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SCHOOL FEE ELIMINATION AND EDUCATIONAL INEQUALITY IN TANZANIA

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School Fee Elimination and Educational Inequality in Tanzania

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Abstract

In this paper, we investigate the impacts of the elimination of primary school fees in Mainland Tanzania. We use 2002 and 2012 census data and conduct a difference-in-differences analysis. Spatial and temporal variation in the implementation process of the reform is generated by distinguishing between cohorts that were exposed to the reform and cohorts that were not, and by considering the intensity of the reform, which is defined based on pre-reform educational outcomes at the gender-district level. We find that exposure to an average of 1.7 years of free primary education has reduced educational inequality by 0.66 standard deviations. We also find that this outcome has been mainly driven by a reform-induced reduction of 6.8 percentage points in the proportion of people who have never attended primary education. The benefits of fee removal have been relatively larger for females compared to males. Therefore, the educational gender gap has been narrowed. Nevertheless, policy-makers should be wary of the reform's limited reach in preventing dropouts and its diminishing effects as time elapses from the year when the reform was rolled out.

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1 Introduction

Policymakers in developing countries agree that improvements in education are necessary to achieve economic development. The attainment of Universal Primary Education has been regarded as a first step toward this target. Since the 1990s, several sub-Saharan countries have implemented policies that promote Universal Primary Education. For instance, governments have removed primary school fees, as user fees have been found to limit school access (Deininger, 2003; Holla & Kremer, 2009).

To date, the literature has mainly focused on the role of free primary education in improving average educational outcomes. We argue that since the elimination of school fees is a pro-poor policy, its benefits extend beyond improvements in average education. For instance, the elimination of user fees can reduce educational inequality, as disadvantaged groups are impacted disproportionately. Gender-based discrimination may also be mitigated if households favour the education of boys over girls in the context of limited resources for school fees. However, the implementation of free primary education can also engender unwanted consequences such as the segregation between public and private schools based on students' socio-economic status (Bold et al., 2015), the deterioration of services and infrastructure due to a rapid increase in the demand for education (Deininger, 2003) or the (re)introduction of other charges to compensate for the elimination of user fees (Somerset, 2009; Vavrus & Moshi, 2009; Lindsjö, 2018).

In this paper, we conduct a cohort analysis to study the impacts of the 2002 fee elimination in Tanzania.[1] We find that the reform, evaluated at its average intensity of 1.7 years of free education, has reduced within gender educational inequality by 0.66 standard deviations. This decrease was chiefly made possible by attracting children into school for the first time. In fact, we document an increase in net enrolment of 6.8 percentage points. Moreover, we show that girls have experienced larger benefits and the educational gender gap has narrowed. Finally, we argue that despite bringing children into school for the first time, the reform has not, however, been sufficient to keep them in school. We find an increase of 0.51–0.71 percentage points in dropouts after each grade, starting grade 4.

To identify the causal effect of the reform, we have exploited the fact that some individuals are treated while others are not, depending on one's year of birth. Additionally, for those who are treated, the intensity of their exposure to the reform depends on their district's pre-reform educational performance. For instance, in districts with low pre-reform performance, exposure to the benefits of free primary education will be at its highest. The result is a difference-in-differences estimation. This identification strategy has also been used to evaluate the impacts of fee elimination in Ethiopia (Chicoine, 2019, 2020), Kenya (Lucas & Mbiti, 2012a,b) and Malawi (Zenebe Gebre, 2019).

We further argue that initial success does not guarantee long-term success. If the implementation of the policy is losing steam — schools start charging fees under different guises, capitation grants are kept constant despite inflation and only a fraction of their

[1] Any mention of Tanzania in this study refers to the country's Mainland territory.

amount reaches schools — then households will update their beliefs regarding the true cost of educating their children and progress can be reversed. Several countries in sub-Saharan Africa have attempted to remove school fees more than once. (World Bank & UNICEF, 2009; Somerset, 2009). Tanzania is one of them. Moreover, the country's most recent attempt may be facing the same problem for the second time.

We compile evidence that the impact of the reform appears to be decreasing over time, whereby the youngest cohort has benefited the least among the treated cohorts. Our estimations suggest that the implementation of free primary education in Tanzania is showing signs of fatigue, but more recent data is necessary to issue stronger conclusions. We interpret these findings against the background of incomplete removal of fees, as some parental contributions continue to be charged (Vavrus & Moshi, 2009; Lindsjö, 2018).

Our research contributes to the expanding niche in development economics which studies Universal Primary Education and the elimination of school fees. For instance, Chicoine (2019) finds that the elimination of fees in Ethiopia, evaluated at the reform's average intensity, has increased educational achievement by 0.7 years. Zenebe Gebre (2019) finds that in Malawi, the average reform intensity increased enrolments by 18.5 percent and improved average educational achievement by 1.2 years. Deininger (2003), Nishimura et al. (2008) and Grogan (2009) evaluate the elimination of primary school fees in Uganda in 1997. They show that enrolment and grade completion improved, late enrolments were reduced, and girls have experienced larger improvements. In contrast, evaluations of the Kenyan reform estimate a higher impact on the graduation rates of boys compared to girls, thus contributing to a widening of the gender gap (Lucas & Mbiti, 2012b).

Regarding evidence on Tanzania, Hoogeveen & Rossi (2013) have already attempted to evaluate the impacts of free primary education. They exploit the staggered absorption of out-of-school children between 2002 and 2005. In line with our own findings, their results show that the elimination of fees has increased the probability of children being in school at age 7. We refine their findings by using a different identification methodology and examining educational inequality and completed education as opposed to mere presence in school. Finally, Valente (2019) also studies free primary education in Tanzania. Her focus is on the impact of accelerated enrolment growth and its consequences for the quality of school inputs. Although she documents a deterioration in teacher knowledge and student-to-teacher ratios, she rejects a substantial worsening in student performance.

2 Context

In 1974, the Musoma Resolution declared Universal Primary Education a national priority. The first attempt of Tanzania at removing primary school fees was in 1973-74 (Galabawa, 1990; World Bank & UNICEF, 2009). Shortly after, primary education was declared compulsory in 1978. In 1981, the net and gross primary school enrolment rates were 69.7 and 98.3 percent, respectively (Ishumi, 2014). These numbers were encouraging for the 1980s; however, the government's expansionary efforts have also exacted a toll on the quality side of education (Omari et al., 1983; Galabawa, 1990), which deteriorated further against the background of high population growth rates and the adverse economic conditions of the 1980s (Galabawa, 1990; Jerve, 2006).[2] Fees were reinstated and education-related costs increased (Jerve, 2006). By 1990, the net and gross enrolment rates had decreased to 59.6 and 80.7 percent, respectively (Ishumi, 2014).

The government of Tanzania started looking into fresh reforms. The Primary Education Development Plan (PEDP) (Government of Tanzania, 2001) was put forward. Per this plan, primary school fees and all other mandatory parental contributions were removed as of January 2002.[3,4] Just before the elimination of fees, the Tanzanian government was estimating that 4.8 million children were in primary education while 3 million children aged 7–13 were out of school (PEDP, Chapter 1). The expected surge in enrolment motivated the Tanzanian government to adopt several measures to avoid overwhelming the educational system, such as implementing a staggered absorption of out-of-school children and expanding the educational infrastructure. The government built 29,922 classrooms and hired 32,325 teachers during 2002–04. Although impressive, these efforts barely managed to maintain educational services at pre-reform level. See Table 1.

We argue that it was the removal of fees rather than these infrastructure developments which paved the way for improvements in educational outcomes post-reform. Hoogeveen & Rossi (2013) adopt the same approach for the case of Tanzania, and so do Deininger (2003), Grogan (2009) and Nishimura et al. (2008) in their studies of the Ugandan fee reform. Per this argument, infrastructure improvements have chiefly served the role of enabling the absorption of new enrolments, which were driven by the removal of fees. This statement is based on the comparison of pre- and post-reform infrastructure in Table 1. While the increase in enrolments at the start of 2002 was significant and a considerable break from previous trends, the change in infrastructure was minimal in 2002. If anything, relative to the number of students, the available infrastructure shrank. Due to the timing mismatch between the enrolment surge, the construction of classrooms and the hiring of teachers, it becomes apparent that parents decided to send their children to school in larger numbers as a consequence of the immediate removal of fees rather than the slower infrastructure developments. What is more, given the phrasing in the PEDP, it is very likely that parents sent their children to school under the impression that not just school fees, but all parental contributions would be eliminated. It was only later that it became apparent that some contributions were still required.

[2]For instance, the oil crises of the 1970s, Tanzania's war with Uganda and agricultural stagnation (Jerve, 2006; Vavrus and Moshi, 2009).

[3]Zanzibar is an autonomous administrative region and has followed a different reform schedule. The timing of the census data does not allow the study of the reform in Zanzibar.

[4]Before their elimination, school fees were estimated to have been roughly USD 4.6 per child per academic year (Valente, 2019). For instance, monthly food and non-food expenditure per capita was estimated at TZS 10,120 (USD 12.5) for Mainland Tanzania in 2000–01 (National Bureau of Statistics Tanzania, 2002). This included the monetary equivalent of the food grown by the household. Moreover, Sumra (2017) documents that the overall education-related costs, fees and parental contributions included, were USD 8 to 16 per year per child — the equivalent of one to two months' worth of agricultural wages.

Table 1: Pre- and Post-Reform Infrastructure

		PRE-REFORM		POST-REFORM	
		1999	2001	2002	2005
(1)	Population of 7–13 children[a]	5,427,156	5,679,676	5,810,309	6,220,512
(2)	Gross enrolment rate	77.2%[b]	84%[c]	99%[c]	105.41%[d]
(3) = (1) × (2)	Children in school	4,189,764	4,770,928	5,752,206	6,557,042
(4)	Stock of classrooms start of year	57,367[e]	est. 60,000[e]	est. 60,000[e]	89,875[f]
(5)	Stock of teachers start of year	103,966[g]	102,313[h]	est. 109,665[i]	134,638[j]
(6) = (3) ÷ (4)	Student-to-classroom ratio	73:1	80:1	96:1	73:1
(7) = (3) ÷ (5)	Student-to-teacher ratio	40:1	47:1	52:1	49:1

[a]World Bank (2001), Annex H, Table 3 and PEDP, Annex 3, Table 1. [b]World Bank (2001), Annex H, Table 3. [c]World Bank (2005), Chapter 4, Table 1. [d]World Bank DataBank and UNESCO Institute for Statistics, ID: SE.PRM.ENRR. [e]World Bank (2001), Annex H, Table 5. [f]Stock of 1999 plus the project-declared output of 29,922 classrooms and plus the output of 2,586 classrooms built under a related World Bank Project (World Bank, 2005). [g]World Bank (2005), Annex H, Table 4. [h]Based on the 1999 stock less attrition at 1.59%. Teacher hirings were frozen (World Bank, 2001). [i]Considering the 1999 stock and attrition rates of approx. 1.59% per year between 1999 and 2000, the stock would have been of 100,665 at the end of 2001 (World Bank, 2001). However, the government planned to hire approx. 9,000 teachers by 2002. This stock of unemployed teachers is likely to have existed because of a prior freeze on teacher hirings (World Bank, 2001). The target would have been missed only if deployment had failed. In a bid not to underestimate the 2002 teacher capacity, we assume all 9,000 teachers were recruited. [j]Stock of 2001 plus the project-declared output of 32,325 teachers (World Bank, 2005). The strategy of temporarily employing double-shift teaching has underperformed. (World Bank, 2005).

To compensate schools, the PEDP introduced a capitation grant of USD 10 (TZS 9,000) per child per year as well as an investment grant to build the necessary classrooms, sanitation facilities and teachers' accommodation. The capitation grant was increased to TZS 10,000 in 2006 (Government of Tanzania, 2006) and has since remained constant (Mbiti et al., 2019) despite yearly inflation rates above 5 percent. Had the grant been re-evaluated at the TZS equivalent of USD 10, then current exchange rates would have put its value at about TZS 23,000 in 2020.

Although the PEDP announced the removal of both user fees and parental contributions, there is anecdotal evidence that only user fees have been removed (Mushi, 2013; Vavrus & Moshi, 2009; Lindsjö, 2018). Despite being mandatory in practice, parental contributions are voluntary in official terms (Lindsjö, 2018). Moreover, Vavrus & Moshi (2009) have noted the confusion of parents with respect to the distinction between school fees and other mandatory contributions. Parental contributions range from food for school meals to monetary contributions for the building of schools or teachers' accommodation. Parents usually subsume contributions to school fees because the former are compulsory in practice (Vavrus & Moshi, 2009; Lindsjö, 2018). The realization of parents that fee elimination does not include all mandatory contributions can arguably explain why enrolments have increased and then decreased post-reform. In addition, the investment grant has also made some mandatory contributions redundant temporarily. Net enrolment was 99 percent in 2008, then 92, 88, 84, 83 and 84 percent in 2010, 2012, 2013, 2014 and 2017, respectively.[5]

[5]UNESCO Institute of Statistics. Data by theme. Education theme - Participation - Enrolment ratios - Net enrolment rate, primary, both sexes. data.uis.unesco.org. Accessed September 5, 2020.

3 Data and Summary Statistics

This study is based on the analysis of the 2002 and 2012 Tanzanian census waves, which were made available by the Tanzanian Bureau of Statistics and distributed by the Minnesota Population Center (2018). The dataset is collapsed such that the units of observation consist of gender-age-district-year groups. The administrative borders of districts in Tanzania have changed between census years, and the tendency has been one of splitting larger districts into smaller ones. In order to present the 2002 and 2012 data in terms of the same administrative demarcations, we use the 1988 district borders, as both the 2002 and 2012 administrative units can be traced back to their 1988 polygons, which encompass a larger geographical area. The dataset is a panel where gender-age-district groups are observed twice.

For each census wave (period), gender and age group in each district, we measure inequality using the Generalized Entropy index for $\alpha = 1$, which is also known as Theil's T index. We have opted for this index because it can include zero values, i.e., individuals without education, in the computation of inequality (Morrisson & Murtin, 2013). See Equation 1. As a robustness check, we use the Gini index which is amenable to a similar correction for the inclusion of zero values (Morrisson & Murtin, 2013).[6]

$$Theil_{y\in(0,7)} = Theil_{y\in(1,7)} - ln[1 - Pr(y = 0)] \tag{1}$$

where $Pr(y = 0)$ is the proportion of individuals who have never enrolled in primary education in a given period-gender-age group in the concerned district. Theil T's index for non-zero educational values is presented in Equation 2. N is the number of individuals in each period-gender-age group in a district.

$$Theil_{y\in(1,7)} = \frac{1}{N}\sum_{i=1}^{N}\frac{y_i}{\bar{y}}ln\left(\frac{y_i}{\bar{y}}\right). \tag{2}$$

Table 2 shows that the magnitude of educational inequality is notably larger if individuals who have never enrolled in school are included in the computation. This suggests that measures aimed at reducing the percentage of out-of-school individuals are likely to bear an important impact on educational inequality in Mainland Tanzania. Moreover, Table 2 points to several other important observations. For instance, there is a statistically significant difference between women and men in terms of their school attendance, with 19 percent of women never having attended school, while only 14 percent of men experienced the same situation. Furthermore, once in school, females seem to acquire more years of education than their male counterparts. The difference is slight but significant. However, because girls are more likely to be out of school than boys, the overall girl-boy educational gap is significant, and girls need to catch up.

[6] $Gini_{y\in(0,7)} = Pr(y = 0) + [1 - Pr(y = 0)] \times Gini_{y\in(1,7)}$ as per Morrisson & Murtin (2013). Same notation as for the Theil index in Equation 1. Then, $Gini_{y\in(1,7)} = 1 - \frac{1}{N}\sum_{i=1}^{N}(y_i + y_{i-1})$.

Table 2: Summary Statistics and T-tests

	Mean Sample	σ	**Min**	**Max**	**Mean Fem.**	**Mean Males**	**Diff.**	**t-stat**	**p-value**
Theil's T index (all children)	0.211	0.172	0.001	1.702	0.242	0.180	0.062	15.50	0.00
Theil's T index (enrollees)	0.019	0.011	0.000	0.065	0.018	0.021	-0.003	-11.51	0.00
Ratio without any education	0.164	0.123	0.000	0.816	0.188	0.141	0.047	16.33	0.00
Years primary educ. (all children)	5.471	0.939	1.236	6.979	5.358	5.585	-0.227	-10.19	0.00
Years primary educ. (enrollees)	6.530	0.360	4.091	6.989	6.577	6.482	0.095	11.11	0.00
Gender-age-district-period groups	7,004	7,004	7,004	7,004	3,502	3,502	-	-	-

Sample weights have been used to compute all variables. 103 Mainland districts in 1988 $\times$ 2 genders $\times$ 2 periods (2002 and 2012) $\times$ 17 age groups (14 to 30 years old) gives 7,004 observations. The Morrisson & Murtin (2013) correction has been used for the inequality index which uses values from 0 to 7 in terms of years of primary education, i.e., all children. For the t-test, $H1$ is Difference $\neq 0$.

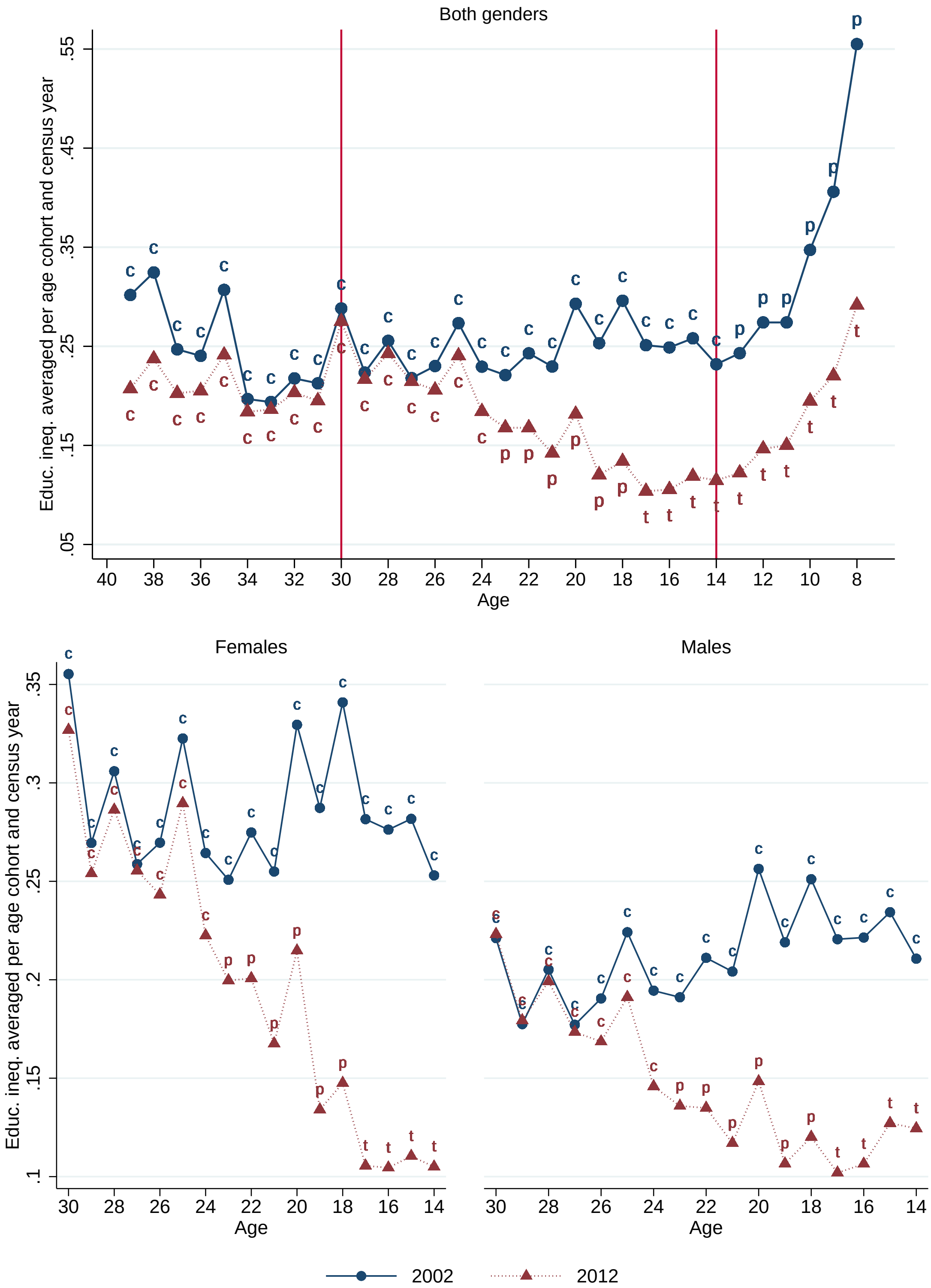
Primary school fees were eliminated at the start of the academic year in 2002. Thus, children aged 7 or younger in 2002 are the treatment group, while individuals aged 8–13 in 2002 are considered partially treated, as presumably they were already in school at the time of the reform. Finally, those aged 14 or older in 2002 are the control group.[7]

Figure 1 presents these treatment groups with educational inequality plotted on the y-axis. The figure shows that inequality among control groups is the same regardless of the period of observation. When both 2002 and 2012 data is plotted for control cohorts, we see that the lines follow the same pattern, i.e., they are parallel. This vindicates the parallel trends assumption. Immediately after cohorts are classified as treated, a divergence is noticeable between the inequality outcomes of 2002 and 2012. The gap is at its widest when the treatment groups of 2012, ages 14–18, are compared with their control counterparts of 2002. Moreover, Figure 1 also suggests that educational inequality among women was higher before the reform was implemented in 2002 and that the elimination of primary school fees has seemingly benefited females more than males. High inequality among the out-of-sample 8-to-13 age groups is illustrative of the fact that late enrolment is common. We also see that inequality has decreased significantly for the 8–13 age cohorts in 2012, which is in line with the findings of Hoogeveen & Rossi (2013). The authors find that the probability of being in school at age 7 has increased significantly after 2002.

Figure 2 presents the same information as in Figure 1 from the point of view of year-of-birth cohorts instead of age groups. The 1982–1988 cohorts have been observed during both periods, 2002 and 2012, at ages 14–20 and 24–30, respectively. All other year-of-birth cohorts have been observed only once. This figure shows how the control age groups, regardless of their birth cohort, are characterized by steady and similar inequality values which are generally above 0.2 units of inequality, i.e., slightly more that the value of one standard deviation over the 2002–12 sample. Thereafter, the partially and fully treated cohorts exhibit a pronounced decreasing trend, thus reaching values as low as 0.1 units.

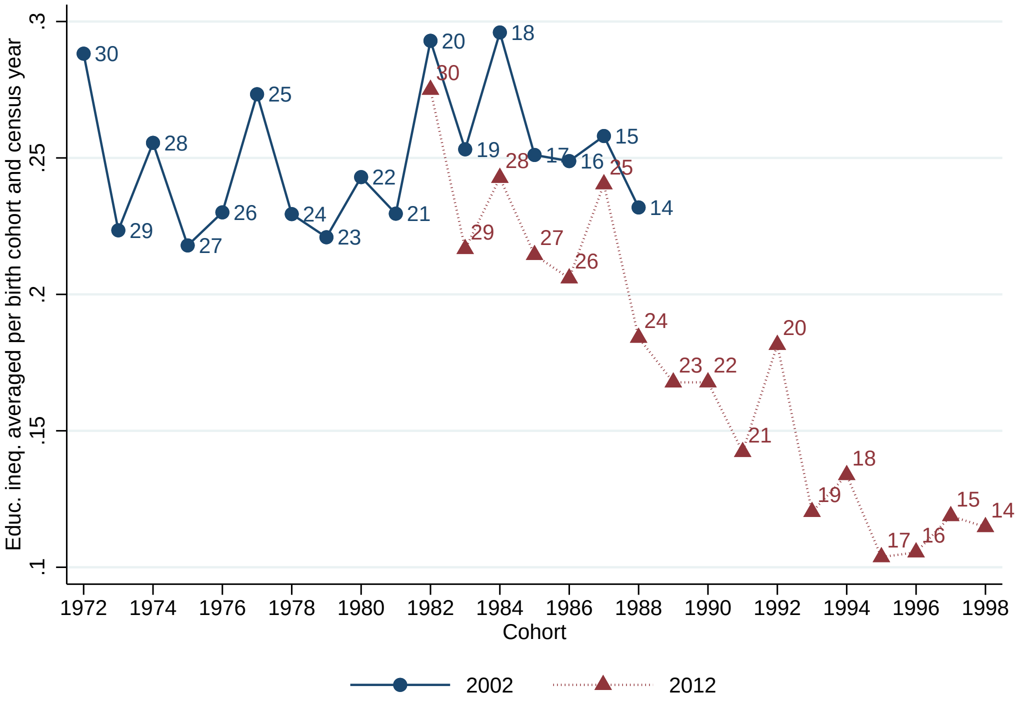
[7] To the extent that children start school at later ages, we are underestimating the impact of the reform, as some treated individuals might be classified as control.

Figure 1: The Evolution of Educational Inequality: Age and Gender Groups
Parallel Trends



Educational inequality is based on years of primary education. It is computed as per Theil T's index with the Morrisson and Murtin correction. The vertical lines in the first block of graphs denote the first and last of the age groups that are included in the analysis. The reform happened in 2002. t denotes the treated, p is for partial treatment and c is for control.

Figure 2: The Evolution of Educational Inequality: Birth Cohorts



Educational inequality is based on years of primary educational attainment and computed as per the Theil's T index with the Morrisson and Murtin correction for zero values. The labels of line markers are the age at which each cohort was observed depending on the census wave.

4 Identification Strategy

The strategy to identify the impact of free primary education is informed by two main aspects: the country-wide simultaneous implementation of the reform and the fact that the dataset is comprised of age-period-cohort observational units. The former issue puts the evaluation of the reform at risk of confounding country-wide changes post-2002 with the elimination of primary school fees. Moreover, the latter problem arises due to the fact that the period (year of observation), age and the year-of-birth variables are linearly related. For instance, birth cohorts are obtained by subtracting age from the year of observation. Consequently, not all cohort, age and period effects can be identified simultaneously (Heckman & Robb, 1985). We discuss below the solutions to these problems.

To address the issue raised by the simultaneous elimination of school fees in all of Mainland Tanzania's administrative units, we use the methodology proposed by Lucas (2010, 2013), Lucas & Mbiti (2012a,b) and Chicoine (2019, 2020). The strategy is to rely on the fact that some individuals are treated while others are not, depending on one's year of birth and consequent age at the time when fees were eliminated. Moreover, for those who are treated, their potential response to the reform depends on their district's pre-reform educational performance. This translates into saying that the reform has had various degrees of geographical intensity: districts which were performing poorly pre-reform will have a higher potential to improve following the elimination of primary school fees, while those with already satisfactory educational indicators will have relatively less room for improvement, thus the intensity of the reform will be at a lower value for the latter districts. This methodology was initially used to evaluate the impacts of anti-malaria interventions on education and fertility in Sri Lanka and Paraguay (Lucas, 2010, 2013). Ultimately, it was also applied to evaluate educational reforms that were implemented country-wide at the same time (Lucas & Mbiti, 2012a,b; Chicoine, 2019, 2020).

Reform intensity takes value zero for all age groups that were 14 or older in 2002 (born in or before 1988) and non-zero increasing values for cohorts aged 13–8 in 2002 (born between 1989 and 1994), when primary school fees were eliminated. Seven-year-olds and younger age groups (born in or after 1995) are assigned the highest values of reform intensity. At the same time, intensity varies across districts and genders, as pre-reform educational attainment is averaged for each gender in each district.[8] For the benchmark indicator, the pre-reform gender-district educational attainment is computed using the 2002 census, namely data from 14 cohorts that were older than 18 at the time of the reform. These are individuals born between 1970 and 1983, 19–32 years old in 2002. As a robustness check, we have also computed the intensity indicator based on (i) the 1970–76 cohorts and (ii) the 1977–1983 cohorts only.[9]

Following Chicoine (2019), the equations in System 3 summarize the reform intensity variable in district d_n for gender s_i; $n \in \{1, ..., 103\}$. Reform intensity can be interpreted as the maximum number of additional years of schooling for each gender in district d that can ensue from the elimination of primary school fees. Thus, the theoretical magnitude ranges from 0 to 7 and is inversely related to pre-reform educational performance.

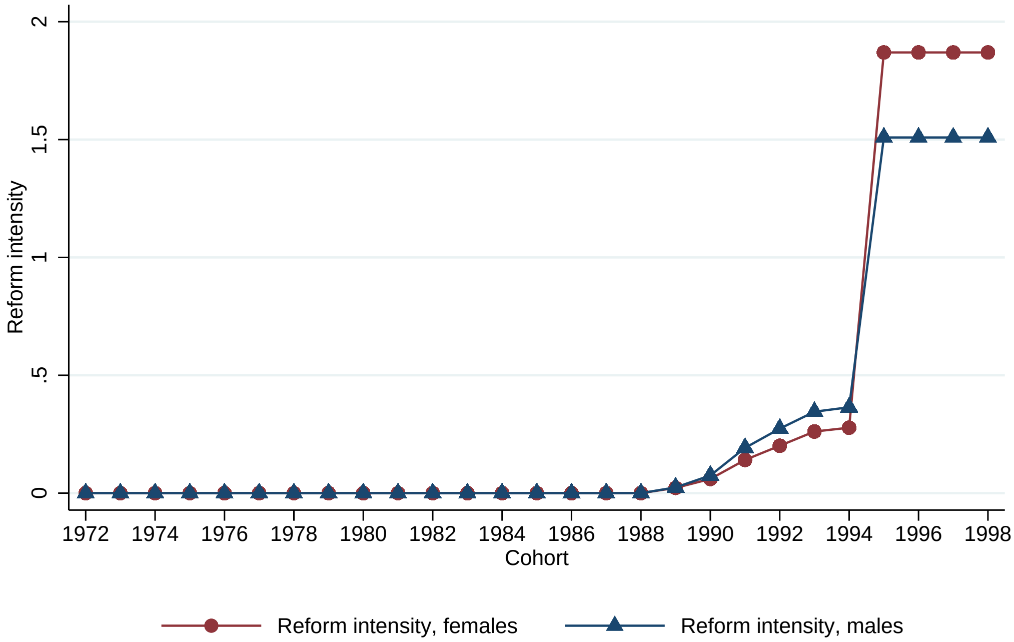
$$Intensity_{c,d_n,s_i} = \begin{cases} \sum_{g=0}^{7}(7-g) \times F_{g,d,s} & \text{if } c \geq 1995 \\ \sum_{g=1995-c}^{7}(7-g) \times F_{g,d,s} & \text{if } 1989 \leq c \leq 1994 \\ 0 & \text{if } c \leq 1988 \end{cases} \quad (3)$$

where d denotes the district, c is for year of birth and g is for years of primary education (i.e., 0 to 7). F_{g,d_n,s_i} is defined as in Chicoine (2019). In each district d_n, some pre-reform gendered fraction F of individuals has never attended school: F_{0,d_n,s_i}. Intuitively, these individuals would have benefited the most from the elimination of fees, as they had a seven-year gap to remedy, wherefrom the pre-multiplication of F by $7 - g = 7$. Moreover, some fraction of individuals of gender s_i in district d_n only completed 1 year of primary education: F_{1,d_n,s_i}. Analogously, F_{7,d_n,s_i} denotes the fraction of people of gender s_i who have completed primary education in district d_n. This fraction of the population would have had nothing to gain from the removal of fees, and this is reflected by the $7 - g$ pre-multiplication, whereby g is 7 in this case. Figure 3 graphs the national average of the reform intensity variable per gender and year-of-birth cohort for the analysis sample.

[8] There is significant heterogeneity between districts in terms of reform intensity. Appendix A.2 is suggestive of the amount of variation, whereby averages of reform intensity are mapped for each district.

[9] See Appendix A.1 and Table 4.

Figure 3: Reform Intensity by Gender and Birth Cohort



Census 2002 data is used to gauge the potential impact (reform intensity) of free primary education. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. The graph takes means over districts per gender and birth cohort. The analysis sample is comprised of the graphed cohorts. 1989 is the first partially treated cohort and 1995 is the first fully treated cohort.

The design of the intensity variable leads to a difference-in-differences framework where age groups are the treatment units and where the year-of-birth cohort, gender and the district of residence inform the intensity of treatment.[10] All age groups are control units in 2002. In 2012, some remain control (ages 24 to 30, born 1982–88), others become partially (ages 18 to 23, born 1989-94) or fully treated (ages 14 to 17, born 1995–98).[11]

Finally, because the analysis relies on age groups, year-of-birth cohorts and period information, we use the literature that deals specifically with age-period-cohort (APC) analyses to inform the setup of the empirical model. The APC identification issue is derived from the fact that any one variable among the age, cohort and period effects can be determined as a linear combination of the remaining two. The age, period and cohort controls are all meant to act as proxies for variables that are relevant to the empirical model but which are not observable. These underlying variables are not themselves linearly dependent (Heckman & Robb, 1985).

Heckman & Robb (1985) mention that the simplest solution is to assume that one of the age, period or cohort effects is zero. This strategy, of course, comes at an important cost if the assumption is wrong. However, due to the peculiarities of this study, we argue that this risk is acceptable. Unlike the usual APC studies, we are not concerned with the

[10]The census dataset does not include a variable documenting the district of birth of individuals.

[11]See Appendix A.3 for an age-cohort-period table.

exact magnitude of the age, cohort or period effects. We do not interpret these effects or base any conclusions on their magnitude. Instead, our interest lies with the impact of the primary school reform. Since we are not interested in any of these effects individually, we run specifications that confront all possible specifications: (1) cohort effects are zero, (2) age effects are zero and (3) year effects are zero. Namely, Equations 4, 5 and 6, respectively, to which district and gender effects are also added.

$$Education_{a,p,d,s} = \beta_1 Intensity_{a,p,d,s} + \gamma_{1,a} + \tau_{1,p} + \delta_{1,d} + \eta_{1,s} + \epsilon_{1,a,p,d,s} \tag{4}$$

$$Education_{c,p,d,s} = \beta_2 Intensity_{c,d,s} + \rho_{2,c} + \tau_{2,p} + \delta_{2,d} + \eta_{2,s} + \epsilon_{2,c,p,d,s} \tag{5}$$

$$Education_{a,c,d,s} = \beta_3 Intensity_{c,d,s} + \gamma_{3,a} + \rho_{3,c} + \delta_{3,d} + \eta_{3,s} + \epsilon_{3,a,c,d,s} \tag{6}$$

where $Intensity$ is the interaction variable of a typical difference-in-differences framework. a stands for age (14 to 30 year olds), c is for cohort (1972 to 1998), p is for period (2002, 2012), d denotes the district of observation (103 districts) and s is for gender. $Education$ can be any outcome of interest.

The resulting framework is a high-dimensional fixed effects model. We expand the above specifications with additional level effects in the form of interactions. Since the age and period variables define the panel, we focus on these as opposed to cohort effects. Then, the strategy is to include district-period, age-period and age-district effects progressively for specifications (4), (5) and (6), which are summarized in Equation 7. The objective is to account for trends and further heterogeneities in the data.

$$Education_{a,p,d,s} = \beta_4 Intensity_{a,p,d,s} + \gamma_{4,a} + \tau_{4,p} + \delta_{4,d} + \eta_{4,s} + \pi_{4,a,p} + \omega_{4,a,d} + \theta_{4,p,d} + \epsilon_{4,a,p,d,s} \tag{7}$$

The identification assumption per which cohort effects are zero, has been commonly employed in the literature. In the case of Krueger & Pischke (1992), the authors argue that since their cohorts of interest are close together, they must be similar, and thus cohort effects are assumed zero. Their study is of a pension reform and its impacts on labour force participation. Similarly, Machin et al. (2011) also explore an age-year panel in their study of the impact of compulsory school reforms on crime. They control for age and year effects, and assume cohort effects are zero.

5 Results

5.1 Free Primary Education and Inequality

5.1.1 Benchmark results

Among the fully treated cohorts, reform intensity is 1.7 years on average and the standard deviation is 0.9 years. The minimum value is 0.2 and the maximum is 5.1 years. We find that the implementation of the reform has reduced educational inequality. See Table 3.

Table 3: Free Primary Education and Inequality

Explanatory variables	**THEIL'S T INDEX - Years of primary education, 0 to 7 -** ($\mu = 0$ and $\sigma = 1$) (1)		(2)		(3)		(4)		(5)		(6)	
Reform intensity (years)	-0.40*** (0.02)	-0.32*** (0.03)	-0.49*** (0.04)	-0.40*** (0.05)	-0.49*** (0.04)	-0.40*** (0.05)	-0.35*** (0.02)	-0.26*** (0.02)	-0.37*** (0.03)	-0.25*** (0.04)	-0.39*** (0.05)	-0.22*** (0.07)
Reform intensity × Female		-0.12*** (0.01)		-0.11*** (0.02)		-0.11*** (0.02)		-0.14*** (0.01)		-0.14*** (0.01)		-0.15*** (0.02)
Fixed Effects												
District	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort	-	-	Yes	Yes	Yes	Yes	-	-	-	-	-	-
Age	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes
District × Period	-	-	-	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Age × Period	-	-	-	-	-	-	-	-	Yes	Yes	Yes	Yes
Age × District	-	-	-	-	-	-	-	-	-	-	Yes	Yes
Adjusted within R^2	0.18	0.19	0.13	0.14	0.13	0.14	0.16	0.17	0.08	0.10	0.07	0.09
F	257	341	160	236	160	236	321	350	182	250	71	219
Nr. clusters	103	103	103	103	103	103	103	103	103	103	103	103
N	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. The dependent variable is standardized. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–1994 cohorts have been partially treated and the 1995–1997 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. The sample only includes districts from Mainland Tanzania.

A one-year increase in reform intensity triggers a reduction of 0.39 standard deviations in educational inequality. At its average intensity of 1.7 years of free education, the reform leads to a decrease in inequality of 0.66 standard deviations. These are the results put forward by column (6), which is the most comprehensive. The other specifications also point to significant and negative coefficients in the range of 0.35 to 0.49 standard deviations for one additional year of free education.

Table 3 shows that the reform has been heterogeneous along gender lines, as females appear to have benefited significantly more. While the impact of the reform on inequality among males was of -0.22 standard deviations, the absolute magnitude for females was 0.15 units greater, amounting to -0.37 standard deviations per 1 additional year of exposure to free primary education. As pointed out in Section 3, this may reflect the fact that females are characterised by higher within gender inequality than males. Thus, there is more room for them to improve relative to male cohorts. Results support the hypothesis that free primary education has reduced the educational gender gap.

In the following sections, we show that the results of Table 3 are robust to changes in the measurement of reform intensity and educational inequality. We alter the cohorts whose educational outcomes are used to compute the variable gauging reform intensity, and we use a different inequality index to measure the dependent variable. Lastly, we account for the fact that internal migration can impact the outcome variable. Thus, we remove the individuals who moved from their region of birth, as their educational outcomes do not genuinely describe the educational situation of their region of residence.

5.1.2 Robustness checks: Reform measurement

In Table 3, the benchmark reform intensity is gauged based on the educational performance of all individuals born between 1970 and 1983, aged 19–32 at the time of the reform in 2002. We start at 19 because we want to avoid any issues created by late enrolments and their ensuing late graduation. The scope of the reform intensity variable is to describe pre-reform performance. Therefore, cohorts that include individuals who might have been impacted by the reform must be removed. We do not go beyond the 1970 cohort because against the background of rapid and important changes in Tanzania post-independence, older cohorts are too detached from Tanzania's educational situation after 1980.

As a robustness check, we use two different reform intensity variables. Instead of focusing on all 14 cohorts between 1970 and 1983, first we take the 7 most recent cohorts, 1977–83, and then the 7 oldest cohorts, 1970–76.[12] Table 4 shows that the benchmark results are

[12]Appendix A.1 compares these three cohort-based measurements, 1970–83, 1970–76 and 1977–83. Essentially, pre-reform educational achievement and reform intensity are inversely related. While the educational situation of females has not changed significantly across the 1970–83 cohorts, for males, however, the reform intensity variable suggests a marked improvement. For instance, the variable is lower if the 1977–83 cohorts are considered, which is a consequence of these cohorts' superior educational results relative to their older counterparts. Similarly, the variable reaches its highest magnitude if the 1970–76 cohorts are employed, as they had more room for improvement, and reform intensity is thus estimated to be stronger. Benchmark intensity sits midway between the aforementioned specifications.

Table 4: Robustness to the Measurement of Reform Intensity

Explanatory variables	**THEIL'S T INDEX - Years of primary education, 0 to 7 -** ($\mu = 0$ and $\sigma = 1$)											
	(1)		(2)		(3)		(4)		(5)		(6)	
Reform intensity† (years)	-0.38*** (0.02)	-0.29*** (0.03)	-0.47*** (0.04)	-0.37*** (0.04)	-0.47*** (0.04)	-0.37*** (0.04)	-0.33*** (0.02)	-0.24*** (0.02)	-0.35*** (0.03)	-0.23*** (0.03)	-0.37*** (0.05)	-0.21*** (0.06)
Reform intensity† × Female		-0.15*** (0.01)		-0.14*** (0.01)		-0.14*** (0.01)		-0.16*** (0.01)		-0.16*** (0.01)		-0.17*** (0.02)
Adjusted within R^2	0.17	0.19	0.12	0.14	0.13	0.14	0.15	0.17	0.08	0.10	0.06	0.09
F	259	357	145	245	145	245	323	379	157	257	67	232
Reform intensity‡ (years)	-0.41*** (0.03)	-0.35*** (0.04)	-0.50*** (0.04)	-0.43*** (0.05)	-0.50*** (0.04)	-0.43*** (0.05)	-0.36*** (0.02)	-0.28*** (0.03)	-0.38*** (0.03)	-0.27*** (0.04)	-0.39*** (0.05)	-0.23*** (0.08)
Reform intensity‡ × Female		-0.09*** (0.02)		-0.08*** (0.02)		-0.08*** (0.02)		-0.11*** (0.02)		-0.12*** (0.02)		-0.13*** (0.03)
Adjusted within R^2	0.18	0.18	0.13	0.14	0.13	0.14	0.16	0.16	0.09	0.10	0.07	0.08
F	243	289	164	206	163	206	298	292	191	223	67	191
Fixed Effects												
District	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort	-	-	Yes	Yes	Yes	Yes	-	-	-	-	-	-
Age	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes
District × Period	-	-	-	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Age × Period	-	-	-	-	-	-	-	-	Yes	Yes	Yes	Yes
Age × District	-	-	-	-	-	-	-	-	-	-	Yes	Yes
Nr. clusters	103	103	103	103	103	103	103	103	103	103	103	103
N	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. The dependent variable is standardized. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–1994 cohorts have been partially treated and the 1995–1997 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity.

† The intensity variable is computed based on the educational performance of individuals who were aged 19–25 at the time of the reform (7 birth-year cohorts, 1977–83). ‡ The intensity variable is computed based on the educational performance of individuals who were aged 26–32 at the time of the reform (7 birth-year cohorts, 1970–76). Reform intensity is district-gender-birth-cohort specific. The sample only includes districts from Mainland Tanzania.

robust to these alternative specifications of reform intensity. The magnitude of coefficients and the qualitative implications of Table 3 are maintained.

5.1.3 Robustness checks: Outcome measurement

We swap Theil's T index for Gini. Results are presented in Table 5. The magnitude of the impact is lower when Gini is employed, i.e. -0.30 vs. -0.39. Nevertheless, these results are in accordance with those put forward by Table 3, and the conclusions of the benchmark analysis hold.

5.1.4 Robustness checks: Analysis sample

In the benchmark analysis, we relied on an individual's district of residence, as opposed to his or her district of birth, to compute the district-aggregated educational outcomes. Therefore, the dependent variable is likely to suffer from measurement error. Moreover, this measurement error might be non-random if there is a tendency for some districts to attract individuals that are better educated, while other districts are left with individuals that have lower educational attainment. We do not have data on district of birth. To mitigate some of the measurement error, we exclude from the computation of the aggregated outcomes of interest those individuals for whom the region of residence does not match their region of birth. Admittedly, this solution is inferior to the one whereby district mismatches are removed, as district data is more granular. There are 103 districts in the sample but only 18 regions. Data limitations do not allow us to be more precise. Nevertheless, we argue that districts within regions are similar and share commonalities. Thus, we argue this correction meets its intended purpose. Consequently, we remove 18 percent of the individual-level sample, as these individuals have moved regions since birth. The aggregated variables are then re-computed. The new regression results support our previous findings. See Table 6. Differences in magnitude between Tables 3 and 6 are negligible.

5.1.5 Falsification analysis

To build further confidence in the results of Table 3, we falsify the treatment variable by intentionally misplacing the timing of the reform. The true timing is 2002, however, we run iterations whereby the reform is assumed to have been implemented in each of the years between 1992 and 2005. To adapt to the false timing of the reform, the sampled cohorts will also be shifted backward and forward to have the same balance of presumed control, partial and full treatment cohorts. Appendix A.3 shows which cohorts are considered for each falsification test. The results of the falsified regressions are plotted in Figure 4 for specifications (5) and (6) from Table 3.

Figure 4 shows how both the magnitude and significance of the reform's impact are strengthened the closer the falsified timing of the reform is to 2002, the true implementation year. This is as much as can be demanded from a falsification test in this context. In fact, for most of the falsification regressions, the majority of the analysed cohorts are

Table 5: Robustness to the Measument of Inequality

Explanatory variables	**GINI INDEX - Years of primary education, 0 to 7 -** ($\mu = 0$ and $\sigma = 1$)											
	(1)		(2)		(3)		(4)		(5)		(6)	
Reform intensity (years)	-0.41*** (0.02)	-0.32*** (0.02)	-0.38*** (0.03)	-0.26*** (0.03)	-0.38*** (0.03)	-0.26*** (0.03)	-0.38*** (0.02)	-0.29*** (0.02)	-0.30*** (0.02)	-0.16*** (0.03)	-0.30*** (0.03)	-0.09** (0.04)
Reform intensity × Female		-0.13*** (0.01)		-0.15*** (0.02)		-0.15*** (0.02)		-0.14*** (0.01)		-0.17*** (0.02)		-0.19*** (0.02)
Fixed Effects												
District	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort	-	-	Yes	Yes	Yes	Yes	-	-	-	-	-	-
Age	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes
District × Period	-	-	-	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Age × Period	-	-	-	-	-	-	-	-	Yes	Yes	Yes	Yes
Age × District	-	-	-	-	-	-	-	-	-	-	Yes	Yes
Adjusted within R^2	0.22	0.23	0.10	0.12	0.10	0.12	0.21	0.23	0.07	0.10	0.05	0.09
F	583	346	199	192	199	192	611	341	226	216	130	234
Nr. clusters	103	103	103	103	103	103	103	103	103	103	103	103
N	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. The dependent variable is standardized. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–1994 cohorts have been partially treated and the 1995–1997 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. The sample only includes districts from Mainland Tanzania.

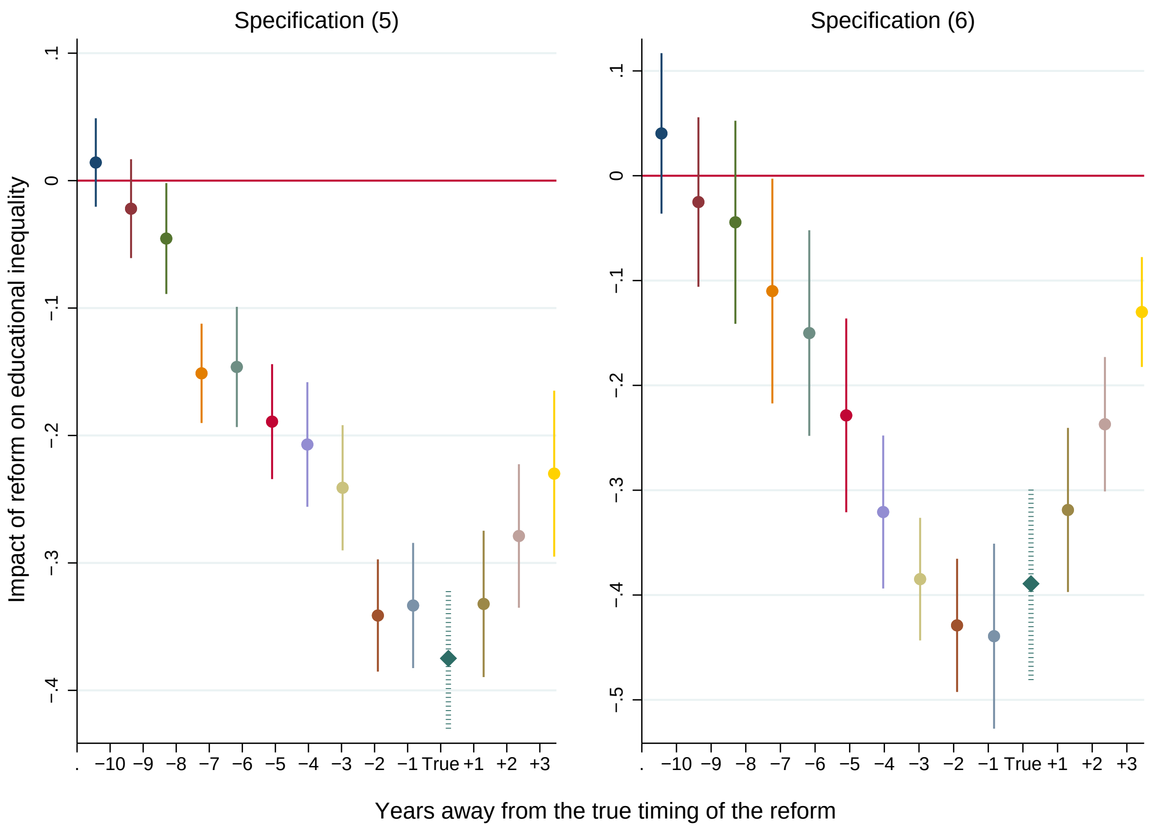
Table 6: Robustness to Removing Individuals Residing in a Region Different from their Region of Birth

Explanatory variables	**THEIL'S T INDEX** **- Years of primary education, 0 to 7 -** *- Only individuals for whom their region of birth coincides with their region of residence -* ($\mu = 0$ and $\sigma = 1$)											
	(1)		(2)		(3)		(4)		(5)		(6)	
Reform intensity (years)	-0.40***	-0.32***	-0.50***	-0.41***	-0.50***	-0.41***	-0.36***	-0.27***	-0.40***	-0.29***	-0.39***	-0.23***
	(0.03)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.02)	(0.03)	(0.03)	(0.04)	(0.05)	(0.07)
Reform intensity × Female		-0.12***		-0.11***		-0.11***		-0.13***		-0.13***		-0.14***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)		(0.02)
Fixed Effects												
District	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort	-	-	Yes	Yes	Yes	Yes	-	-	-	-	-	-
Age	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes
District × Period	-	-	-	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Age × Period	-	-	-	-	-	-	-	-	Yes	Yes	Yes	Yes
Age × District	-	-	-	-	-	-	-	-	-	-	Yes	Yes
Adjusted within R^2	0.18	0.18	0.13	0.14	0.13	0.14	0.16	0.17	0.09	0.11	0.06	0.08
F	243	252	137	180	136	180	316	296	174	207	74	202
Nr. clusters	103	103	103	103	103	103	103	103	103	103	103	103
N	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. The dependent variable is standardized. It was computed based on the sample of individuals who have not changed their region of residence since birth. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–1994 cohorts have been partially treated and the 1995–1997 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. The sample only includes districts from Mainland Tanzania.

rightly classified as control, partial or full treatment (see Appendix A.3), thus falsification tests can only show that estimations perform incrementally worse at identifying the impact of fee elimination the further away the falsified reform is from its true timing.

Figure 4: Falsification Analysis



Estimates are with 95% confidence intervals. The reform impact coefficient is plotted for 14 regressions. The left-hand side block employs specification (5) from Table 3. The right-hand side block presents specification (6). Control and treatment cohorts are pushed forward or backward each time by 1 year.

5.2 Mechanisms

Morrisson & Murtin (2013) have argued that worldwide reductions in educational inequality between 1870 and 2010 have been chiefly attained by reducing the share of people who have never enrolled in school. We confirm this is also the case of Tanzania. We investigate the mechanisms via which the elimination of fees has impacted educational inequality. We estimate the reform's impact on (i) the ratio of people who have never enrolled in school, (ii) the average educational achievement of *all* individuals, whether they have or have not enrolled in school, and (iii) the average educational achievement of those who enrolled.

Summary statistics have suggested that inequality is significantly smaller if the computation of the index does not include the individuals who have never enrolled in school. Thus, there is little room for improvement in terms of educational inequality among enrollees since baseline levels are satisfactory. However, the proportion of people who have never enrolled in school is large, an average of 13 percent, and it has decreased by 6 percentage points between 2002 and 2012. This is suggestive evidence that better enrolment rates, rather than improvements in grade progression, have triggered the drop in inequality.

Figure 5: Evolution of the Never-Enrolled Population Ratios

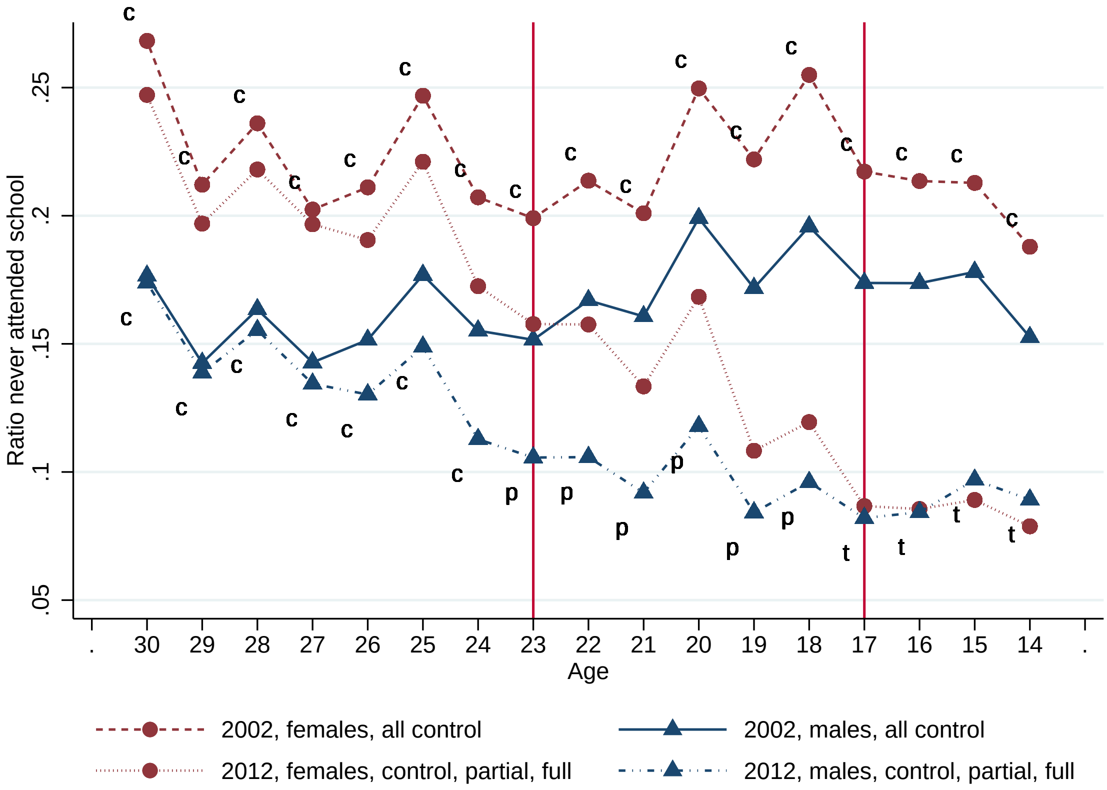


Figure 5 brings further suggestive evidence of the impact of free primary education on the percentage of individuals who have never attended school. First, it is visible that among the control groups, there is no significant difference between the out-of-school gendered ratios of 2002 and 2012. Then, starting with age 23, when these age groups are classified as partially treated for the first time in 2012, it becomes apparent that the 2012 outcomes diverge from their 2002 counterparts by following a downward path. Second, a higher share of control women have never attended school compared to males. However, the difference between treated females and males appears to have been eliminated. What is more, the proportion of individuals who have never enrolled in school is also significantly lower for the treated compared to control groups.

Table 7 confirms the insights from Figure 5. The elimination of fees has had a significant causal impact on the ratio of people who have never attended school. The average magnitude of this effect is a reduction of 4 percentage points for a one-year increase in reform exposure, and 6.8 percentage points for the average reform intensity of 1.7 years, with females, once again, benefiting almost twice as much as their male counterparts. However, females are also 5 percentage points more likely to be uneducated. Thus, this can partially explain the difference in impacts between females and males.

Table 7: Mechanisms

Explanatory variables	RATIO OF NEVER-ENROLLED INDIVIDUALS - All individuals -											
	(1)		(2)		(3)		(4)		(5)		(6)	
Reform intensity (years)	-0.05***	-0.04***	-0.05***	-0.04***	-0.05***	-0.04***	-0.04***	-0.03***	-0.04***	-0.03***	-0.04***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Reform intensity × Female		-0.01***		-0.01***		-0.01***		-0.02***		-0.02***		-0.02***
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Fixed Effects												
District	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort	-	-	Yes	Yes	Yes	Yes	-	-	-	-	-	-
Age	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes
District × Period	-	-	-	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Age × Period	-	-	-	-	-	-	-	-	Yes	Yes	Yes	Yes
Age × District	-	-	-	-	-	-	-	-	-	-	Yes	Yes
Adjusted within R^2	0.19	0.20	0.13	0.14	0.13	0.14	0.17	0.19	0.09	0.11	0.07	0.09
F	516	379	263	203	263	203	548	370	294	236	199	258
Nr. clusters	103	103	103	103	103	103	103	103	103	103	103	103
N	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. The dependent variable is standardized. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–1994 cohorts have been partially treated, and the 1995–1997 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. The sample only includes districts from Mainland Tanzania.

Table 7: Mechanisms (Continued)

Explanatory variables	(1)		(2)		(3)		(4)		(5)		(6)	
YEARS OF PRIMARY EDUCATION - All individuals -												
Reform intensity (years)	0.44***	0.36***	0.34***	0.22***	0.34***	0.22***	0.41***	0.33***	0.26***	0.11***	0.29***	0.10**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)
Reform intensity × Female		0.11***		0.15***		0.15***		0.12***		0.17***		0.17***
		(0.01)		(0.02)		(0.02)		(0.01)		(0.02)		(0.02)
Adjusted within R^2	0.26	0.27	0.08	0.10	0.09	0.11	0.26	0.27	0.06	0.09	0.05	0.10
F	621	322	165	192	165	191	615	310	169	214	174	233
YEARS OF PRIMARY EDUCATION - Only enrollees -												
Reform intensity (years)	0.17***	0.16***	-0.01	-0.06***	-0.01	-0.06***	0.18***	0.17***	-0.03***	-0.08***	0.07***	0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Reform intensity × Female		0.01*		0.06***		0.06***		0.01*		0.06***		0.04***
		(0.00)		(0.01)		(0.01)		(0.00)		(0.01)		(0.01)
Adjusted within R^2	0.17	0.17	0.00	0.01	0.00	0.02	0.18	0.19	0.00	0.02	0.02	0.03
F	151	89	2	48	2	48	154	90	12	50	22	59
Fixed Effects												
District	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort	-	-	Yes	Yes	Yes	Yes	-	-	-	-	-	-
Age	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	Yes	Yes
District × Period	-	-	-	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Age × Period	-	-	-	-	-	-	-	-	Yes	Yes	Yes	Yes
Age × District	-	-	-	-	-	-	-	-	-	-	Yes	Yes
Nr. clusters	103	103	103	103	103	103	103	103	103	103	103	103
N	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004	7,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. The dependent variable is standardized. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–1994 cohorts have been partially treated, and the 1995–1997 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. The sample only includes districts from Mainland Tanzania.

The impact of free primary education on the ratio of individuals who have never attended school informs the impact of the reform on average years of education. If the percentage of people who have never enrolled in primary education is reduced, then the average education of the concerned population will automatically increase, and the magnitude of this increase will be amplified by the efficacy of the reform in improving the educational outcomes of those enrolled. Table 7 confirms that the elimination of fees has increased the average educational achievement of the sampled cohorts by 0.29 years on average for a one-year increase in reform intensity, which is the equivalent of a 0.49 increase evaluated at average reform intensity. The gendered pattern is maintained, with girls benefiting significantly more than boys. Nevertheless, if the educational outcomes are only averaged over populations who have previously enrolled in primary education, then the aforementioned positive effects are no longer visible in the data. Table 7 shows that the elimination of fees has not had a consistent impact on the average educational outcomes of those enrolled.

Overall, Table 7 suggests that the documented reduction in educational inequality was chiefly facilitated by the reform's strong impact on reducing the ratio of people who never enrol in primary education. Thus, while this is an important step forward, it also suggests that the elimination of fees may be insufficient to improve retention rates and reduce dropouts. This also means that dropouts are caused by a wider set of factors, which extend beyond school fees. We tentatively conclude that fees have been an important obstacle in the achievement of full enrolment; however, fees do not appear to have been the leading problem behind dropouts for those who do enrol in primary education. A complementary explanation is that the average educational achievement of those who would have enrolled in school regardless of the reform may be watered down by the performance of individuals who enrol as a consequence of the removal of fees, but for whom the elimination of fees is insufficient to allow them to complete primary education, and thus they drop out. Consequently, the latter group of students cancels any improvement in grade achievement for those who would have anyway enrolled, fee or no fee. We document suggestive evidence of the latter explanation in Section 5.3.1, which does not exclude the potential validity of the former explanation.

5.3 Reform Limitations

The elimination of school fees, while successful at reducing educational inequality via improvements in enrolment rates, it has also had a more limited if not problematic impact on dropout rates. Moreover, we find that the magnitude of the reform's impact has declined for the last of the treated cohorts, born 1998, compared to those born 1995-97. This suggests that reform impacts may be diminishing as time elapses from the year when the reform was first rolled out. This is corroborated by summary statistics from UNESCO, per which net enrolment in primary education has decreased by 15 percentage points between 2008 and 2017. Nevertheless, more recent census data is needed to issue conclusive evidence.

5.3.1 Dropout rates

We estimate the impact of fee elimination on the probability of dropping out after each grade using the most comprehensive of specifications, column no. (6) from previous tables. Table 8 shows that the reform has not impacted dropout rates at the level of grades 1, 2 or 3. However, starting grade 4, we see an increase in dropout rates after the implementation of free primary education. At the end of grade 4, children also sit an exam to proceed to grade 5. At the average level of reform intensity, dropout rates have increased by 0.61, 0.51 and 0.71 percentage points after grades 4, 5 and 6 respectively. As hypothesized before, while the reform was successful in bringing children into school for the first time, it has not been sufficient to keep them in school until the end of grade 7. If all the evidence is put together, we argue that children who enrolled as a consequence of fee elimination have had relatively higher dropout rates.

Table 8: Free Primary Education and Dropout Rates

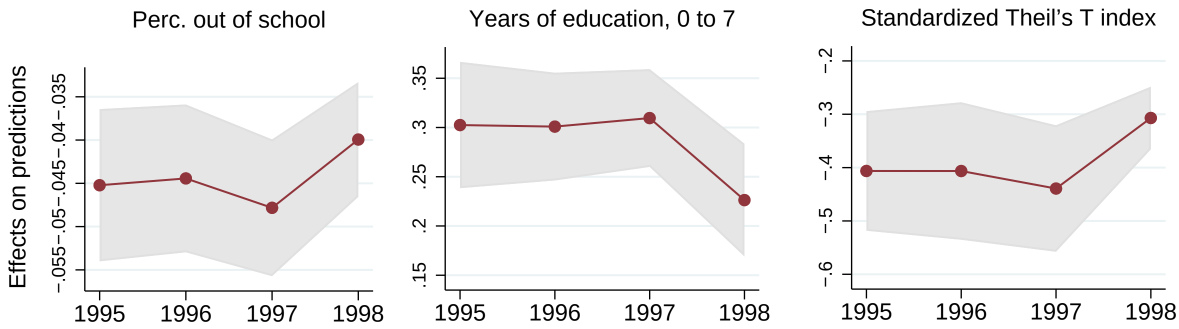
	PERC. ENROLLEES DROPPING OUT AFTER GRADE #					
	1	**2**	**3**	**4**	**5**	**6**
Explanatory variable	%	%	%	%	%	%
Reform intensity (years)	-0.07	0.03	-0.06	0.36**	0.30***	0.42***
	(0.06)	(0.06)	(0.10)	(0.15)	(0.10)	(0.12)
Fixed Effects						
District	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort	-	-	-	-	-	-
Age	Yes	Yes	Yes	Yes	Yes	Yes
Period	Yes	Yes	Yes	Yes	Yes	Yes
District × Period	Yes	Yes	Yes	Yes	Yes	Yes
Age × Period	Yes	Yes	Yes	Yes	Yes	Yes
Age × District	Yes	Yes	Yes	Yes	Yes	Yes
Mean explained var. (%)	0.74	1.98	2.68	4.08	3.07	2.90
σ explained var. (%)	0.90	1.55	2.03	2.76	2.33	2.36
F	1	0	0	5	9	13
Nr. clusters	103	103	103	103	103	103
N	7,004	7,004	7,004	7,004	7,004	7,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–1994 cohorts have been partially treated, and the 1995–1997 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific.

5.3.2 Diminishing effects

As described in Section 2, there is anecdotal evidence that the implementation of free primary education is losing momentum because schools cannot cope with the burden that the reform has imposed on their income (Lindsjö, 2018). Thus, schools have informally resumed asking students for financial contributions. Although not compulsory in theory, students are refused participation in classroom teaching if they fail to pay (Vavrus & Moshi, 2009). Therefore, one can intuit that the strength of the reform is diminishing the more one moves away from the announcement year and the more chances parents get to update their beliefs regarding the cost of free primary education.

Figure 6: Cohort Marginal Effects of Reform Intensity on Outcome of Interest



Confidence intervals are at 95 percent. Each plot is the result of a regression. Each regression includes the covariates of specification (6) plus interaction terms between reform intensity and the cohorts that have been partially (1989, 1990, 1991, 1992, 1993 and 1994) or fully treated (1995, 1996, 1997 and 1998). The reference category is the interaction with the 1998 cohort. Errors are clustered at the level of districts.

With the data that is available, Figure 6 is suggestive of a smaller effect for the youngest of cohorts compared to the other treated but older cohorts. The difference between the magnitude of the effect for the 1998 cohort on the one hand, and the 1995-97 cohorts on the other, is generally statistically significant and points to a reduction in absolute values.

Table 9: Tests of Cohort Effects

	Coeff. 1								
	$H_0 : Coefficient_1 = Coefficient_2$ and $H_1 : Coefficient_1 \neq Coefficient_2$								
	PERC. OUT OF SCHOOL			**YEARS OF EDUCATION**			**THEIL'S T INDEX**		
	Reform intensity × 1995	Reform intensity × 1996	Reform intensity × 1997	Reform intensity × 1995	Reform intensity × 1996	Reform intensity × 1997	Reform intensity × 1995	Reform intensity × 1996	Reform intensity × 1997
Coeff. 2	t-stat (p-value)	t-stat (p-value)	t-stat (p-value)	t-stat (p-value)	t-stat (p-value)	t-stat (p-value)	t-stat (p-value)	t-stat (p-value)	t-stat (p-value)
Intensity	0.04	-	-	0.00	-	-	0.00	-	-
× 1996	(0.84)	-	-	(0.95)	-	-	(1.00)	-	-
Intensity	0.25	0.93	-	0.04	0.14	-	0.40	1.15	-
× 1997	(0.62)	(0.34)	-	(0.84)	(0.71)	-	(0.53)	(0.29)	-
Intensity	1.06	0.96	5.29**	3.53*	4.25**	8.59***	2.89*	2.75*	7.21***
× 1998	(0.31)	(0.33)	(0.02)	(0.06)	(0.04)	(0.00)	(0.09)	(0.10)	(0.01)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows the results of testing coefficients against each other within each regression that was plotted in Figure 6. Regressions include the covariates of specification (6) plus interaction terms between the reform intensity variable and the cohorts that have been partially (1989, 1990, 1991, 1992, 1993 and 1994) or fully treated (1995, 1996, 1997 and 1998). The reference category is the interaction with the 1998 cohort. Errors have been clustered at the level of districts.

This draws attention to the importance of ensuring that schools have access to sufficient funds to obviate the need to ask for student contributions. A policy eliminating mandatory fees and contributions is not sufficient in and of itself. It requires long-term commitment to ensuring that schools do not revert to old habits. With more recent data, it is important to investigate if younger generations are indeed being burdened by newly reinstated fees, which dissuade enrolment for those who are disadvantaged due to their socio-economic status, gender or other such factors.

6 Conclusion

Studies on free primary education in sub-Saharan Africa have focused on the reform's impact on average outcomes. We have complemented this literature by providing causal evidence of the impact of free primary education on the distribution of educational achievement in Mainland Tanzania. We find that exposure to an average of 1.7 years of free primary education has successfully reduced educational inequality by 0.66 standard deviations. We also find that this outcome has been mainly driven by a reform-induced reduction of 6.8 percentage points in the proportion of people who have never attended primary education. We document evidence that free primary education has benefited females more than males. Thus, the educational gap between genders has narrowed.

The elimination of primary school fees is an important policy to improve and equalize access to primary education. At the same time, we argue that it should be part of a package of interventions because, as efficient as it might be at solving some problems, the reform is not without limitations. For instance, dropout rates at the end of grades 4, 5 and 6 have increased by 0.51–0.71 percentage points following the elimination of fees. Moreover, the beneficial impacts of the reform seem to be diminishing for the youngest of the treated cohorts compared to the other treated cohorts. Lastly, we do not find any consistent impact on the educational achievement of those who would have presumably enrolled even without the reform. However, this may be due to satisfactory baseline performance for this group of beneficiaries, which limits their opportunities for improvement.

Our study is complementary to Hoogeveen & Rossi (2013). The authors focus on school-age cohorts, while we investigate completed education among cohorts of post-primary age. Valente (2019) is also an excellent complement to our study, as she investigates the impacts of the reform on student performance and rejects any significant deterioration due to the infrastructure challenge brought on by the post-2002 surge in enrolments.

Our paper underscores the importance of monitoring and implementing a reform with the same enthusiasm, energy and resources as when it was first rolled out. Otherwise, the concerned stakeholders can revert to previous behaviours and progress reversed. As already documented in this paper, net enrolment in primary education was 99 percent in 2008 but only 84 percent in 2017. This comes against the background of qualitative studies finding evidence that parental contributions are still being requested and they are mandatory. Finally, the amount of the per-capita capitation grant has not been revised despite inflation, and there are documented cases whereby only a fraction of the capitation grant has reached schools (Vavrus & Moshi, 2009; Mushi, 2013; Lindsjö, 2018). Therefore, it is not surprising that parents update their beliefs with respect to the true cost of free primary education, which in return informs their decision to send children to school.

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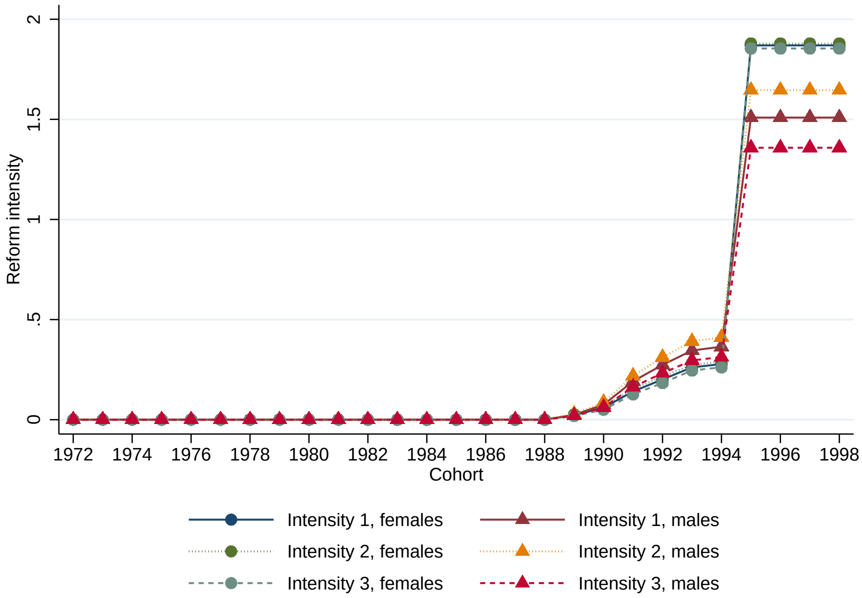
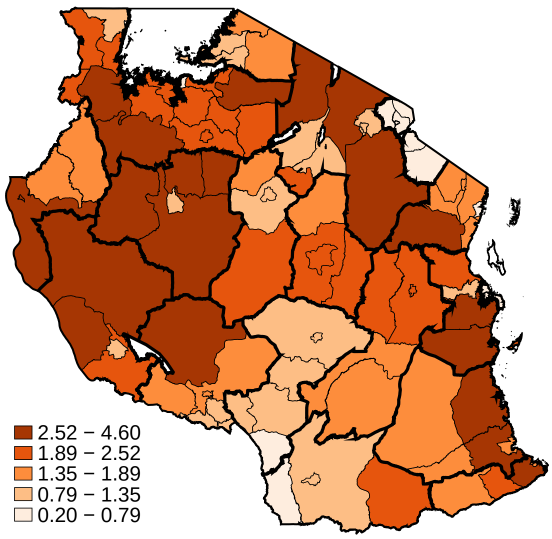
Appendix

A.1 Reform Intensity Variations



Reform intensity is computed based on the educational performance of: (**Intensity 1**) individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83); (**Intensity 2**) individuals who were aged 19–25 at the time of the reform (7 birth-year cohorts, 1977–83); and (**Intensity 3**) individuals who were aged 26–32 at the time of the reform (7 birth-year cohorts, 1970-76). Reform intensity is district-gender-birth-cohort specific. The graph takes means over districts per gender and birth cohort. 1989 is the first partially treated cohort and 1995 is the first fully treated cohort.

A.2 District and Regional Distribution of Reform Intensity



The reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. Only the intensity for the fully treated cohorts is mapped. We take averages over genders. 103 Mainland districts (thin contours) and 18 regions (thick contours).

A.3 Age-Period-Cohort Table

	AGE OR BIRTH COHORT																													
Age in 2002	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**	**20**	**21**	**22**	**23**	**24**	**25**	**26**	**27**	**28**	**29**	**30**	**31**	**32**	**33**	**34**	**35**	**36**	**37**	**38**	**39**	**40**
Birth cohort (year)	91	90	89	88	87	86	85	84	83	82	81	80	79	78	77	76	75	74	73	72	71	70	69	68	67	66	65	64	63	62
True treatment status	p	p	p	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c
Age in 2012	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**	**20**	**21**	**22**	**23**	**24**	**25**	**26**	**27**	**28**	**29**	**30**	**31**	**32**	**33**	**34**	**35**	**36**	**37**	**38**	**39**	**40**
Birth cohort (year)	01	00	99	98	97	96	95	94	93	92	91	90	89	88	87	86	85	84	83	82	81	80	79	78	77	76	75	74	73	72
True treatment status	t	t	t	t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c
False, +3, 2002 status	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c													
False, +3, 2012 status	t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c													
False, +2, 2002 status		c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c												
False, +2, 2012 status		t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c												
False, +1, 2002 status			c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c											
False, +1, 2012 status			t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c											
False, -1, 2002 status					c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c									
False, -1, 2012 status					t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c									
False, -2, 2002 status						c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c								
False, -2, 2012 status						t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c								
False, -3, 2002 status							c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c							
False, -3, 2012 status							t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c							
False, -4, 2002 status								c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c						
False, -4, 2012 status								t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c						
False, -5, 2002 status									c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c					
False, -5, 2012 status									t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c					
False, -6, 2002 status										c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c				
False, -6, 2012 status										t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c				
False, -7, 2002 status											c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c			
False, -7, 2012 status											t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c			
False, -8, 2002 status												c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c		
False, -8, 2012 status												t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c		
False, -9, 2002 status													c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	
False, -9, 2012 status													t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c	
False, -10, 2002 status														c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c	c
False, -10, 2012 status														t	t	t	t	p	p	p	p	p	p	c	c	c	c	c	c	c

Where t denotes the treated, p is for partial treatment and c is for control.