The Impact of the Universal Primary Education Program on Labor Market Outcomes: Evidence from Tanzania*

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Abstract

The purpose of this paper is to study the impact of education on labor market participation and on household consumption in a rural environment. To do so, I use the Universal Primary Education (UPE) program, whose intensity was varying across locations and over time . This program proved to be efficient at reducing inequalities of access to education and at providing basic agricultural skills. Based on a difference-in difference approach, and exploiting these two exogenous variations to instrument education and I find that education raises household consumption, especially in agriculture. I also provide evidence that education increases the probability of working in agriculture at the expense of non-agricultural self-employed activities. These results illustrate the particularity of the program and suggest that returns to education in agriculture are positive, provided that the curriculum at school is suitable agriculture.

Keywords: Human capital investment, Returns to Education, labor market organization, schooling reforms, Tanzania.

JEL Codes: H52, I24, I26.

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1 Introduction

Education is a cornerstone for economic growth, and proves to be crucial to eradicate poverty and counter the transmission of inequalities between generations. This situation convinced policymakers to put education at the top of their agenda. More specifically, several governments of developing countries have implemented policies to universalize primary education. These programs turned out to be useful in increasing the access to education, but not necessarily in improving attendance (Deininger, 2003), especially in rural areas where children can work in the fields. This situation raises new challenges in finding policies that increase the level of education, and insure high returns to education in a rural environment.

The main difficulty in estimating the returns to education in a rural environment is twofold. First, education is likely to be endogenous, which prevents to obtain unbiased estimates with simple OLS. Card (2001) reviews papers that aim to identify the causal impact of education on earnings. Among the eleven papers included in the survey, only two of them focus on developing countries, the paper of Duflo (2001) where education is instrumented by a school construction program in Indonesia, and the study by Maluccio (1998) where education is instrumented by the distance to school in rural Philippines.

Yet, both authors restrict their analysis to wage-earners. This brings up to the second difficulty: the representativeness of the samples. Indeed, wage-earning individuals are likely to be self-selected and to have specific characteristics. Maluccio (1998) does not deal with this sample selection issue, but Duflo (2001) adopts an imputation technique to compute a wage for individuals from the self-employment sector. While this method is suitable for countries with a developed formal sector, it is less adapted to countries that are mainly agriculture-based and where few individuals are wage-earners.

To address this important issue, another strand of the literature estimates the returns to education among agricultural households by considering the agricultural production. Lockheed *et al.* (1980) review papers estimating the impact of education on agricultural production and find very mixed results depending on the country and the specification of education. However, these papers do not consider the endogeneity of education of the household head. Using the Universal Primary Education (UPE) program implemented in Tanzania from 1974 to 1978 as a natural experiment, this paper builds on this literature and investigates the benefits of education in a rural economy.

The first contribution of this paper is to assess the efficiency of the massive UPE program by testing whether it ensured the expansion of the education system and contributed to reducing inequality in access to education.

The second contribution is to estimate the returns to education in developing countries for the entire population. Since developing countries are often characterized by the large size of both the non-agricultural self-employed sector and the agricultural sector, I use consumption aggregates that are available for all sample households. To account for the potential endogeneity of education, I instrument education of the household head by the exposition to the UPE program. In 1974, educational levels were low at the national level, with wide variation between regions. The strict enforcement of the UPE program led to substantial results: 3.3 million children aged 7 to 13 were enrolled in 1980, compared to 1.2 million in 1974 (Bonini, 2003). To reduce disparities in access to education, the Tanzanian socialist government gave priority to deprived areas, which led the latter to experience higher schooling expansion. Therefore, the exposure to the UPE program varied according to the age of the individual at the time of the reform and according to the educational level by regions before the introduction of the program. Thus, the UPE program gives rise to an exogenous variation in education that I exploit to instrument education and to determine the effect of education on consumption. In order to capture variability in the returns to education, I also distinguish between the returns to education for subgroups: the agricultural sector, the non-farm self-employed sector, and wage-work activities.

The third contribution of this paper is to address the effect of education on the labor market organization, more precisely, on the probability of working in each sector of activity. The motivation behind is that education can not only increase earnings, but also gives access to better paid activities and eases mobility between sectors. In order to deal with the endogeneity of education, I adopt the same identification strategy and I instrument education by exploiting the exposition to the UPE program.

The main findings of this paper suggest that the UPE program reduced inequalities of access to education and that returns to education are positive in every sector. Counter-intuitively, they are especially high in agriculture. I justify this finding by the design of the program which were directed toward agriculture by providing a specific curriculum with agricultural classes. In these specific conditions, this paper also demonstrates that education decreases the probability of working in non-farm self-employed activities in favor of farm activities.

The remainder of the paper is organized as follows: section 2 provides a broad picture of the evolution of education in Tanzania and describes the data and the main variables of the analysis. Section 3.1 introduces the identification strategy; section 3.2 presents the effect of the UPE program on education; section 4.1 and section 4.2 respectively, focus on the effect of education on consumption and on labor market participation. Finally, section 5 concludes.

2 The program

2.1 Historical background and the UPE program.

When colonization ended in 1961, access to education in Tanzania was very unequal between regions (Court and Kinyanjui, 1980). These spatial disparities were based on ecological endowments and were exacerbated by colonial activities and transport networks.[1] At this time, the purpose of primary education was to prepare for secondary education and was to encourage a small number of rural students to find white-collar jobs in urban areas (Kinunda, 1975). The arrival in power of the Prime Minister Nyerere in 1964 marked a radical political and economic change.

In 1967, the policy of Education for Self-Reliance (ESR) was approved. Education became the mainstay of the Tanzanian socialist economy that would ensure economic growth. This policy should have led to radical changes but, in practice, was slowly enforced. It was only in 1974 that the government committed itself to reach at a forced-march Universal Primary Education (UPE) by 1978. The aim of this program was three-fold: i) to improve the equity of access to education, ii) to teach agricultural skills that would be relevant in a rural society, and iii) to offer a political and civic education (Nyerere, 1967). To achieve the UPE goal, the government made a series of changes. First, it implemented a villagization program in order to provide access to schools and social services. From 1968, villagization consisted of constructing community villages commonly called ujamaa, but from 1974, households living in remote areas were forced to move (see

[1] The most privileged zones were the Arusha-Kilimanjaro-Tanga and the Mwanza-Shinyanga corridors, and the Coast Morogoro-Kigoma (Maro and Mlay, 1979).

Table 2).[2] As a result, more than 10 million people were moved and 2,650 ujamaa were built (Martin, 1988) from 1974 to 1977. During this period, the Tanzanian government invested massively in primary education and concentrated its efforts on deprived areas. Local resources were mobilized for classrooms and a large number of new schools were built. Thus, the UPE program combined with villagization greatly reduced distances to schools. Simultaneously, teachers' recruitment and teacher training were restructured. To deal with the growing number of pupils, the government trained 10,000 teachers. Despite this, there was still a shortage of primary school teachers, which may have affected the quality of education, especially in the beginning of the UPE plan (Sabates *et al.*, 2011). The government also made additional adjustments to improve schools' attractiveness. Tuition fees were eliminated, primary education became mandatory, and Swahili, most pupils' mother tongue, was designated as the language of instruction.

To attain agriculture self-sufficiency defined as one of the main priority: «kilimo cha kufa na kupona», *Agriculture for Life and Dealth*, the Prime Minister Nyerere (1987) exploited primary school. Agriculture classes were introduced in the schooling curriculum, the starting age was postponed from 5 to 7 years old, and the examination in the middle of the primary cycle was removed. Consequently, pupils leaving the primary schools would be old enough and would have acquired the abilities to work in the fields. To go along with these changes and encourage people to start working after primary school, access to the secondary cycle was drastically limited by regional quotas (Martin, 1988).[3] The results of this UPE plan were considerable: from 1974 to 1978, enrolled children aged 7 to 13 rose from 43.1 to 90.4 percent, and disparities among regions were reduced (Bonini, 2003). By construction, individuals were differently exposed according to their age. Since the official exit age to primary education was 13, individuals older than 13 years old at the beginning of the program (in 1974) should not have been impacted by the program. However, school enrollment in Tanzania often takes place two or three years late (Bommier and Lambert, 2000), and several pilot programs started in 1968. As a consequence, some regions benefited from financial support and from the villagization procedure between 1968 and 1974.

To address these issues, I define a pre-treatment group T_0 to be household heads not affected by the UPE reform, individuals who were older than 13 in 1968 (born between

[2] Most of the time, the distance to their prior dwelling was less than five kilometers.
[3] Despite this policy, no significant drop of the secondary enrollment rate is observed.

Table 1: Villlages in Tanzania

Year	Number of villages	Number of residents
1968	180	58 000
1969	650	300 000
1970	1200	50 000
1971	4484	1 595 240
1972	5556	1 980 862
1973	5631	2 028 164
1974	5008	2 560 474
1975	6944	9 140 229
1976	7658	13 067 220
1978	7768	13 847 000
1979	8200	13 905 000

Source: Shao (1982)

1945 and 1954) and I distinguish T_{pt}, the group that consists of household heads that were likely to be partially treated by the UPE program (born between 1945 and 1960). Then, I define T_{tot}, the treatment group, to include all children who should have been affected by the program. This group, composed of children who were younger than 13 in 1974, can be delineated into several sub-groups (T_1,T_2, and T_3) according to the age of the household head (see Table 2). T_1 gathers children who were likely to be treated at the beginning of the reform (born from 1961 to 1966), while T_2 is composed of children who were likely to be treated at the end of the reform (born from 1967 to 1971). Both the T1 and T2 cohorts were affected by the reform before it ended in 1978. After 1978, school attendance started flattening (King, 1984) but children still benefited from the UPE program infrastructure. To test whether the effect of the UPE program was persistent over time, I define T_3 which includes children who were to young to go to school at the time of the program (born between 1972 to 1978).

2.2 Data

2.2.1 Data sets

This study uses three data sources: a census data set, a household panel survey, and administrative data. First, the census data used are a 10 percent IPUMS sample from the 2002 Population and Housing Census in Tanzania. These data, carried out by the National Bureau of Statistics (NBS), are exaustive and contain basic information on dwelling characteristics, individual demographics and socio-economics for 500, 519 households.

To complete the analysis and provide accurate measure of households' wealth, I combine the LSMS-ISA (LSMS-Integrated Surveys on Agriculture) data, a household panel survey collected by the World Bank in 2008-2009, 2010-2011 and 2012-2013.[4] The LSMS-ISA data include 3265 households in 2008, 3924 households in 2010 and 5015 households in 2012. [5] This dataset gives detailed information on labor activities, on household consumption, and on other individual characteristics. Despite a district reorganization between the dates of the two datasets, both data cover the 26 Tanzanian regions and are representative at the regional and national levels. Finally, I use administrative data collected by the Ministry of Economic Affairs and Development Planning and recorded in Jensen *et al.* (1968). These data gather information on the distribution of primary schools and on GDP[6] by regions and districts for mainland Tanzania, in 1967, just before the introduction of the UPE program. These data are particularly interesting while investigating the effect of the UPE programn because they constitute, to the best of my knowledge, the only source of information on primary school provision in Tanzania at this time. [7]

Restricting the data to the pre-treatment cohort T_0 (individuals born between 1945 and 1954) and to the treatment cohort T_{tot} (individuals born between 1961 and 1978), I obtain from the census data two samples composed of 111, 818 and of 388,701 individuals. Table 2 presents information on the other sub-groups used for robustness checks.

Table 2: Age Cohorts

Age cohorts	Year of birth	Age in 1974	Potential education level during the UPE plan	Obs. IPUMS	Obs. LSMS
T_b	1935-1945	29-39	over postsecondary	77,115	1,083
T_0	1945-1954	20- 29	postsecondary and over	111,818	1,706
T_{pt}	1945-1960	14-19	secondary and postsec.	83,937	1324
T_1	1961-1966	8 -13	primary-secondary	113,063	1,,555
T_2	1967-1971	3-7	no education- primary	103,406	1,408
T_3	1972-1978	not born-2	no education	172,232	2,156
T_{tot}	1961-1978	not born-13	no education-secondary	388,701	5,119

As the purpose of this article is to assess the extent to which the UPE program changes the level of eduation and to estimate the returns to education, it is necessary

[4]From October 2008 to December 2009 for the first wave, from October 2010 to December 2011 for the second wave, and from October 2013 to December 2013 for the third wave.

[5]The number of households is increasing over the three waves due to the high number of split-off households and to the low attrition rate that does not exceed 5 % over the three rounds.

[6]GDP records are divided in sub-activities such as crops, livestock, mining, manufacturing, construction, public utilities, transport, rent, and other services.

[7]The National Bureau of Statistics gives access to the number of schools by region only from 2002.

to first describe how the intensity of the UPE program and the households' wealth are measured.

2.3 Measuring intensity of the UPE program

The UPE program was applied during a limited time frame and targeted regions with poor access to education. Hence, exposure to the program can be captured by two types of variation, across locations and over time.

Since the UPE program seeks to equalize access to education across all locations, the intensity of the program should be a decreasing function of the education level in 1967, the year before the introduction of any program.

This spatial variation can be measured at different administrative geographical units. The smaller the unit, the higher will be the accuracy of the instrument. In this respect, the census district is best suited for the analysis. However, census data report only the district of residence. Because individuals could have moved from one place to another during the UPE program, the place of residence might be endogenous. To avoid this endogeneity issue, I use instead the region of birth, which was determined prior to the program.[8] Consequently, the education level of region j in 1967, $S_{j,1967}$, is the best indicator of the initial education level.[9] Figure A4a maps this variable, and it is clear that, indeed, education was unequal across regions: Zanzibar West and Kilimanjaro had already reached the maximum years of primary education while the average education in others regions was low. In contrast, education levels were higher and more homogenous by the end of the UPE program in 1978 (see figure A5b). Although $S_{j,1967}$ gives a good picture of access to education by region in 1967, it also reflects the demand for education, and so it might be endogenous. To get around this problem, I also use data on school infrastructure and I construct $N_{j,1967}$, the number of primary schools per square kilometer, by region. Figure A4c depicts the distribution of $N_{j,1967}$. Two striking features are evident. Firstly, the schooling supply was very unequal between regions in 1967 and secondly, the supply of schools seems correlated with the distribution of the education level at this time.

[8]Results that use the district of residence as geographical unit are similar and are available upon request.

[9]Given that primary school in Tanzania ends at 13 years old, $S_{j,1967}$ is computed from the education level of individuals born in 1954.

2.3.1 Measuring household wealth

Usually, living standards are measured either by income or by consumption. In developing countries where agriculture is widespread, incomes are very sensitive to current shocks and may not be representative of household well-being (Meyer and Sullivan, 2003) while consumption can be smooth through formal or informal mechanisms. In this respect, consumption has the advantage of being more representative of long-run well being. The second interest of using consumption stems from the fact that income is not similarly measured between activities,[10] which calls into question the reliability of comparison between sectors. Last, but not least, consumption is available for all households, which allows one to avoid selection and imputation issues. Thus, these features advocate the use of consumption rather than incomes data in developing countries. Deaton and Zaidi (2002) propose guidelines to construct a very detailed consumption variable from household survey data. They consist of defining a weighted per capita consumption variable composed of four components, food items, non-food items (electicity, health expenditure, etc), housing consumption (derived from imputing a rent for each household) and consumption from consumer durables. To adjust household consumption for variation in household composition, the consumption variable is divided by an equivalence scale.[11] The Living standard Measurement Study (LSMS) data are particularly well suited for constructing the consumption index since they collect exhaustive information on consumption expenditures. However, the serious limitation of the Deaton and Zaidi (2002) method is such accurate data are costly to collect and are often not included in large datasets. As in most census data, the 2002 Tanzanian census excludes income and expenditures, but records a list of dwelling characteristics and durable goods. A large number of authors ((McKenzie, 2005), (Vyas and Kumaranayake, 2006)) adopt a factor analysis which consists in constructing an asset index based on access to utilities and housing characteristics. This index proves to be useful for measuring inequalities between households and are good proxies for long-term wealth. The additional advantage of these indexes is that they limit measurement errors (Sahn and Stifel (2003), (Filmer and Pritchett, 2001)). Thus, I construct a consumption variable from the Tanzanian

[10]Self-employment income is rarely a wage and agricultural income is measured through production.

[11]The equivalence scale is made from the household's size: every adult represents one unit and each child represents 0.3 units.

LSMS data[12] and a consumption proxy from the 2002 census. To take advantage of the large sample size of the census data and obtain a monetary value of consumption which eases comparision with the literature (Duflo (2001) and Maluccio (1998)), I construct a consumption proxy from the census data by adopting the method developed by (Elbers *et al.*, 2003) and (Tarozzi and Deaton, 2009)) from census data and household survey matching. By following a two step-procedure, I predict household consumption from a set of predictors X that are common to both data sources.[13] The idea behind this method is first to estimate the joint distribution of the consumption, $logC$, and of X from the household survey:

$$LogC_i = bX_i + \nu_i \tag{1}$$

Where ν_i is the error term of household i. Then, I use the estimated distribution to predict $\widehat{logC}$ in the census data. This method is valid only if several assumptions hold. The predictors X should be similarly measured in both data sets. Questions regarding the predictors have the same wording for the two questionnaires, but as Tarozzi and Deaton (2009) suggest, differences may still persist due to differences in questionnaire length or interviewer training.[14] Table A2 gathers coefficients of equation 1 in the LSMS data. As predicted, all dwelling characteristics have a positive and significant impact on consumption. The R-squared coefficient is high (0.53), meaning that the predictors have good explanatory power. Graph A3 plots the relationship between the expected utility $\widehat{logC}$ and $logC$ in the LSMS data.[15] I find a clear positive linear relationship between these two variables. However, $\widehat{logC}$ may not capture all the variation in consumption, especially at the ends of the distribution where large consumption gaps can exist between households who have all the basic dwelling characteristics. As a result, the dispersion for extreme values of $\widehat{logC}$ is larger, but this effect seems negligible. To account for the artificially low variance of $\widehat{logC}$ compared to $logC$, I adopt the method proposed by Barham and Boucher (1998) and Gubert *et al.* (2010). This recommends adding to $\widehat{logC}$, an error term drawn from a normal distribution with the same variance that $\widehat{\nu_i}$ observed in the survey data. To make sure that the results are independent from the random draw,

[12]The data report detailed spending, except for consumer durables.

[13]The number of rooms in the dwelling, whether the household has drinking water, electricity, a phone, a flush toilet, a high quality roof, high quality walls, etc.

[14]To avoid anachronism issues, I do not include in the list of predictors "having a phone" that may have a different meaning across time and across the data.

[15]For each value of $\widehat{logC}$, I compute the average value of $logC$ depicted by a dot.

this procedure is replicated a large number of time. Thanks to this method, the standard errors can be normally interpreted. However, this assumes that ν_i is exogenous, which may not be the case in practise.[16]

2.4 Descriptive statistics

Table 3 reports descriptive statistics computed from the 2002 census for the whole sample, and for the sub-groups of interest : the pre-treatment (old cohort), the treatment group (young cohort), "regions +" and "regions -" that gather regions where the education level in 1967 was above and below the national average, respectively. The data indicate

Table 3: Descriptive statistics from the 2002 census

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	T_0	T_{Tot}	T_0-T_{Tot}	Region$^-$	Region$^+$	Region$^-$-Region$^+$
Age	41,699	51,948	32,113	20,64***	41,806	41,498	0,000271
	(16,252)	(2,800)	(5,051)	(0,0124)	(16,395)	(15,977)	(0,0205)
Men	0,663	0,647	0,692	0,00385**	0,660	0,667	-0,000619
	(0,473)	(0,478)	(0,462)	(0,00130)	(0,474)	(0,471)	(0,000548)
Urban areas	0,425	0,385	0,461	-0,0676***	0,409	0,455	-0,0373***
	(0,494)	(0,487)	(0,498)	(0,00129)	(0,492)	(0,498)	(0,000536)
Years of primary edu,	4,484	3,403	5,612	-2,424***	4,215	4,991	-0,616***
	(3,071)	(2,987)	(2,602)	(0,00732)	(3,147)	(2,854)	(0,00366)
Ended primary edu	0,542	0,321	0,742	-0,439***	0,506	0,610	-0,0729***
	(0,498)	(0,467)	(0,437)	(0,00118)	(0,500)	(0,488)	(0,000582)
Man doesn't work	0,094	0,067	0,062	0,0156***	0,102	0,080	0,0282***
	(0,292)	(0,250)	(0,241)	(0,000802)	(0,303)	(0,271)	(0,000371)
Man works in agri	0,583	0,633	0,560	0,0799***	0,619	0,515	0,0842***
	(0,493)	(0,482)	(0,496)	(0,00148)	(0,486)	(0,500)	(0,000630)
Man is self-emp,	0,177	0,119	0,219	-0,0817***	0,156	0,216	-0,0473***
	(0,382)	(0,323)	(0,414)	(0,00115)	(0,363)	(0,412)	(0,000474)
Man is a wage worker	0,146	0,182	0,159	-0,0138***	0,123	0,190	-0,0652***
	(0,353)	(0,386)	(0,366)	(0,00110)	(0,328)	(0,392)	(0,000453)
Woman doesn't work	0,258	0,202	0,257	-0,0514***	0,255	0,264	0,00241***
	(0,438)	(0,402)	(0,437)	(0,00120)	(0,436)	(0,441)	(0,000503)
Woman works in agri	0,595	0,665	0,567	0,0874***	0,617	0,553	0,0428***
	(0,491)	(0,472)	(0,495)	(0,00135)	(0,486)	(0,497)	(0,000564)
Woman is self-emp,	0,096	0,076	0,117	-0,0319***	0,088	0,110	-0,0175***
	(0,294)	(0,265)	(0,321)	(0,000814)	(0,283)	(0,313)	(0,000331)
Woman is a wage worker	0,051	0,057	0,058	-0,00412***	0,040	0,073	-0,0277***
	(0,221)	(0,231)	(0,234)	(0,000620)	(0,196)	(0,261)	(0,000250)
log ($\widehat{consumption}$)	14,050	14,135	14,083	-0,0530***	13,965	14,211	-0,206***
	(0,720)	(0,771)	(0,680)	(0,00201)	(0,690)	(0,747)	(0,000852)
$\widehat{consumption}$	1702123,000	1961921,000	1688983,000	-45370,1***	1535861,000	2014851,000	-496063,2***
	(1872784)	(2456112)	(1624089)	(8069,4)	(1699485)	(2126491)	(3284,7)
GDP in 1967					231,015	318,822	-91,44***
					(98,673)	(243,845)	(0,199)
Observations	3676116	59326	221677	1026701	529033	280666	3676116

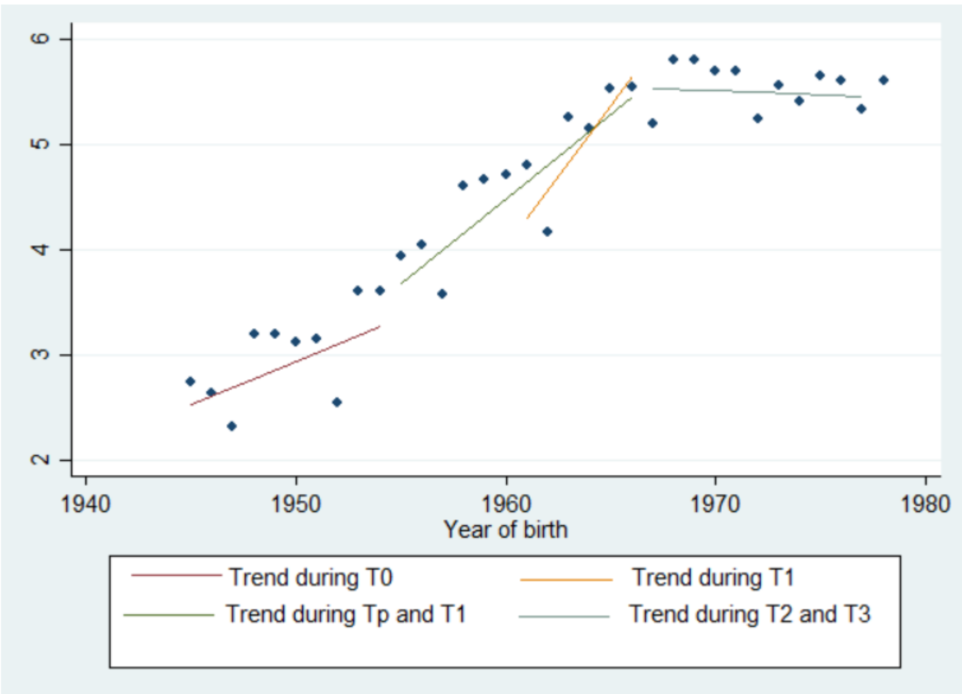
Sources: The 2002 census (IPUMS data). ***,**,* means respectively that the coefficient is significantly different from 0 at the level of 1%, 5% and 10%. Standard deviations are reported in parentheses columns (1) to (3), and (5) to (6). Standard error for average difference are reported in parentheses columns (4) and (7).

that the majority of households are head by men and live in rural areas, although the prevalence of rural households decreases overtime. Regarding the access to education,

[16] In section 4.1, consequences of relaxing this hypothesis will be further studied.

household heads have validated less than five years of education, meaning that some of them did not reach the end of the primary education cycle. However, the comparison the young and the old cohorts informs that the education level has been rising : the number of years has increased by 65 %, and the percentage of households heads who ended the primary education cycle has more than doubled. Graphic 1 reveals that the primary education level has been gradually increasing betweeen age-cohorts, but that the trend has changed. The slope is steeper for the treatment groups (including or excluding the partially treated group) but becomes flat for the following age-cohort. Turning to household heads' activities, one notices that agriculture is still the main activity even though this sector has been shrinking in favor of self-employed activities. Next, I examine whether "regions +" and "regions -" significantly differ and have characteristics that could explain different trends. The data highlight that regions with a low initial education level are more rural, more agricultural intensive and are poorer. These results are not surprising since the density of school in rural areas is epxected to be lower.

Figure 1: Primary education level (in years) by age cohorts.



Sources: The 2002 census.

Table A1 gathers similar descriptive statistics, but computed from the LSMS panel data. Overall, the results tell the same story, but provide two additional information. First, they show that household heads do several activities at the same time, and that this strategy, which can be a useful to protect against economic shocks, is more widespread among the young cohort. Second, the LSMS data allows to compute the households' consumption and the predicted households' consumption. The comparision between these variables informs about the share of the consumption explained by the observable dwelling characteristics included in equation 1. In the whole sample, it amounts about two-third, suggesting that dwelling characteristics have a high explanatory power.

Although education has been increasing between age-cohorts, it does not imply that the UPE program had a causal effect on the education expansion. The following section presents the identification strategy that I use to estimate the effect of the reform and to measure the returns to education.

3 Empirical strategy

To analyze the effect of education on labor market outcomes, I rely on the following basic equation:

$$Y_i = \theta_1 S_i + \theta X_i' + \varepsilon_i \tag{2}$$

where Y_i , the outcome of interest, denotes the consumption per capita or the occupational status. S_i is the years of schooling of the head in household i, X_i' is a set of controls and ε_i is the error term.

As the literature underlines (Card, 2001), education depends on individual choices that may be endogenous. Thus, unobserved omitted variables may influence both education and the outcome interest and simple OLS estimations would lead to inconsistent estimates of θ_1. To address this endogeneity issue, I adopt an instrumental variable approach.

3.1 Identification strategy

The instrumental variable approach that I use is based on the UPE program. This assumes that being exposed to the program increases the probability of being enrolled in school but is orthogonal to unobserved household characteristics that determine labor market outcomes. Because the UPE program is an exogenous source of education variation, it should be a reasonable instrument.

To capture exposure to the UPE program, I adopt a difference-in-differences strategy based on variations in time and in space. It consists of comparing pre-treatment cohorts

($T = 0$) with treated cohorts ($T = 1$), for whom the intensity of the program varied across regions.

Since the program aimed at improving equity of access to education, the intensity should be a decreasing function of the education level before the program, S_{67}. Zanzibar West, that experienced the lowest increase in years of schooling between 1967 and the end of the program in 1978 is considered as untreated and I define the intensity of the program as:

$$I_{j,67} = (S_{Zanzibar\,West,67} - S_{j,67})$$

When $S_{j,67}$ is close to the education level in Zanzibar West, the intensity of the treatment is expected to be small. Inversely, when $S_{j,67}$ is small, the intensity of the treatment is expected to be high. Thus, I instrument education by $T * I_{j,67}$, which captures the UPE program's exposure. This variable is a valid instrument (IV) if two conditions are satisfied: i) the IV is correlated with education, and ii) the IV explains the outcome of interest only through education. In such cases, IV estimates correspond to the Local Average Treatment Effect (LATE). Otherwise, IV estimates give inconsistent and biased results. Since the interpretation of IV estimates relies on the quality of the instrument, I now discuss whether the interaction term $T * I_{j,67}$ is a valid instrument.

The IV variable is a relevant candidate if it is highly correlated with the endogenous variable. To check whether $S_{j,67}$ explains the education expansion by regions, I plot in figure A6 the education increase from the pre-treatment group T_0 to the program period T_1 according to the regional education attainment in T_0. Each dot depicts a region of birth. I show that, indeed, there is a clear negative relationship between the initial education level and the education increase: the UPE program was more intense in regions with low schooling enrollment at T_0. I find the same result in figure A7, where I consider the supply of school instead of the education level. Likewise, the same conclusion can be drawn at the district level in figure A8: the lower the education in T0, the higher is the education increase from T_0 to T_1.[17] However, this relationship is not necessarily causal. The main concern is that the education expansion between T_0 and T_1 is not exclusively due to the UPE program but to other factors correlated with the instrument and the outcome of interest. Despite the fact that the exclusion restriction could not be tested, I try to identify all potential sources that could discredit this condition, and I provide

[17]Each dot represents a district of residence because the districts of birth are not available.

evidence that the instrument is exogenous.

First, I check whether the education expansion is due to the introduction of the UPE program and not to a convergence phenomenon. In case of convergence, less educated regions could have had a higher education increase in order to catch up with the more educated regions. If this were to be true, this phenomenon would be observed before and after the introduction of the UPE program. Subsection 3.2 addresses this question and confirms that during the pre-treatment program, the education progression was not statistically different between educated and non-educated regions, whatever their initial education level. On the contrary, there is a trend reversal during the UPE program period and the education expansion was statistically higher for deprived regions.

The exclusion restriction can still be invalidated if other region's characteristics generate the same trend reversal or are correlated to the outcomes of interest. In order to insure the exogeneity of the instrument, I add a set of controls. Among these control variables, I add the number of children aged 7 to 13 to account for possibility that the education expansion may depend on the size of the cohort. Furthermore, the level of wealth may have non negligible impacts on the development of the schooling supply: wealthy regions can have higher needs in skilled labor and invest more in education. In addition, when a region becomes specialized in a given sector of activity, this region becomes more vulnerable to all the specific shocks of that sector. For instance, regions with a developed agricultural sector are more likely to be sensitive to commodity price shocks while mining regions are probably more perturbed by variations in energy prices. These variations in the labor market organization can also bias the results if they affect the demand for education and the returns to education. Hence, if regions are not homogeneously affected by sectoral economic shocks, the exclusion restriction is not valid and IV estimations are inconsistent. One way to ensure the validity of the instrument is to control for heterogeneity in order to capture variations in shocks between regions. In this respect, I add regional GDP by sector of activity interacted with a time trend. Among these sectors, I distinguish between the following economic activities: crops, livestock, mining, manufacturing, construction, and activities from the tertiary sector, including public utilities, transport, rent, and other public services.

In addition, De Chaisemartin and d Haultfoeuille (2015) highlight that IV estimates can be far from returns to education in any location when the homogeneity assumption does not hold. However, they show that difference-in-difference (DID) methods with fuzzy treated groups[18] should provide unbiased estimates without relying on any homogeneity assumption, as long as 1) the common trend assumption is valid; 2) there is a control group for which the treatment does not change overtime. According to the above results, these two assumptions seem to be satisfied. In this study, 2) implies there is at least one region where education has not evolved between the pre-treatment period and the treatment period, which is precisely the case of Zanzibar West.[19] This lack of education increase has two explanations: education has already reached the maximum years of primary education in 1967[20] and access to secondary education was cut at time of the UPE program.

Last but not least, IV estimates are biased if the program has influenced outcomes other than education that explain the level of consumption. Thinking of forced villagization, this is very likely to happen. Among the possible channels, the program could have changed the access to other social services and the living conditions. Nonetheless, this should not call into question the validity of the instrument in this particular case. Indeed, the specificity of the villagization program is that all individuals were concerned since entire families were asked to move. As a consequence, the treatment group and the control group were similarly affected by these changes. On the contrary, the education policy consisting in education reforms was beneficial only for the treatment group and had no reason to affect outcomes other than education.

3.2 The impact of the UPE program on education expansion

Since education may be endogenous, I adopt a two-stage procedure, the first stage of which is :

$$S_{ijt} = \alpha + \beta_j + \beta_t + \gamma T * I_{j,1967} + \delta t * X_{j,1967} + \mu_{ijt} \tag{3}$$

β_j and β_t are region-of-birth fixed effects and birth-cohort fixed effects to account for permanent differences across regions and over time. T is a dummy taking the value 0 for people belonging to T0 and 1 for people belonging to either T1, T2, T3 or T_{tot}. $I_{j,1967} * T$ captures the intensity of the UPE program and $X_{j,1967}$ is a set of region characteristics.

[18]This refers to DID when the intensity of the treatment varies between treated groups.
[19]The education level decreases by 0.1 year between 1967 and 1978 which is negligible.
[20]Zanzibar, independent in 1964, benefited from a better access to education.

It includes the log of population aged 7 to 13 and regional GDP by sectors of activity in 1967. These controls are interacted with a time variable t. The coefficient of interest, γ, represents the effect of the UPE program on education (years of schooling). When $I_{j,1967}$ increases, the education expansion between T0 and the treatment groups are expected to be larger.

Table 4: Effect of the program on education: γ coefficients of equation (3)

Dependant variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	T_{tot}		T1		T2		T3	
Years of education	0.369***	0.336***	0.277***	0.226***	0.418***	0.373***	0.435***	0.443***
	(0.064)	(0.060)	(0.057)	(0.053)	(0.050)	(0.078)	(0.065)	(0.089)
R-squared	0.276	0.273	0.318	0.315	0.339	0.337	0.298	0.297
F-test	33.38	31.83	23.70	18.60	70.14	23.34	45.55	25.97
Primary completion	0.038***	0.030**	0.029**	0.015	0.045***	0.031*	0.042***	0.036**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.017)	(0.013)	(0.017)
R-squared	0.238	0.238	0.289	0.291	0.329	0.332	0.283	0.285
F-test	8.940	5.314	5.181	1.467	12.89	3.367	10.22	4.472
Sample	T0 +T1+T2+T3		T0 +T1		T0 +T2		T0 +T3	
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP Controls	no	yes	no	yes	no	yes	no	yes
Observations	440,676	423,419	199,450	190,148	190,466	182,454	247,502	238,723

Source: the 2002 census. Notes: Standard errors are clustered at the region of birth level and are reported in parentheses. ***,**,* mean respectively that the coefficient are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household size and the principal sector of activity of the household head.

Table 4 reports the results of equation 3. I distinguish the effect for the whole treatment group T_{tot} and by age cohorts T1, T2 and T3. To consider the possible serial correlation in errors, I cluster standard errors at the regional level (Bertrand *et al.*, 2004). Without GDP controls, I find that when the predicted intensity $I_{j,67}$ is raised by one additional year, education increases by 0.34 between T_0 and T_{tot}. This result is consistent with the idea that the UPE program targeted regions with low initial education attainment and contributed to the equalization of access to education among regions. We can deduce from columns (3), (5) and (7) that the effect of this program was progressive: when $I_{j,1967}$ increases by one year, the education expansion increases by 0.28 years of education from T0 to T1, by 0.42 years of education from T0 to T2 and by 0.44 years of education from T0 to T3. These results are in line with the expectations: the effect becomes larger when the exposure to the treatment increases. For all estimations, F-test values are large. When I introduce GDP controls interacted with a time trend (columns (2), (4), (6) and (8)). The effect of $I_{j,1967}$ is still positive and significant but the coefficient is slightly lower. It is worth noting that the introduction of this control changes the sample of the analysis since the regional GDP variables are only available for mainland Tanzania. The bottom panel of Table 4 indicates whether the UPE plan had fully reached its goal by convincing people not only to enroll in school but also to complete primary education. It is not obvious since primary education lasts seven years and the UPE plan was strictly implemented for four years. When I control by regional GDP, reported result for T_{tot} indicates that $I_{j,1967}$ significantly increases the primary completion by 3.1 percent age points. However, F-test values are lower, suggesting that $I_{j,1967}$ is a much stronger predictor for the number of years of education than for primary completion.

As a robustness check, I also test the impact of the density of schooling infrastructures $N_{j,1967}$. Since I do not have information of the number of schools in Zanzibar, I choose Kilimanjaro as the reference[21] and the intensity is rewritten as a decreasing function of the number of schools $I'_{j,67} = (N_{Kilimanjaro,67} - N_{j,67})$. Thus, the first-stage equation becomes:

$$S_{ijt} = \alpha + \beta_j + \beta_t + \gamma T * I'_{j,1967} + \delta_t X_{j,1967} + \mu_{ijt} \tag{4}$$

Table A3 presents the estimates for mainland Tanzania, which are consistent with the previous results. Coefficients are positive and significant for all treatment groups. When the intensity increases by one additional school per square kilometer, the education expansion between the control and the treatment groups is raised by 0.05 year to 0.08 years of education, depending on the specification.

I also estimate equations 3 and 4 using the LSMS data. Results, gathered in Table A4, provide a less clear message. F-test values are much lower, and the effects are not robust to the introduction of GDP controls and to the change of instrument. This is probably explained by the smaller sample sizes of the sub-samples.[22] Thereafter, I use only the 2002 census to study the impact of UPE program on education.

I also estimate a more flexible regression that allows the effect of the UPE program to vary according to the time exposure to the program:

$$S_{ijt} = \alpha + \beta_j + \beta_t + \sum_{t=1945}^{1954} \gamma_t I_{j1967} + \sum_{t=1961}^{1978} \gamma_t I_{j1967} + \delta_t X_{j1967} + \mu_{ijt} \tag{5}$$

[21]Kilimanjoro is the second most educated region in 1967, and it also experienced a negligible education expansion from 1967 to 1978.

[22]Few observations are available by region and by year, which prevents to capture any significant effect.

In this equation, γ_t indicates age-cohort coefficients. It measures the effect of the reform by age-cohort. The difference between γ_t and γ_{t+1} represents the education expansion between t and t+1 generated by the education level in 1967. For the pre-treatment group, $I_{j,67}$ should have no impact on education expansion and γ_t values should be close to 0, while for treated groups, regions with a high predicted intensity $I_{j,67}$ should benefit from a larger education progression and γ_t values should be increasing . This is precisely what

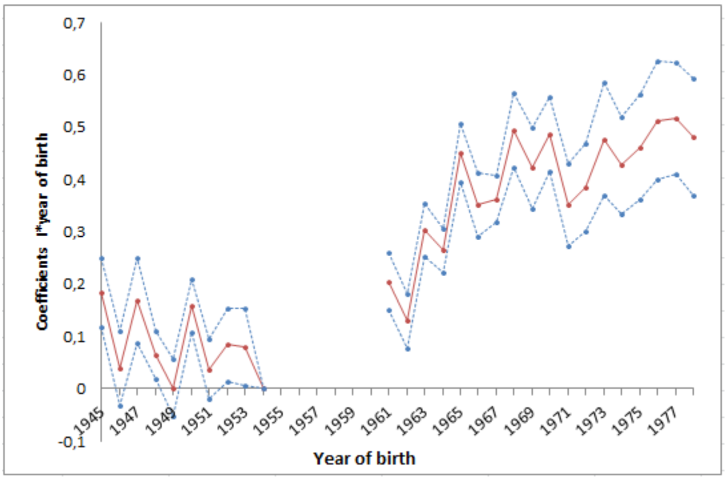


Figure 2: γ_t coefficients of the interaction between age-cohorts and education level by region in 1967.

Source: 2002 census (IPUMS data).

is shown in figure 2. Each dot depicts the γ_t coefficients of equation 5. The reference year is the year before the introduction of the UPE program, in 1967, which corresponds to the age-cohort born in 1954. As expected, most of the coefficients in the pre-treatment program were not statically different from 0. From 1961 to 1978, γ_t coefficients steadily increased by 0.4 point. Cohorts born after 1968 were still exposed but the slope declines afterward. This graph confirms that the identification strategy is reasonable: the trend was not present before the program and the UPE program had a significant impact on education for the treated cohorts (all coefficients are significant at 1% level). Thus, if no regional time-varying characteristics correlated with the program's intensity are omitted, these fuzzy difference-in-difference results should correctly estimate the impact of the UPE program.

4 The results

This section presents the main results. The first sub-section is devoted to the returns to education for the entire population. To better understand the distribution of these returns, I also compare in sub-section 4.1.3 the returns to education across different sectors of activity. Education may also have the benefit of increasing the probability to work in sectors that are better paid. Then, subsection 4.3 investigates whether the returns to education come from the returns "within sectors" or from a "distribution effect".

4.1 The returns to education

4.1.1 For the entire population

I measure the returns to education by looking at the effect of education S_{ijt} of household head i born in region j at year t on current consumption C_{ijt}. The main equation is:

$$Log(C_{ijt}) = \alpha + \beta_j + \beta_t + \theta S_{ijt} + \delta_t X_j + \epsilon_{ijt} \tag{6}$$

where β_j and β_t are, respectively, region-of-birth and year-of-birth fixed effects. Regional controls X_j are also included. For the sake of comparison with my earlier results, this equation is estimated separately for the same samples (T_{tot}, T1, T2 and T3).

I first ignore the potential endogeneity of education and run OLS regressions. Several conclusions can be drawn from Table 5. In the top panel, the estimates indicate that education increases the log consumption aggregate (constructed using the Deaton and Zaidi method) by 7%. Comparing the middle and bottom panels, the estimates from the consumption proxy $\widehat{LogC_{ijt}}$ are very similar for the LSMS data and for the census data and, around 4.2%. Notwithstanding, there is an important gap between consumption estimates and consumption proxy estimates. To correctly interpret these results, it is necessary to clarify under which conditions the proxy for consumption is a relevant outcome.

In traditional settings, θ in equation 6 captures the causal impact provided that S_{ijt} is not correlated with ϵ_{ijt}. In contrast, using the proxy for consumption introduces an additional assumption: $\hat{b}$ and $\widehat{\nu_{ijt}}$ estimates from equation 1 should be unbiased. Recall that $\widehat{\nu_{ijt}}$ was drawn from a normal distribution and was assumed to be exogenous.

Table 5: OLS estimates of the returns to education

	T_{tot}		T1		T2		T3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LSMS: Deaton and Zaidi consumption aggregate $log(C)$								
	0.070***	0.071***	0.069***	0.069***	0.070***	0.070***	0.072***	0.073***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)	(0.007)	(0.005)	(0.005)
R-squared	0.425	0.425	0.478	0.480	0.443	0.447	0.463	0.464
LSMS: $\widehat{log(C)}$								
	0.041***	0.041***	0.043***	0.043***	0.044***	0.044***	0.042***	0.0427***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.00325)
R-squared	0.570	0.569	0.633	0.634	0.604	0.606	0.583	0.584
Observations	5,820	4,982	2,699	2,282	2,556	2,138	3,215	2,816
IPUMS: $\widehat{log(C)}$								
	0.042***	0.042***	0.045***	0.045***	0.046***	0.046***	0.043***	0.043***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R-squared	0.574	0.575	0.601	0.603	0.595	0.595	0.581	0.582
Sample	T0 +T1+T2+T3		T0 +T1		T0 +T2		T0 +T3	
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP Control	no	yes	no	yes	no	yes	no	yes
Observations	440,676	423,419	199,450	190,148	190,466	182,454	247,502	238,723

Notes: Standard errors are clustered at the birth region level and are reported in parentheses. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household size and thesector of activity of the household head.

Yet, ν_{ijt}, the consumption part not explained by households' dwelling characteristics, results from households' preferences and is likely to be correlated with education of the households head. For instance, educated household heads may be more willing to spend money for the education or health of their children. If so, there is a remaining endogenous part of the residual ν''_{ijt} ($\nu''_{ijt} = \nu_{ijt} - \widehat{\nu}_{ijt}$) that is not captured by the drawn residual $\widehat{\nu_{ijt}}$. Thus, equation 6 can be rewritten:

$$LogC_{ijt} = \hat{b}X_{jt} + \widehat{\nu_{ijt}} = \alpha + \beta_j + \beta_t + \theta S_{ijt} + \delta_t X_j + \epsilon_{ijt} - \nu''_{ijt} \tag{7}$$

And $\widehat{\theta} = \theta + \frac{cov(\epsilon_{ijt}, S_{ijt})}{V(S_{ijt})} - \frac{cov(\nu''_{ijt}, S_{ijt})}{V(S_{ijt})}$.

The positive correlation between education and ϵ_{ijt} leads to the traditional upward bias, while the positive correlation between education and ν''_{ijt} causes downward bias in the coefficient of interest. In conclusion, if ν_{ijt} is not purely exogenous, using the proxy for consumption adds an additional source of bias.

To obtain consistent estimates of θ, I instrument education and I rely on the first-stage equations 5 and I impose that each γ_{jt} equals 0 for the pre-treatment cohorts:

$$S_{ijt} = \alpha + \beta_j + \beta_t + \sum_{t=1961}^{1978} \gamma_t I_{j1967} + \delta_t X_j + \mu_{ijt}$$

In this equation, γ_t identifies the effect of the UPE program by age-cohort in comparison with the pre-program period T_0. If the instrument is not correlated with ν nor with ϵ, and if the relevance condition is valid, IV estimates identify the causal impact of education. Even though this first condition cannot be empirically tested, the intensity of the treatment has no obvious reason to be correlated with ν and ϵ, except though education.

Table 6: Estimations of education on $\widehat{consumption}$

	T_{tot}		T1		T2		T3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS estimates								
	0.042***	0.042***	0.045***	0.045***	0.046***	0.046***	0.043***	0.043***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R-squared	0.574	0.575	0.601	0.603	0.595	0.595	0.581	0.582
IV estimates with $T * I_{j,1967}$								
$S_{j67} * T$	0.075***	0.082***	0.077***	0.067**	0.061***	0.071***	0.079***	0.088***
	(0.019)	(0.022)	(0.028)	(0.029)	(0.020)	(0.017)	(0.016)	(0.019)
R-squared	0.455	0.446	0.510	0.523	0.511	0.505	0.459	0.443
F-test	33.38	31.83	23.70	18.60	70.14	23.34	45.55	25.97
IV estimates with $\sum_{t=1961}^{1978} \gamma_t * i_{j,1967}$								
	0.068***	0.073***	0.065**	0.049**	0.060***	0.067***	0.078***	0.087***
	(0.017)	(0.016)	(0.026)	(0.019)	(0.021)	(0.017)	(0.015)	(0.019)
R-squared	0.468	0.463	0.527	0.537	0.517	0.513	0.464	0.450
F-test	188.7	131.6	24.18	17.73	35.21	9.094	21.86	7.266
Observations	440,676	423,419	199,450	190,148	190,466	182,454	247,502	238,723
IV estimates with $I'_{j,1967} * T_{tot}$								
$N_{j67} * T$	0.076***	0.093***	0.0916***	0.0887***	0.068***	0.078***	0.0721***	0.081***
	(0.020)	(0.025)	(0.0185)	(0.0265)	(0.017)	(0.023)	(0.0207)	(0.026)
R-squared	0.232	0.189	0.305	0.312	0.326	0.312	0.259	0.242
F-test	69.16	21.55	42.31	35.36	97.80	27.36	82.75	20.75
IV estimates with $\sum_{t=1961}^{1978} \gamma_t * I'_{j,1967}$								
	0.067***	0.076***	0.082***	0.072***	0.066***	0.073***	0.072***	0.081***
	(0.019)	(0.021)	(0.016)	(0.018)	(0.018)	(0.021)	(0.021)	(0.026)
R-squared	0.247	0.233	0.326	0.344	0.327	0.318	0.258	0.242
F-test	37.90	39.75	23.67	12.52	36.92	6.640	18.05	7.303
Observations	423,419	423,419	190,148	190,148	182,454	182,454	238,723	238,723
Sample	T0 +T1+T2+T3		T0 +T1		T0 +T2		T0 +T3	
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP Controls	no	yes	no	yes	no	yes	no	yes

Source: the 2002 census. Notes: Standard errors are clustered at the birth region level and are reported in parentheses. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

Table 6 reports the 2-SLS estimates for the 2002 census. When I consider the entire treatment period T_{tot} and I add controls for GDP by sectors of activity, I find that one additional year of education of household head increases the log of household consumption between 7.1 and 9.3 %. F-statistics are high which suggests that the instruments have strong predictive power, and results are robust to the instrument and to specifications.

In comparison with OLS estimates, coefficients are larger. With regard to the ability bias, this result is counter-intuitive: educated individuals have higher abilities, θ captures both the education and the ability effect and OLS estimates are over-estimated. However, the opposite effect can be observed when education is measured with error (Griliches, 1977) and when returns to education are heterogenous[23] (Card, 2001). In this framework, the most plausible explanation is that instrumenting education removes the negative bias from the correlation between education and ν.

To test whether IV estimates are unbiased, I implement a series of robustness checks. First, I test whether results are robust when I use the primary education level by region in 1967, $P_{j,1967}$ instead of $S_{j,1967}$.[24] Table A5 show that IV estimates are not very sensitive to this choice: the returns are about 1% lower, but the difference is not statically significant. Finally, I test whether the introduction of partial treated individuals T_{pt} in the treatment group changes the results and I deduce from Table A6 that the estimates are very similar and are not statically different. In addition, is worth emphasizing that these IV estimates are close to those in the existing literature (Maluccio (1998), Duflo (2001)).

Heretofore, standard errors are clustered at the level at which the instrument is defined, in other words, at the regional level. Yet, a small number of clusters can lead to over-rejection of standard asymptotic tests (Cameron *et al.*, 2008). To check whether I under-estimate the standard errors, I instrumenting education with the intensity $I_{j',67}$ defined at a lower scale, the district of residence j'.[25]. Since I construct this variable from the district of residence , this method provides biased estimates in case of selective migration.[26] Table A7 shows that results are very similar. Coefficients are close and are still significant at the 1% level. This entails that selective migration and over-rejecting issues are negligible.

[23]When the instrument affects the education choices of less-educated subgroups, which have high marginal returns to education, IV estimates are upward-biased. Regarding the UPE program that focused on individuals with restricted access to primary schools, IV estimates may over-estimate the average marginal returns to education of the entire population.

[24]$P_{j,1967}$ represents the education level but cannot be above 7, the length of primary education.

[25]In 2012, there were 31 regions against 169 districts in Tanzania.

[26]I construct the instrument from the district of residence instead of the district of birth, which is not available.

4.1.2 Quality bias

The analysis of these IV estimates is controversial if the UPE program affected both the quantity and the quality of education (Duflo, 2001). If so, the UPE program's results would confound these two entangled effects. This massive education program could have lowered the quality of education if it had led to overcrowded rooms (Little, 2008) or if the quality of teachers and the status of teaching had fallen (Towse *et al.*, 2002). To test whether it is true, I compare the returns to education from different treated age-cohorts (T1, T2 and T3) that were affected by the UPE program with different intensities. For instance, the T1 cohort was exposed to the program at the end of their education while the T2 cohort was exposed to the program from the beginning of their education. At this period, primary schools had to face up to an increasing number of pupils. From 1974 to 1978, the number of students in grade 1 increased from 200,000 to 901,770 students and these rapid changes could have lowered the quality of education (King, 1984). On the contrary, the T3 cohort was only indirectly treated by the program and primary enrollment started flattening out for the T3 age-cohort (King, 1984) (see Table 1). In the meantime, primary education was exposed to structural changes [27] and quality of education was defined as the new priority (Bonini, 2003). Then, one would expect that quality of education would be lower for T1 and T2 and would be higher for T3. If so, returns to education in T1 and T2 would constitute a lower bound while returns to education in T3 would constitute a higher bound. This is precisely what is observed in Table 6. Whatever the instrument and the specification, returns to education are higher for the T3 cohort. However, these differences are small and are almost never significant. This comparison from different age-cohorts suggests that the UPE program may have lowered quality of education but to a small extent.

4.1.3 Returns to education by sector of activity

So far, returns to education have been estimated for the whole population. However, they can vary from one sector of activity to another. In this subsection, I investigate this

[27]The structural changes started in 1986 when Tanzania signed agreements with the IMF and the World Bank.

question and I estimate the consumption equation for each sector:

$$Log(C_{iajt}) = \alpha_a + \beta_{aj} + \beta_{at} + \theta_a S_{ijt} + \delta_{ta} X_j + \epsilon_{iajt} \tag{8}$$

where the subscript "a" depicts the household head main activity and indicates whether he: 1) does not work or is unpaid, 2) works in agriculture, 3) works in non-farm self-employed activities, or 4) is a wage-worker.

Table 7: IV estimates of the returns to education by sectors

Activity	Not paid Don't work (1)	agri. (2)	self employed (3)	wage-work (4)	Not paid Don't work (5)	agri (6)	self employed (7)	wage-work (8)
OLS estimates								
	0.030***	0.033***	0.062***	0.055***	0.029***	0.033***	0.064***	0.055***
	(0.006)	(0.002)	(0.003)	(0.002)	(0.006)	(0.002)	(0.003)	(0.002)
R-squared	0.545	0.415	0.505	0.457	0.543	0.416	0.511	0.464
IV estimates: $T * I'_{j,1967}$								
	0.491	0.074***	0.096***	0.098***	0.116	0.097***	0.103***	0.048**
	(0.871)	(0.024)	(0.023)	(0.024)	(0.119)	(0.028)	(0.025)	(0.024)
F-test	0.400	25.75	72.65	37.01	3.842	8.315	15.69	65.22
IV estimates with sample selection correction (IV for occupation equation: $T * I'_{j,1967}$								
	0.312	0.075***	0.095***	0.087***	0.190	0.096***	0.110***	0.048*
	(0.437)	(0.024)	(0.026)	(0.025)	(0.183)	(0.028)	(0.029)	(0.027)
Mills no work	-0.008				-0.010			
		(0.003)			(0.008)			
Mills agri.		-0.011***				-0.010***		
	(0.010)					(0.003)		
Mills self.			0.004				0.004*	
			(0.003)				(0.003)	
Mills wage				0.025**				0.020
				(0.012)				(0.014)
F-test	0.547	25.85	56.73	28.88	6.677	8.569	13.21	51.74
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP control	no	no	no	no	yes	yes	yes	yes
Observations	3,518	277,747	81,396	60,536	3,518	277,747	81,396	60,536

Source: the 2002 census. Notes: Standard errors are clustered at the region of birth level and are reported in parentheses. In IV estimations, standard errors are bootstraped. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

The first panel of Table 7 presents the OLS results. It shows that returns to education are much lower in agriculture than in the non-farm self-employment activities and in the formal (wage work) sector. However, 2SLS estimates (Table 7 and A9) do not lead to the same conclusion: returns to education are higher in agriculture and in non-farm self-employed activities than in the wage-activities.

By comparison, IV estimates are three times, 32% and 15% larger than OLS estimates in agriculture, non-farm self-employed activities and wage-workers activities, respectively. These ratio of IV to OLS estimates represents the size of the bias. Several possible explanationss can explain these discrepancies.

A first possible explanation is that the correlation between education S_{ijt} and the error term ϵ_{ijt} differs across sectors. For instance, if working in the formal sector requires higher abilities, the ability bias will be higher for wage-workers. Second, it may be explained by different correlation between S_{ijt} and the error term ν_{ijt}, the determinants of households' consumption unexplained by dwelling characteristics.[28]

Finally, these differences of bias can be explained by the nature of the UPE program itself. The aim of this program was to improve agricultural skills and to boost rural productivity through agricultural classes (Kinunda, 1975). Thus, IV estimates probably capture a LATE, representative of the schooling curriculum at this time. This could explain why individuals who benefited from this program have higher returns to education in agriculture.

4.1.4 Sample selection bias

I do observe household consumption for the population, but once I estimate the returns to education for non-random sub-samples such as sectors of activity, I may encounter sample selection issues. To address this, I adopt the two-stage model proposed by Wooldridge (2010) that deals with the endogeneity of education and the selection issue by using an exclusion restriction variable in the first stage equation and in the selection equation (see appendix A for more details). Since the UPE program was expected to influence both access to education and the labor market organization, I use the intensity of the UPE program, as the excluded variable for both equations.

Results with sample selection correction are reported at the bottom panel of Table 7 and A9. The introduction of sample selection corrections does little to change the 2SLS estimates: the returns are still much higher in the agricultural sector and in the self-employed sector while they are lower in the formal sector.[29] Furthermore, there is little evidence of sample selection bias. Coefficients of the Mill's ratio are close to 0 and are not statistically significant except in the agricultural sector. Thus, estimating the consumption by sectors of activity does not seem to generate significant sample selection

[28] Yet, this does not seem an empirical issue. Even though expenditures are slightly lower in agriculture, the distributions of education spending and food items (see figures A1 A2) which capture schooling preferencesn are similar across sectors.

[29] When instrumenting education with $I'_{j,67}$, the difference between agriculture and wage-work activities is still statistically significant.

issues.

4.2 Effect of education on the labor market participation

Education can also ease the access to sectors that require skilled labor. In a rural country where the government promoted education to increase the agricultural productivity, it could be interesting, both from a macroeconomic and a microeconomic perspective, to identify the effect of education on the distribution between the sectors of activity. I estimate a multinomial logit model where A_{ijt} is the sector of activity, taking the value of 1 if the individual does not work or are unpaid, 2 if the individual works in the agricultural sector, 3 if the individual is self-employed in non-farm activities and 4 if he has a wage-work. The activity equation has the following functional form:

$$A_{ijt} = \alpha + \beta_j + \beta_t + \theta S_{ijt} + \delta_t X_j + \epsilon_{ijt} \tag{9}$$

To avoid endogeneity issues, I instrument education by the exposure to the UPE program and I follow a two-step Control Function approach (Wooldridge, 2014). After obtaining the predicted residual from the first stage equations, I plug it into equation (9). This predicted residual is also used to test the endogeneity of education.

Results are reported Table 8. From OLS estimates, I observe that education decreases the probability of being unemployed, of working in agriculture and in non-agriculture self-employed activities while it increases the probability of having a wage work. However, IV estimates show a completely different picture. Education raises the probability of working in agriculture and reduces the probability of being self-employed in non-farm activities. These effects, robust to the instrument used, probably identify the LATE explained by the specificity of the UPE program. In most estimations, the predicted residuals are statistically different from 0 which confirms the importance of dealing with the endogeneity of education.

4.3 Decomposition of the education effects

To investigate the relative impact of education on consumption and on the probability of working in each sector of activity, I base my analysis on the following expected consumption: $E(C) = \sum_{a=1}^{n} P_a * C_a$, where P_a is the probability of working in the sector

Table 8: Average marginal effect of education on the probability of working in each sector of activity (mult. logit)

Activity	Don't paid Don't work (ref) (1)	agri. (2)	self employed (3)	formal (4)	Don't paid Don't work (ref) (5)	agri (6)	self employed (7)	formal (8)
OLS	-0.001***	-0.003*	-0.004***	0.007***	-0.001***	-0.003*	-0.004**	0.007***
	(0.000)	(0.002)	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)
Observations		443,541				426,261		
Instrument: $I_{j1967} * T_{tot}$	0.000	0.017***	-0.020***	0.002	-0.000	0.015**	-0.010*	-0.005
	(0.001)	(0.004)	(0.005)	(0.003)	(0.001)	(0.007)	(0.006)	(0.004)
$\hat{\mu_{ijt}}$		0.045	0.336***	0.312***		0.059	0.300*	0.374**
		(0.074)	(0.080)	(0.076)		(0.158)	(0.160)	(0.157)
F-test		33.25				30.96		
Observations		443,541				426,261		
Instrument: $I'_{j1967} * T_{tot}$	0.001	0.010**	-0.013***	0.003*	0.001	0.006*	-0.007**	-0.001
	(0.001)	(0.004)	(0.004)	(0.002)	(0.001)	(0.003)	(0.003)	(0.002)
$\hat{\mu_{ijt}}$		0.095	0.284***	0.287***		0.111	0.3066***	0.328***
		(0.059)	(0.071)	(0.069)		(0.074)	(0.103)	(0.088)
F-test		69.53				48.14		
Observations		426,261				426,261		
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP control	no	no	no	no	yes	yes	yes	yes

Sources: 2002 census. Notes: Standard errors, reported in parentheses, are bootstraped and clustered at the birth region level. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. CF-IV: IV estimates with control function method. Additional controls are the population aged 7 to 13 in 1967 and the household's size.

of activity a and C_a is the consumption level of individuals working in activity a. If education impacts both the choice of the sector of activity and the level of consumption in these respective sectors, I can decompose the education effect in two parts:

$$\frac{\delta E(C)}{\delta S} = \overbrace{\sum_{a=1}^{n} \frac{\delta P_a}{\delta S} * C_a}^{distribution\ effect} + \overbrace{\sum_{a=1}^{n} P_a * \frac{\delta C_a}{\delta S}}^{intra\ sector\ effect} \tag{10}$$

The first term represents the monetary benefit of education due to the change in the distribution between sectors and the second term corresponds to the returns to education within sectors. More specifically, $\frac{\delta P_a}{\delta S}$ is the effect of education on the probability of working in activity a (see Table 8) and $\frac{\delta C_a}{\delta S}$ is the return to education by activity (see Table A9 and Table 7). C_a and P_a are approximated by the predicted values of $\hat{C_a}$ and $\hat{P_a}$ from equation 8 and equation 9, respectively.

Table 9 provides results from equation 10. OLS estimates show that both the distribution and the intra-return effects are positive and significant. I conclude from this decomposition that total returns to education come only from the intra-returns effect. IV estimates suggest that the intra-returns effect is the main effect while the distribution

Table 9: The cumulative effect of education

	(1)	(2)	(3)	(4)	(5)	(6)
Model	OLS		IV: $S_{j,1967} * T_{tot}$		IV: $N_{j,1967} * T_{tot}$	
Distribution effect	0.0018***	0.002***	-0.0023***	-0.002 ***	-0.002***	-0.002 ***
	(0.001)	(0.001)	(0.003)	(0.003)	(0.005)	(0.001)
Intra sector effect	0.042***	0.042***	0.081***	0.093***	0.081***	0.091***
	(0.010)	(0.010)	(0.0029)	(0.014)	(0.007)	(0.014)
Cohort FE	yes	yes	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes	yes	yes
GDP Control	no	yes	no	yes	no	yes
Observations	440,452	423,191	440,452	423,191	423,191	423,191

Source: the 2002 census. Notes: Standard errors are clustered at the region of birth level and are reported in parentheses. Since results are produced from a multi-stage procedure, standard errors are bootstraped. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

effect is much small and slightly negative. Since education increases the probability of working in the agricultural sector (see section 4.2), and average consumption is lower in this sector, this effect is not surprising and illustrates the specificity of the UPE program.

5 Conclusion

This paper studies the benefits of education in Tanzania and considers two particular dimensions, household consumption and the choice of the sectors of activity. To deal with engogeneity issues, I instrument education of household heads by exploiting variation in time and in space of the exposition to the Universal Education Program.

I find that this massive primary education program contributed to a reduction of inequalities among regions. After this program ended, its effects persisted for the next age-cohorts. Despite the controversial means of villagization, the Tanzanian government fulfilled its goals by improving access to basic education, even in remote areas. Unfortunately, several changes were implemented at the same time, which prevents one from identifying the relative efficiency of each policy.

By using a household survey, census data, and records on the number of schools, I find that education increases household consumption between 7.3 and 9.3 percent, depending on the specification and the instrument. This analysis has the advantage of focusing on the entire population, instead of wage-workers, who are in the minority in most developing countries and are very likely to be self-selected. I also compare the returns to education between sectors of activity. I find that the returns to education are higher in agriculture and in non-farm self-employed activities than in wage-work activities. This conclusion, at first sight surprising, is consistent with the Tanzanian governmental policy that aimed to put education at the service of agriculture by teaching agricultural skills. Compared to the few studies on the benefits of primary education in agriculture in African countries that find low returns (Appleton *et al.*, 1996; Jolliffe, 2004), I argue that returns to education in agriculture are positive, provided that the curriculum at school is consistent with agriculture. This gets closer to Foster and Rosenzweig (1996)'s results suggesting that returns to education are positive only during specific contexts such as during technological changes, when education helps farmers to adopt new technologies.

This suggests that the introduction of agricultural classes could help households to escape poverty by increasing the farmers productivity. In terms of public recommendations, this result is all the most relevant in a context where the large majority of indidivuals work in rural activities and where the government has a limited range of intervention tools to support farmers.

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A Measuring the effect of education by sector of activity with the Heckman selection model

To overcome endogeneity issues and selection issues, I follow the Wooldridge (2010) 's method. It consists in estimating three different equations :
$Log(C_{iajt}) = \alpha_{1a} + \beta_{1aj} + \beta_{1at} + \theta_{1a} S_{ijt} + \delta_{1ta} X_j + \epsilon_{1isjt}$

$A_{ijt} = \alpha_{2a} + \beta_{2aj} + \beta_{2at} + \theta_{2a} I_{j,67} * T + \gamma_{2a} N_{ijt} + \delta_{2ta} X_j + \epsilon_{2iajt}$

$S_{ijt} = \alpha_{3a} + \beta_{3aj} + \beta_{3at} + \theta_{3a} I_{j,67} * T + +\delta_{3ta} X_j + \epsilon_{3iajt}$

The first equation is the equation of interest, the consumption equation by sector of activity a. The second equation is the selection equation. A_{ijt} is the main sector of activity of households head (unemployed or domestic unpaid workers, wage-workers , self-employed workers in non agricultural activities and self-employed workers in agriculture). The third equation represents the endogenous education equation of househould i.

I_{j1967}, is the instrument for both S_{ijt} and for A_{ijt}. To obtain unbiased estimates of the impact of education on consumption, I compute the inverse Mills ratios $\hat{\lambda_{ia}}$ with from the predicted probabilities in the selection equation. Then, I introduce the inverse Mills ratios into the consumption equation :

$$Log(C_{iajt}) = \alpha_{1a} + \beta_{1aj} + \beta_{1at} + \theta_{1a} S_{ijt} + \delta_{1ta} X_j + \gamma_{1a} \hat{\lambda_{ia}} + \epsilon_{1iajt} \quad (11)$$

This equation is estimated by 2SLS, using $I_{j,67}$ as an instrument. Standard errors have to be bootstraped to account that it is a two-step procedure. The sample selection issue can be tested by checking whether γ_{1a} is significantly diffferent from 0.

As Wooldridge (2010) underlines, if the same instruments are used for the occupational equation and for the consumption equation, the introduction of the Mills ratio generates collinearity that may affect performance in the case of small samples. Since, sub-samples' size are very large in this analysis, this collinearity issue should be limited.

B Sample and statistic descriptives

Table A1: Descriptive statistics from the LSMS panel data

	(1) All	(2) T_0	(3) T_{Tot}	(4) T_0-T_{Tot}	(5) Region$^-$	(6) Region$^+$	(7) Region$^-$-Region$^+$
Age	46,992	60,283	40,396	19,89***	47,129	46,778	0,351
	(15,875)	(3,128)	(5,470)	(0,139)	(16,257)	(15,314)	(0,292)
Men	0,248	0,307	0,211	0,0952***	0,255	0,238	0,0163*
	(0,432)	(0,461)	(0,408)	(0,0118)	(0,436)	(0,426)	(0,00793)
Urban areas	0,352	0,309	0,364	-0,0541***	0,306	0,416	-0,110***
	(0,477)	(0,462)	(0,481)	(0,0133)	(0,461)	(0,493)	(0,00871)
Years of primary edu,	4,921	3,856	5,763	-1,906***	4,577	5,403	-0,826***
	(2,898)	(3,011)	(2,428)	(0,0728)	(3,026)	(2,637)	(0,0531)
Ended primary edu	0,598	0,387	0,749	-0,362***	0,547	0,669	-0,122***
	(0,490)	(0,487)	(0,434)	(0,0125)	(0,498)	(0,471)	(0,00894)
Man's activity							
Wage worker	0,242	0,211	0,250	-0,0388**	0,202	0,296	-0,0940***
	(0,428)	(0,408)	(0,433)	(0,0149)	(0,402)	(0,457)	(0,00943)
Self-employed	0,160	0,107	0,178	-0,0704***	0,139	0,188	-0,0497***
	(0,366)	(0,310)	(0,382)	(0,0129)	(0,346)	(0,391)	(0,00809)
Works in agriculture	0,317	0,435	0,268	0,167***	0,344	0,279	0,0658***
	(0,465)	(0,496)	(0,443)	(0,0159)	(0,475)	(0,448)	(0,0103)
Wage-worker and self-employed	0,026	0,023	0,027	-0,00428	0,027	0,024	0,00331
	(0,158)	(0,150)	(0,163)	(0,00560)	(0,162)	(0,152)	(0,00350)
Wage-worker and agriculture	0,123	0,108	0,133	-0,0245*	0,132	0,110	0,0226**
	(0,328)	(0,311)	(0,340)	(0,0117)	(0,339)	(0,313)	(0,00726)
Self-employed and agriculture	0,133	0,115	0,144	-0,0287*	0,155	0,103	0,0520***
	(0,340)	(0,319)	(0,351)	(0,0120)	(0,362)	(0,304)	(0,00750)
Woman's activity							
Wage worker	0,159	0,114	0,156	-0,0415*	0,153	0,168	-0,0144
	(0,366)	(0,318)	(0,363)	(0,0202)	(0,360)	(0,374)	(0,0150)
Self-employed in non-agri	0,227	0,196	0,272	-0,0762**	0,210	0,256	-0,0457**
	(0,419)	(0,397)	(0,445)	(0,0249)	(0,407)	(0,437)	(0,0172)
Works in agriculture	0,359	0,464	0,259	0,205***	0,369	0,343	0,0264
	(0,480)	(0,499)	(0,438)	(0,0265)	(0,483)	(0,475)	(0,0197)
Wage-worker and self-employed	0,027	0,007	0,036	-0,0294**	0,023	0,034	-0,0110
	(0,161)	(0,083)	(0,187)	(0,00943)	(0,149)	(0,180)	(0,00664)
Wage-worker and works in agri	0,093	0,079	0,097	-0,0178	0,101	0,082	0,0188
	(0,291)	(0,270)	(0,296)	(0,0167)	(0,301)	(0,274)	(0,0120)
Self-employed and works in agri	0,134	0,140	0,180	-0,0401	0,144	0,118	0,0259
	(0,341)	(0,347)	(0,384)	(0,0216)	(0,352)	(0,323)	(0,0140)
log ($\widehat{consumption}$)	13,790	13,983	13,932	0,0515	13,675	13,948	-0,273***
	(1,046)	(1,107)	(0,944)	(0,0276)	(1,033)	(1,044)	(0,0191)
$\widehat{consumption}$	1924560	2901587	2032611	868975,8**	1862755	2010673	-147918,3
	(8956441)	(18700000)	(7569948)	(319670,2)	(11100000)	(4636514)	(164533,7)
consumption	3424040	3654149	3782781	-128631,4	3130704	3835226	-704522,7***
	(8707449)	(4886988)	(11700000)	(290802,0)	(9612158)	(7248920)	(159844,2)
Observations	12195	1706	5119	6825	7096	5092	12188

Sources: The 2002 census (IPUMS data). ***,**,* means respectively that the coefficient is significantly different from 0 at the level of 1%, 5% and 10%. Standard deviations are reported in parentheses columns (1) to (3), and (5) to (6). Standard error for average difference are reported in parentheses columns (4) and (7).

C Construction of the proxy for consumption

Table A2: Effect of dwelling characteristics on consumption

VARIABLES	log (consumption)
Solid wall	0.148***
	(0.015)
Housing water	0.124***
	(0.019)
Flush toilet	0.040**
	(0.016)
Electricity	0.388***
	(0.019)
Permanent floor	0.379***
	(0.017)
Solid roof	0.478***
	(0.055)
Nb. of bedrooms	0.093***
	(0.005)
Age HH head	-0.002***
	(0.000)
Gender HH head	-0.107***
	(0.014)
Nb. child aged 5-15	0.092***
	(0.004)
Nb. adult aged 16-65	0.157***
	(0.004)
Constant	12.566***
	(0.041)
R-squared	0.532
Observations	12,178

Sources: The three pooled waves of the LSMS data. Notes: additional controls: Regions dummies, survey year dummies. Standard errors are reported in parentheses. ***,**,* mean respectively that the coefficient are significantly different from 0 at the level of 1%, 5% and 10%.

Figure A1: Distribution of education spendings.

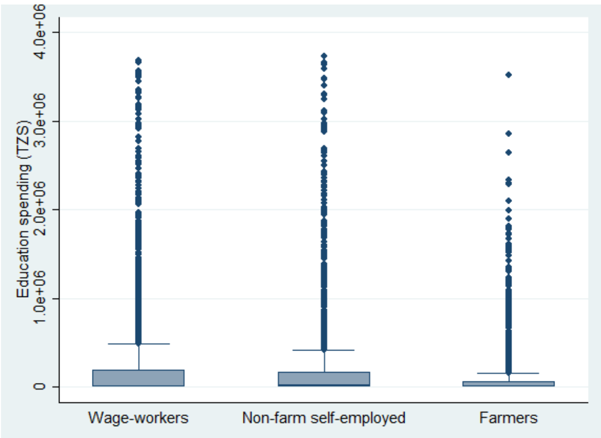
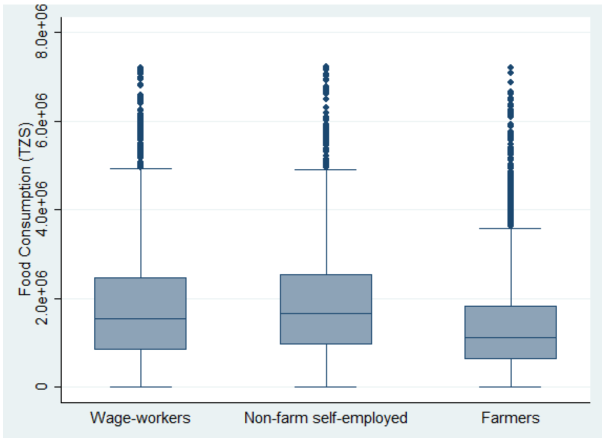
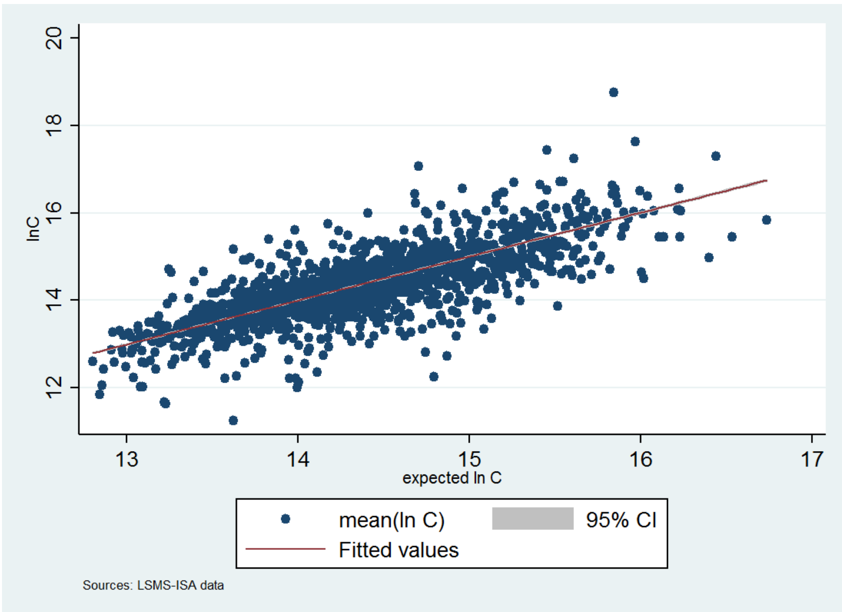


Figure A2: Distribution of food spendings.



Sources: The LSMS data (2008, 2010, 2012).

Figure A3: Relationship between the expected consumption $\widehat{lnC}$ and lnC.



Source: LSMS data (2008, 2010, 2012)

D First stages

Table A3: Effect of the program on the education level: γ coefficients of 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcomes	T_{tot}		T1		T2		T3	
years of	0.052***	0.045***	0.041***	0.038***	0.060***	0.058***	0.063***	0.065***
education	(0.006)	(0.010)	(0.006)	(0.006)	(0.006)	(0.011)	(0.007)	(0.014)
R-squared	0.272	0.273	0.315	0.315	0.336	0.337	0.296	0.297
F-test	69.53	21.66	42.44	34.62	99.29	28.42	82.55	20.73
Primary	0.006***	0.006***	0.005***	0.005***	0.008***	0.008***	0.007***	0.008***
completion	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
R-squared	0.238	0.239	0.290	0.291	0.332	0.333	0.285	0.286
F-test	30.45	19.10	15.48	32.71	35.75	30.60	28.03	17.83
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP Control	no	yes	no	yes	no	yes	no	yes
Observations	423,419	423,419	190,148	190,148	182,454	182,454	238,723	238,723

Source: the 2002 census. Notes: Standard errors are clustered at the birth region level and are reported in parentheses. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

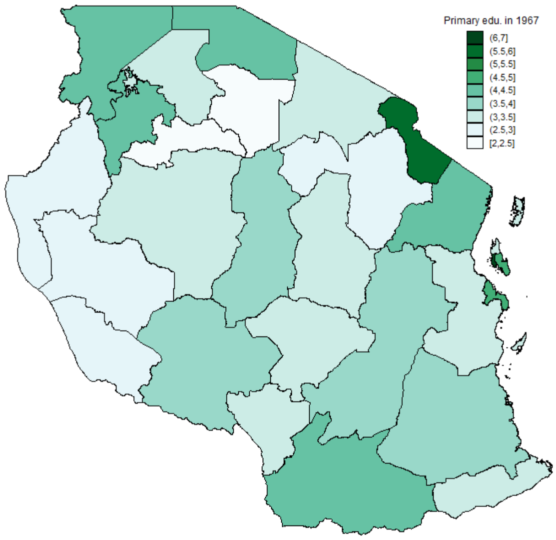
Table A4: Effect of the program on the education level from the LSMS data (γ coefficients of 3).

Instrument	(1)	(2)
$I_{j67} * T_{tot}$	0.198*	0.370
	(0.103)	(0.239)
R-squared	0.203	0.198
F-test	3.690	2.399
Observations	5,820	4,982
$I'_{j67} * T_{tot}$	0.00700	0.0281
	(0.0179)	(0.0182)
R-squared	0.176	0.178
F-test	0.152	2.395
Observations	4,982	4,982
Cohort FE	yes	yes
Regiont FE	yes	yes
GDP Control	no	yes

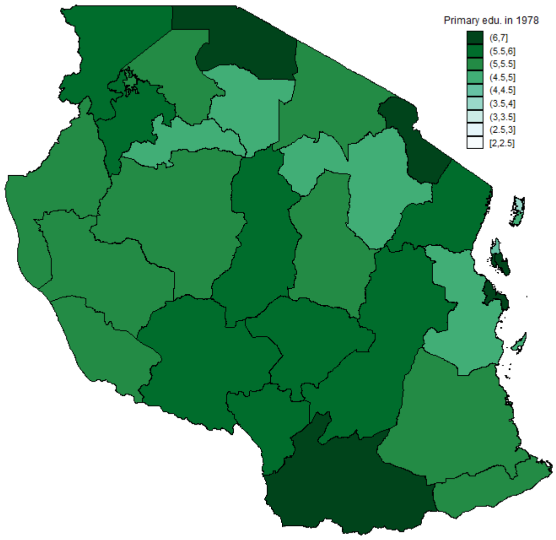
Source: the pooled LSMS survey (2008, 2010, 2012). Notes: Standard errors are clustered at the region of birth level and are reported in parentheses. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

Figure A4: Access to education in Tanzania

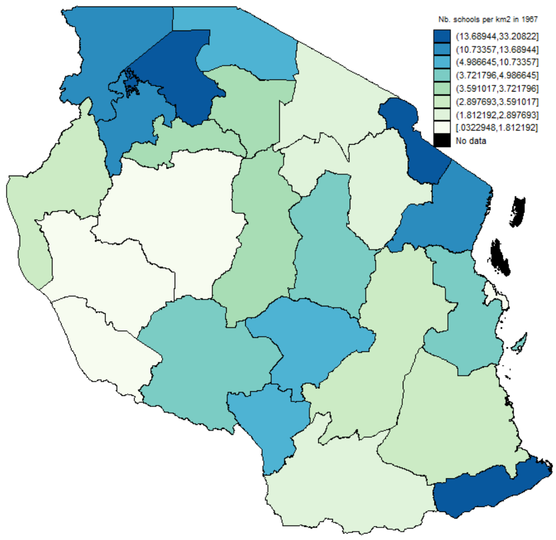
(a) Education level in 1967



(b) Education level in 1978



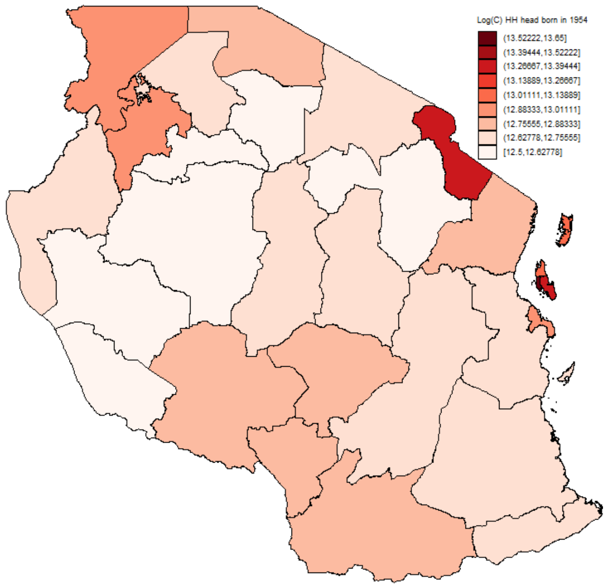
(c) distribution of primary school by region in 1967



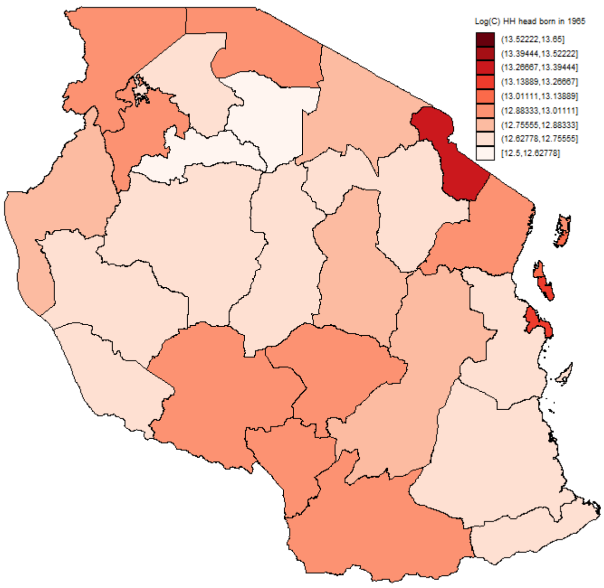
Source: Jensen & al.'s record (1968) and 2002 census.

Figure A5: Consumption level of household heads

(a) Consumption of household heads born in 1967



(b) Consumption of household heads born in 1978



Source: LSMS-ISA data (2008, 2010, 2012).

E Robustness Checks :

Table A5: Effect of education on consumption : IV estimations with education level and primary education level.

	T_{tot}		T1		T2		T3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IV estimates with $I_{j67} = S_{Zanzibar\ West,67} - S_{j67}$								
	0.075***	0.082***	0.077***	0.067**	0.061***	0.071***	0.079***	0.088***
	(0.019)	(0.022)	(0.028)	(0.029)	(0.020)	(0.017)	(0.016)	(0.019)
R-squared	0.455	0.446	0.510	0.523	0.511	0.505	0.459	0.443
F-test	33.38	31.83	23.70	18.60	70.14	23.34	45.55	25.97
IV estimates with $P_{Zanzibar\ West,67} - P_{j67}$								
	0.071***	0.075***	0.073**	0.059*	0.059***	0.065***	0.077***	0.082***
	(0.020)	(0.023)	(0.030)	(0.031)	(0.021)	(0.017)	(0.016)	(0.020)
R-squared	0.241	0.233	0.340	0.358	0.334	0.329	0.251	0.239
F-test	27.08	26.14	17.98	13.43	51.24	19.18	34.04	22.15
IV estimates with $\sum_{t=1961}^{1978} \gamma_t * I_{j1967}$								
	0.068***	0.073***	0.065**	0.049**	0.060***	0.067***	0.078***	0.087***
	(0.017)	(0.016)	(0.026)	(0.019)	(0.021)	(0.017)	(0.015)	(0.019)
R-squared	0.468	0.463	0.527	0.537	0.517	0.513	0.464	0.450
F-test	188.7	131.6	24.18	17.73	35.21	9.094	21.86	7.266
IV estimates with $\sum_{t=1961}^{1978} \gamma_t * (P_{Zanzibar\ West,67} - P_{j67})$								
	0.064***	0.065***	0.061**	0.040*	0.056**	0.060***	0.075***	0.079***
	(0.018)	(0.017)	(0.028)	(0.021)	(0.022)	(0.017)	(0.016)	(0.019)
R-squared	0.251	0.250	0.354	0.363	0.335	0.334	0.255	0.245
Additional CC	YES	YES	YES	YES	YES	YES	YES	YES
F-test	297.0	174.3	27.39	24.90	41.74	7.430	31.61	7.827
Sample	T0 +T1+T2+T3		T0 +T1		T0 +T2		T0 +T3	
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP Controls	no	yes	no	yes	no	yes	no	yes
Observations	440,676	423,419	199,450	190,148	190,466	182,454	247,502	238,723

Source: the 2002 census. Notes: standard errors are clustered at the birth region level and are reported in parentheses. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1958, the percentage of people living in rural areas in 1958, the household's size and the sector of activity.

Table A6: Effect of education on consumption: IV estimations with partially treated.

	T_{tot}		T_{tot} and partial treated	
	(1)	(2)	(3)	(4)
$I_{j67} * T$	0.075***	0.082***	0.083***	0.097***
	(0.019)	(0.022)	(0.018)	(0.031)
R-squared	0.234	0.220	0.234	0.196
F-test	33.38	31.83	26.34	13.50
Sample	$T_0 + T_1 + T_2 + T_3$		$T_0 + T_{pt} + T_1 + T_2 + T_3$	
Cohort FE	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes
GDP Control	no	yes	no	yes
Observations	440,676	423,419	516,061	495,082

note: Source: the 2002 census. Standard errors are clustered at the birth region level and are reported in parentheses. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

Table A7: IV estimations at different geographical scales

Instrument	(1)	(2)
$I_{j,1967} * T$	0.075***	0.082***
	(0.019)	(0.022)
R-squared	0.455	0.446
F-test	33.38	31.83
$I_{j',1967} * T$	0.074***	0.073***
	(0.014)	(0.020)
R-squared	0.243	0.262
F-test	52.81	74.00
Sample	$T_0 + T_1 + T_2 + T_3$	
Cohort FE	yes	yes
Regiont FE	yes	yes
GDP Control	no	yes
Observations	440,676	423,419

Source: the 2002 census. Notes: standard errors are clustered at the birth region level and are reported in parentheses. ***,**,* means respectively that the coefficient is significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

Table A8: Effect of education on the wealth index (Income index constructed from the 2002 census data)

	(1)	(2)
OLS estimates		
	0.051***	0.051***
	(0.003)	(0.003)
R-squared	0.470	0.465
IV estimates		
$I_{j67} * T$	0.105***	0.128***
	(0.0158)	(0.0263)
R-squared	-0.007	-0.066
F-test	33.37	31.82
$\sum_{t=1961}^{1978} \gamma_t * I_{j1967}$	0.096***	0.116***
	(0.017)	(0.023)
R-squared	0.011	-0.033
F-test	195.8	132.7
Sample	T0 +T1+T2+T3	
Cohort FE	yes	yes
Regiont FE	yes	yes
GDP Control	no	yes
Observations	440,683	423,426

Source: the 2002 census. Notes: standard errors are clustered at the birth region level and are reported in parentheses. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

Table A9: IV estimates of the returns to education by sector

Activity	Don't paid Don't work	agri.	self employed	formal	Don't paid Don't work	agri	self employed	formal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS estimates								
	0.030***	0.033***	0.062***	0.055***	0.029***	0.033***	0.064***	0.055***
	(0.006)	(0.002)	(0.003)	(0.002)	(0.006)	(0.002)	(0.003)	(0.002)
R-squared	0.545	0.415	0.505	0.457	0.543	0.416	0.511	0.464
IV estimates :$T * I_{j,1967}$								
	0.173	0.086***	0.095***	0.089***	0.100	0.101***	0.085**	0.064*
	(0.306)	(0.019)	(0.024)	(0.027)	(0.110)	(0.026)	(0.033)	(0.034)
F-test	0.181	18.63	38.89	21.74	1.619	10.11	21.66	25.08
IV estimates with sample selection correction (IV for occupation and first-stage equation: $T * I_{j,1967}$)								
	0.179	0.088***	0.100***	0.087***	0.103	0.103***	0.086**	0.068**
	(0.313)	(0.019)	(0.025)	(0.026)	(0.109)	(0.026)	(0.034)	(0.034)
Mills no work	-0.011*				-0.011			
	(0.006)				(0.009)			
Mills agri		-0.009***				-0.010***		
		(0.003)				(0.003)		
Mills self.			0.005**				0.003	
			(0.002)				(0.003)	
Mills wage				0.026**				0.022
				(0.011)				(0.014)
F-test	0.181	19.05	33.21	22.18	1.625	10.18	20.80	23.79
Cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Regiont FE	yes	yes	yes	yes	yes	yes	yes	yes
GDP control	no	no	no	no	yes	yes	yes	yes
Observations	3,576	284,127	87,582	65,167	3,518	277,747	81,396	60,536

Source: the 2002 census. Notes: standard errors are clustered at the region of birth level and are reported in parentheses. In IV estimations, standard errors are bootstraped. ***,**,* mean respectively that the coefficients are significantly different from 0 at the level of 1%, 5% and 10%. Additional controls are the population aged 7 to 13 in 1967, the household's size and the sector of activity.

Figure A6: Evolution of education attainment by region from T0 to T1 according to the education level in T0.

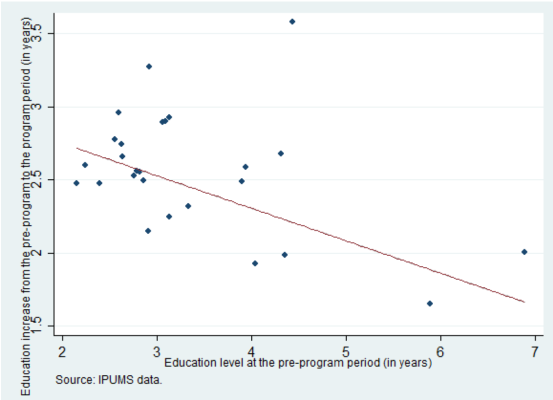


Figure A7: Evolution of education attainment by region from T0 to T1 according to the number of schools in 1967.

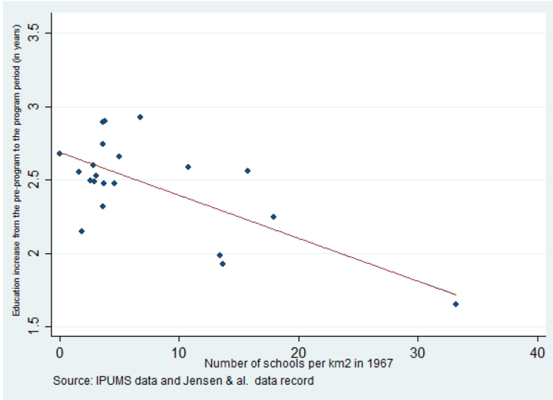
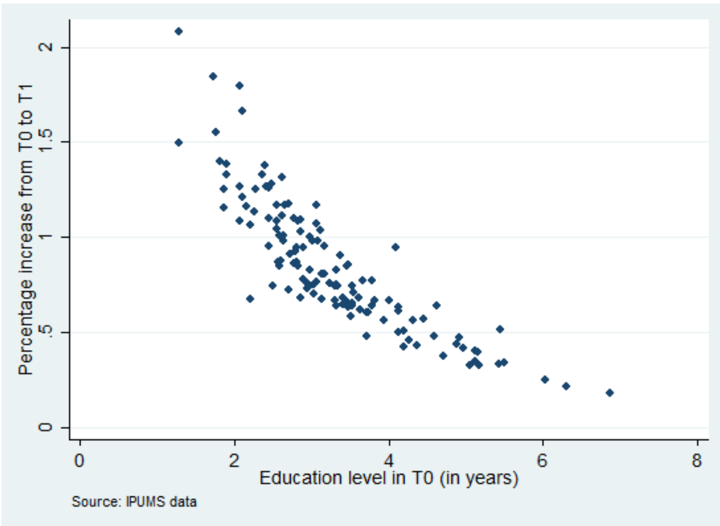


Figure A8: Evolution of the education attainment by district according to the education level in 1967.



Sources: The 2002 census.

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Figure A9: Education level by district in 1967.

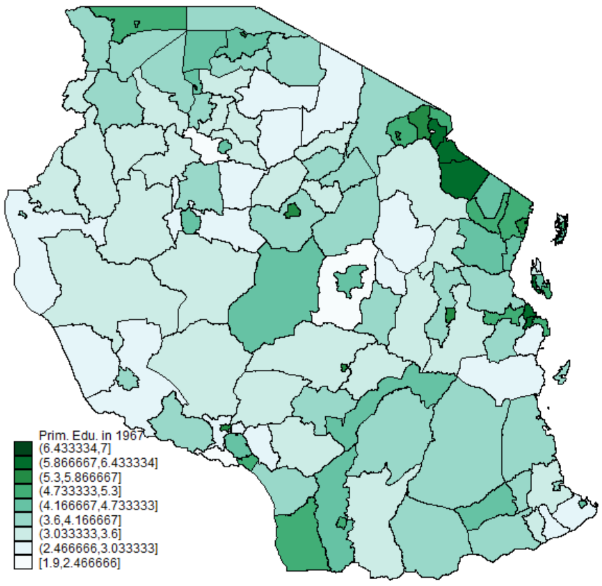
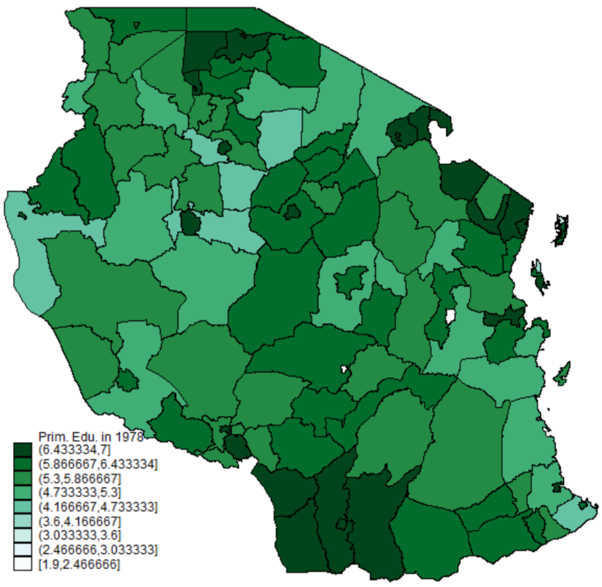


Figure A10: Education level by district in 1978.



Sources: The 2002 census.