1980. This represents a large effect: 26.5 percent relative the mean of the entire sample. This is consistent with previous work using different datasets. Combining data from the three Zimbabwean Demographic and Health Surveys (1999, 2005-06 and 2010-11) and restricting the sample to black women, Agüero and Bharadwaj (2014) find that the reform increased by 25 percent the number of completed years of schooling for women aged 14 in 1980, compared their 15 year old counterparts. To put these estimates in perspective, in her seminal study for Indonesia, Duflo (2001) finds an increase of 0.12 to 0.19 years of schooling for each primary school constructed per 1,000 children. Against her findings, the results for Zimbabwe are large. Panel B, column 1, confirms these findings for the probability of having at least Form 1 and report an increase at the threshold of 54.1 percent ( $=.276/.510$ ) with respect to the mean.

Table 1 also helps validate the exclusion restriction. In Panel A, column 2, I ran the same regression (Equation 1) but for non-black Zimbabweans (whites and Asians), the racial group that should not be affected by the policy. The estimated parameter shows no discontinuity around the cutoff and if anything, non-blacks aged 14 in 1980 have *fewer* years of schooling than their slightly older counterparts. Columns (3) and (4) combine blacks and non-blacks and estimate the *difference-in-discontinuity* by adding a

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<sup>5</sup> Future versions of the paper will explore alternative clustering scenarios.

binary variable for blacks (=1) and well as the interaction  $I(AGE1980_i \leq 14) * Black$ . This interaction uses the non-black population as an additional control group. In column (3) I use a linear spline for the running variable and find an effect of 4.10 additional years of schooling for black Zimbabweans aged 14 in 1980 relative to 15 year olds and the non-blacks. Column (4) uses a quadratic spline and reports a similar effect: 4.11 additional years of schooling. In Panel B, I found analogous effects when the schooling outcome is the probability of having at least Form 1. While the effect of the reform for blacks was an increase in 27.6 percentage points, for non-blacks the effect is negative and the comparison between races yields an effect of 45.8 percentage points for the linear splines (column 3) and 46.0 for the quadratic specification (column 4). These findings suggest not only a very strong first stage but also provide important supportive evidence in favor of the validity of the exclusion restriction.

Agüero and Bharadwaj (2014) presented additional support for the exclusion restriction using a sample of women from the DHS<sup>6</sup>. For instance, they showed that women's height is smooth around the cutoff point for black Zimbabweans. This indicates that the reform affected schooling but not health outcomes directly. The authors also show that the observed discontinuity in education for 14 year olds in 1980 in Zimbabwe does not exist for neighboring countries. For instance, they found no discontinuities for South Africa or Zambia. I expand their analysis by examining whether there is a discontinuity in seven other sub-Saharan African counties using population censuses

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<sup>6</sup> Non-blacks represent less than five percent of the population in Zimbabwe. Thus, the sample size for non-blacks in the DHS is nearly zero. In the ICDS I was able to identify less than 300 observations for those aged between 0 and 40 in 1980. See Table 1.

closest to the 1997 ICDS (available from IPUMS online after registration). For these countries (Kenya, Rwanda, Senegal, South Africa, Uganda, Tanzania and Zambia) I used all observations from natives (men and women) aged between 0 and 40 in 1980. As shown in Figure 6, there is no discontinuity for these other countries either. The regression counterparts for these figures are shown in Appendix Table A.2 and confirm the lack of a discontinuity in the years of schooling for these seven countries at the threshold. All these evidence provides a stronger support for the identification strategy. The results of using 2SLS to estimate the effect of education on internal migration — based on the fuzzy RD— are shown next.

## **5.2. Effect of education on migration**

I now present the estimates for the effect of schooling on the three migration outcomes described in section three and for the sample of black Zimbabweans only. I start by showing the reduced form effects graphically for the preferred specification: those aged zero to forty in 1980. In Figures 7A-7C I plot the average value of the migration outcome by age in 1980. All figures show a clear discontinuity at the threshold.

In Tables 2 to 4, I expand this analysis and show the 2SLS estimates for several specifications regarding the bandwidth: those aged 0-40 in 1980 (column 1); 10-20 (column 2) and 12-17 (column 3). The latter is possible thanks to the larger sample size available in the ICDS, but only when considering all ages in between. I also show robustness checks by dropping from the analysis 14 years olds (Panel B), 15 year olds (Panel C) and both at the same time (Panel D).

I find that education increases the probability of internal migration and these associations are statistically different from zero. For instance, more schooling is associated with a higher probability of living (in 1997) in a district different from a person's district of birth. Table 2, column 1, Panel A, shows that for the preferred sample and using all the observations, an additional year of education is associated with an increase of 7.6 percent (with respect to the mean) in that outcomes ( $=0.039/0.510$ ). For those born in rural areas, schooling also increases the probability of migrating to urban areas by 14.2 percent ( $=0.032/0.225$ ) as show in Table 3, column 1, Panel A. Schooling is also associated with an increment in the probability of migrating to the largest cities (Harare and Bulawayo) by 8.2 percent.

These findings remain robust when considering all the alternative specifications. In columns (2) and (3) I reduced the bandwidth to those aged 10-20 or to 12-17 in 1980, respectively. In fact, the 2SLS point estimates *increase* when the bandwidth is reduced. For example, using the sample of those aged 12-17 in 1980; an additional year of schooling increases the probability of migrating by 18.2 percent, the probability of migrating to urban areas by 18.2 percent and the probability of migrating to the largest cities by 42.6 percent.

The possibility of overage enrollment created by the reform also could affect the schooling of those aged 15 in 1980. Dropping from the analysis those aged 14, 15 or both does not alter the main findings. I still find a large and statistically significant association between education and migration.

## 6. Conclusions

This paper uses the exogenous variation in schooling generated by the 1980 education reform in Zimbabwe and shows that geographical mobility responds, causally, to more education. An additional year of education also increases the probability to migrate out of rural areas (by 14 percent) and the probability to move to the largest cities in the country by more than eight percent. Several robustness checks, including varying the bandwidth, and the sample confirm these findings. Also, placebo tests using populations not affected by the reform (e.g., white Zimbabweans and people living in other Sub-Saharan African countries) help confirm the validity of the identifying assumptions.

The use of a fuzzy RD design implies that these findings should be interpreted as a local average treatment effect. This is important given the context. Prior to the reform Zimbabwe was characterized by an apartheid-type system where very few black Zimbabweans had access to secondary education. When compared to the findings in advanced economies my effects suggest larger effects. For instance, McHenry (2013) finds a negative effect on migration and Machin et al (2012) find a positive but smaller effect.

Several issues have not been addressed in this version of the paper but should be included in future versions. It is possible to explore whether the migration patterns estimated here represent longer or shorter changes. By exploring location five years prior to the survey one could explore richer patterns of migration. Also missing for the analysis is the exploration of the possible channels that could affect migration. Machin et al (2012) hypothesized about the role of the family size but found no effect of education on the

number of children. Preliminary evidence suggests that in the context of Zimbabwe this could be an important mechanism.

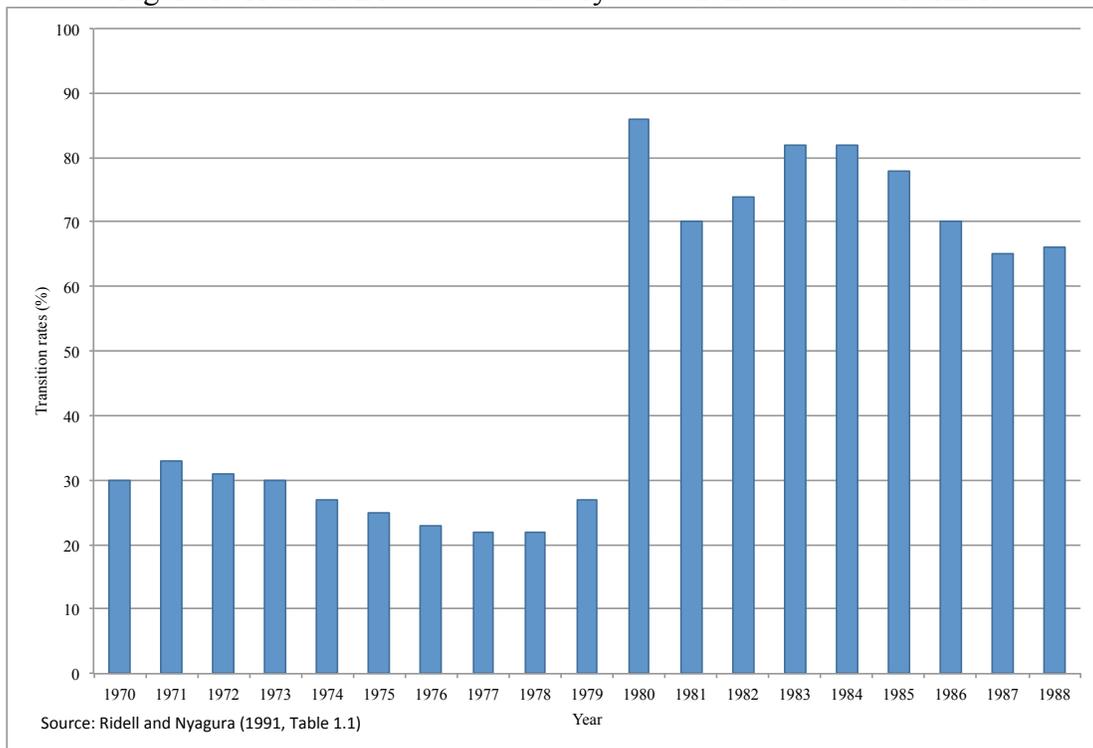
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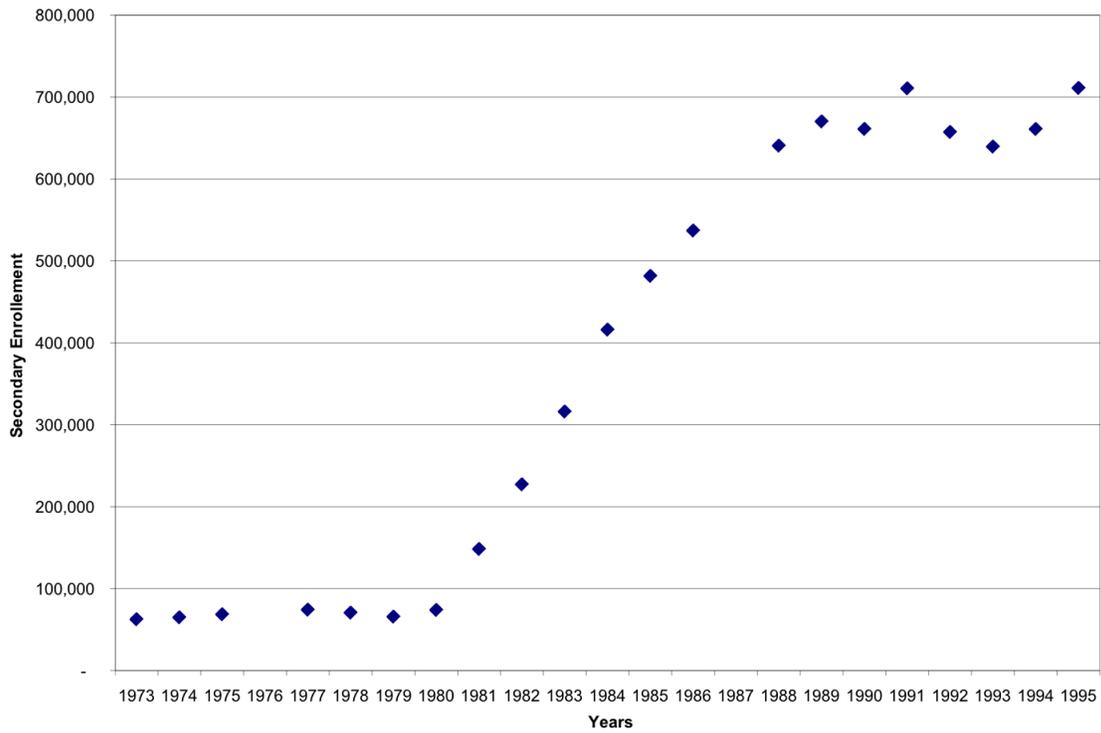
Figure 1. Transition rates to secondary education: Grade 7 to Form 1



Data source: Riddell and Nyagura (1991), Table 1.1.

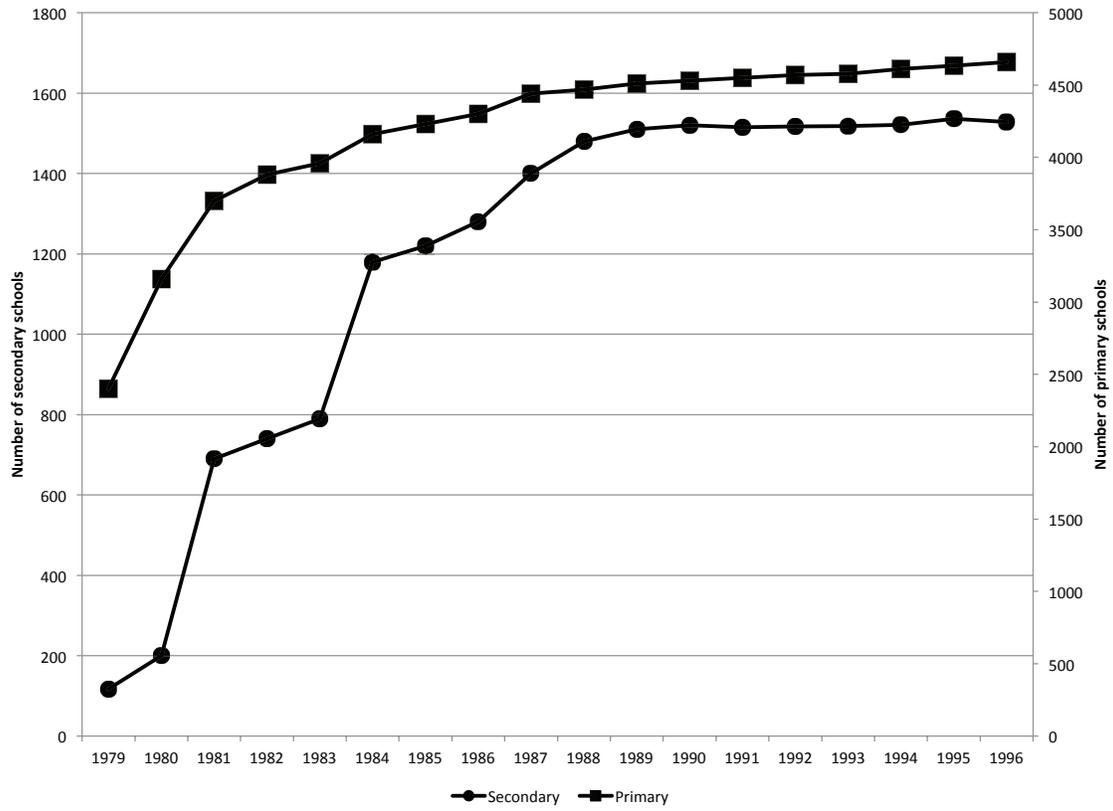
Note: Grade 7 is the last year of primary education and Form I is the first year of secondary education.

Figure 2. Secondary enrollment by year



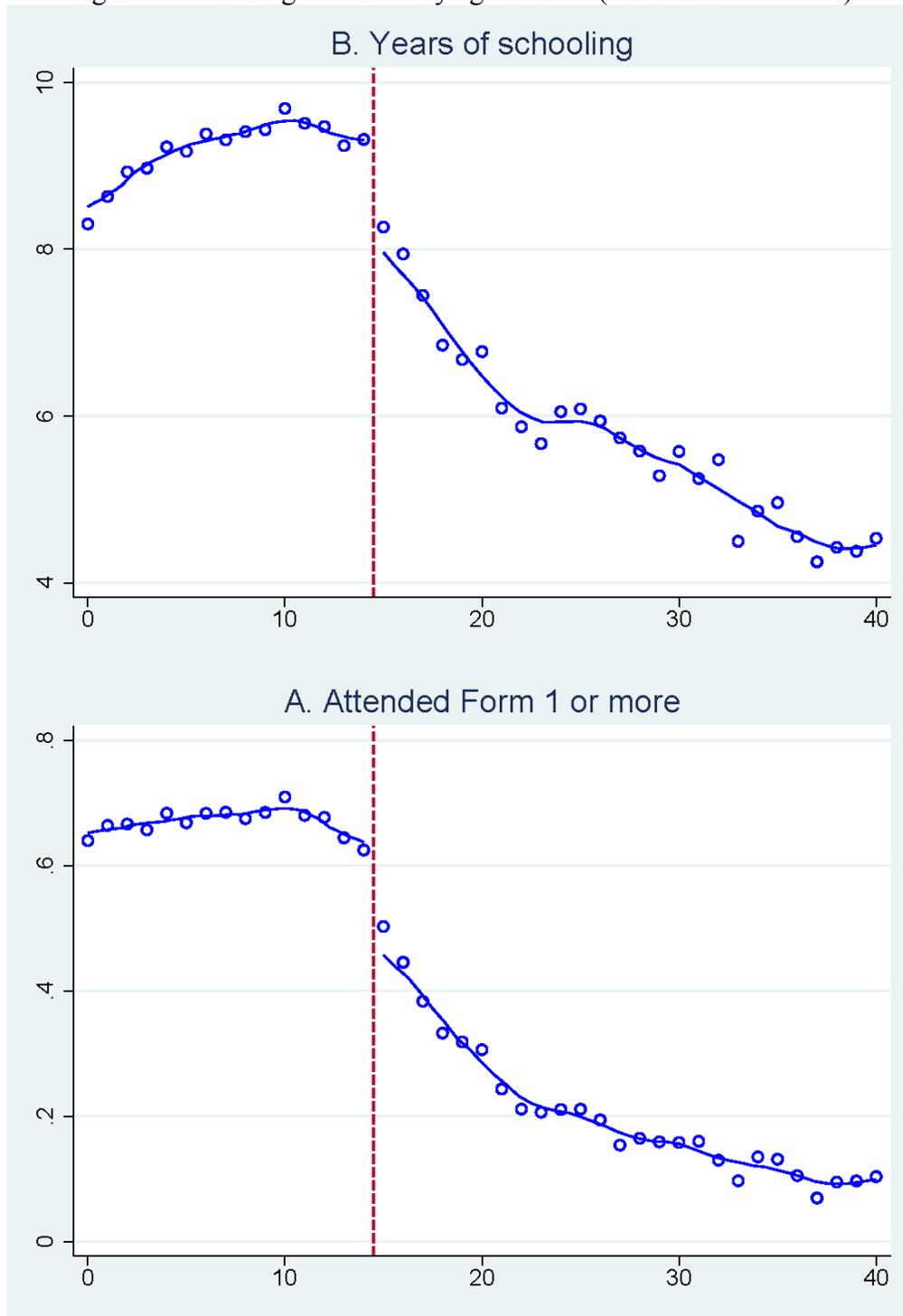
Note: Elaborated based on data from United Nations Statistical Yearbook, various years

Figure 3. Trends in school construction by educational level, 1979-1996



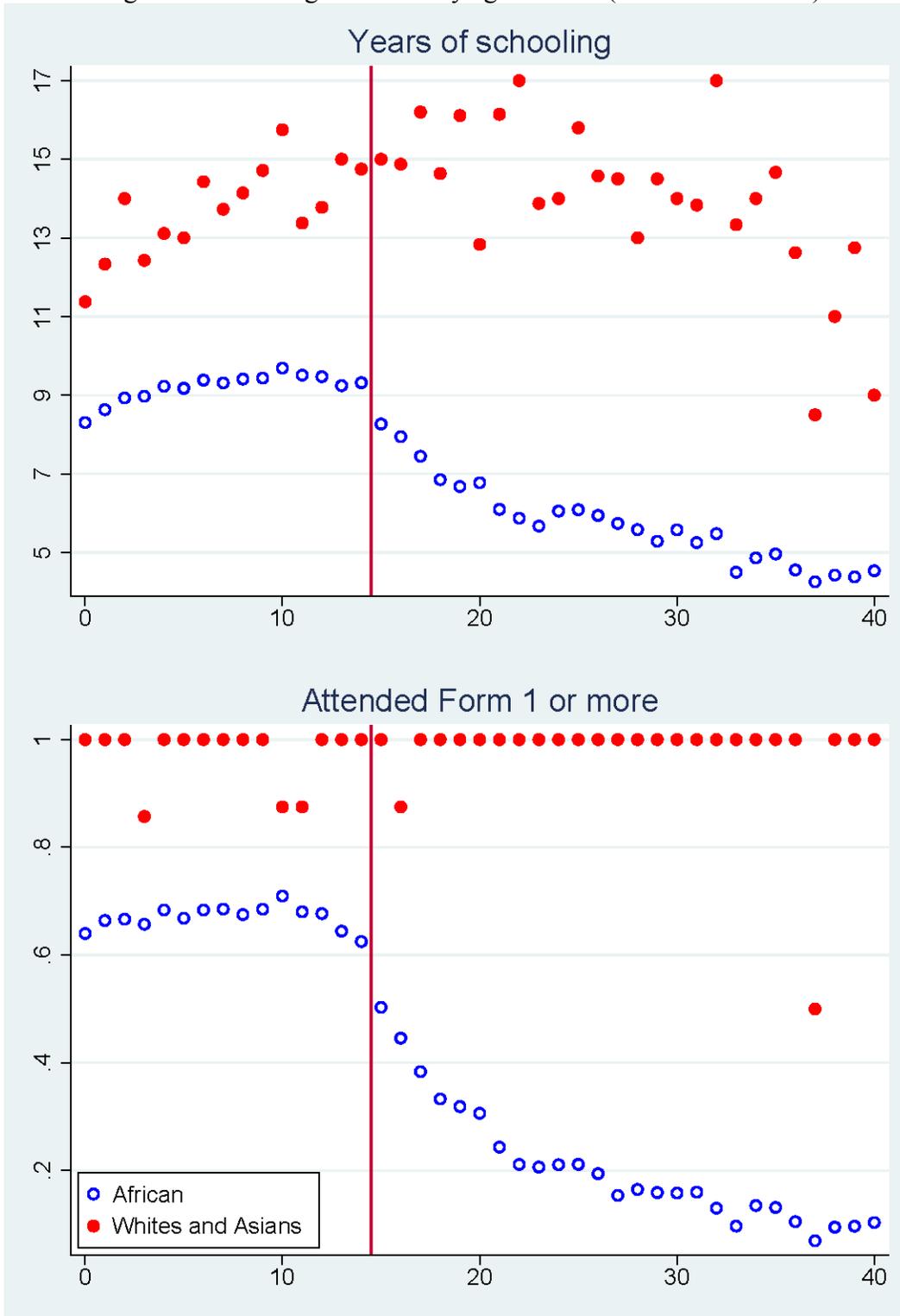
Note: Elaborated based on Zimbabwe Ministry of Education, Culture, and Sports, Annual Education Report, various years.

Figure 4. Schooling outcomes by age in 1980 (Black Zimbabweans)



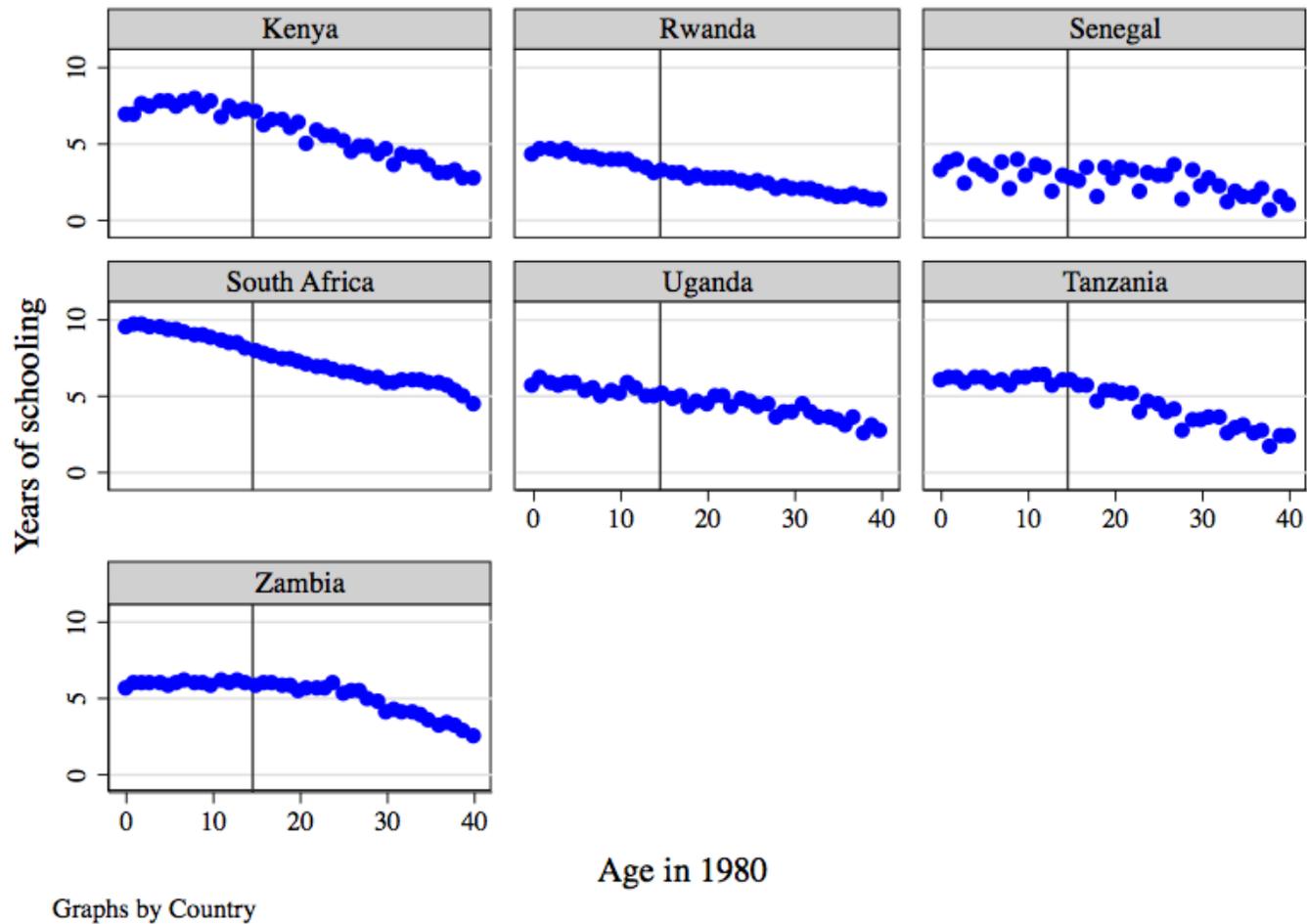
Note: Each symbol represents the average by age in 1980 for the selected variables. Sample is restricted to blacks born in Zimbabwe using the 1997 ICDS. The lines are local polynomials estimated for each side of the cutoff.

Figure 5. Schooling outcomes by age in 1980 (All Zimbabweans)



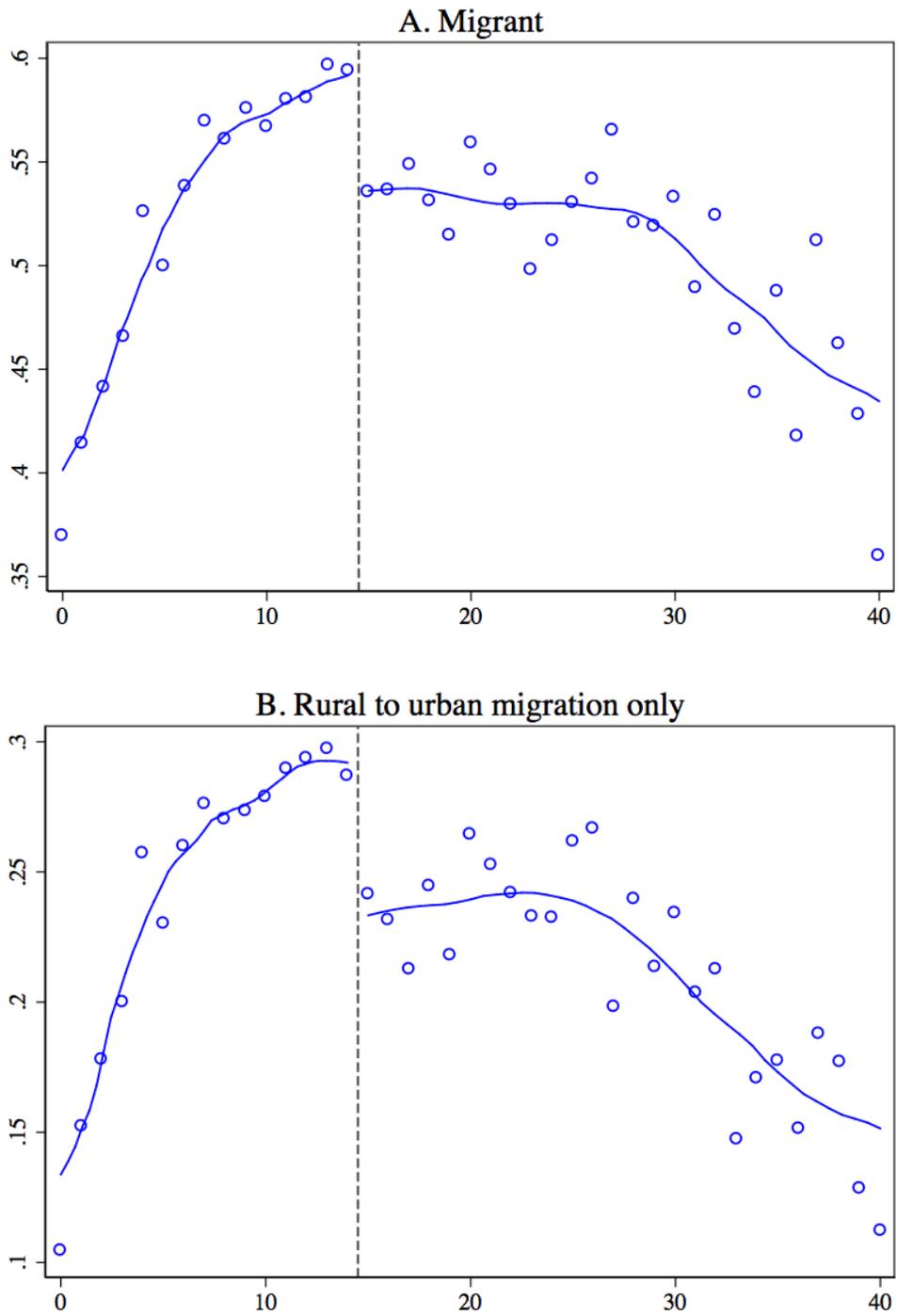
Note: Each symbol represents the average by age in 1980 for the selected variables for the corresponding race. Sample is restricted to those born in Zimbabwe using the 1997 ICDS.

Figure 6. Age in 1980 and schooling in other African countries

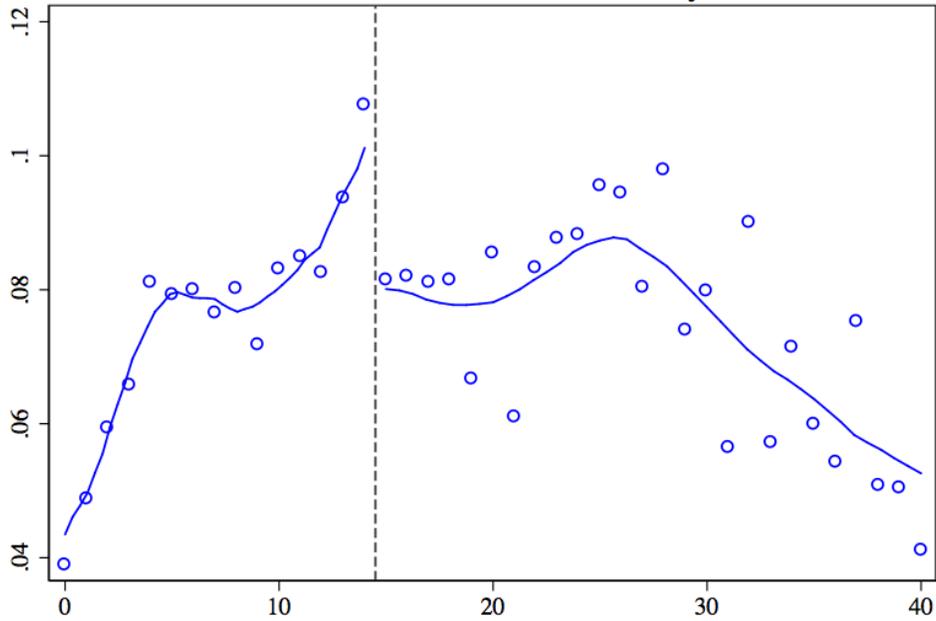


Note: Each symbol represents the average number of completed years of schooling by age in 1980 using the latest census in each country.

Figure 7. Migration outcomes and age in 1980 (Black Zimbabweans)



### C. Moved to Harare or Bulawayo



Note: Each symbol represents the average by age in 1980 for the selected variables. Sample is restricted to black born in Zimbabwe using the 1997 ICDS. Panel A includes all black Zimbabweans. Panel B is for those born in rural areas and Panel C for those born outside Harare or Bulawayo. Lines are local polynomials estimated for each side of the cutoff.

Table 1. Age in 1980 and schooling outcomes (First Stage)

	Blacks	Non-Blacks	All	
	Linear (1)	Linear (2)	Linear (3)	Quadratic (4)
Panel A. Dependent variable: Completed years of schooling				
1(AGE1980≤14)	2.112*** (0.253)	-1.106 (0.707)	-1.985*** (0.394)	-2.997*** (0.408)
1(AGE1980≤14)*Black			4.101*** (0.384)	4.111*** (0.380)
Black			-8.587*** (0.305)	-8.592*** (0.302)
Obs.	42,844	229	43,073	43,073
Panel B. Dependent variable: Probability of attending Form 1				
1(AGE1980≤14)	0.276*** (0.0308)	-0.0530 (0.0319)	-0.181*** (0.0289)	-0.307*** (0.0295)
1(AGE1980≤14)*Black			0.458*** (0.0319)	0.460*** (0.0312)
Black			-0.759*** (0.0271)	-0.760*** (0.0263)
Obs.	42,844	229	43,073	43,073

Note: Robust standard errors clustered by age in 1980 (41 clusters) are shown in parenthesis. Sample is restricted to Zimbabwe-born individuals using the 1997 ICDS. The bandwidth corresponds to individuals aged between 0 and 40 in 1980. All regressions include (not shown) linear (columns 1-3) or quadratic splines (column 4). \*\*\* Significant at the 1 percent level; \*\* Significant at the 5 percent level; \* Significant at the 10 percent level.

Table 2. Schooling and the probability of migrating (Two Stage Least Squared)

Bandwidth	Dependent variable: Migrated		
	0-40 (1)	10-20 (2)	12-17 (3)
Panel A: All ages in 1980			
Years of schooling	0.039*** (0.008)	0.092*** (0.014)	0.103*** (0.017)
Mean of dep. var.	0.510	0.560	0.566
Observations	42,844	11,791	6,152
F-stat (first stage)	676.8	15.98	6.114
Panel B: Excludes 14 years olds in 1980			
Years of schooling	0.041*** (0.008)	0.110*** (0.015)	
Mean of dep. var.	0.509	0.557	
Observations	42,020	10,967	
F-stat (first stage)	696.8	9.448	
Panel C: Excludes 15 years olds in 1980			
Years of schooling	0.034*** (0.007)	0.086*** (0.020)	
Mean of dep. var.	0.510	0.562	
Observations	41,782	10,729	
F-stat (first stage)	699.4	9.972	
Panel D: Excludes 14 and 15 years olds in 1980			
Years of schooling	0.036*** (0.008)	0.103*** (0.024)	
Mean of dep. var.	0.508	0.560	
Observations	40,958	9,905	
F-stat (first stage)	719.8	6.719	

Note: Robust standard errors clustered by age in 1980 are shown in parenthesis. Sample is restricted to black Zimbabwean using the 1997 ICDS. Dependent variable is equal to one if the person lives in a district different from birth district and zero otherwise. Coefficients were estimated instrumenting completed years of schooling with  $1(\text{AGE}1980 \leq 14)$ . The bandwidth changes by column: those aged between 0-40 in 1980 (column 1), between 10 and 20 (column 2) and between 12 and 17 (column 3). There are not enough observations to correctly estimate the 2SLS for columns three for the panels where additional years are removed (Panels B-D). All regressions include (not shown) linear splines and a gender indicator.  
 \*\*\* Significant at the 1 percent level; \*\* Significant at the 5 percent level; \* Significant at the 10 percent level.

Table 3. Schooling and the migration to urban areas (Two Stage Least Squared)

Bandwidth	Dependent variable: Migrated to urban areas		
	0-40 (1)	10-20 (2)	12-17 (3)
Panel A: All ages in 1980			
Years of schooling	0.032*** (0.008)	0.081*** (0.017)	0.049*** (0.011)
Mean of dep. var.	0.225	0.262	0.262
Observations	33,573	9,124	4,694
F-stat (first stage)	639.1	19.92	6.753
Panel B: Excludes 14 years olds in 1980			
Years of schooling	0.035*** (0.008)	0.102*** (0.016)	
Mean of dep. var.	0.224	0.260	
Observations	32,970	8,521	
F-stat (first stage)	652.1	11.57	
Panel C: Excludes 15 years olds in 1980			
Years of schooling	0.028*** (0.008)	0.096*** (0.022)	
Mean of dep. var.	0.225	0.264	
Observations	32,745	8,296	
F-stat (first stage)	647.9	11.61	
Panel D: Excludes 14 and 15 years olds in 1980			
Years of schooling	0.031*** (0.008)	0.118*** (0.026)	
Mean of dep. var.	0.223	0.262	
Observations	32,142	7,693	
F-stat (first stage)	662.0	7.713	

Note: Robust standard errors clustered by age in 1980 are shown in parenthesis. Sample is restricted to black Zimbabwean born in rural areas using the 1997 ICDS. Dependent variable is equal to one if the person lives in an urban district and zero otherwise. Coefficients were estimated instrumenting completed years of schooling with  $1(\text{AGE}_{1980} \leq 14)$ . The bandwidth changes by column: those aged between 0-40 in 1980 (column 1), between 10 and 20 (column 2) and between 12 and 17 (column 3). There are not enough observations to correctly estimate the 2SLS for columns three for the panels where additional years are removed (Panels B-D). All regressions include (not shown) linear splines and a gender indicator.  
 \*\*\* Significant at the 1 percent level; \*\* Significant at the 5 percent level; \* Significant at the 10 percent level.

Table 4. Schooling and the migration to largest cities (Two Stage Least Squared)

Bandwidth	Dependent variable: Migrated to Harare or Bulawayo		
	0-40 (1)	10-20 (2)	12-17 (3)
Panel A: All ages in 1980			
Years of schooling	0.006** (0.003)	0.025*** (0.005)	0.037*** (0.004)
Mean of dep. var.	0.073	0.084	0.087
Observations	41,248	11,377	5,919
F-stat (first stage)	678.3	16.13	6.390
Panel B: Excludes 14 years olds in 1980			
Years of schooling	0.005** (0.002)	0.017** (0.008)	
Mean of dep. var.	0.073	0.082	
Observations	40,467	10,596	
F-stat (first stage)	699.0	9.432	
Panel C: Excludes 15 years olds in 1980			
Years of schooling	0.005* (0.003)	0.023*** (0.007)	
Mean of dep. var.	0.073	0.084	
Observations	40,229	10,358	
F-stat (first stage)	696.5	9.242	
Panel D: Excludes 14 and 15 years olds in 1980			
Years of schooling	0.004 (0.003)	0.015 (0.009)	
Mean of dep. var.	0.073	0.082	
Observations	39,448	9,577	
F-stat (first stage)	717.4	6.103	

Note: Robust standard errors clustered by age in 1980 are shown in parenthesis. Sample is restricted to black Zimbabwean born outside Harare or Bulawayo using the 1997 ICDS. Dependent variable is equal to one if the person lives in Harare or Bulawayo and zero otherwise. Coefficients were estimated instrumenting completed years of schooling with  $1(\text{AGE1980} \leq 14)$ . The bandwidth changes by column: those aged between 0-40 in 1980 (column 1), between 10 and 20 (column 2) and between 12 and 17 (column 3). There are not enough observations to correctly estimate the 2SLS for columns three for the panels where additional years are removed (Panels B-D). All regressions include (not shown) linear splines and a gender indicator. \*\*\* Significant at the 1 percent level; \*\* Significant at the 5 percent level; \* Significant at the 10 percent level.

## Appendix Tables

Table A.1 First Stage: Age in 1980 and Completed Years of Schooling (Blacks only)

Bandwidth:	Dependent variable: Completed years of schooling		
	0-40 (1)	10-20 (2)	12-17 (3)
Panel A: All ages in 1980			
1(AGE1980≤14)	2.112*** (0.253)	0.707*** (0.153)	0.548*** (0.0881)
Observations	42,844	11,791	6,152
Adjusted R-squared	0.176	0.068	0.030
Panel B: Excludes 14 years olds in 1980			
1(AGE1980≤14)	2.191*** (0.262)	0.615*** (0.144)	0.313*** (0.0470)
Observations	42,020	10,967	5,328
Adjusted R-squared	0.178	0.070	0.030
Panel B: Excludes 15 years olds in 1980			
1(AGE1980≤14)	2.294*** (0.238)	0.855** (0.281)	0.313** (0.0766)
Observations	41,782	10,729	5,090
Adjusted R-squared	0.183	0.075	0.034
Panel D: Excludes 14 and 15 years olds in 1980			
1(AGE1980≤14)	2.373*** (0.247)	0.763** (0.278)	0.0779*** (0.0245)
Observations	40,958	9,905	4,266
Adjusted R-squared	0.185	0.077	0.037

Note: Robust standard errors clustered by age in 1980 are shown in parenthesis. Sample is restricted to black Zimbabwean using the 1997 ICDS. The bandwidth changes by column: those aged between 0-40 in 1980 (column 1), between 10 and 20 (column 2) and between 12 and 17 (column 3). All regressions include (not shown) linear splines.  
 \*\*\* Significant at the 1 percent level; \*\* Significant at the 5 percent level; \* Significant at the 10 percent level.

Table A.2 Age in 1980 and Completed Years of Schooling in other African countries

	Dependent variable: Completed years of schooling						
Country:	Kenya	Rwanda	Senegal	South Africa	Uganda	Tanzania	Zambia
Census year:	1998	2002	2002	2001	2002	2002	2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$1(\text{AGE}_{1980} \leq 14)$	-0.224	0.016	0.174	0.126	0.199	-0.054	0.110
	[0.345]	[0.090]	[0.664]	[0.090]	[0.242]	[0.314]	[0.130]
<i>N</i>	574554	249464	356777	1720606	831325	1359930	385693
Adjusted $R^2$	0.135	0.102	0.042	0.106	0.096	0.151	0.054

Note: Robust standard errors clustered by age in 1980 are shown in parenthesis. Sample is restricted to natives in each country using the latest census available. The bandwidth includes those aged between 0-40 in 1980. All regressions include (not shown) quadratic splines and a binary variable for gender.

\*\*\* Significant at the 1 percent level; \*\* Significant at the 5 percent level; \* Significant at the 10 percent level.