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The Effects of Free Primary Education on Occupational Choice and Internal Migration in Sub-Saharan Africa

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ABSTRACT
This paper studies the relationship between education, occupational choice and regional migration in Sub-Saharan Africa. I assemble micro-data from household surveys and micro-censuses and use a differences-in-differences approach to evaluate the effects of the Free Primary Education reforms on individuals' education levels, choices of occupation and migration decisions in Ethiopia, Kenya, Malawi, Nigeria, Tanzania, Uganda, and Zambia. The findings suggest that the reforms caused significant improvements in educational attainment in terms of primary school completion rates and average years of schooling. In all but one country (Ethiopia), the reforms also led a significant proportion of individuals to shift away from agriculture as their main occupation. Significantly positive effects of the reforms on internal migration support the hypothesis that regional mobility may be an important channel for occupational change in these countries. The results also suggest heterogeneity in the effects of the reforms, which are significantly larger for women than men in all countries except for Nigeria.
Keywords: *Regional migration, education, occupation*

1. INTRODUCTION

Access to education is considered to be both a basic human right and a critical factor in the eradication of poverty[1]. Education has also been recognized as an important contributor to economic growth[2, 3]. The economic impact of reforms aiming to improve educational attainment in developing countries is therefore of crucial interest to policymakers. Focusing on Sub-Saharan Africa, this paper examines an important channel through which such reforms could help the growth of developing economies: occupational change. In Sub-Saharan Africa, most countries have yet to see their occupational structure shift away from one where the majority of the population is employed in agriculture. Policies seeking to improve education levels therefore hold the potential to give young people living in rural, agriculture-dominated areas the means to work in higher-productivity sectors. Since occupational change has been shown to play a significant role in long-term growth and development[4, 5], precisely estimating the impacts of education reforms on individuals' choice of sector of employment could yield important conclusions from a policy perspective.

This study evaluates the effects of school fee abolition policies, known as the Free Primary Education (FPE) reforms, on individuals' choice of occupation and propensity for regional migration in seven Sub-Saharan African countries, using two different data sources. After decolonization, fees were collected at the primary school level in many countries. In an effort towards universal primary education, many Sub-Saharan African governments have since officially abolished school fees. This paper investigates whether these policies, which significantly improved school enrolment in all seven countries, also increased the proportion of young adults finding employment in non-agricultural sectors. Since internal migration may play an important role in facilitating occupational change, this paper also studies the impacts of the reforms on regional mobility.

Turning to mechanisms, this paper argues that the dominant channel driving the positive effects of the reforms on occupational choice and internal migration is human capital. The validity of this assumption is discussed in section 5, where this paper tests for the most plausible alternative channel through which these policies could have affected occupations: the wealth effect of school fee abolition on households who would likely have sent their children to school even without the reforms. For this paper, there were used education of older siblings and education of fathers as indicators of such households to test for differential effects of the FPE reforms on these households. This paper finds no evidence that a wealth effect is driving the impact of the reforms on occupations in the samples in which the paper can perform these tests. This lends support to the hypothesis that the FPE policies affected occupational choice primarily via the human capital channel.

This paper fits within a large body of work on the relationship between education and economic growth. In the growth literature, endogenous growth models such as those put forward by Romer[6], recognize human capital accumulation as an important source of long-term growth. Young's work[7, 8] on the NICs in East Asia also suggests that human capital explains a relatively large share of the

high growth rates of these economies in the postwar period. In the micro-development literature, many papers have studied the returns to education in developing countries[9, 10, 11, 12]. An important contribution in terms of causal evidence is Duflo's work[13] in Indonesia, in which she uses variation in the timing of a nationwide school construction program across different regions to estimate the impact of education on wages. She finds positive returns to schooling in terms of higher wages for the individuals who benefited from improved access to primary education, and her cost-benefit analysis suggests net economic gains for the Indonesian economy from this school expansion program. None of these studies, however, focus on individuals' choice of sector of occupation in relation to their exposure to an education reform.

On the relationship between education and internal migration, the literature includes one notable attempt to measure the impacts of a schooling reform on regional mobility: a study by Machin et al.[14] that examines the effects of a Norwegian reform increasing the length of compulsory education in 1960. The authors find evidence of a causal link between education and internal migration. To the best of my knowledge, however, the impact of policies seeking to increase educational attainment on regional migration has not yet been investigated in a developing country setting.

The remainder of this paper is structured as follows: section 2 describes the different datasets used for each country. In section 3, this paper discusses the identification strategy. The results on the average effects of the reforms and the differential effects by gender are described in section 4. Section 5 discusses the possible mechanisms driving these results. Section 6 concludes and brings forward some potential avenues for future research.

2. DATA

In all seven countries examined in this paper, the objective of the FPE policy was simple: abolish all fees that might present a barrier to primary education for children from poor households. According to a comparative study on the implementation of the reforms in several countries, before the abolition reforms, the fees were used to fund all school-level inputs except for teacher salaries[15].

2.1. Sample Countries

Out of the 51 countries in Sub-Saharan Africa, 26 have officially documented FPE reforms. These countries are listed in Table 1. Publicly available datasets containing information on education, occupation and migration were available for 14 of these countries. This paper choses to analyse the effect of the reforms in the seven countries for which the lapse of time between the launch of the FPE reform and the date of data collection was long enough to allow for the identification of effects on both primary school completion rates and labour market outcomes. In countries where more than one primary school fee abolition policy was implemented since independence, this paper analyses the latest reform (except for Kenya; details are provided in the Appendix). For each of these seven countries, this paper restricts the sample to all individuals who were in age of having completed primary school at the time of the data collection.

The two data sources exploited in this study are the Integrated Public Use Microdata Series (IPUMS) and Living Standards Measurement Surveys (LSMS) datasets. Both are nationally representative samples measuring a range of socioeconomic indicators at the household and individual level. IPUMS data was available for Malawi (2008), Zambia (2010), and Kenya (1989), while LSMS data was available for Ethiopia (2006), Uganda (2009-2010), Nigeria (2010 and 2012) and Tanzania (2012-2013). The following two sub-sections describe the variables used in the analysis.

2.2. Outcome Variables

2.2.1. Education

In all seven samples, this paper uses the 'educational attainment' measure (included in each dataset) to construct two outcome variables on education. This educational attainment measure is, in every dataset, a self-report of the highest school grade ever completed by surveyed individuals. The first outcome variable that this allows me to construct is a binary variable equal to 1 for individuals who completed to primary school. The second is the total number of years of schooling completed by each individual.

2.2.2. Occupation

In both the IPUMS and LSMS samples, surveyed individuals were asked to report their main occupation and the enumerators recorded the sector of occupation to which this type of activity belonged using survey-specific code sheets. These code sheets allow me to define my main outcome variable on occupation: a binary variable equal to 1 when an individual's main occupation is classified as belonging to a non- agricultural sector for each sample.

The three IPUMS censuses also include an 'employment status' variable which classifies individuals as employed, unemployed, or inactive. This paper uses this classification to construct another binary outcome variable indicating whether an individual is recorded as economically active. As can be seen in Table 2, a significant proportion of the surveyed individuals in the three IPUMS countries are recorded as economically inactive, ranging from 29% of the sample in Malawi to 47% of the Zambia sample.

2.2.3. Migration

The migration variable that can define for six out of seven countries using the LSMS and the IPUMS data is a binary variable equal to 1 if the individual's district of birth is located in a different region or province of the country than their current district of residence.

2.3. Covariates

The seven country surveys all report gender and urban/rural classification of household, which constitute my main two individual-level control variables. In the samples where it is available, religion is also included in my vector of controls. Six of the seven datasets included a district or region of birth variable, which uses as a proxy for the district or region where the individual went to primary school. In the Nigeria surveys, however, no information on the location of birth of sampled individuals was available, so this paper follows Larreguy and Marshall[16] and use current Local Government Authority (LGA) - a geographical unit similar to that of district in the other countries - as a proxy for LGA od birth.

3. IDENTIFICATION STRATEGY

3.1. Reduced Form Estimation

The identification strategy of this paper exploits the variation in the intensity of the reforms across districts of birth (which proxy for district of education) and in individuals' eligibility for the reforms as determined by their year of birth to evaluate the impact of the FPE policies on education, occupation and internal migration outcomes. The average treatment effects of the reforms are estimated using the following differences- in-differences specification:

$$Y_{idt} = \alpha_t + \delta_d + \beta Treatment_d \times Post_{it} + \gamma x_i + \epsilon_{idt} \quad (1)$$

Where Y_{idt} is the outcome variable for individual i born in district d in year t (education, occupation and migration outcomes); α_t is a year of birth fixed effect; δ_d is a district of birth fixed effect; x_i is a vector of individual-level characteristics (urban/rural status, gender, and religion where available); $Post_{it}$ and is a binary variable indicating whether individual i born in year t was eligible to benefit from the FPE reform - i.e. whether they were aged 6 or 7 (depending on the country) or less at the time of the reform. The main effects ($Post_{it}$ and $Treatment_d$) are not visible in equation (1) because they are controlled for by the year of birth and district of birth fixed effects. When running the specification on occupation and migration outcomes, this paper also controls for current district fixed effects. Since some people moved away from their district of birth, this controls for any unmeasured differences in the labour markets where individuals were holding their occupations at the time that the surveys were administered. The individuals whose age was less than or equal to the official primary school entry age in the year that the reform was implemented constitute the 'post-reform' group, and those who were old enough to have finished primary school at the time of the reform constitute the 'pre-reform' group. Throughout the analysis, this paper excludes the 'partially treated' cohorts, i.e. individuals older than the official age of entry into primary school but young enough at the time of the reform to have still been enrolled in primary school.

The $Treatment_d$ variable is constructed using pre-reform education levels in individuals' district of birth, similarly to Larreguy and Marshall[16]. This variable is equal to 1 for individuals born in districts where the proportion of people having completed primary school is below the national median primary school completion rate before the reform. The treatment and control areas are thus defined based on their difference in education levels pre-reform.

The main identifying assumption that has to hold in this framework to ensure that the differences-in-differences strategy will yield estimates of the causal effects of the reforms is that of 'parallel slopes'. That is, it must be plausible that the difference in education levels between treatment and control areas would have remained the same after the date of the FPE reforms if these policies had not been implemented. If this assumption holds, any change in the relative trends in education levels between the treatment and control regions after the FPE reforms can be credibly attributed to the causal impact of the reforms.

It is therefore necessary to examine the trends of the control and treatment groups before the implementation of these policies to be able to draw accurate conclusions about any changes in the relative trends in education levels of treatment and control regions post-reform. To test for the validity of this identification assumption, this paper therefore estimates:

$$Y_{idt} = \alpha_t + \delta_d + \sum_t \beta_t Treatment_d \times Birthyear_{it} + \gamma X_i + \epsilon_{idt} \quad (2)$$

Where $Birthyear_{it}$ is a dummy equal to 1 if individual i was born in year t. Evidence that the parallel slopes assumption holds will be reflected in β_t estimates that are insignificantly different from zero for all years preceding the year of birth of the oldest cohort of individuals exposed to the reform (i.e. the 'first' of the 'post-reform' cohorts). The results of this test are discussed in section 4.1. Another possible threat to this identification strategy would be the simultaneity of the FPE reforms and other policies that could have affected the treatment and control areas differentially and around the same time. However, this paper investigates the factors determine no other policy changes that could have affected the difference in education levels, occupations and migration patterns across treatment and control areas were being implemented around the dates of the FPE reforms in any of the seven countries in this sample. Finally, it is

important to note that this paper restricts analysis to a reduced-form approach. First, this study seeks to estimate the average effects of the FPE reforms on education, occupation and migration indicators, whereas using these policies as an instrumental variable for education would imply estimating the causal effect of education on occupational choice and migration decisions, which is beyond the scope of this paper. It is also arguable that the reform would not make a plausibly excludable instrument for education, given that any effects it might have had on any 'second stage' outcomes could have occurred through different channels, the most obvious one of which being the wealth effect for families who were already planning to send their children to school and could have reallocated the funds saved as a result of the reforms towards other productive investments (see section 5).

3.2. Differential Effects by Gender

One goal of the FPE reforms that was made explicitly clear by some of the governments which implemented these policies was to improve the educational attainment of women and close the gap between differences in schooling levels between men and women[17]. Before the launch of the FPE reforms, educational attainment measures were distinctively lower for females in all seven countries in the sample, with female primary school completion rates being on average 33% lower than that of males. If girls' education is more price elastic than boys' in Sub-Saharan African countries[18], then one would expect the effect of the FPE reforms to be larger for girls. This paper tests this hypothesis using the following specification:

$$Y_{idt} = \alpha_t + \delta_d + \eta Treatment_d \times Post_{it} + \beta Treatment_d \times Post_{it} \times Female_i + \gamma x_i + \epsilon_{idt} \quad (3)$$

Here, the coefficient of interest is thus β : finding significantly positive estimates for this coefficient would suggest that the reforms helped reduce the gap in educational attainment between girls and boys. The covariates are all the same as in equation (1). (Note that the $Female_i$ main effect is included in the vector of individual controls x_i here.) This paper also runs specification (3) on the same labor market and migration outcomes as those studied for average treatment effects.

4. RESULTS

4.1. Testing for Parallel Trends

Figure 1 plots the estimates of the β_t coefficients from specification (2), which tests the validity of the parallel slopes assumption, for each of the seven samples. The outcome variable is a binary variable equal to 1 for individuals who completed primary school, for all countries except Kenya and Nigeria, for which the estimates plotted in Figure 1 are obtained using years of schooling as the outcome variable. The standard errors are notably smaller for the countries for which IPUMS data is analyzed, but given the difference in sample sizes this is not surprising - the LSMS datasets contain about 3% of the number of observations of the IPUMS ones, so the estimates are expected to be less precise.

In all but one (Nigeria) of the seven samples, the treatment and control areas seem to follow similar trends in education levels before the FPE policies, with almost all estimates of the coefficients on the interaction between the $Treatment_d$ variable and the year of birth variable lying in a 95% confidence interval around zero. In these six samples, the parallel trends assumption therefore seems to hold.

The country for which the parallel slopes assumption seems to be violated, however, is Nigeria, where the coefficients suggest that the treatment regions were already catching up to the control regions in terms of education levels before the FPE reform. To address concerns that the 1999 FPE reform is not the only cause of the differential trends between treatment and control areas after the implementation of the policy, this paper controls for state trends in all my specifications for the Nigeria sample. Evidence suggests that the implementation and financing of educational reforms is very much decentralized at the state level in Nigeria; further, there seems to have always been large discrepancies in education levels and trends across states since independence[19, 20]. However, this cannot rules out the possibility that the FPE reform might not have been the driver of the change in differences in education levels between treatment and control regions in Nigeria.

4.2. Average treatment effects

Figure 1 provides evidence of the positive effects of the FPE reforms on education levels. These six countries exhibit a clear trend break in educational attainment (measured as primary school completion rates or years of schooling) after the implementation of the FPE reforms: in each country, education levels in the treatment areas depart from the pre-reform trend in the upward direction after the launch of the FPE policy. This suggests a positive causal effect of the FPE reforms on primary school completion rates and years of schooling.

The reduced form estimates of the average effects of the FPE reforms, obtained by estimating specification (1), are presented in Tables 4-6. They provide further evidence that the FPE reforms caused positive and significant effects on educational attainment. The effects of the reforms on the probability that an individual completed primary school range from an increase in 3.7 percentage points in Malawi to 17.4 percentage points in Nigeria. Since the sample means vary substantially across countries, it is also important to report the size of the effects relative to the country means: these effects range from a 7.3%

increase in primary school completion rates relative to the sample mean in Zambia to a 23% increase relative to the mean in Uganda and Nigeria. The effect on average years of schooling is also significantly positive in all countries, ranging from a 7.7% increase relative to the sample mean in Kenya to a 28.9% increase in Nigeria. In years of education, the countries for which the estimates are the lowest are Malawi and Kenya, where the reforms caused an in- crease in 0.563 and 0.458 years of schooling respectively. The countries for which the estimates are highest are Zambia and Nigeria, where the reforms led to an increase in 1.567 and 2.316 years of schooling respectively. For the countries where the lapse of time between reform and data collection is long enough to allow for the identification of the impact of the reforms on secondary schooling, I also find large and significant effects. In Malawi, the FPE reform led to an increase in secondary school completion rates by 73.8% relative to the sample mean. In Kenya, the estimate is 48.9%; in Nigeria, it is 63.7%.

The FPE policies also had significant impacts on individuals' sector of occupation in six out of seven countries - the exception being Ethiopia, where this paper investigates no effects on labor market outcomes. In the six countries where the estimates are significantly positive, the effect of the FPE reforms on the probability that an individual's main occupation does not belong to the agricultural sector range from a 2.2 percentage point increase in Kenya to 8.8 percentage point increase in Zambia. Comparing these estimates to the countries' sample means shows that the effects range from a 4.85% increase in the proportion of people reporting that their main occupation is not in the agricultural sector in Kenya to 33.7% in Uganda. It is important to note that the effects we see in Kenya were recorded in 1989, whereas the outcomes of young adults in the other countries were all collected between 2006 and 2013. Sub- Saharan Africa has experienced substantial growth since 1989[21] and so one might want to challenge the comparability of the Kenya estimate with that of the other countries. Aggregating the results across the seven countries, the estimates of the effect of the reforms on the probability to work in a non-agricultural sector average around 22.3% of the sample mean.

In the IPUMS data, the effects of the FPE policies on the likelihood that an individual is recorded as economically active are also strongly significant, ranging from a 4.1% increase relative to the sample mean for Kenya to a 13.81% increase in Malawi. In the two LSMS datasets for which this paper explores on wage employment, the estimates also suggest large increases in the probability that an individual holds a wage job. The effect is a 13.6 percentage point, or 45.6% increase relative to the mean in Uganda, and a 5.8 percentage point, or 45% increase relative to the mean in Tanzania.

The results on self-reported self-employment status are not consistent across the four countries in which this outcome is measured. These are the four countries for which the analysis uses LSMS data. In Tanzania, Uganda and Ethiopia, the effect is positive, though only significant at the 10% significance level in Tanzania, where the probability that an individual reports being self-employed increases by 30% relative to the mean. In Nigeria, the effect is negative, albeit not significant. In the face of these conflicting results and the fact that this paper cannot attest for the comparability of this variable across the four surveys, this paper does not propose an interpretation of the effects of the reform on the propensity for self-employment.

The effects on internal migration are overall positive but slightly less consistently significant across countries, with only 4 out of 6 countries exhibiting significantly positive effects. These countries are Kenya, Malawi, Tanzania and Zambia. In these four countries, the effects of the reforms on the migration indicator (equal to 1 for individuals who migrated from their region of birth) range from an increase by 2.6 percentage points in Malawi to an increase by 6 percentage points in Tanzania. Comparing these estimates to the sample means, this paper finds that the effects range from an 18.7% increase in the probability that an individual migrated from their region of birth relative to the mean in Zambia (where the individuals included in the treatment group are on average a few years younger than in the other countries) to a 40% in- crease in Malawi. In the two countries for which the LSMS data includes a question on whether the individual migrated for work, this paper finds positive but insignificant effects: a 41% increase relative to the mean in Tanzania and an 18.3% increase in Uganda.

Taken together, these results suggest that the FPE reforms had a strong impact not only on educational attainment, but also on labor market participation, wage employment, the proportion of individuals working in non-agricultural sectors, and regional mobility.

Given that it is the only country out of the seven in this sample with no positive effects on any labor market outcomes, the results for Ethiopia, however, are puzzling. One possible interpretation, assuming that the main channel via which the reforms could have affected occupational choice is the human capital hypothesis, is that the abolition of fees increased enrollment at the cost of education quality. However, the reliability of the variable this paper assumes to measure individuals' sector of occupation is very questionable for this particular sample, since this variable was missing for over 90% of the raw data and had to be imputed using other sections of the survey. This statistic is much higher in the Ethiopia dataset than in the other countries for which I use LSMS (Uganda, Tanzania, and Nigeria).

Some contextual factors specific to Ethiopia over the period of study should also be considered when interpreting these results. The sample contains individuals born between 1968 and 1993, with individuals born between 1968 and 1979 constituting the 'pre-reform' group. All of these individuals would have been either children or teenagers at the time of the 1984-85 Ethiopia famine. The famine led to over half a million deaths[22], as well as the forced relocation of 600,000 subsistence farmers by the government from the northern regions affected by the famine, to the south of the country in 1985

and 1986[23]. 2.5 million people also fled the country to seek refuge in neighboring countries[23].

This has a number of implications for my analysis. The first is that among the group of individuals in our sample affected by the famine (i.e. the 'pre-reform' group), a lot of these are 'survivors'. It is possible that this makes them less comparable to the 'post-reform' group than in the other countries. For instance, they could be on average of a more resistant physical constitution, or they could have been born in wealthier families if the deaths counted a large proportion of the poorest subsistence farmers. The second is that the forced displacement of rural populations is also highly likely to have perturbed the trend of occupational change in Ethiopia. It is rather plausible that individuals in the 'pre-reform' group are on average less likely to be farmers, not by choice, but because a significant fraction of them were forced to move away from their land of origin and, consequently, must have had recourse to other occupations as a source of livelihood. This would make the individuals born before 1979 an unreliable counter-factual for our 'post-reform' group, i.e. individuals born in 1987 and after, who did not live through the famine.

In the face of these different concerns about the data and the Ethiopian context, this paper does not consider that the lack of effects of the FPE reform constitutes reliable evidence that the abolition of school fees actually caused a decrease in the number of non- agricultural occupations in Ethiopia.

4.2. Heterogeneity of the Effects by Gender

The interaction of the $Treatment_d \times Post_{it}$ variable with a Female dummy in specification (3) yields striking results, presented in Tables 7-9. The estimates show that in all countries except Nigeria, the effects of the FPE reforms on educational attainment were significantly larger for women. The estimates of the impact of the FPE policies on the probability of completing primary school are between 7.3 percentage points (in Tanzania) and 15.6 percentage points (in Zambia) higher for females in the six countries where the effects of the policies are significantly different across genders. The effect of the reforms on years of schooling range from 0.521 more years for women in Tanzania to 1.468 more years in Ethiopia. The results on secondary school completion rates are also stronger for girls in every country except for Nigeria. In Nigeria, the coefficient estimates for the differential effect of the reforms on females is actually negative for both primary school completion and years of schooling (it is positive for secondary school completion), albeit not significant. In the six other countries, the results lend support to the hypothesis that girls' education is more price-sensitive than boys' in Sub-Saharan Africa, and suggest that the FPE reforms were successful in bridging some of the pre-existing gender gap in education in these countries[18].

The effects of the reforms on labor market outcomes are also higher for females: women are more likely than men to be working in a non-agricultural sector as a result of the reforms, they are more likely to be employed for a wage, and they are more likely to be classified as economically active in the IPUMS datasets. The largest estimate for this variable, in percentage points, is Zambia, where the effect of the FPE reform on the probability that a woman is classified as economically active in the IPUMS data is 24 percentage points higher than it is for a man. These results highlight a very positive outcome of the FPE policies, namely that they led to a sizable increase in women's participation in labor markets. This could have important long-term impacts on these countries' development, since studies such as Young's[7, 8] have put forward female labor force participation as being a key contributor to overall economic growth.

Finally, women are also more likely (though here the estimates of the differential effects are lower) than men to have migrated from their region of birth in three out of the four countries where the reforms caused a significant average treatment effect on this variable: Malawi, Zambia, and Kenya. In Uganda and Tanzania, the effects on work-related migration are also significantly more positive for women.

5. MECHANISMS

5.1. Possible Channels

The results laid out in the previous section can be interpreted as the causal effects of the FPE reforms on education, occupation and migration outcomes. To better inform policy, it would be useful to identify the dominant mechanism driving these effects. One may want to interpret these results as evidence that changes in education levels cause a shift in the distribution of occupations and regional mobility by allowing individuals to accumulate more human capital. To be able to claim that these effects are evidence of causality in the relationship between education and choice of occupation, one must assume that the FPE reforms affected occupations primarily via their improvement of average education levels. However, one possible other important channel than the human capital mechanism that might explain the effects of the reforms on occupation and regional mobility is the wealth effect from the removal of fees on parents who were already sending their children to school before the reforms.

Estimates of the percentage of household expenditure allocated to education of children in Sub-Saharan Africa prior to the reforms range from 6.5% to about 30%[18]. Therefore, households who were already planning to send their children to school prior to the reforms must also have benefited from a sizable wealth increase as a result of the reforms. It is plausible that some households reallocated some of these funds towards other investments in their children's futures, which could have influenced the type of occupations they would then grow up to hold. Examples of such investments might include health-care and capital

investments in non-farm household businesses. Health costs can influence the level of skills that someone can acquire by improving their propensity to attend school[24] or increasing their productivity[25]. Capital constraints are often cited as a barrier to entrepreneurship in developing countries[26, 27]. It is therefore conceivable that an increase in household wealth due to the FPE reforms could have affected the choice of occupations of the children exposed to these policies.

A number of my results support the assumption that the impact of the re- forms operated through education rather than an income effect. First, finding higher effects for women on occupational outcomes as well as education levels challenges the plausibility of the wealth effect channel. If it is through other household monetary investments that the reforms affected the distribution of occupations, it is difficult to think of an explanation for more of these investments to have been allocated to women, when evidence on educational investments suggests that boys' education is given priority over girls'[18]. Second, the significantly positive effects of the reforms on outcomes such as secondary schooling favor the hypothesis that human capital is the dominant link between the reforms and the observed changes in occupations. Secondary school fees were not abolished at the same time as primary school fees in these countries, and so higher enrolment in secondary schools suggests that the reforms raised the valuation of education. This could be attributed to the fact that the reforms led more households to enjoy the returns to education.

5.2. Testing for the Wealth Effect Channel

Nonetheless, this paper cannot completely rule out the possibility that the impact of the reforms was driven by an income effect. The ideal test for this alternative channel would involve checking whether the change in occupational outcomes occurred only for children coming from households who would have sent their children to school even without the FPE reforms. These households saved a significant expense thanks to the FPE policies and could thus have benefited from a wealth 'surplus'. Since this paper does not have data on such a counter-factual, this paper proposes the following two tests instead.

The first test involves restricting the sample to families with children in both the 'pre-reform' and the 'post-reform' groups. In other words, the sample is restricted to households of which some children were able to benefit from the FPE reform and some children could not, because they were older than the primary school age at the time of the reform's implementation. This allows for the use of the education of the older siblings as an indicator for families who would have sent their children to primary school even without the FPE reforms. This paper therefore uses the older siblings as a counter-factual for the parents' investment in the 'post-reform' children's education if the reforms had not taken place. The test then requires estimating specification (4) below:

$$Y_{idt} = \alpha_t + \delta_d + \eta Treatment_d \times Post_{it} + \beta_1 Treatment_d \times Post_{it} \times S_i + \gamma x_i + \epsilon_{idt} \quad (4)$$

where S_i is 1 if the older sibling(s) of the child(ren) eligible for the reform completed primary school, and 0 otherwise. The S_i main effect is not visible in the equation because it is included in the vector of individual-level characteristics x_i. (4) is run both on primary school completion and occupation outcomes, to test whether having a sibling who went to primary school augments the effect of the FPE reforms on the probability that the 'post-reform' child is working in a non-agricultural sector or is reported as economically active.

The second test this paper proposes is to use data on education of fathers to proxy for the likelihood that children would have been sent to school even without the FPE reforms. It is rather plausible that parents who went to primary school themselves are more likely to send their children to school. Here, this paper runs specification (5):

$$Y_{idt} = \alpha_t + \delta_d + \eta Treatment_d \times Post_{it} + \beta_2 Treatment_d \times Post_{it} \times F_i + \gamma x_i + \epsilon_{idt} \quad (5)$$

where F_i is 1 if the individual's father completed primary school. (The F_i main effect is not visible in the equation because it is included in the vector of individual-level characteristics x_i.)

The 'siblings test' is run on the Zambia IPUMS dataset, since the younger age of the individuals in the 'post-reform' group relative to other countries implies that a large proportion of individuals in the sample are still living with at least some of their older siblings. The 'father test' is run on Uganda (LSMS 2009), since this is the only dataset which includes information on the education levels of adult individuals' parents. Thus, in the Uganda dataset the test does not restrict the sample to individuals who are still living with their families of origin.

The results of both placebo tests are presented in Table 10. Estimating specification (4) on the restricted Zambia sample yields negative estimates for the β_1 coefficient in (4), whether Y_{it} is an education outcome, the probability of working in a non-agricultural sector, or the probability of being classified as 'economically active'. Thus, the data does not show any evidence that 'post-reform' children from families that would have likely sent them to school even without the FPE reforms (i.e. families with educated older siblings) are any more likely to choose a non-agricultural occupation than 'post-reform' children whose older siblings did not attend primary school. In the Uganda sample, specification (5) also yields negative coefficient estimates for β_2 when the outcome variable is an education, occupation or migration variable.

In view of these results, it seems reasonable to conclude that a human capital channel is a more plausible explanation for the shift in occupations caused by the reforms than a wealth effect. Nonetheless, more tests and more research should be undertaken before one can

conclude that these policies significantly increased the cognitive skills of the children who benefited from them.

First, due to data limitations, in this paper only performs tests of the likelihood that a wealth effect could be driving the effects of the reforms in two out of seven datasets. This paper plans to explore other sources of data in order to further test the plausibility of the wealth effect channel in each of the seven countries in this sample.

Second, theoretically, education can affect labor market outcomes through other channels than one's level of skills. These include signaling of ability[28] or the non-cognitive effects of schooling[29]. It is possible that one of these channels was at play in this setting, especially given the concerns cited in policy reports[30,15] that the FPE reforms improved access to education at the cost of education quality. One other possible mechanism that could drive the results of the reforms on individuals' choice of occupations is aspirations. For instance, going to primary school is likely to be instrumental in at least making children aware of other labor market opportunities than the occupations held by their parents.

6. CONCLUSIONS

In the seven Sub-Saharan countries examined in this paper, the abolition of primary school fees led to higher primary and secondary school completion rates, and an increase in individuals' average years of schooling. The causal effects of the reforms also include a shift in occupations from the agricultural to the non-agricultural sector. A possible mechanism for this shift in occupations is the rise in internal migration resulting from these reforms in most of the countries in this sample. The use of placebo tests did not produce any evidence that the effects of the reforms on occupations are driven by an income effect channel, which is the most plausible alternative to the initial hypothesis that the abolition of school fees leads to occupational shifts via higher educational attainment.

Thus, the reduced form estimates of the impact of the FPE reforms are largely consistent with the hypothesis that a positive shock to education levels can induce a shift of labor from the agricultural to the non-agricultural sector `a la Lewis[31]. The results from this analysis thereby shed light on the important role that human capital may play in the structural transformation of an economy. They also highlight important avenues for future research. For instance, it would be a valuable contribution to assess the effect of the FPE reforms on average cognitive ability, to isolate the cognitive skills channel from other possible effects of schooling on occupational choice such as aspirations. One way to achieve this might be to compare the effects of reforms like FPE which aim to give access to schooling to more children with those of educational policies that seek to improve education quality and increase cognitive abilities on the intensive margin. Such a study could involve the evaluation of the teacher recruitment and teacher training programs that were launched years after the introduction of FPE[30] in some of the countries in this sample.

This paper's findings on internal migration also leave room for further investigation. As mentioned in the previous sections, the main migration outcome variable analyzed in this study does not include any information about the timing of migration, its purpose, or whether migrants' region of provenance was predominantly rural or urban. This paper plans to explore these channels in a subsequent study, by using GPS data, collected in the LSMS surveys, to build indicators of rural-to-urban migration. This would allow me to investigate the mechanisms driving the effects of the FPE reforms on regional migration and could lend further insight into the relationship between education and regional mobility in the Sub-Saharan African context. Examining the effects of the FPE policies on rural-to-urban migration would also provide a more precise test of the hypothesis that regional mobility is an important channel for occupational change, with individuals using internal migration to shift away from the agricultural sector.

The occupational shift observed in this paper also deserves closer attention. It would be useful to identify the different types of employers that the newly educated individuals went on to work for. Stylized facts on the topic include the trend that a significant proportion of the highest educated individuals work for the public sector[32] in several Sub-Saharan countries, for instance. If verified, this would challenge the hypothesis that human capital accumulation is a driver of structural change in Sub-Saharan African economies.

Finally, it must be highlighted that this study's main limitation is that the effects described in this paper are all relatively short-term effects. Indeed, the labor market outcomes of individuals who benefited from the reforms are all observed between one and seven years after the individuals completed their primary education. They are therefore still very young at the time of the data collection and so their occupational choices might only be temporary. For instance, they might have migrated to an area to find work in the non-agricultural sector and send remittances back to their parents for a few years, all the while planning to come back to the household farm when they get older. Assessing whether the FPE reforms led to a permanent occupational shift is left for future research.

Table 1 Dates of the FPE reforms and data availability

Country	FPE	Date of reform(s)	IPUMS	Year of data collection	LSMS	Year of data collection
Angola	No		No		No	
Benin	Yes	1990	No		No	
Botswana	Yes	1987	No		No	
Burkina Faso	No		No		Yes	2014
Burundi	Yes	2005	No		No	
Cameroon	Yes	2000	Yes	2005	No	
Cape Verde	No		No		No	
Central African Republic	No		No		No	
Chad	No		No		No	
Comoros	No		No		No	
Congo(Brazzaville)	Yes	1995	No		No	
Congo(Democratic Republic)	No		No		No	
Cote d'Ivoire	No		No		Yes	1988
Djibouti	No		No		No	
Equatorial Guinea	No		No		No	
Eritrea	No		No		No	
Ethiopia	**Yes**	**1994**	**Yes**	**2007**	**Yes**	**2006**
Gabon	No		No		No	
The Gambia	Yes	1998	No		No	
Ghana	Yes	2005	Yes	2010	Yes	2009
Guinea	No		Yes	1996	No	
Guinea-Bissau	Yes	2002	No		No	
Kenya	**Yes**	**1974,1979,2003**	**Yes**	**1989,1999,2009**	**No**	
Lesotho	Yes	2000	No		No	
Liberia	Yes	2007	Yes	2008	No	
Madagascar	No		No		No	
Malawi	**Yes**	**1994**	**Yes**	**2008**	**Yes**	**2013**
Mali	No		Yes	2009	No	
Mauritania	No		No		No	
Mauritius	No		No		No	
Mozambique	Yes	2004	Yes	2007	No	
Namibia	Yes	2013	No		No	
Niger	No		No		Yes	2011
Nigeria	**Yes**	**1976,1999**	**Yes**	**2010**	**Yes**	**2010,2012**
Reunion	No		no		No	
Rwanda	Yes	2003	Yes	2002	No	
Sao Tome and Principe	No		No		No	
Senegal	No		Yes	2002	No	
Seychelles	Yes	1981	No		No	
Sierra Leone	Yes	2004	Yes	2004	No	
Somalia	No		No		No	
South Africa	No		Yes	2011	Yes	2011
Sudan	Yes	2011	Yes	2008	No	
Swaziland	Yes	2005	No		No	

Country	FPE	Date of reform(s)	IPUMS	Year of data collection	LSMS	Year of data collection
Tanzania	**Yes**	**2001**	**Yes**	**2002**	**Yes**	**2012**
Togo	Yes	2008	No		No	
Uganda	**Yes**	**1997**	**Yes**	**2002**	**Yes**	**2009,2011**
Western Sahara	No		No		No	
Zambia	**Yes**	**2002**	**Yes**	**2010**	**No**	
Zimbabwe	yes	1980	no		No	

Table 2 Summary Statistics

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia	Nigeria
Completed primary school	0.211	0.415	0.364	0.728	0.453	0.327	0.746
Completed secondary school	0.060		0.078				0.315
Years of schooling	3.936	5.196	4.079	6.916	6.390	4.967	7.972
Economically active	0.708	0.526	0.679				
Non-agricultural occupation	0.333	0.334	0.402	0.344	0.261	0.363	0.348
Wage earner				0.312	0.139		
Self-employed				0.107	0.135	0.198	0.372
Migrated from region of birth	0.053	0.172	0.126	0.182	0.112	0.099	
economic migrant				0.031	0.114		
Data source	IPUMS	IPUMS	IPUMS	LSMS	LSMS	LSMS	LSMS
Year of data collection	2008	2010	1989	2012-2013	2009-2010	2006	2010-2012

Table 3 Primary School Completion Rates Pre- and Post-FPE

Country	Year of FPR	Treatment status	Pre-FPE	Post-FPE
Kenya	1974	Treatment	0.37	0.66
Kenya	1974	Control	0.62	0.82
Malawi	1994	Treatment	0.27	0.31
Malawi	1994	Control	0.47	0.51
Ethiopia	1994	Treatment	0.17	0.34
Ethiopia	1994	Control	0.30	0.49
Uganda	1997	Treatment	0.41	0.46
Uganda	1997	Control	0.57	0.67
Nigeria	1999	Treatment	0.43	0.66
Nigeria	1999	Control	0.93	0.95
Tanzania	2001	Treatment	0.64	0.7
Tanzania	2001	Control	0.83	0.83
Zambia	2002	Treatment	0.58	0.42
Zambia	2002	Control	0.84	0.71

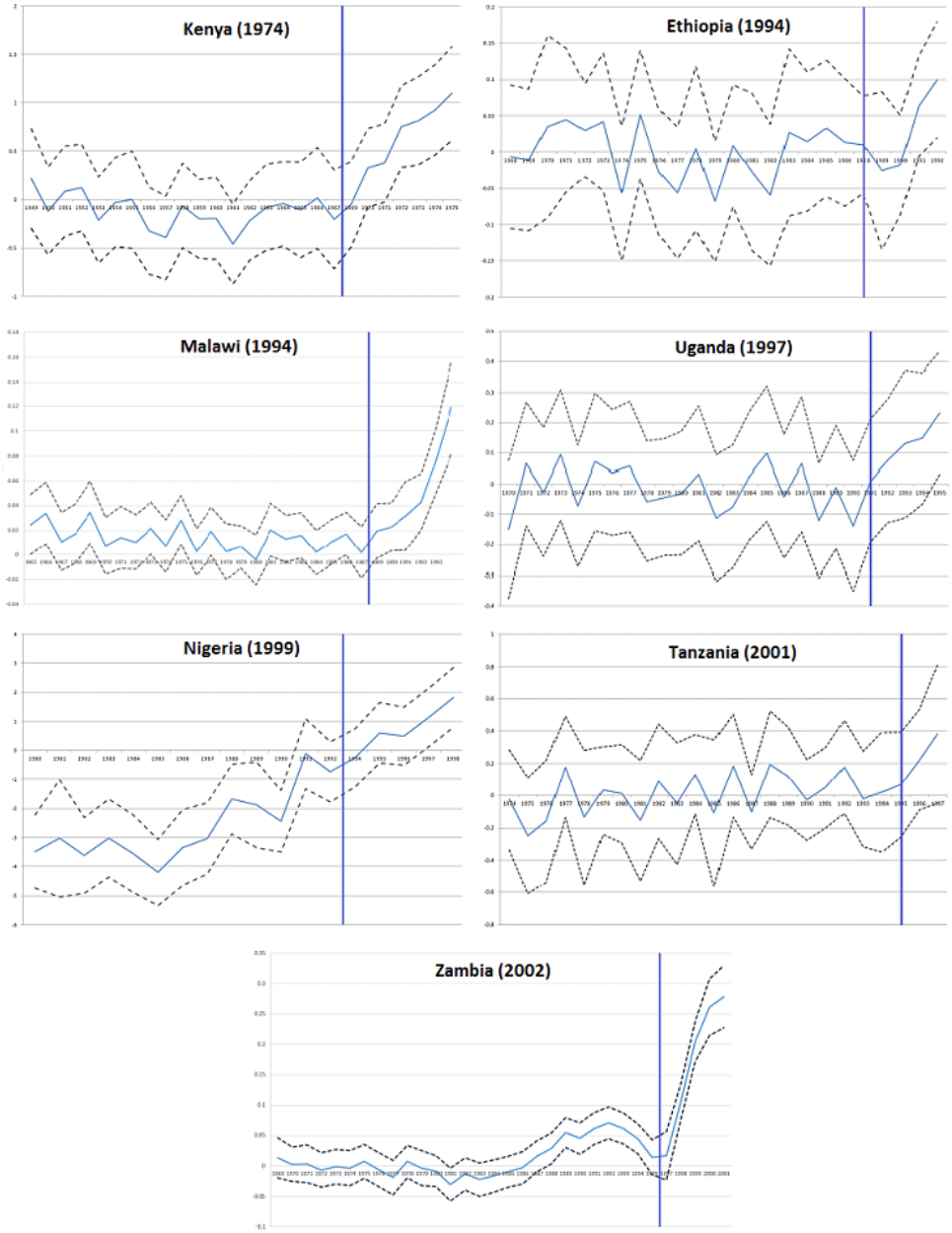

Figure 1 Treatment x Year of Birth Coefficients Estimates

Table 4 Average Treatment Effects: Education Outcomes

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia	Nigeria
Completed primary school							
Treatment×Post	0.037***	0.042***	0.078***	0.061**	0.089**	0.046**	0.174***
Sample Mean	0.332	0.575	0.605	0.688	0.390	0.256	0.749
Completed secondary school							
Treatment×Post	0.059***		0.066***				0.215***
Sample Mean	0.080		0.135				0.337
Years of schooling							
Treatment×Post	0.563***	1.567***	0.458***	0.581***	0.856***	1.005***	2.316***
Sample Mean	5.691	6.821	5.920	6.610	5.850	4.507	8.010
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia	Nigeria
District of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	No
District FE	No	No	No	No	No	No	Yes
Observations	313150	201039	236057	4901	4947	7203	9018

Table 5 Average Treatment Effects: Occupation Outcomes

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia	Nigeria
Non-agricultural occupation							
Treatment×Post	0.059***	0.088***	0.022***	0.059**	0.083***	-0.023	0.075***
Sample Mean	0.357	0.351	0.453	0.344	0.246	0.201	0.390
Observations	130330	98322	124587	3774	4211	3283	4929
Economically active							
Treatment×Post	0.075***	0.050***	0.031***				
Sample Mean	0.543	0.507	0.758				
Observations	313150	201039	232706				
Wage worker							
Treatment×Post				0.136***	0.058**		
Sample Mean				0.300	0.127		
Observations				3774	4211		
Self-employed							
Treatment×Post				0.035*	0.009	0.009	-0.017
Sample Mean				0.116	0.145	0.097	0.417
Observations				3774	4211	3283	4916
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	No
District FE	Yes	Yes	Yes	Yes	yes	No	Yes

Table 6 Average Treatment Effects: Migration Outcomes

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia
Migrated from region of birth						
Treatment×Post	0.026***	0.040***	0.041***	0.060**	0.022	0.002
Sample Mean	0.065	0.214	0.194	0.182	0.106	0.089
Economic migrant						
Treatment×Post				0.014	0.021	
Sample Mean				0.034	0.115	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
District of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	No
Observations	313150	201039	236057	5001	5006	7203

Table 7 Heterogeneous Treatment Effects: Education Outcomes

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia	Nigeria
				Completed primary school			
Treatment×Post×Female	0.082***	0.156***	0.075***	0.073***	0.128***	0.109***	-0.036
Treatment×Post	-0.006	-0.036***	0.038**	0.025	0.026	-0.003	0.191***
Female	-0.114***	-0.109***	-0.177***	0.022	-0.072***	-0.074***	-0.077**
Female Mean	0.287	0.539	0.519	0.708	0.372	0.221	0.688

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia	Nigeria
Completed secondary school							
Treatment×Post×Female	0.054***		0.079***				0.030
Treatment×Post	0.031***		0.024*				0.202***
Female	-0.064***		-0.092***				-0.082***
Female Mean	0.055		0.091				0.282
Years of schooling							
Treatment×Post×Female	0.903***	1.273***	0.767***	0.521***	0.952***	1.468***	-0.093
Treatment×Post	0.096	0.931***	0.044	0.320	0.387	0.344	2.359***
Female	-1.227***	-1.099***	-1.839***	-0.116	-0.730***	-1.022***	-1.174***
Female Mean	5.223	6.431	5.022	6.595	5.578	3.726	7.280
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	No
District FE	No	No	No	No	No	No	Yes
Observations	313150	201039	236057	4901	4947	7203	9018

Table 8 Heterogeneous Treatment Effects: Occupation Outcomes

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia	Nigeria
Non-agricultural occupation							
Treatment×Post×Female	0.071***	0.033***	0.088***	0.085***	0.152***	0.066***	0.186***
Treatment×Post	0.020***	0.072***	-0.026**	0.017	0.012	-0.044**	0.002
Female	-0.137***	-0.030***	-0.159***	-0.080***	-0.115***	-0.050**	0.120**
Female Mean	0.257	0.295	0.328	0.315	0.210	0.356	0.478
Observations	130330	98322	124587	3774	4211	3283	4929
Economically active							
Treatment×Post×Female	0.095***	0.241***	0.071***				
Treatment×Post	0.026***	-0.071***	-0.007				
Female	-0.032***	-0.246***	-0.112***				
Female Mean	0.535	0.416	0.710				
Observations	313150	201039	232706				
Wage worker							
Treatment×Post×Female				0.186***	0.122***		
Treatment×Post				0.044	0.001		
Female				-0.175***	-0.139***		
Female Mean				0.233	0.083		
Observations				3774	4211		
Self-employed							
Treatment×Post×Female				-0.019	0.036	-0.004	0.120***
Treatment×Post				0.044**	-0.008	0.010	-0.064
Female				-0.003	-0.025	0.023	0.126***
Female Mean				0.118	0.141	0.264	0.507
Observations				3774	4211	3283	4929
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	No
District FE	Yes	Yes	Yes	Yes	Yes	No	Yes

Table 9 Heterogeneous Treatment Effects: Migration Outcomes

	Malawi	Zambia	Kenya	Tanzania	Uganda	Ethiopia
			Migrated from region of birth			
Treatment×Post×Female	0.004**	0.007***	0.017***	-0.015	-0.001	-0.015
Treatment×Post	0.024***	0.036***	0.032	0.013	0.023	0.008
Female	-0.006***	0.002	-0.017***	0.018	0.014	0.003
Female Mean	0.060	0.216	0.161	0.198	0.114	0.105
			Economic migrant			
Treatment×Post×Female				0.035***	0.075***	
Treatment×Post				-0.004	-0.017	
Female				-0.023***	-0.082***	
Female Mean				0.028	0.092	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
District of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	No	Yes	Yes
Observations	313150	201039	236057	5001	5006	7203

Table 10 Testing for the Wealth Effect Channel: Results

	Zambia estimate	p-value	obs.	Uganda estimate	p-value	obs.
Completed primary school						
Treatment×Post×FatherPrim				-0.066	0.42	3373
Treatment×Post×SiblingPrim	-0.013	0.414	13269			
Years of schooling						
Treatment×Post×FatherPrim				-0.502	0.061	3373
Treatment×Post×SiblingPrim	-0.278	0.035	13269			
Non-agricultural occupation						
Treatment×Post×FatherPrim				-0.117	0.001	2887
Treatment×Post×SiblingPrim	-0.005	0.699	4611			
Economically active						
Treatment×Post×SiblingPrim	-0.084	0.001	13269			
Wage earner						
Treatment×Post×FatherPrim				-0.1	0.001	2887
Migrated from region of birth						
Treatment×Post×FatherPrim				0.001	0.931	3388

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